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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

THERMAL-HYDRAULIC PHENOMENA SUBCOMMITTEE MEETING

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THURSDAY

MARCH 20, 2003

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ROCKVILLE, MARYLAND

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The Subcommittee was convened in closed session at the Nuclear Regulatory Commission, Two White Flint North, Room T2B3, 11545 Rockville Pike, at 1:06 p.m., Dr. Graham Wallis, Chairman, presiding.

COMMITTEE MEMBERS PRESENT:

DR. GRAHAM WALLIS, Chairman

DR. SANJOY BANERJEE, ACRS Consultant

DR. THOMAS S. KRESS, Member

DR. DANA A. POWERS, Member

DR. VICTOR H. RANSON, Member

DR. JOHN SIEBER, Member

ACRS STAFF PRESENT:

MICHAEL R. SNODDERLY, ACRS Staff Engineer

I N D E X

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

(1:06 p.m.)

CHAIRMAN WALLIS: Okay. We will come back into session. We will now have an open session, and we will hear from Westinghouse again, and try to wind up some things.

MR. CORLETTI: The next discussion is that I think we are going to first go over some of the comparison plots from NOTRUMP that we discussed yesterday. We put at least one comparison plot, and we have some additional plots that we can show you. I think we have handed out the packet. Do you all have the handouts?

CHAIRMAN WALLIS: I don't think any of us the handouts.

MEMBER SIEBER: No, we don't.

MR. CORLETTI: Then I will turn it over to Andy Gagnon to go through some of these comparison plots.

MR. GAGNON: Okay. One of the requests that we had yesterday was to provide a comparison plot of the core collapsed level and some void fractions. We have a comparison of the core collapsed level for the DVI line break.

As you can see the RELAP is about a foot lower than the NOTRUMP at its minimum, and trending in the same direction. NOTRUMPS and IRWST injection time for this simulation is approximately 2,076 seconds, and so we are not starting to really start injecting IRWST here.

One of the other plots presented yesterday was the core void fraction. One of the things to note is that the NOTRUMP core contains 14 axial segments, and NOTRUMP was also validated against the G2 14 foot course facility, as well as Achilles.

G2 was run in a pressure range from 700 down to near atmospheric conditions. One of the things that you will note here is that I think that we are lower than the predicted value at mid-plane for RELAP-5 for Walt's presentation yesterday, and we are at near the similar values for near the top of the core.

One of the things to note is that we have a 14 foot axial core power shape that we are utilizing in this simulation, which the top core nod is at substantially lower power than the previous core nods. So the peak power NOTRUMP is core node 13.

DR. BANERJEE: Core node 13 is the highest?

MR. GAGNON: Yes. It is also the peak

power.

DR. BANERJEE: Is it also the highest core average void fraction?

MR. GAGNON: Yes. This is the core average void fraction for this case. You can see where the core average is roughly between 50 and 60 percent.

MEMBER KRESS: What causes the periodicity? It looks like it is fixed --

MR. WRIGHT: We are getting pressure oscillations as to when we start slugging flow out of ADS-4. We have periodic discharges out of four, and at that point --

MEMBER KRESS: And is that calculated in the NOTRUMP?

MR. GAGNON: Yes.

DR. BANERJEE: This is increased resistance in the ASD-4 line?

MR. GAGNON: Yes.

CHAIRMAN WALLIS: This is the build up of liquid in the riser part of the line; is that what it is?

MR. GAGNON: Yes, we get liquid into contact and we suck it out, and we pressurize.

MEMBER SIEBER: And push it.

MR. GAGNON: And then it turns itself back off.

CHAIRMAN WALLIS: And when it gets to the valve, it increases the pressure and drops the flow a bit?

MR. GAGNON: Yes.

DR. BANERJEE: And what is keeping the level up? Is it the CMT flows then for the first slide that you showed?

MR. GAGNON: The CMT for NOTRUMP, if you go back to the slide presentation that we had yesterday, Slide 65, which is some of the slides that we skipped over, the CMT continues to inject until 2006, the impact CMT.

DR. BANERJEE: So that is what is keeping up the level then?

MR. GAGNON: Yes, that is what is keeping the level up. There is a slight injection gap for the 14.7 psi containment of approximately 70 seconds for AP-1000.

DR. BANERJEE: Slide 65 you said?

MR. GAGNON: Slide 65, yes.

DR. BANERJEE: Slide 65 is the integrated flow, right?

MR. GAGNON: This the sequence of events.

CHAIRMAN WALLIS: It is the timing.

MR. GAGNON: It is the first presentation from yesterday.

CHAIRMAN WALLIS: That is the wrong one. The covers look the same on these presentations?

MR. CORLETTI: One of them is proprietary.

CHAIRMAN WALLIS: One is proprietary and one isn't, and you have to look for that.

DR. BANERJEE: So it is CMT around 2005, right?

MR. GAGNON: Yes. And the IRWST.

CHAIRMAN WALLIS: So what is keeping it up after 2000?

MR. GAGNON: Well, it is only 70 seconds. The void fraction is increasing after 2000 and --

CHAIRMAN WALLIS: Well, the liquid level is going up after 2000. I was looking at the collapsed liquid level.

DR. BANERJEE: Is it 70 seconds later that the IRWST comes on, right?

MR. GAGNON: Yes.

CHAIRMAN WALLIS: No, it is 700 seconds.

MR. GAGNON: No, 70.

DR. BANERJEE: 76 seconds.

MR. GAGNON: 76 seconds.

CHAIRMAN WALLIS: Where did you have that coming in again?

MR. GAGNON: 2076.

CHAIRMAN WALLIS: Oh, okay.

MR. GAGNON: Now, if we look at a more realistic containment back pressure, IRWST injection comes on at roughly 1,400 seconds, which is considerably earlier. So we have an injection from both paths at the same time, which we believe to be the more realistic case for DEDVI.

This just happens to represent for the DEDVI case.

CHAIRMAN WALLIS: And going back to the presentation yesterday, when you did it with COBRA/TRAC, it had a very different picture of integrated mass flow, and it must have been at COBRA/TRAC that the IRWST came on much earlier.

MR. GAGNON: Yes, very shortly after, and approximately -- what was it, Bob, about a hundred seconds after ADS-4?

MR. KEMPER: 110.

MR. GAGNON: 110 seconds after ADS-4. Whereas, NOTRUMP would delay that up to about 1,400 seconds. I believe that is the case.

MEMBER SIEBER: It really doesn't make a

difference in the outcome though what the timing is, right?

MR. GAGNON: No. It has a slight impact on core void fractions.

CHAIRMAN WALLIS: It just depressurizes quite dramatically.

MEMBER SIEBER: In the grand scheme of things.

MR. GAGNON: In the grand scheme of things, it has essentially none.

DR. BANERJEE: Because you have CMT.

MEMBER SIEBER: Right.

MR. GAGNON: This is the DVI line pressure, compared to our IRWST injection flow, and as you can see when we slip down to about 25, I believe, psi in order to get our IRWST injection flow. You can see the kind of behavior that we are getting as we plug the ADS-4, and vent it, and with the pressurization we get a blip, and it comes in and plugs it again.

And once we can get it clear sufficiently, we start getting more stable IRWST flow. This is the 10 inch break, and we don't have the RELAP curve on here, but --

CHAIRMAN WALLIS: This is the purple

curve?

MR. GAGNON: That was the purple curve from yesterday, and they were predicting approximately a 30 percent core level as their minimum. And you can see with NOTRUMP that the collapse level is down around -- oh, half a foot. This is in the active shield area, and so it is very highly voided, and that is why we did the adiabatic to heat up the calculation.

And while it didn't predict the traditional mixture level uncovery, we said these void fractions are just to high to say that it is not going to be a brief blowdown. I mean, this is a high pressure type uncovery.

DR. BANERJEE: And five foot is the minimum roughly?

MR. CORLETTI: In the long term, yes.

CHAIRMAN WALLIS: And so 30 percent is something like 4 foot? It is about the same when you get out further. The RELAP calculation would be about the same or the minimum.

MR. GAGNON: The void fraction for that case -- this is core average void fraction. And you can see the initial blowdown was very high, and then it settled pretty quickly, and we --

CHAIRMAN WALLIS: And all these oscillations are -- do you think they are real or numerical?

MR. GAGNON: I think that in a combination that some are real and I would say a good chunk of them of fairly numerical.

CHAIRMAN WALLIS: Is this for RELAP, and does this play the same kind of fluctuations? The purpose code?

DR. BANERJEE: They seem to.

MR. SNODDERLY: I'm sorry, Walt, but can you come to the microphone?

CHAIRMAN WALLIS: Maybe you could show us the purple. While we are waiting for Walt, is there something else?

MR. GAGNON: Not really. This is what I was talking about when you were looking at that pressure, the pressure oscillation. You can see that this is one of the ADS-4 paths, and here is the other ADS-4 path and how it pressurizes and you start getting -- I believe this is -- and I can't remember which one, but I believe that this is probably the single failure path. I don't remember off the top of my head without the nodding diagram, and which one is 50 percent and which one is the 100 percent path.

DR. BANERJEE: This is with the three ADS-4?

MR. GAGNON: This is with 3 ADS-4 paths. And you have a single failure.

CHAIRMAN WALLIS: We are going back to look at the purple curve

DR. JENSEN: The purple curve.

CHAIRMAN WALLIS: Upside down.

DR. JENSEN: It looks better that way.

CHAIRMAN WALLIS: So the purple curve is qualitatively -- it does show that initial big dip, but it is nowhere near as big?

MR. GAGNON: Not as deep, no.

CHAIRMAN WALLIS: And then it goes way up to high value, and then after a thousand it has a second dip. Qualitatively it is different.

DR. BANERJEE: And the second dip in NOTRUMP is about 1,500, right?

MR. GAGNON: I can't see that too well. Wrong one, I'm sorry.

DR. BANERJEE: One over the other.

CHAIRMAN WALLIS: And so the qualities, there is a significant difference there.

DR. BANERJEE: Not that much.

CHAIRMAN WALLIS: RELAP goes up much more and then has a second evacuation to about half, and then it goes back up to 60. Whereas --

MR. GAGNON: This is actually the same time period, this evacuation, and is roughly the same time period as --

DR. BANERJEE: And you are filling this up to 14 feet, which is a hundred percent, right?

CHAIRMAN WALLIS: And maybe it is also a different scale?

MR. GAGNON: Yes, the time scales are different.

CHAIRMAN WALLIS: So I guess what we have is a kind of general uneasiness about three codes predicting things that look to be quite different. Now, maybe this can be resolved in some way that is pleasing to everybody.

DR. BANERJEE: By putting the relap points on the other one.

MR. GAGNON: On the 10 inch?

CHAIRMAN WALLIS: No, I mean in the future. This just raises a question in my mind is what do we do with something when you see these different things, and maybe the staff and Westinghouse can put together an answer which makes --

DR. JENSEN: Well, the staff was

(inaudible) could understand some of the phenomena that were going on, and give you insights to ask the proper questions to Westinghouse on what was going on in the process in very small break LOCAs.

And what our goal is to license the plant based on Westinghouse's calculations, and based on their comparison of their codes to experimental data. And they have done some comparisons, of course, to the AP-600, and potentially the 14 foot level swell test.

But then we think that they need some more data as to the core containment. So we are hoping that Westinghouse will do a good job and benchmark the codes against the (inaudible) data, and that is how we perceive --

CHAIRMAN WALLIS: You license based on their predictions. but there has to be a probability of their predictions, which is enhanced tremendously if you can do independent calculations giving the same prediction.

MEMBER KRESS: Or to explain why they didn't.

DR. JENSEN: Yes, that would be very helpful. It could be that the difference is the way that we perceive the input. We might have put different numbers.

CHAIRMAN WALLIS: I guess I might say, and I don't know if my colleagues agree, but I think it is sort of essential that we don't license entirely based on curves shown by Westinghouse, but that you have some independent assessment of what the curve should be. That is why you run your code, or their code, or both codes, in order to get this independent assessment of what the curve should be.

DR. JENSEN: But I ran that code.

CHAIRMAN WALLIS: Or run your code, or something, but we just don't believe a figure entirely coming from Westinghouse. If all the evidence comes from Westinghouse, there is no independent check of it.

DR. JENSEN: If our code showed core (inaudible), we would have looked at or tried to find the cause a lot more carefully, and our codes showed that everything was fine, except for some differences, but if we had Westinghouse's code, we could have put them side by side and looked for perhaps differences in the input.

CHAIRMAN WALLIS: So you have got two different plays. You have got Romeo and Juliet, and Hamlet, and the hero dies in both, or doesn't die in both, and the same conclusion, and therefore it is okay, There ought to be some comparison of what goes along the way as well, and what happens along the way.

DR. JENSEN: I think we have some similarity.

CHAIRMAN WALLIS: Well, is the fundamental reason that you are getting these differences that we have here a passive system design? If you had an injection which was forced by a pump, or an accumulator, or something, it would be much crisper and deterministic wouldn't they than this passive design?

DR. JENSEN: You are correct. We are dealing with very small pressure drops.

CHAIRMAN WALLIS: That's right. So it can go this way or that way, and in a way or in the traditional active safety system design. Isn't that why we are getting these differences at the times that things happen, but the overall conclusion is the same.

DR. JENSEN: I suspect that could be the cause of some of the differences, but some models are different than what you would expect the models to predict.

CHAIRMAN WALLIS: Well, what is the reality is the question isn't it?

DR. JENSEN: That is the question. I hope that the OSU test will give us something closer to reality. We don't have plans to benchmark RELAP against the OSU test. I don't believe the Office of Research plans to do that.

DR. JENSEN: Well, Research might run the TRAC-M code though in the way that you have here, or in some other way, to give us an independent assessment.

DR. JENSEN: I would hope that they would. That would be helpful.

CHAIRMAN WALLIS: Okay.

MR. CUMMINS: If I could make a comment. We have subcontractors in Italy that run RELAP and does some of the same analysis that we do, and we find that there is general agreement between the results of our codes and the results of their RELAP analysis, though I though where you evolve as you evaluate this kind of question is what does that mean.

What is general, and it is sort of like how do you know that scaling within .5 and 2 is okay. And I am not sure that there is a clear answer to that, but I think that you are seeing in this particular instance an aberration between the codes that is more than typical, and I would expect that the staff, if they saw a continual aberration, would have a source of our RAIs that they would have asked us about the cause of that.

MR. CORLETTI: I am not sure that we have concluded anything here, and I guess --

MR. CUMMINS: And we also don't have the staff's results. So maybe we could ask the staff to -- they have our results, and we don't have the staff analysis results, and I am not sure that we are supposed to have them. I don't think we are. I think that it is for their independent assessment.

So maybe we could ask that in the future that they make a plot of our results and their results together.

CHAIRMAN WALLIS: Maybe it is up to the staff in their safety evaluation report to explain to us and to the public how they reached their conclusions in the light of these differences and in the details predicted in the codes, which if you picked the right place looked to be fairly large, though the outcome is the same.

And maybe they would explain why this happens, and why it is okay if it is okay, and maybe that is where it should be. But then you have two codes predicting different results, too. So you have to fill that obligation of explaining.

MR. CUMMINS: Yes, I think we have the obligation to explain the differences between our codes.

CHAIRMAN WALLIS: Right.

DR. BAJOREK: Dr. Wallis --

CHAIRMAN WALLIS: Yes.

DR. BAJOREK: Just to kind of go on, I think this is one of the reasons that the staff feels that a well-scaled experimental test is a necessity here for the AP-1000.

We do see differences between these 3 codes, and if we were to run TRAC-M at this point, we would probably see differences in four codes. Maybe we should just average them all together. But I am not sure that would help.

But that is why we think there is enough uncertainty here, and the big difference in these passive plans is the uncertainty is not due to the functioning of the equipment, whether you lose a diesel generator, and we can make an assumption on an ADS-4 valve failing.

But the big uncertainty comes in these thermal hydraulic codes, and how they handle these very, very small pressure drops and very small deltas on the calculations, and those get magnified from code to code.

CHAIRMAN WALLIS: And which we have said I think three times in our research report, in three separate places.

MR. CORLETTI: I guess if I could make a comment. If you just look back at AP-600, we ran properly scaled experiments, and we developed NOTRUMP and validated NOTRUMP to those tests.

The staff did the same for those tests, and we did the same with COBRA/TRAC for those tests. And I bet if you take similar transients for AP-600, I don't know if they are laid on top of each other, or that there was these similar differences, but in all, we always had a large margin to core cooling, and I don't know what the criteria is for code to code similarities.

We tend to validate each of these codes separately against test data that we hope is suitable, and I don't know. Walt, if you can comment on -- are these differences in our code comparisons for AP-1000 any more or less different than they were for 600?

DR. JENSEN: I didn't do the analysis for AP-600, but I think that for the most part the staff's calculations for AP-600 were best estimate calculations; and that we ran the same models that we ran the test data for, which is also best estimate.

Whereas, Westinghouse, I suspect, ran conservative Appendix K calculations for small break LOCA. So for that reason, I don't think that those results could be compared directly.

MR. CORLETTI: Okay. But that is not what you did here, and here you did it in more of an Appendix K approach.

DR. JENSEN: I tried to make the RELAP model on the first cut to Westinghouse's NOTRUMP calculations with the same failure, with 20 percent AMS, and atmospheric containment, and so, yes. The noding is different. The input, we derived that from (inaudible) independently, and if I am sure that we had the same numbers in our codes, then I would be looking for more of a code problem, but I think there may be differences, but likely there is not.

Maybe Westinghouse was using a conservative or more conservative levels in some of their tanks, and in IRWST perhaps, and we probably used more best estimate in a lot of the plant features. I don't know.

MEMBER RANSOM: Walt, can I ask you a question about the AP-1000 RELAP model?

DR. JENSEN: Sure.

MEMBER RANSOM: Was it just a direct conversion of the AP-600 model? And the reason that I ask that is that I know that from what of the AP-600 model, it was sort of an overzealous attempt to put in some 3-D effects and everything, and the model was very complex.

DR. JENSEN: You are correct, Dr. Ransom. The AP-1000 model is a derivative of the AP-600 model. There were several AP-600 models, but the first one was extremely complex, and it had a split core, I believe.

MEMBER RANSOM: Right.

DR. JENSEN: And that introduced artificial circulations within the core. A later model that was produced was simpler, and it only had a single flow path to the core to avoid the circulation problem.

But the downcomer is still split into eight radial segments. And the AP-1000 model is the same as or was based on the second AP-600 model.

MEMBER RANSOM: Did they turn off momentum flux in the downcomer then?

DR. JENSEN: I believe they did.

MEMBER RANSOM: That is one fix of the recirculation that I know that I have heard.

DR. JENSEN: So it doesn't circulate in the downcomer, and I know that I did check that.

MEMBER RANSOM: Thanks.

MR. CORLETTI: At this point in time, we would discuss maybe potential -- whether actions are coming out of this meeting, and one of the ones that I had on my list was this code to code comparison, and maybe looking for advice. What are your expectations from either Westinghouse or the staff for our next following meeting, or have we satisfied -- has this discussion satisfied it, or --

CHAIRMAN WALLIS: Well, I don't think it will go away. I think that you should address it in whatever the next meeting is. I am not sure whether it is going to be a thermal hydraulics meeting or whatever, or the other advanced reactor subcommittee.

MEMBER KRESS: The easy answer to the code to code comparison is to try to explain why they are different. The hard answer is -- and that I know that you can't get to, is that these codes should have uncertainties associated with the results, and if the uncertainties more or less overlap, then you have a pretty good feeling that you can deal with the differences, in terms of uncertainties.

But you are not going to be able to do

that, and so just explain the differences to some extent.

CHAIRMAN WALLIS: Don't get into problems where you tell us that you got the same steam flow, but you got more water flow, and yet the pressure drops less.

MEMBER SIEBER: Won't a lot of this be answered when the APEX data, the new data, becomes available, and is benchmarked?

MEMBER KRESS: Well, we always have the question that Sanjoy was going to bring up, is how good is that scale to the full AP-1000, and we have to look at that, I guess.

MEMBER SIEBER: Well, it would be better than what they have now.

DR. BANERJEE: Well, after that, in spite of the fact that at some point AP-600 was blessed, I hope that it was blessed with scaling, because there was a whole lot of other facilities available, including ROSA.

MEMBER SIEBER: Yes.

DR. BANERJEE: What we found wrong with the APEX for the AP-600 was that the CMT flows were wrongly scaled, and as you see, CMT flow is vital in some of these breaks between the time that the ADS-4 goes on and IRWST comes on.

MEMBER SIEBER: Both in timing and in flow rate.

DR. BANERJEE: Yes. It is very, very important, and what happened in APEX was the flow was about three times too high, and so it gave you the wrong resistances in those lines, and I am just speaking from memory here, and it ended up giving you less core uncovery, or much less of a level depression than you would find in a properly scaled case, such as ROSA.

So one has to look at that aspect fairly carefully and not just say that this is properly scaled, because it wasn't properly scaled. Now, I hear that we said it was okay for some reason, but I was on the other side. I wasn't on the NRC side at that time, and we identified that very clearly.

MEMBER RANSOM: Well, the way you would answer some of these problems it would seem is that if these codes really embody all of the physics of the access process, is to model the facility directly, and compare the results of the calculation to the facility, and then of course use the code to model the plant and then compare it to that.

As opposed to saying to directly compare the data to the said plant behavior.

DR. BANERJEE: Well, that --

MEMBER RANSOM: I mean, that helps explain some of those differences.

DR. BANERJEE: And in fact when we looked at the adequacy of the database for the AP-600, we came to the conclusion that the database was adequate for scaling, and in fact the RELAP-5 version that was used in the ISL presented it extensively for the PTS, and it came out fine.

I mean, there was nothing wrong with it. So that's why we said it was okay.

MEMBER RANSOM: Well, Oregon State does quite a bit of modeling, and Professor Reyes, could you comment on that?

DR. REYES: Well, we found that -- and again we were using the RELAP-5 version that was used for the AP-600 work to perform some calculations for the pressurized thermal shock study that we did, and simulating the Palisades Nuclear Plant.

And what we found was that we got very good comparisons with a wide range of transients.

MEMBER RANSOM: For your data.

DR. REYES: For our data, right. For comparisons to our data, and so it seemed to predict our data very well. Part of it that we noticed was that our modeler was right there at the site. So he could look at the volume, and he could see exactly what we had modeled in the experiment.

He had access to the initial conditions and the boundary conditions. So, there was a lot of information that was available to him because he was right there.

And that did seem to make a difference between our calculations and even some of the ISL calculations. Later on we went back and showed them where some things were different than what they had modeled, and so we did get good comparisons. On the ADS-4, or the CMT, I am a little vague on exactly what the scaling problem was on that one, but I know that we did model for a single phase, but I thought we were within the range.

MR. CORLETTI: Dr. Banerjee, our scaling studies that we performed for AP-1000 in WCAP-15613 was docketed last year, and I think that concluded that we were not poorly scaled for CMT flow, and for the tests or for the sum total of the available information for AP-1000.

And therefore our codes have been validated against SPES and OSU, or were validated against SPES and OSU, and that we had at least one of the scaled facilities that were properly scaled for CMT flow.

MR. CUMMINS: And we further checked the scaling against ROSA, which indicated that the ROSA tests were applicable to the AP-1000.

DR. BANERJEE: I am not saying that the AP-1000 is not properly scaled, because I don't know that. I am just saying that the AP-600, there was a potential issue.

Now, also the sort of thing about ROSA was that it was -- that ROSA was well-scaled for AP-600, and now I don't know if it was well-scaled for AP-1000, and if you had gone through that analysis and found that it was, that's fine.

The differences are, of course, related to your core power being higher and relative resistance to out flow from the core through the hot leg and so on, because those have been tighter now compared to, say, AP-600. So is the back pressure going to be more, and those are the two main issues in my mind.

MR. CORLETTI: Well, I don't know if you are saying the same thing that Steve has been saying, is that the one area was upper plenum and hot leg entrainment, was the one issue that was found to be not --

DR. BANERJEE: Maybe I am saying it in a different way. I am saying what is of interest to me is the pressure drop.

MR. CORLETTI: The pressure drop.

DR. BANERJEE: But maybe it is the same thing, in the sense that between the outlet of the ADS-4 and the top of the core, because everything is a bit tighter, I would expect a higher pressure drop.

MR. CORLETTI: You have to look at it that it is not really. In ADS-4 to the hot leg is significantly larger core power, but from the hot leg to the upper plenum and in that area, you are right. We have higher velocity.

However, if you look at the contribution to pressure drop, I think you will find that you are either choking in the ADS-4, or you have your line losses in your ADS-4 piping.

I don't think that your significant line losses from a subcritical pressure drop point of view is going to be in the hot leg piping.

DR. BANERJEE: Well, if that is true and the scaling shows that, then that is less of an issue to me. But if the core power density had been higher, to me at least I expect more entrainment from the core itself.

DR. BAJOREK: Dr. Banerjee, I guess I would like to compare notes with you on the CMT flow for APEX, because we did use your analysis method in taking a look at that, and we did not come to the conclusion that APEX was poorly scaled out for long term cooling in those periods after the ADS-4 and IRWST transition.

However, I think I would agree with you that in general leading up to or just after ADS-4 to IRWST transition, ROSA actually, looking at all of the parameters, may have been a more suitable choice in looking at all of the different periods.

DR. BANERJEE: Well, I am only talking of the period between ADS-4 being opened, and IRWST coming on, which is sort of a critical period. For the long term cooling, I completely agree with you. There is no problem.

MR. CORLETTI: Well, if I may continue. I am not sure -- that discussion is that, Steve, you and Dr. Banerjee are going to compare notes on that, and whether we have an issue on CMT flow scaling or not, and maybe that can resolve that.

The other notes that I had, unless you want to go down your notes, Dr. Wallis.

CHAIRMAN WALLIS: Well, you have the floor. Maybe we should go through what you have.

MR. CORLETTI: My notes? Okay. We heard a question about the long term cooling analysis that we did, and that we performed with COBRA/TRAC and with a two-node core, and I think that the ACRS had -- I think that you questioned that, whether that was a suitable model.

And I think that it was also recognized that you probably had large margins, because I think that is what we did say. And I think that you also recognized that it is probably a pretty easy hand calculation to perform, or along those lines.

I am not sure that I could do it, but it is easy for some people, like Steve, to do, or Bill Brown, or our people. But I am sure that we could do it.

So I think we would like an opportunity to do an assessment of -- do a calculation to --

CHAIRMAN WALLIS: I think it would be appropriate to just pick some window or a couple of windows, and do a more thorough nodalization of the core.

MR. CORLETTI: And do some sort of a calculation at that point.

CHAIRMAN WALLIS: And as you point out, you probably skipped a lot of flow regimes by having two nodes, and it is not particularly representative of what happens in the middle, and where it may get most of the pool swell.

MR. CORLETTI: And that leaves me with -- I only have one other thing on my list, and that is in the entrainment issue that we heard so much about, and I think what the recommendation or the perception was that perhaps the detailed modeling code that we used for COBRA/TRAC was going to -- that some day we might be able to demonstrate that we had everything absolutely correct there, but that was going to be a rather long path.

And that also that we recognized that probably a bounding approach, some sort of a bounding approach, where we demonstrated large margins in this area, would be maybe a way to try to show in reality how bad the problem could be in that regard.

And I think that we will take that under advisement. Also, I think that leads us to the use of the test data that we saw in the first test, the APEX-1000 facility, and I think that there we can provide more presentation in the future as far as some of the results that those tests are showing, and try to put that into context of what we believe that tells us about plant safety for AP-1000.

MEMBER KRESS: I think we also suggested that you look at the 14 foot test data.

MR. CORLETTI: Yes.

MEMBER KRESS: And if there was an action there or not.

MR. CORLETTI: Yes. The action on the 14 foot, and I think maybe we could follow up with Dr. Banerjee, but we provided an REWIRE response that at least outlined our validation of NOTRUMP against the 14 foot G2 test data that we were speaking of.

I think maybe we can follow up if there are more questions next time from that, but I think there are other WCAPs, NOTRUMP validation WCAPs, that probably speak to that in more detail. Andy, is that true? Is there even more detail in -- and maybe we can get the list of those topical reports.

MR. GAGNON: Yes, WCAP-14807, Revision 5, and I believe it is either Section 5 or 6, has the notes on the two-phase

MEMBER KRESS: Again?

DR. REYES: WCAP-14807, Revision 5.

MR. CORLETTI: And I believe our RAI response references that; is that right, Andy?

MR. GAGNON: I believe that's correct. What is in the RAI response is specifically void fractions. The remainder of the information, including the background and the modeling techniques, are described in the final validation report.

DR. BANERJEE: Now, with these experiments, do you have a write-up on the experiments in terms of data where you can interpret given this inlet flow rate, or whatever the situation is with a temperature at this level, apart from, let's say, NOTRUMP? But just data to look at to get a feel for whether you get dryout, or you don't get dryout, and things like that?

For example, there must be some conditions under which get dryout in the experiments?

MR. GAGNON: These were like boil out type experiments.

DR. BANERJEE: Right, but at some point --

MR. GAGNON: The heater rod should increase in temperature, yes. I have not looked at them in a long time.

DR. BANERJEE: So more or less what I am trying to understand is what is the margins to this, and what core level do you start to get dryout and temperature excursions, and things like that.

MR. GAGNON: In terms of collapse level?

DR. BANERJEE: Yes, collapse levels or whatever. So I can see that this is trying to make NOTRUMP -- compare NOTRUMP with some of the experiments done here. But leaving aside NOTRUMP, just the physical feel at what point somebody runs into trouble. Is it 20 percent, or 30 percent, 40 percent?

That is the source thing that would make me feel comfortable that your 40 percent level is the correct level, or even if you made a mistake and it was down to 30 percent, it wasn't going to get you into trouble.

MR. GAGNON: Yes, we should be able to look at that.

DR. BANERJEE: I think that is more of a comfortable factor that would increase strongly.

CHAIRMAN WALLIS: Any other member want to raise any matter at this time?

MEMBER RANSOM: One issue that I have heard about is these 14 foot tests I guess are -- and I am just guessing, but they are bundle tests which are more heat transfer tests. Do they have the tip of the upper plenums? Is there any data from the entrainment point of view that would be valuable there, or is there nothing there?

DR. BANERJEE: They have collapsed liquid levels, and so you know what is being entrained there.

CHAIRMAN WALLIS: If it is coming out of the bundle, you wouldn't know.

DR. BANERJEE: But it is not flow resistance you are saying.

MEMBER RANSOM: Well, the upper plenum, and configuration, is it more or less typical of the AP-1000, or --

DR. BANERJEE: I don't know.

MR. GAGNON: I don't remember the back pressure of these.

DR. BANERJEE: The outlet wouldn't be, but there would be a back pressure I think.

MR. GAGNON: I don't remember off the top of my head, but there is a description of the facility, including drawings of the facility, and the NOTRUMP validation report, but I don't think it is prototypic in the upper plenum.

DR. BAJOREK: Andy, one of the problems with the G2, and what it was like up there, it was actually originally designed for looking at upper head injection tests, and up at the top there were a set of thimbles to mimic this upper head injection.

And the other aspect of that, where the upper plenum area becomes very confusing and maybe not very useful. That is where they brought all the thermal coupled cables out.

And if you take a look at some of the photos that are available from that, it is a little bit of a spaghetti pot appearance from that. So I think it is useful for level swell and heat transfer from the core, but probably not for de-entrainment and for things that are going on up at the very top.

DR. BANERJEE: Well, there are two separate problems. I mean, unless you believe there is a lot of back flow from the upper plenum into the core, it is a one-way street that it is going in. So the upper plenum is seeing the flow resistance by the core, and then of course the details of the de-entrainment or whatever happens in the upper plenum is a separate issue.

But from the core point of view, as long as there was some resistance to it, that would probably be quite educational to see that. A coupling backwards is not through the flow back. I don't think there is a lot of liquid coming back.

CHAIRMAN WALLIS: So let me go over what we did yesterday. We had some general introductions which were helpful, and then we go into the safety analysis results, which I think we actually came here mostly expecting to talk about the entrainment issues, and that seemed to be the one outstanding issue.

And then we got these other things put on the program, of the large break LOCA, and small break LOCA, and containment. That is where I think it was a little bit surprising.

We thought it was going to be a breeze, and you guys had worked it all out, and the large break LOCA was okay. It looked like a large break LOCA in any PWR. There didn't seem to be issues there.

But it was a bit surprising that we had issues with the long term cooling, and the two-nodal model did seem -- I think struck us as being too crude to be relied upon completely.

And you agreed to sort that one out. The small break LOCA again was surprising, because when you fixed the momentum flux, it wasn't quite clear why it went out and came back again. We never saw any equations, and so maybe somebody should look at the equations that you used for this momentum flux.

Presumably they are around somewhere where you fixed this momentum flux terms in the ADS-4 line.

Do we have that written up somewhere?

MR. CORLETTI: Well, we have. We wrote it up on AP-600, and submitted that. We could also find you -- is this committee interested in that, as far as providing you a copy of that?

CHAIRMAN WALLIS: Well, it had quite a big difference to get quite a big difference in the answers, and that's why I was kind of curious as to how that could be.

MR. GAGNON: That is also documented in the NOTRUMP final validation report, and all the RAIs are included as the appendix.

CHAIRMAN WALLIS: So we can find that in the RAIs?

MR. GAGNON: That's correct.

MR. CORLETTI: Instead of having them go through this mountain of pages, do we know which one?

MR. GAGNON: It is really the AP-600 RAI, which is maybe a little bit of a confusing thing here. It is RAI 440796F, Part A, I believe.

MR. CORLETTI: And the NOTRUMP report, what is the WCAP, 14807, Revision 5?

MR. GAGNON: The same one.

MR. CORLETTI: Okay.

CHAIRMAN WALLIS: Well --

DR. BANERJEE: Is it documented in that WCAP?

MR. CORLETTI: Yes, it is documented actually in several places.

CHAIRMAN WALLIS: Mike would you like to pull out the pages that are relevant so that we don't have to look through a mountain of paper and see if there is anything that I ought to look at there.

MR. SNODDERLY: Okay.

CHAIRMAN WALLIS: So I guess we were surprised because this went off on quite a discussion about how these codes all gave different answers, and you agreed to address that in the future.

MR. CUMMINS: Could we clarify how we agreed to do that? I am not -- I guess my point before was that the staff is the only organization with all of these data, and it would seem to me that the staff would have the lead to --

CHAIRMAN WALLIS: Well, you had two codes. You had NOTRUMP and WCAP, and they were predicting very different flows at the break, and there are very different depressurization curves, and refiling codes, and the rate at which REFRAN was occurring, and it was very much more rough with one than the other.

And so I think that this gives or raises questions as to the believability of the codes, although the bottom line is the same, and I think that has to be addressed at some time.

You have got two codes, and they have got another one, and so they have three to compare with, and then they even have four if they run their -- and so I think you and the staff have to -- well, the thing that surprised me was that we ran into this.

We didn't expect to have issues with these things which weren't the entrainment issue, which was the big issue. So it was a little disconcerting perhaps that issues arose, or so many questions arose about what looked like being a more routine presentation that we could just be pleased with.

The containment analysis, and I think that Sanjoy agreed to review the heat master transfer and the falling film, which looked like a pretty straightforward problem. It doesn't look like it is bedeviled with a phase flow and so on. So that should be very straightforward.

DR. BANERJEE: But somebody has to provide me with that.

CHAIRMAN WALLIS: And he needs to get whatever.

MR. SNODDERLY: Let's clarify then what it was. I had it in my notes, but I would like to go over it. It was WCAP15806. We have that WCAP, the one that Dana Powers has, but --

MR. CORLETTI: Dana has actually all the WCAPs related to WGOTHIC, and so that ought to be in this.

MR. SNODDERLY: But again if you could help us. What was the specific RAI or chapter?

MR. CORLETTI: Rick, do you know?

MR. WRIGHT: I don't know offhand, but we can check it out. I was talking about the WGOTHIC applicability report.

MR. CORLETTI: Well, there is the main WGOTHIC WCAP.

MR. WRIGHT: Right.

CHAIRMAN WALLIS: So describing the equations for the heat mass transfer.

MR. CORLETTI: So it is the main WGOTHIC submittal.

DR. BANERJEE: There was interest from the outside.

CHAIRMAN WALLIS: From the outside of it.

MR. THROM: Ed Throm with the staff. WCAP15846, I believe, is the one. It is rather large, and it is three volumes. Well, this is what you want, and as I remember, it is Chapter 2 which discussed what Westinghouse refers to as the CLIME model, and that is basically taking you from the containment atmosphere inside through the condensate on the shell, and through the shell, and into the PCS water on the outside of the shell, and into the riser downcomer section of the PCS system.

So that is pretty much the definition of the heat transfer package that deals with both sides of the containment. It is 10 or 11, but it is in one of these specific sections.

DR. BANERJEE: The area that I am specifically interested in, or the one that I have been charged to look at is for the liquid film falling along the outside of the containment and --

MR. THROM: I am pretty sure that is in Section 10.

CHAIRMAN WALLIS: The mass temperature in the liquid film and so on.

MR. SNODDERLY: Now, we have Rev. Zero. Is that --

MR. THROM: That is the current revision.

CHAIRMAN WALLIS: Well, the next technical issue that I had was --

MR. SNODDERLY: I'm sorry, Graham, but

before we leave containment, we forwarded some comments and some information that Dr. Powers wanted, and I think yesterday off the record you had said that you were going to provide some information for that.

MR. CORLETTI: Yes, and I think I said that all of that information is in the WCAP, but what I think I said was that I would provide -- that instead of making him hunt through the three volumes, we will try to pull it out and provide that.

It is all docketed information, and so I don't think there is an issue of public information, but maybe I will work with either yourself, Mike, or John, on how we get that information to Dana.

MR. SNODDERLY: That would be great. Thank you.

CHAIRMAN WALLIS: Then there is the matter of boron precipitation, and this was raised I think by the staff. It seemed to be a key issue that needed to be resolved, and there is no RAI on the boron precipitation issue? So this is something which is being resolved and I guess we will hear about how you resolved that.

So then we got into the liquid entrainment issues, which are sort of the main reason why we are here. You were working on them, and you are working to resolve them with the staff. And I think we will probably get involved when you have reached hopefully that resolution.

So I don't think it is our job to give consulting advice on how to do modeling and things like that.

MR. CORLETTI: If I just could interject. I believe the APEX-1000 test are very well scale tests for the AP-1000. I think we would look to see a scaling report provided. I think we want to look at the overall system performance, because that is what has really been our contention all along, is that the overall system performance is really the most important aspect of this issue.

And look at how it does in that regard, and whether we are going to be able to predict and define everything that we examined going on between the core and the upper plenum, and to that level, I am not sure it is the right --

CHAIRMAN WALLIS: Well, you don't want to hang your hat on that, because that is where you might get in a lot of trouble because it might be easy to shoot down parts of that structure, and destroy parts of that structure, and then you wonder what you have for the whole, unless you have the whole perspective.

MR. CORLETTI: Yes, that is the approach that I believe that we will be pursuing.

MR. GAGNON: Yes, and I think that what was discussed yesterday is that there is a possibility that you could resolve this even without tests if you did some sort of bounding calculations, and in fact that was the first approach taken by Westinghouse, and we are halfway there or whatever.

I don't believe that we have satisfied the staff, and I am not sure that we quite understand the part that we have not satisfied, but I don't think we need the ACRS to help there. I think we can deal with the staff.

So that is one way of resolving it, and the next way if that doesn't quite get worked out is by showing and reaffirming that the integrated safety predictions of the codes are similar to the test results.

CHAIRMAN WALLIS: So maybe we ought to think about where we go from here, in terms of schedule and meetings, and so on. You presented a schedule suggesting that it would be two meetings of the AP-1000 subcommittee in May and June; is that what I saw?

MR. CORLETTI: Med, is that consistent

with your thinking? That is what I put there. That was my understanding.

MR. EL-ZEFTWAY: That is what it was based on, was the fact that this thermal hydraulic subcommittee would actually conclude all the issues that we are talking about now, and the May subcommittee is the future planned design subcommittee meeting on the other issues.

CHAIRMAN WALLIS: Do we need to have a meeting in April to wrap up some of this?

MR. EL-ZEFTWAY: Well, that is probably what you have to do right now.

CHAIRMAN WALLIS: Can we attach it to some other meetings that we have in April and in other subcommittee meetings that we are going to be attending?

MR. SNODDERLY: We could try, but as I remember, April was pretty heavy, and May was not.

CHAIRMAN WALLIS: So we might have a meeting of the thermal hydraulics committee say the day before the AP-1000 subcommittee?

MR. SNODDERLY: Or joint.

CHAIRMAN WALLIS: Or joint.

MEMBER KRESS: Well, when do you plan on meeting?

CHAIRMAN WALLIS: Spend half-a-day on thermal hydraulics and go on?

MR. CORLETTI: That sounds like a good idea.

MR. EL-ZEFTWAY: We can have two meetings, one after the other.

CHAIRMAN WALLIS: So we will do that. So maybe half a day is what I think we need.

MR. EL-ZEFTWAY: I think the time frame was we were thinking about the week of May 19th. I mean, that is for the future plan design.

MR. CORLETTI: And you see the list of items that I have on that slide? I think that is generally from feedback that I have received from the ACRS in general on some of the issues. Reliability of ADS-4 squib valves, which we talked about quite a bit at the PRA meeting.

And the containment structural design. I guess I am a little bit -- I think we can go over what our containment structural design is. I don't know of any issues with it, but I know that there was interest from this committee, and I believe it is taller, and how did you design it. I could see a short --

CHAIRMAN WALLIS: The thing with the seismic thing, and --

MR. CORLETTI: Yes, and we could present you with our seismic analysis results that we performed.

MEMBER KRESS: Now, this was raised by Peter Ford, and I am not sure that is what he had in mind, but I would certainly be interested in that part of it.

MR. CORLETTI: Okay. We can check back with Peter, too, to see what his concern was as well.

MEMBER KRESS: Yes, and I think we should. I don't really recall what he had in mind.

MR. CORLETTI: Okay.

MR. SNODDERLY: I believe that Dr. Bonaca has the lead for that review, but we should check with both, and I think Peter might have had the materials.

But the point is that we need to make sure that the agenda addresses the needs of the committee.

CHAIRMAN WALLIS: And the experience yesterday might indicate that in discussing these that other things might appear.

MR. CUMMINS: We hope not. In Pittsburgh, at least we have the experts.

CHAIRMAN WALLIS: We expect to go to Pittsburgh in May.

MR. CORLETTI: The materials is on there

was one, and --

CHAIRMAN WALLIS: And shutdown maintenance is one.

MR. CORLETTI: Shutdown maintenance, and I think I would like some feedback, and maybe not right now, Mike, but maybe we can work between you, I, and Med, as far as what the issue on materials might be.

Shutdown maintenance, I think I understand what some of the issues were that they might want to hear.

CHAIRMAN WALLIS: Well, I presume that May is going to be to resolve some issues that have been raised. Is June going to be where you put together our final picture of how it all fits together?

MR. CORLETTI: Actually, Dr. Wallis, I thought that May was to be to discuss these issues, and June was going to be on open items, and --

CHAIRMAN WALLIS: Well, what concerns me is that we have over 2 or 3 years now gone over this AP-1000, and it goes back to the AP-600, too. And we have talked about various things along the way, and I would like to see the whole picture again, and not with a lot of unnecessary detail, but the whole picture.

And how does all this stuff that we have talked about over 2 years fit together. And when we say we discussed that 2 years ago, and so we don't need to do that again, well, I think you do, because we have lost how it all fits together. So that is my comment and I don't know what my colleagues want to say on that.

MR. CORLETTI: Is that for the May meeting or the June meeting?

MEMBER RANSOM: That would be good.

MEMBER KRESS: For the June meeting.

CHAIRMAN WALLIS: And put it all together as a kind of dress rehearsal for the full committee meeting.

MR. EL-ZEFTWAY: We still have to get also a lot of draft sections from the staff on the DSER, and I think we have to look at all of them at the same time.

CHAIRMAN WALLIS: Look at all the issues and how they were resolved, and so on. I mean, the whole story, beginning to end, on why should this be -- why should we accept the design of this device.

And then there is a question of when the SERs is available to us.

MR. SEGALA: This is John Segala.

CHAIRMAN WALLIS: If we are finished with Mike, we should put you up, John, and you do your summary.

MR. SEGALA: Well, I handed out a slide on yesterday, and the last slide, Number 10, and I don't know if you have that in front of you, was sort of my wrap-up. We were looking at right now to get Westinghouse the comments on our unresolved issues to try to see if we can come to a conclusion on those.

We have an internal due date to get our DSER input from the technical reviewers by April 21st. And then as project managers, we have to put that together into one concise document, and get it through the lawyers, and then issue that by June 16th.

And then I guess some of this on the ACRS interactions might change a little bit based on the discussions that we just had, but we I guess the future plant design subcommittee in May will include half thermal hydraulics and half of some of these other issues that we discussed, and we had scheduled a full committee meeting in probably the July time frame, and we were going to present the DSER open items, and try to give you a view of what were the items that were classified as open in our DSER.

And welcome any feedback that you have at that time. And then we would have --

CHAIRMAN WALLIS: Well, we have a letter to the full-committee in July.

MR. SEGALA: Well, our schedule as of right now, I think that Westinghouse wants a more aggressive schedule. Our schedule as it is right now is that that would just be a conclusion of the DSER, and then we would -- our schedule right now has another full committee in July and August of 2004 time frame.

And then the final safety evaluation report would be issued in September of '04.

CHAIRMAN WALLIS: So we have another year?

MR. SEGALA: Hold on. We have talked with Westinghouse and we have agreed that when the DSER goes out that we are going to reassess our schedule and look at, okay, how many open items do we have, and how significant they are, and try to get a feel from the staff on how much effort it is going to take to review that.

So we are going to reassess our schedule. But as of right now, this is our schedule, and we are not going to change it unless there is something obvious that would --

CHAIRMAN WALLIS: So any results from OSU would be available long before the final SER.

MR. SEGALA: Yes, and we have another issue, and that is security, and it is kind of like we have not issued any RAIs on that yet. So that is a whole another unknown right now, and that I don't think we want to change the schedule until we have a handle on that.

CHAIRMAN WALLIS: Is that something that we get involved with?

MR. EL-ZEFTWAY: I think that is going to be handled differently or separately.

MR. SEGALA: We have that on a whole separate schedule, but it affects the issuance of the final safety evaluation report.

MR. SNODDERLY: But, John, I want to go back and clarify that right now the expectation is that in the middle of June, June 16th, that we would receive a DSER with perhaps open items.

MR. SEGALA: Yes.

MR. SNODDERLY: And that in July a full committee meeting, and you are asking for a letter from the committee --

MR. SEGALA: No.

MR. CORLETTI: John, I think that even at the DSER stage, I think that they will be asking for a letter.

MR. SEGALA: I looked back on what we did for the AP-600, and we did not require a letter from the ACRS. You may have sent one, but --

MR. EL-ZEFTWAY: Well, normally at the draft SER level, we write an interim letter.

MR. SEGALA: Okay.

MR. EL-ZEFTWAY: So that is what we thought we were going to have to do, and at the July meeting we are going to write an interim letter on the draft SER.

CHAIRMAN WALLIS: I would think that you would need a letter.

MR. CORLETTI: Yes.

CHAIRMAN WALLIS: And to let you know if we have major concerns, or any additional issues, or whether we think that everything is fine.

MR. SEGALA: Well, we welcome your comments. I don't think that we have a requirement in our schedule right now, but we welcome any feedback.

CHAIRMAN WALLIS: Well, you won't get any ACRS position until you get a committee letter. You can get all kinds of comments from committee members, and even read the transcript and all of that, but you won't get an ACRS position on anything until you get a letter from them.

MR. SEGALA: Okay.

MR. CORLETTI: I would agree, John, that Westinghouse does have a more aggressive schedule, too.

MR. SEGALA: And I think we have to go back and reassess our schedule, and we will have to take it from there, but right now our schedule is fixed. We are working to our schedule.

CHAIRMAN WALLIS: And can we review with Steve where we stand?

DR. BAJOREK: I have a handout. Our plans at this point are to continue supporting NRR and the review. We have been asked at this point to be involved in the liquid entrainment issues. So we will continue to follow those, and if it means evaluating test data, or new analysis methods, we will do that, but we are going to have to wait and see how Westinghouse responds to some of the issues that were raised here today.

Above and kind of beyond the review, we are doing some confirmatory work. We have been running some TRAC-M calculations, primarily in large break. It is premature to discuss those results and at this point we don't see any new issues being raised from them.

We do plan and intend to run a series of tests of APEX, and these tests would be designed to look at beyond design basis accident conditions. Things that would really stress the plant APEX, and provide some rather severe conditions for a thermal hydraulic code to help you assess the models that are in there, and possibly in this case to help in TRAC-M for the same problem that we see in RELAP, NOTRUMP, and COBRA/TRAC, and how do we model these processes in the upper plenum.

So we would hope to try to gain some of that from these tests. That is all that I have as far as our follow-up. Dr. Kress, I do have a brief handout to talk about this scaling idea and the .5 to 2, and if you would like to hear some of that now, I can take about 5 minutes to do that.

MEMBER KRESS: I think that would be good.

CHAIRMAN WALLIS: That would be fine.

DR. BAJOREK: There is a one page handout coming around, and it has three overheads on it, but again I think I can just handle this from here. The topic here are is the range of acceptability for these pi groups.

As has been pointed out at a previous

meeting, well, why is it .5 to 2. Why is it something else; .3 to 3, or something more restrictive than that. We have tried to start some work on this area, and at this point we have not made a whole lot of progress, but I just wanted to try to report our thinking on this, and where we think this may go.

First, and this was the first overhead, one thing that we would do is to rephrase the question slightly. And rather than looking at an acceptability range, it ought to be, well, how large can a distortion become before it starts to invalidate the test results.

CHAIRMAN WALLIS: Well, another question is what is the uncertainty introduced by having this distortion. Is it a quantitative measure, and how long you can be.

DR. BAJOREK: Yes.

MEMBER KRESS: And on what.

DR. BAJOREK: And on what and it is not simple. And by the way, we look at this as a generic problem in scaling, and not something that is AP-1000 specific in this case.

MEMBER KRESS: And another thing that worried me was if there are cliff effects, where you might change flow regimes within the high range, and then that could --

DR. BAJOREK: Yes.

CHAIRMAN WALLIS: And something like AP-600, and as I remember, there was a condition where the CMT might drain, and just throw up a number, like X minutes. And then if you don't quite reach that condition, they don't drain until 10 X minutes.

Well, that is a big change in the scenario for a little balance between some others, and it well could be the difference between 1 and 2 in a pipe and it changes the scenario.

DR. BAJOREK: And is there a bifurcation in here.

MEMBER KRESS: Well, what we were looking for with respect to your comment is that what would be a generic fundamental approach to determining appropriate pi groups for any scaling? And you might be faced with this later on and with other things.

DR. BAJOREK: Well, we have to do scaling evaluations for ACR-700 and SBWR.

MEMBER KRESS: We would like to see some fundamental technical basis for choosing those ranges.

CHAIRMAN WALLIS: Well, I don't think there is. I think it is very system specific.

MEMBER KRESS: It may be system specific, but that may be the answer that you come up with.

CHAIRMAN WALLIS: Then you have to look at the system and do sensitivity tests or something in order to evaluate how important it is.

DR. BAJOREK: We would like of like to formalize our thoughts in there, but we think that the bottom upscaling approach is important, because this is where you look for changes in flow patterns. Is it annular, where before you thought it was stratified.

Now the difficulty that we have had is that the correlations that you would like to use to address those aren't always appropriate for these new sets of conditions.

However, it is in that approach where we think it is prone to introduce a bifurcation, or all of a sudden the drag changes. Now, we think that perhaps the answer of this is to ask the next question, is that if you do have a problem in the scaling group, a distortion, how does it propagate through that transient?

There is two approaches that we have seen in the past that have been able to at least address this. One was by Dr. Banerjee in his scaling for the AP-600, where after he got the scaling groups, went back to the simplified conservation equations, and looked at the experiments in non-dimensional terms, and saw if the plant adhered to what was going on in the experiment.

I mean, that kind of focuses just on the main parameters in the scaling evaluation. Well, once you get it in those terms, you can look at the relative effect of changing one term against the three or four others that may balance it.

A similar one was by Marino Demarzo, where he said, well, let's take a look at a specific period, and develop a simplified model, and if we have a question on a distortion like we did for this entrainment, what is its impact down the stream.

And that is how we identified the flow quality. So the answer may be after you do the spread sheet work with the scaling analysis, go back and look at the scaling equations, and see how those distortions may propagate in a simplified model with time.

Now on a grander scale, I think the question basically asks, well, what happens in the plant analysis, and as we start to think that out, it starts to become CSAU again. Now we have a code, as opposed to a simplified model, and we arrange those terms which are thought to be important.

If you have models in the code, and you are confident that they are predicting something close to reality, that has a lot of merit. An example may have been in the Westinghouse large break methodology, where the PIRT experts had ranked -- I think it was gap conductance as being very large, and they had a decent model for that, and when that was ranged within its experimental uncertainly in a plant calculation, the delta PCT, the effect on the transient was significantly smaller.

And that is good, because that basically means that the developers of the PIRT were being overly sensitive at that point, and it was an issue that they need not be concerned about.

But the problem then is when you go to the full CSAU evaluation, you have got to be able to say that those models were applicable, or very realistic, for the process.

CHAIRMAN WALLIS: And that you could do something to range the uncertainty. One thing that I think came up in discussion with another applicant was the question of how do you range the uncertainties in the room on momentum equations.

Really, there ought to be fudge factors in there or something which say that you really don't know the momentum flux that well, and there is an uncertainty with a factor of two on it, and let's range it in some way.

And at that fundamental level, rather than ranging just some heat transfer coefficient which affects a tiny little bit of the whole scenario over a big range, range the fundamental things that affect how the whole code behaves, which you know are not exact either. And maybe you can work towards that, too, in your work.

DR. BAJOREK: Okay. I think what we would like to do is -- I have been talking with Marino Demarzo on this, and maybe the thing to do is we will try to set up a couple of simple test cases, and see how this works, and write down what we think is some of the appropriate steps to protect against these distortions.

MEMBER KRESS: I think the approach sounds really good to me, Steve.

DR. BAJOREK: Thanks.

MEMBER KRESS: Good luck on it.

CHAIRMAN WALLIS: Yes, I think I should also say that I appreciated your presentation on the entrainment issues. That was very helpful.

DR. BAJOREK: Thank you.

MEMBER KRESS: I think that this would be a significant contribution to the whole process here with coming up with something.

MR. SNODDERLY: Graham, if it is all right with you, I would like to try to take another shot at summarizing the last two days of meetings, because I think that there is three -- well, as I see it, the most important issue that was identified coming into the meeting was as you said the entrainment issue.

And I think that we heard from Westinghouse and also from the staff about why there may be more need for an assessment of the APEX test data. And the staff has documented in their letter of March 18th why that is.

I think that perhaps we could maybe provide some feedback in an hour or in the future, but I think the impression that I got was that they seemed to be heading in the right direction, or that is an important -- it is a good summary of the entrainment issue, and one possible success path.

I think as part of this meeting we also accomplished our other major objective, which was to identify possibly thermal hydraulic issues that may turn into possible open items if not resolved between now and the time of the DSER when we are asked to review it.

Those two issues as I saw them were the issue of the two node modeling for long term cooling, and the treatment of the momentum flux, and I think that Westinghouse has a good understanding of those issues, and that I think the staff is aware of those issues that have been brought up, and they will either have to address those in the DSER or at the time of the writing of the letter on the DSER, those two issues would then again be brought up, if not resolved, between now and then, either through another subcommittee meeting perhaps in May, or at the time that we review the DSER.

And as you mentioned there was the boron precipitation issue that was brought up, and then the other one that I had on my list that had not been brought up was the future presentation possibly on the sump strainer issue.

And as Westinghouse had portrayed it, it was that they had made a submittal, and they feel that their design is adequate, but they have not heard back from the staff. And the staff is in the process of evaluating that.

But that is another issue that perhaps when we review the DSER that that is another area that we may focus on, and I think the staff is aware of that.

So all I was trying to do here was summarize what I saw were the three major issues, which was entrainment, long term cooling, the two node and long term cooling, and the treatment of momentum.

And it appears that the two parties are aware of those issues, and will resolve those either in the DSER or at a future thermal hydraulics subcommittee.

CHAIRMAN WALLIS: I want to put this in perspective though, the two issues of momentum and the codes predicting different things. Sometimes it is not quite there physically and why, and the business of the two nodes long term cooling. These were issues that came up just by chance because Westinghouse made a presentation.

And it seems to me that this is a lesson. I mean, if we looked at it and it said that we had four presentations and on two of them we had surprisingly new questions, that is not a very good thing to happen.

So I think what I would like to see personally is in June when you come before this committee to make your case that it is an absolutely clear case, and that when we ask questions that you have the answers.

Because if you make a presentation and at random we hear about four things and we have major questions, that does not give a very good feeling that you are really on top of everything.

So when you come back in June, you are going to be on top of everything, and there won't be things that rise out of your presentation which require a lot of discussion or whatever, and you give us a feeling that something is not or has not really been thought out, or resolved, or maybe there is something there that needs to be investigated.

MR. CORLETTI: That's fine.

DR. BANERJEE: Nice bounding calculation.

MR. CORLETTI: Mike, the one issue that you mentioned on the sump issue, and I don't know that it will be an issue, but the May meeting would be a good opportunity to have our expert on the issue make a short presentation, if nothing else.

CHAIRMAN WALLIS: And it may be at the May meeting.

MR. CORLETTI: Yes, that would be a good time.

CHAIRMAN WALLIS: And when Brian McIntyre was here, we had this business about the particles of paint, and what was their trajectory when they landed in the pool, and we looked to the maple leafs falling in the air, and how they wobbled around and when off.

MR. CORLETTI: You have to do that in the fall for the maple leafs.

CHAIRMAN WALLIS: Well, you are going to revisit that and have a platform to prevent the debris from falling down, but it doesn't always follow a straight line trajectory. I think that would be appropriate to bring that up in May.

MR. CUMMINS: This is Ed Cummins. I would like to maybe clarify a sense of the ACRS, because I think that it helps us in resolving the big issue, which is the entrainment issue.

And I am going to say that as a straw man comment, and you can agree with it, or disagree with it. I believe after we went through the whole thing that there was a general consensus that trying to be exact on the entrainment and the de-entrainment, and all the phenomena on each tiny place of the plant is less than productive.

And the key is whether you can predict the integrated system performance of the entire safety systems. And if I could get a sense from the ACRS that would help guide our resolution with the staff, I believe.

CHAIRMAN WALLIS: Who wants to start with that?

DR. BANERJEE: Physics is good, or good calculations.

MEMBER KRESS: I will give you what my thought on it was. I think the key is what comes off at the interface between the liquid and the pool, as Ishii gets below the hot leg, and so you do need some sort of a physical model or an empirical correlation, or something that tells you what gets entrained as a function of the steam rates or the gastral rates that comes off of there.

And then to get a bounding issue, and just forget about de-entrainment. Now, I say forget about it, but it may be possible to use that Ishii -- and I can't pronounce the other name, but where you have a function of height in there, and where it is the entrainment, because that is physically very real, because it is just fall back mostly.

So being able to use that as a variable amount that goes into the hot leg line as a function of the position of the collapsed liquid level. But other than entrainment, other than that, I would just forget about and say that all that stuff that goes into the hot leg also goes out your ADS-4 line.

Now you may have to treat the pressure drop differently, and I don't know how to deal with that, but you also will need as a function of the collapsed liquid level this thing that Sanjoy asked for, and that was if it gets too low, what does it give you in terms of margins to dry out.

And what affect does it have on the thermal properties. We have not really seen that exactly what the margins are. And before I quit on this question of how to model the entrainment, I think you still need to think about as the collapsed liquid level gets lower and lower that you do leave a film.

And I would say that is part of the entrainment liquid that goes out, and I don't know how you calculate a film thickness that is left on the fuel elements, but there are models for that. So that to me would be a bounding approach, assuming that you had a good technical basis for this model for what comes off at the collapsed level as a function of the steam rate.

Does that fit with anybody else's thinking on this?

MEMBER RANSOM: Yes, I think I would like to add to that a little bit, in terms of some guidance as far as what things to try to refine and what not to. And that is that when you start trying to model things at a level that is below your ability to describe them in the physical system, such as say 1D models in pipes, or even finite volume models within a core, you can't refine those ad infinitum in spite of the fact that we have very powerful computational tools today. We are pretty much stuck with that.

When you start trying to build ad hoc models for processes that lie below the level of that description, you are going to cause a lot of problems. And I am not sure that you can improve the uncertainty in a calculation.

And so my attitude is that it is important to recognize right from the start that there is uncertainty in these models and in the calculation. And so there are limits to how much refinement that can be made on an ad hoc basis in any given phenomena.

And I am not ready to say that like the entrainment phenomena is at that point. I mean, you do need a model of entrainment, but it can be refined to the point where you see the wriggles in the curves and they interact with a model that you put in there, and you are not sure it is believable, because a lot of these oscillations are caused by the numerical process to actually integrate the equations.

And often times it seems like there is a tendency to go overboard in one area, and maybe the AP-600 model that was originally produced was an example of that, where they were trying to make this thing do multi-dimensional calculations, and dividing up the downcomer, and it was a very complex model.

And probably not -- and I can say definitely not what the codes that they were using were intended for. It was more a -- it is not exactly an integral balance model. I mean, that is another level of crudeness you might say which may be useful at times.

So I believe that it is important to keep that in mind and I know that this drift towards the use of probablistic uncertainty methods in licensing is an area where in these codes we are going to have to address the uncertainty in all of the different models in some way, and if you incorporate that, then the output from those things becomes a band, and it does not become a curve.

And now you can compare, say, one band with another and, and if they substantially overlap, maybe draw the conclusion that they are similar. But

I believe there is an issue there.

DR. BANERJEE: Let me say something.

CHAIRMAN WALLIS: And let's hear from Jack after that.

DR. BANERJEE: I was going to say that having been involved with scaling that I am very skeptical about scaling. Therefore, wherever you can use full-scale data to anchor your case, that to me makes a lot of sense.

And that is why I like the UPTF experiments, and I like the G2 experiments. If you could use those experiments to get a feel for what is going to be the entrainment rate from the core, and the carryover, which I think is the key there, and maybe what capacities the upper plenum might give you in storing a little bit of liquid if that is necessary, and as Tom or Dr. Kress suggested, that if you don't have to appeal to storing liquid in the upper plenum, but can just say that whatever comes out of the core goes out of the ADS-4, then that is even more convincing.

But anchoring the case on full-scale experiments, where you have got at least components that are full-scale, to me would be much more convincing. So that we don't have to go through some of these very convoluted arguments regarding the scaling.

MEMBER SIEBER: I guess my approach to a lot of this is pretty simple. I liked the plants that had active safety systems and (inaudible) came along and closed the switch and the pump started, and you had pressure in the flow, and you could test for that, and you knew from your modeling, and from the installation that you had that the safety systems would work.

It is not clear to me that with these very low pressures in driving heads that you can actually model this with sufficient accuracy to be able to tell what goes on. And the only approach I think that you can use is to take a look at collapsed level or void fracture, or what have you, and calculate how bad can it get when core damage begins to occur.

And then from APEX data and all of your modeling, say I will never get to that condition, and to me that would be good enough, except that you really have not improved the usefulness of the models at all. You have not said too much about uncertainty at all.

But that is sort of a way out of where we are right now to my view point. And it is a different kind of an approach, and it is not a very satisfying approach, but it does give some regulatory assurance that you are still on solid ground. I don't know what anybody else things of that, but that's it.

CHAIRMAN WALLIS: Well, to answer your question there, I agree with Dr. Banerjee that physics is good. I think that full-scale data is better, because even if you don't understand the physics, you sort of have much more confidence in what happens.

In the case of what we are talking about here with entrainment, there are some parts of the system where you need to get it right. I think you need to get right what comes out of the core. You can't really take bounding assumptions like homogenous equilibrium flow or something, and that gives ridiculous answers.

So you have got to get that right, or right enough --

DR. BANERJEE: Out of the core.

CHAIRMAN WALLIS: Out of the core, yes. And now there are other parts where, say, let's take the other extreme, where we are talking about the hot leg entrainment from the ADS-4 line, and where the amount of water that you can store there is not very much, and this is the changes that you make from one extreme to the other, and changes that you have or the effect that you have on the level in the core is not very much.

And where bounding analyses makes a lot of sense, because it is difficult to get it right. If you can get it right, then it is best that you do that, but it is difficult to get it right. And the worst thing you can do is to try to get the physics right, and to bring in Teitel-Duckler or something, which to a knowledgeable reviewer doesn't seem to be appropriate to the problem.

If you are relying on importing correlations, and then Kataoka-Ishii is a little bit like that, and you are importing something from another context, and where it doesn't really cover the range of variables and parameters, and the geometries.

And you can't rely on it very much. So you are liable to have it become unconvincing, and there is nothing so convincing as a really good bounding analysis, which says that by the first law of thermal dynamics, or by some continuity of mass, or something, the worst it could be is X.

And if that is what you need to rely on, then that is a very strong thing to rely on. If you can't rely on that, then you have to do more physics.

And you can't rely on that at every stage, and so I think for some of these phenomena that we have been talking about, it may be appropriate for you to show that whenever your physical model lies between this limit and that limit, and everything is okay over the whole range, that is a good argument for you to use when it is appropriate, yes.

MEMBER KRESS: But I came away with a feeling that the Kataoka-Ishii model might be appropriate for what comes off of the core, and one way to judge that would be to use the 14 foot databases and see how it compares.

CHAIRMAN WALLIS: If it helps to explain the full-scale data, but if you don't have any data at all to invoke it, it is a very weak thing to rely on.

MEMBER KRESS: That would be the connection that I would like to see.

CHAIRMAN WALLIS: Absolutely. The more legs you have to stand on the better. I mean, if you have full-scale data and an understanding of the physics, and a theory to explain those physics, then that is wonderful, and then you really have them all.

MEMBER KRESS: Well, there is no theory for the Ishii thing. It is strictly --

CHAIRMAN WALLIS: But the --

DR. BANERJEE: Then you get the heat transfer and --

CHAIRMAN WALLIS: And the danger from this upper plenum modeling is that you have to patch together so many things which you are not too sure about that it is not a convincing story and good data.

Now, maybe you would be convinced by OSU data. I don't know. It depends on how well we believe the scaling. Does that help?

MR. CORLETTI: Yes, it does.

CHAIRMAN WALLIS: And you can look at the transcripts, too. Do you read transcripts of these meetings afterwards?

MR. CORLETTI: Yes. Yes, we do.

CHAIRMAN WALLIS: Do you go cold and sweaty and say I can't believe I said that or something?

(Laughter.)

MR. SNODDERLY: While Dr. Banerjee was speaking it reminded me that Slide 25 from your proprietary material provided two data points for the UPTF tests, and then the presenter showed six, and I think that you guys had committed to providing those six.

MR. CORLETTI: I will provide that.

MR. SNODDERLY: Thank you.

CHAIRMAN WALLIS: That is very good to follow up on all those things.

MR. SNODDERLY: Yes.

CHAIRMAN WALLIS: Is there anything else, or can we close this session?

MR. CORLETTI: I would just like to say on behalf of Westinghouse our thanks to the members for the past two days, and it has been a challenging review and challenging questions, and for your patience.

Also, I wanted to thank Mike Snodderly and Med for all your help with helping us, and thanks to the staff and the Office of Research, and Jose, Dr. Reyes, for trucking out from Oregon to be with us today. That was very helpful. Thank you.

CHAIRMAN WALLIS: So you have made my closing speech, and I would just endorse what you just said.

MEMBER SIEBER: I will second that.

CHAIRMAN WALLIS: Well, I declare this meeting closed.

(Whereupon, at 2:46 p.m., the meeting was concluded.)

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 Reator 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.

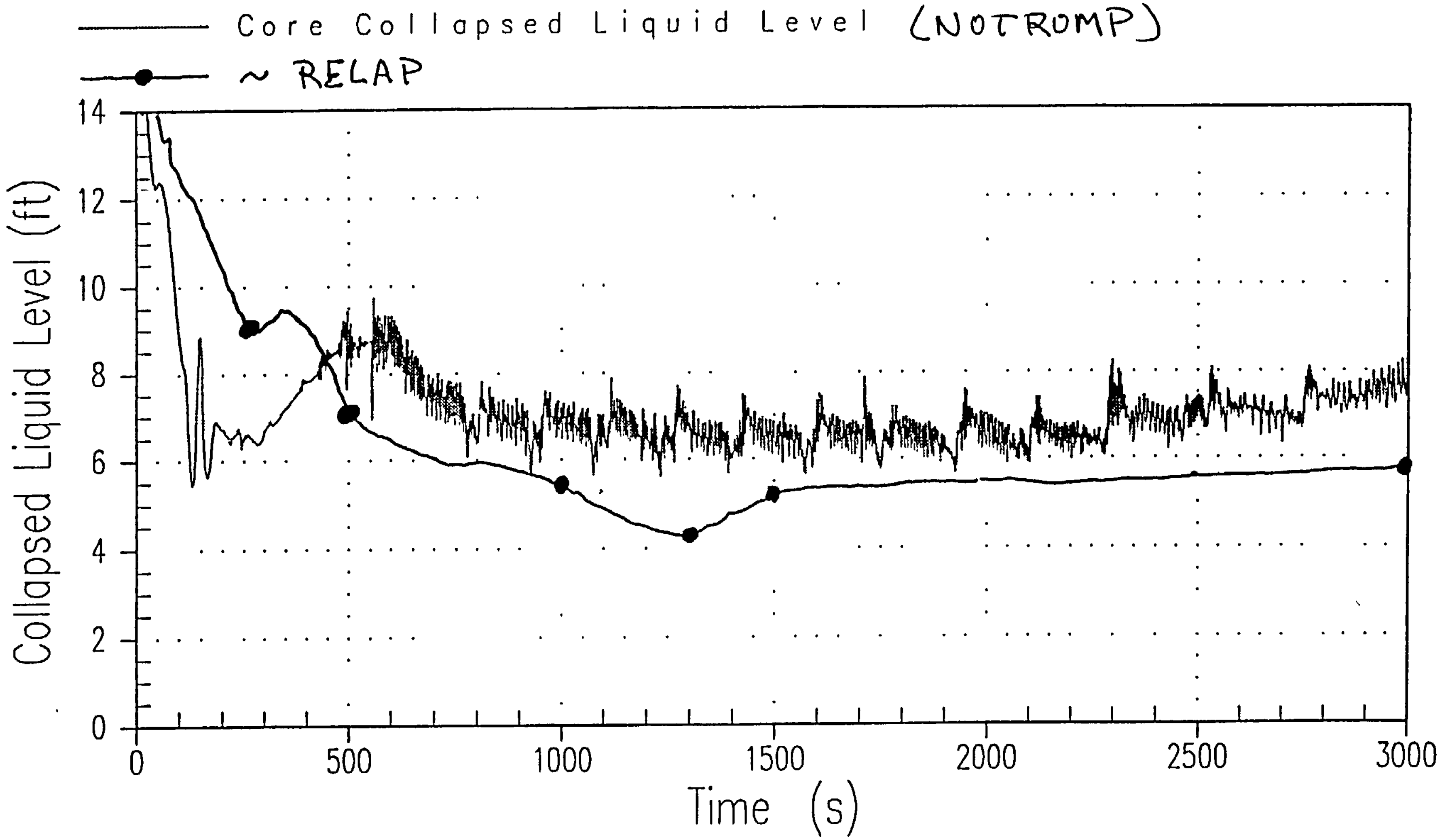
Heather Craycraft
Official Reporter
Neal R. Gross & Co., Inc.

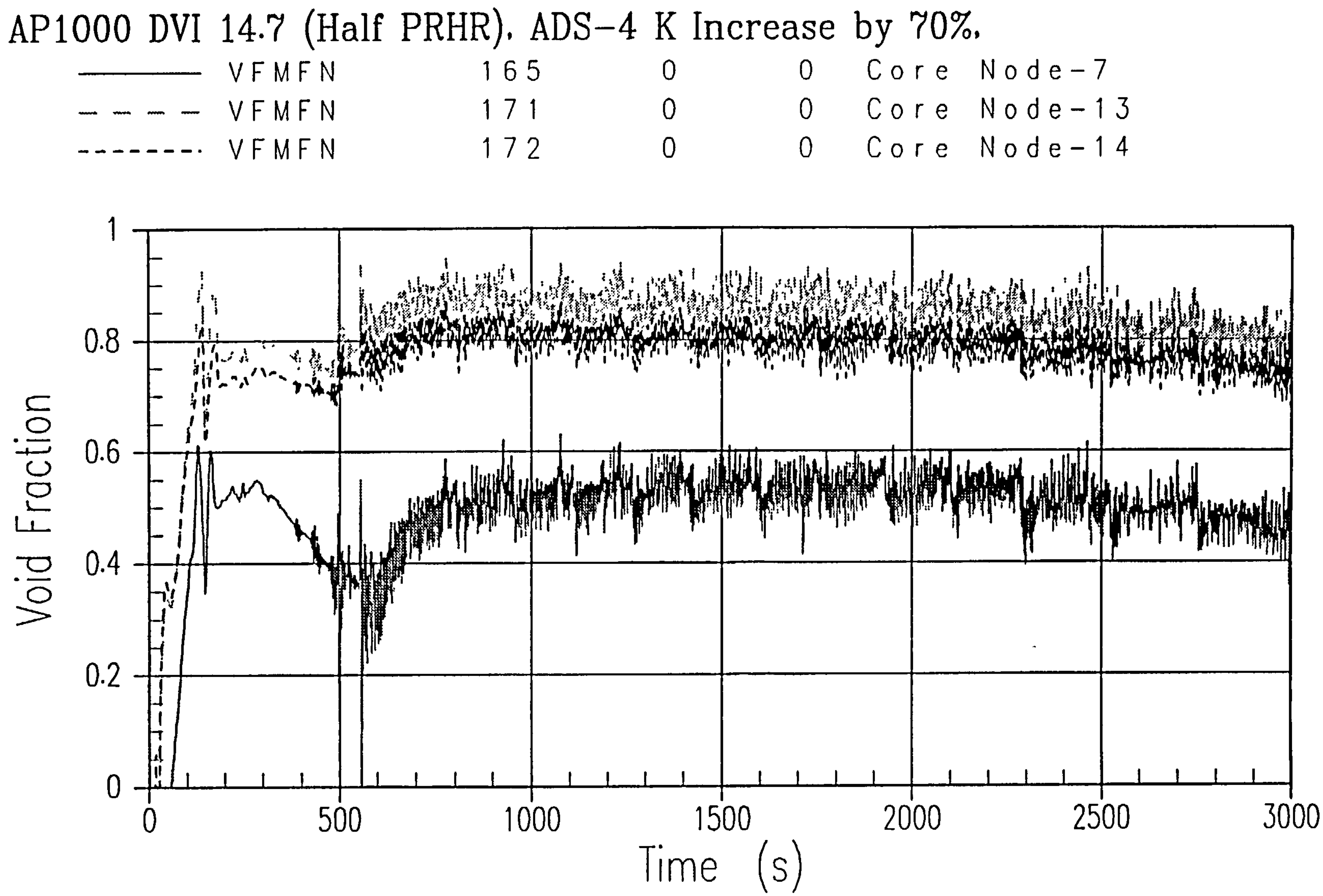
Summary of RES Future Actions

- RES plans to support NRR in resolution of liquid entrainment issues.

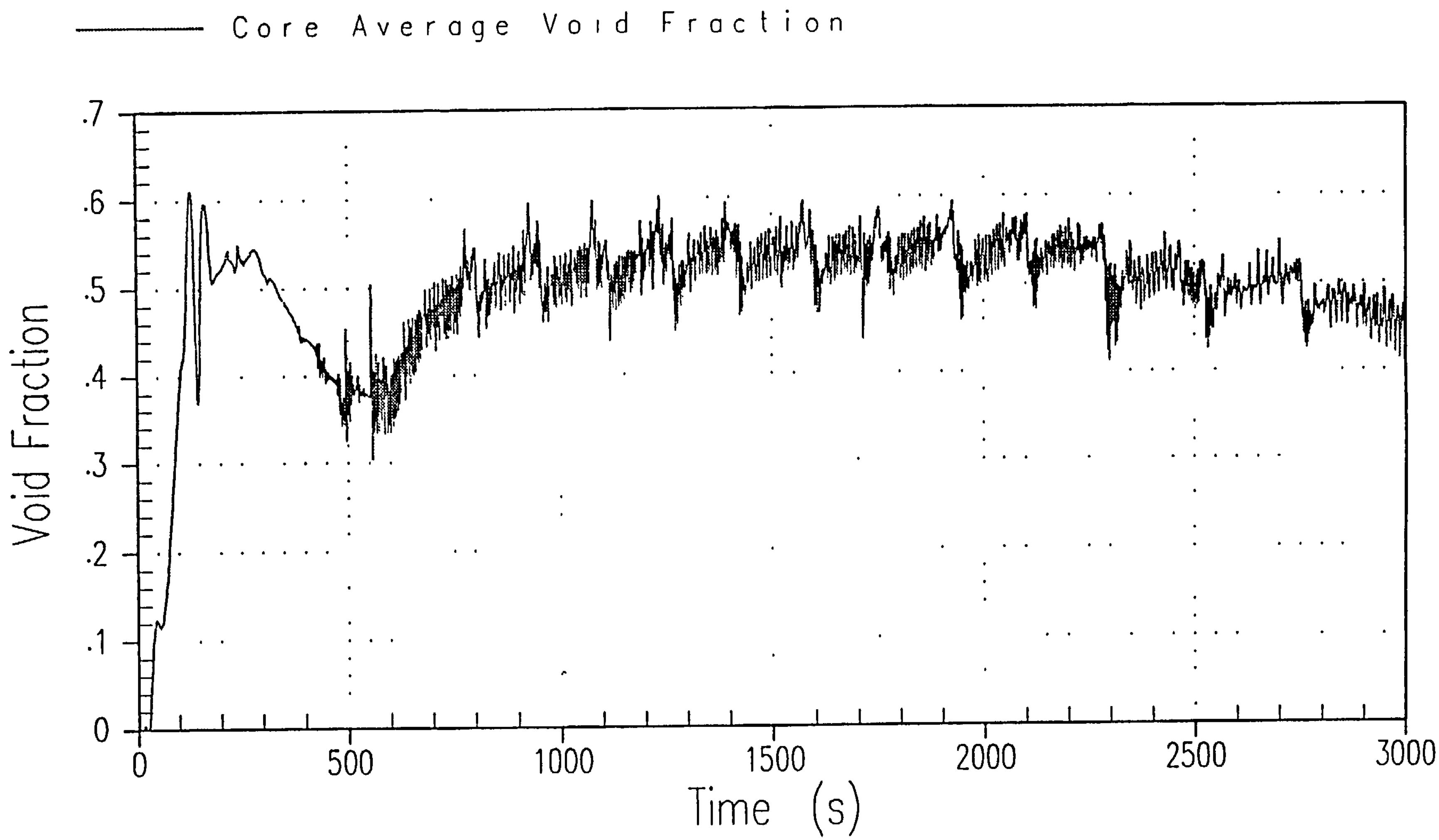
- RES is sponsoring confirmatory research at APEX that is applicable to AP1000. Integral and separate effect experimental tests in the APEX-AP1000 test facility to investigate BDB scenarios and develop improved models for upper plenum processes for TRAC-M.

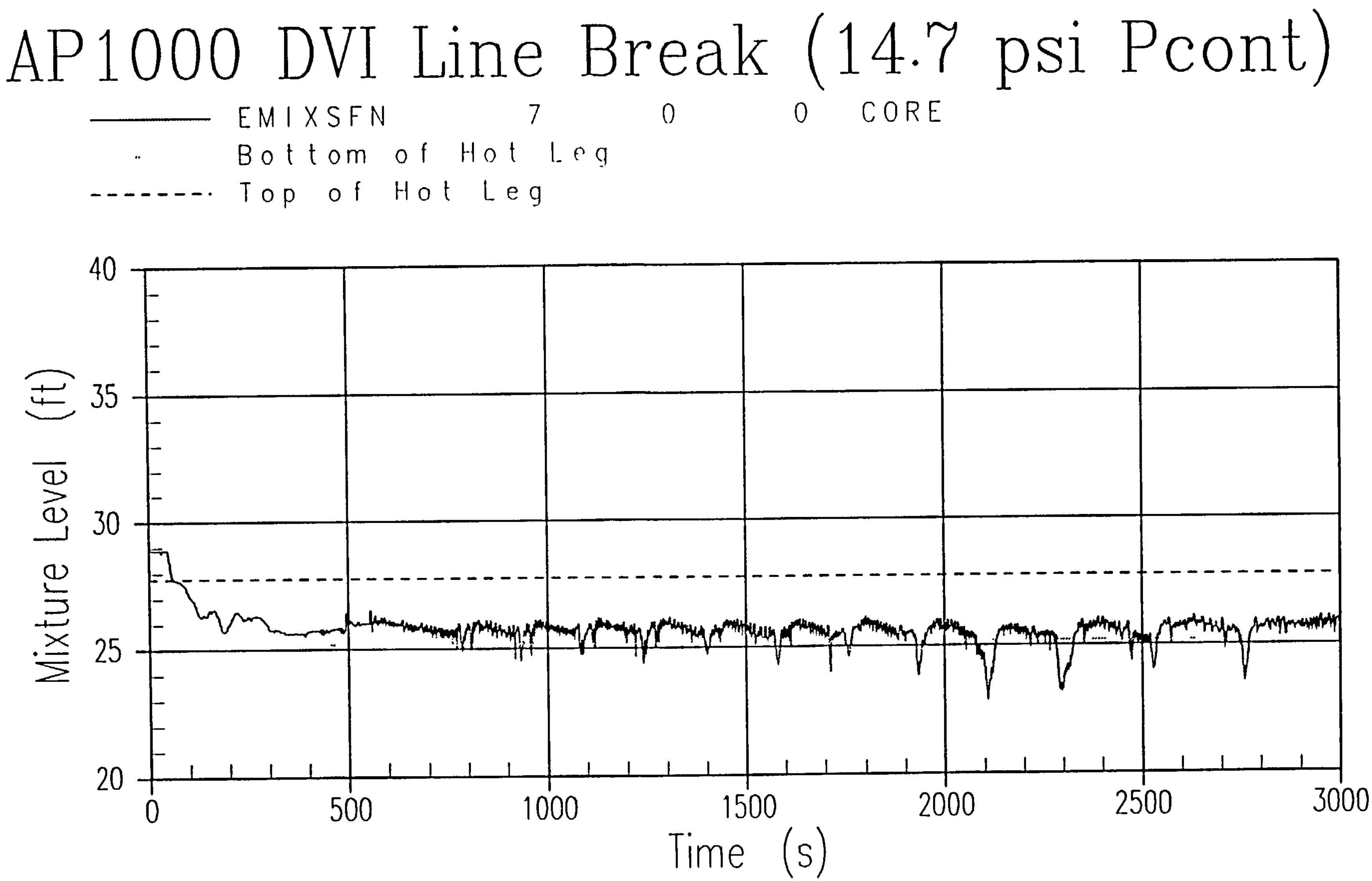
AP1000 DVI Line Break




AP1000 DVI 14.7 (Half PRHR), ADS-4 K Increase by 70%.
VFMFN 165 0 0 Core Node-7
VFMFN 171 0 0 Core Node-13
VFMFN 172 0 0 Core Node-14
Void Fraction
Time (s)
0
0.2
0.4
0.6
0.8
1
0
500
1000
1500
2000
2500
3000

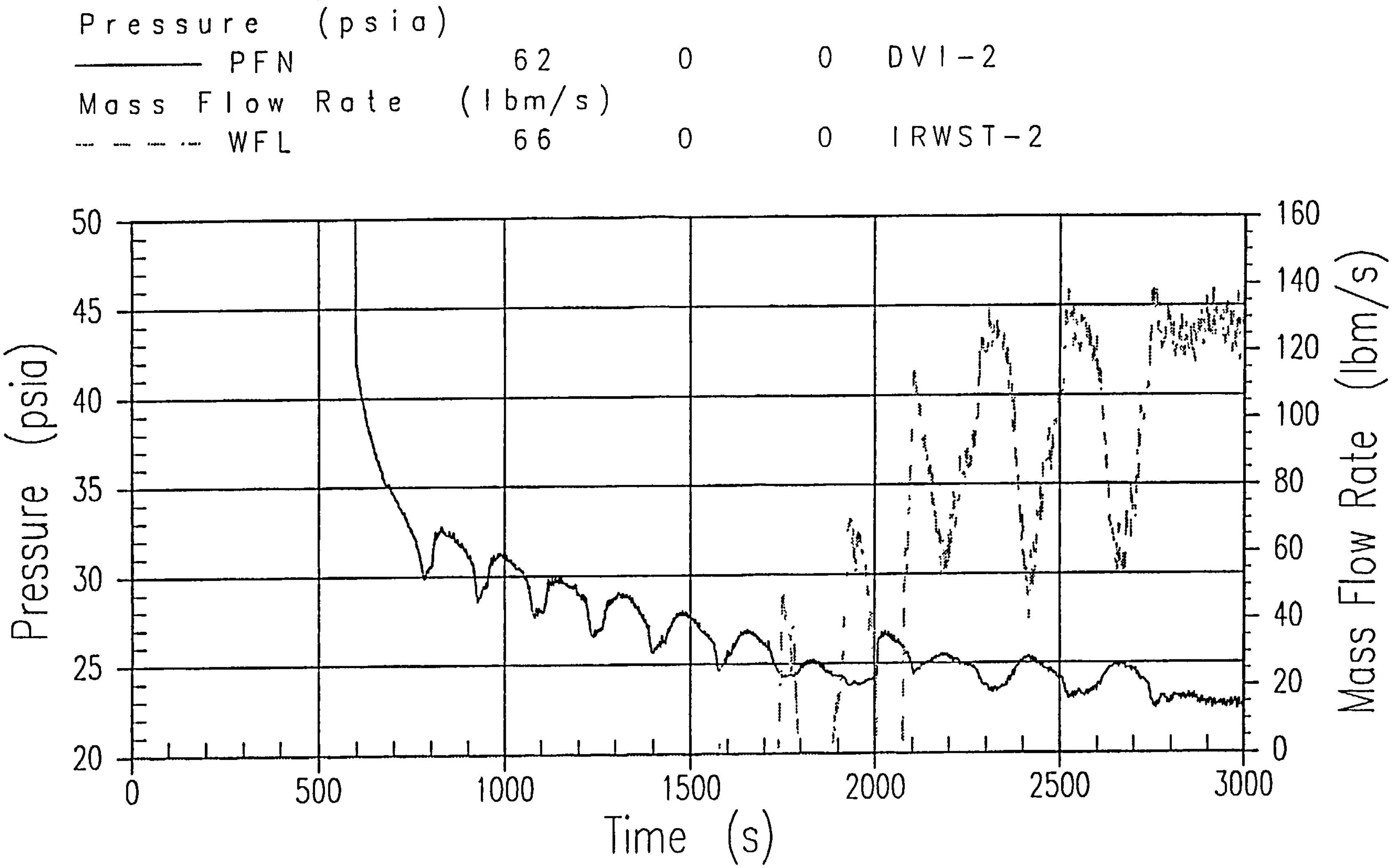
AP1000 DVI Line Break



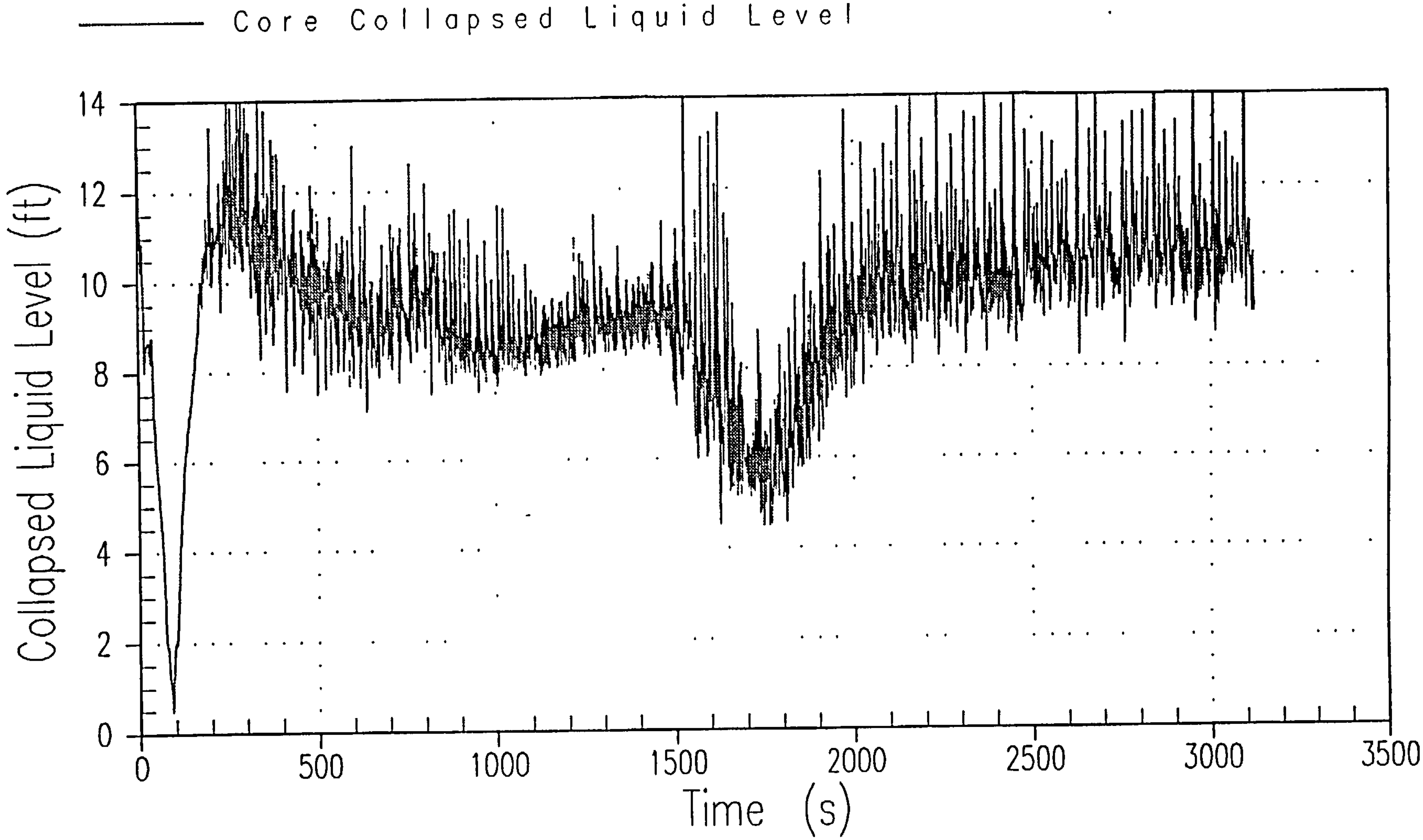


AP1000 DVI Line Break (14.7 psi Pcont)
EMIXSFN 7 0 0 CORE
Bottom of Hot Leg
Top of Hot Leg
Mixture Level (ft)
40
35
30
25
20
0
500
1000
1500
2000
2500
3000
Time (s)

AP1000 DVI 14.7 (Half PRHR), ADS-4 K Increase by 70%.



AP1000 10 Inch Break in Fluid Node 49 (Cold Leg)



AP1000 10 Inch Break in Fluid Node 49 (Cold Leg)