CHAIRMAN WALLIS: But it doesn't give you a measure of success. And I think you really need to think more about what is the proper measure of success for a code.

MR. BAJOREK: Let me jump ahead for that then.

MS. UHLE: Can I answer Professor Schrock's question?

MEMBER SCHROCK: I think he said it okay. That really results in more assessment than less. I have a feeling that it is maybe limiting the amount of assessment.

MS. UHLE: I think it's just focusing on where we're going to start first and then getting gradually to the lower things.

MEMBER SCHROCK: Yes.

MR. BAJOREK: Paul, if you pass out that other set --

MR. BOEHNERT: This one?

MR. BAJOREK: Yes, that one. This is the proposed assessment matrix that will be used following the code consolidated assessment matrix. If it came out well in this, we would continue to do tests looking at tube barometers, types of tests where you know you can do a hand calculation to come up with the answer that maybe the code has to deal with to perform those tasks before it could go on to the others. This is a way of checking to make sure your latest code change goes through appropriately. But the difference between the consolidated matrix and what we would be doing in what I'm calling this first development assessment matrix, we would greatly expand what we are looking at in the FLECHT SEASET facility so that we could look at how the code performs for a forced reflood, when we change the reflood rate --

CHAIRMAN WALLIS: What's your measure of performance? In your two-phase pressure drop here you've got to do some comparisons. How do you know when it's good enough? Maybe a factor of 2 or 10 is good enough two-phase pressure drop. How do you know?

MR. BAJOREK: Part of that comes from what we get out of ranging the bias and uncertainties at the light water -- in the light water reactor. So coming up and let me go -- I'll jump to this, and let me show you --

CHAIRMAN WALLIS: You really have to do the CSAU thing and look at how does it effect things that matter, like peak clad temperature or something. Then say, have we got a good enough code. Don't you have to go to the things you're trying to predict for regulatory purposes and the sensitivity of those are the things that you look for.

MS. UHLE: That comes out of the fact that these models will probably be ranked --

CHAIRMAN WALLIS: But the PIRT doesn't do any of that.

MS. UHLE: Well, sure it does. It tells you what experts are thinking of.

CHAIRMAN WALLIS: It doesn't tell you what's good enough.

MS. UHLE: It does in a sense that --

CHAIRMAN WALLIS: What an expert's thinking is really often self-serving. They say I'm an expert on flow regimes so you need to do more work on flow regimes.

MS. UHLE: And then in our first experiments we focus on those models that people point out as most important.

MR. BAJOREK: We look at the reflood heat transfer to determine how well it behaves, and we have looked at some of the reflood tests and we would see how those uncertainties behave in the full scale.

Now, if we continue to see very large uncertainties, that's an indication that we need to go back --

CHAIRMAN WALLIS: If it effects the regulatory decision.

MS. UHLE: Right.

CHAIRMAN WALLIS: Yes.

MR. BAJOREK: So if we range the reflood heat transfer over its broad range of uncertainty based on how we see it in separate effects, but it doesn't make any effect anymore on the peak cladding temperature, that says we should look more at things like bypass or condensation. I don't think we're at the point where we can rule any of those out.

MEMBER KRESS: Rather than look for what range it's asking as measured as how good is good enough, I think your aim ought to be being able to capture the uncertainties. And then if you can capture then, you can say how good your prediction is with respect to any of the reactions and then your decision process could factor in those uncertainties on whether or not it's good enough. So again an application --

MR. BAJOREK: Once can capture them how well the code's performing based on the separate effects then we can see how it behaves.

MEMBER KRESS: So how is the code going to be able to kick out for you the uncertainties.

MR. BAJOREK: If we don't get to that step and we see a large change or no change in the light water reactor, you don't know whether it's because the code is doing input or not or whether it's exhibiting the right sensitivities.

MS. UHLE: Another thing, too, focusing on the separate effects test is the fact that if you just focus on the integral effects test you're not sure if the answer isn't changing because of compensating errors. And that's what the separate effects tests really highlights.

MR. LAUBEN: No, the point is that there is nothing like the regular development of --

CHAIRMAN WALLIS: No, I'm saying that I think the PIRT is based on experts, the wrong experts.

MR. LAUBEN: Right.

CHAIRMAN WALLIS: They're not your customers. They're just the people who are looking for work. They're the wrong group.

MS. UHLE: Any PIRT contributors here? All right. So any PIRT you were involved in we'll throw out.

MR. LAUBEN: But if you were to go through the process we talked about today, you'd start out with some kind of PIRT and during the process you'd focus in on, at the highest level, like you were saying the ability to predict a regulatory effect on the peak cladding temperature, and all of the top level things. The PIRT may change. The PIRT may start out as something, and what is critical changes throughout your whole process.

MR. BAJOREK: And it does. I mean, if you look at the PIRTs that are designed for conventional PWR, versus AP600 or AP1000; there are small but perceptible changes in all those, and what's important in one transient versus the next --

Our problem is making sure that the code can deal with those things which people have deemed to be very important and then can also deal with those things which are deltas between plants that have been looked at in the past.

Now, I think part of the problem in this assessment, I think has just been pointed out, is a lot of folks have focused on solely the peak cladding temperature as being your sole measure of a code performance. And what I did is I grabbed a couple rolls of technical papers and, actually, I took one out of CSAU NUREG for example, how does your code behave. And the common way of doing it is looking at the peak cladding temperature from the scout point.

A plot, if you were, where you predict the PCT as higher than the measured, you deem that it's being conservative and say that your code's conservative forgetting the fact that there may be other things going on in these experiments, CCTF and SCTF in steam binding and the steam generators that may be contributing to the performance of your core heat transfer.

Another way would be taking these tests, mix them in with separate effects tests, which is done over on this figure on the left hand side, and use that to get a gauge of your code performance, or in this case as this had been designed to, is well let's just get a delta PCT and you would simply put that on as an adder towards some calculation that you would do for, in this case, the PWR.

I think the perception now, and correctly so, is that approach is incorrect because it doesn't deal with compensating errors. It doesn't deal with new ranges and conditions and tells you nothing about whether you're getting things like super heat, drop break-up correct, all of the intricacies of reflood heat transfer that go into calculating that peak cladding temperature.

Now, we intend to expand the test matrix that we're going to for the separate effects tests, but at the same time which when you get away from this type of a measure of the code performance. This is an example of typical practice, and the one I just showed you -- I've got my greater than and equals sign in the wrong direction here. The one where I just pointed out is to take a look at solely the peak cladding temperature, it says your coding is conservative if your predicted is greater than the measured.

Now another way, and Joe alluded to it in his presentation, is to look at the one model in reflood that's perceived as having the greatest effect. Okay. This has been done by taking a look at the dispersed flow film boiling heat transfer coefficient; defining a bias and an uncertainty. And in this particular application then the uncertainty in that particular model was used to range at full scale in order to get delta PCTs in the full scale case. PWR in this case.

We're going to take advantage of more detailed test data like we're getting out of the rod bundle heat transfer program and information that we can glean from other test programs to increase the total number of peak performance parameters before we can claim success in any one of the models, and I'm going to reflood as an example.

Our approach now is we can use multiple parameters is to try to characterize specific models within the package and the package in total. Okay. We will not rely on simply peak cladding temperature as the sole performance indicator. For reflood heat transfer the type of things that we would get out of the assessment after we have done the simulations and comparison to data, the FLECHT SEASET, that larger number of tests; the FLECHT Skewed, the FLECHT ACHILLES, the other ones that I have listed on there, is to look at break things up into heat transfer regimes. Look at those periods where the test and the code were predicting steam cooling heat transfer, and use this as a performance measure by defining a bias and uncertainty essentially to characterize how well the code is characterizing and calculating in a single phase performance. We would still do the dispersal of film boiler heat transfer coefficient as we've done in the lab, in case you're not aware of.

Joe noted that near the quench front, okay, we also have some very important precursory cooling. And we want to know whether the bias and uncertainty in the models that we develop and put into the TRAC-M are reasonable compared to the experimental data that we get out of the rod bundle and we can also get out of some of the other tests. And this is one case where we might want to jump very quickly to take this biased uncertainty and use those or study those in a light water reactor application and give us an indication should we be looking more closely at inverted annular flow, okay? Or, should we continue to focus on steam cooling dispersed flow film boiling which has been more typical of the past.

The answer to that in those simulations would be whether we're seeing very large uncertainties in the light water reactor application, very large delta PCTs. That would be an indication that the bias and uncertainty that you are imposing on the code by a model selection and model development would be unacceptable. It might mean another experimental program or it could at least mean you would have to go back, sharpen our pencils and come up with a better model and do some additional assessment.

MEMBER KRESS: Is the plan incorporating these biases of uncertainties into the code itself and combining in some way with the Monte Carlo, for example?

MR. BAJOREK: In the long run, yes. Right now we don't have any plans to put in into the input structure in the TRAC-M the way of incorporating these biases or uncertainties easily.

MEMBER KRESS: It seems to me like that should be your eventual goal?

MR. BAJOREK: I think if we start to see it -- we did want to have some kind of input or some way within the code structure that we could range these things easily without depending on either the developer or the user to actually go into the code and hard wire the changes, which is the way I've seen this thing done in the past.

MS. UHLE: A good thing about the modernization, too is the architecture. The physical models are isolated from any of the -- associated with the alphanumerics. And so the correlation in a specific sub routine is either divided or multiplied by that value, and have that propagate through the answers.

MR. BAJOREK: It has also quite helped us to get away from relying on that group of experts that helped develop PIRT. Because once we try to develop a larger range of performance perimeters, and have really to range those in the light water applications, now we can go back and say ah-ha, this should have been on your PIRT and this was missed or hopefully you guys did a job.

MEMBER KRESS: I worry about arranging them individually one at a time. That's why I mentioned in Monte Carlo, you can get away from that.

MR. BAJOREK: The way we had done that at Westinghouse was to look at things one at a time and then develop a response service methodology to try to incorporate how combinations of things can change. That also was driven by a couple of different things.

One, it was always nice to go to the user and say "This is what you're going to do because this is what was approved," very clear cut.

Another approach, and I think that has been used more in Europe and we are going to be looking at that in the long run, is I think is a GRS sampling approach or refer to it as a German sampling approach. I thought that was Oktoberfest.

But what this does is it looks at a broad range of uncertainties and simultaneously picks and ranges multiple perimeters, and puts that in your simulation, samples that distribution many times that gives you an uncertainty in your peak cladding temperature, your equivalent clad reaction and also a confidence interval. If you don't like that confidence interval, do it more times.

Now, the nice thing is that it seems as we spend more time in these meetings computers continue to get faster. And what would seem, you know, absolutely insane ten years ago, making a hundred PWR or BWR calculation, is now something that can be done in a reasonable amount of time. So that type of approach now I think is something that can and should be looked at in the long run.

But anyway, we're going to break up, for example, reflood into multiple performance perimeters, in some we are going to look at specific model and processes. I wanted to add a couple on here to try to address the hydraulics, although it doesn't individually get at flow regime transition or interfacial drag, but carry over fractions. Rather than just taking a look at mass affluent and what the code is predicting, it applies an uncertainty for as many of these tests that you can so we can determine if the code is doing a good job or not in calculating things like entrainment drop size.

Level swell, or another way I would say it interfacial drag below the quench front. And see for a given amount of mass has the quench front propagated too high into the bundle.

And in those characterized individual

models or call them packages, we can use things like quench time, clad temp and steam temperatures, these are in the program that was developed at Penn State called ACAP that essentially goes through and takes a look at a predicted trace versus a measured trace and gives you statistics on how well that curve corresponds to one another.

I'd like to think it a little bit more as the integral of this curve behaving much like the integral of the other curve. This, again, starts to get closer towards the peak cladding temperatures you're looking at things that's an aggregate, but we think by defining several key performance perimeters and making our holy grail the idea that we're going to get all of these simultaneously in some reasonable bias and uncertainty where reasonable at this point still is yet to be defined, because when we go through the first cuts and range those in PWRs and BWRs, that's going to start to tell us what is reasonable, whether we're looking at hundreds of degree change in clad temperature or a few degrees.

Most of this work will not begin until late 2002 with the release of the Rev 0.0 version. 2002 is going to be primarily those tests that are being used in the consolidated assessment. What I wanted to note on here is what we would be doing is expanding the database both in the total number of tests that would be looked at in an individual facility and in the total number of facilities that would be factored into the assessment.

Some of these tests will be done in 2002 once we get close enough with the Rev 0 version, because one of our first applications is going to be the AP1000 large break LOCA. So we not only have the work at performance for reflood heat transfer, but also things like bypass, we're going to be very interested in the performance of the code for how well it does for direct vessel injection. So we would be look at tests like UPTF 6 and 21 phase D to get the direct vessel injection, and also one of the CCTF tests that also gets --

CHAIRMAN WALLIS: All right. Can you finish by 12:30?

MR. BAJOREK: Yes.

Integral tests also captured in the assessment matrix would expand the number, the total number of facilities. What we would be looking for there is, does the code give us the type of sensitivities that were observed in these various tests. What would we do if we would look at, for example, SCTF and the difference between flat radial shape and the very peak radial shape, in the sense that we are able to get that same type of a variation in TRAC-M. We wouldn't get that if we just looked at one test.

And you can see some of the other sensitivities we would hope to get out of the integral effects tests.

The eventual goal then is defining the uncertainty for a large number of models, develop the capability of range and base and assessing their importance in the full scale plants, peak cladding temperature and their effect on normal clad reaction. As we start to see plants being operated, they're now staying at higher temperatures for longer periods of time. Our concern from a risk based regulation is that maybe peak declading temperatures is what we're going to have to look at in the future, so we're going to have to start looking at clad ductility and clad reaction rate in a lot more detail than it had been in the past.

As we mentioned, if we start seeing large uncertainties in the light water reactors, that's an indication that we either have to look at test data closer or we have to go back to develop better models for the process we're interested in.

We see this as being one of our major activities over the next 3, 4 or 5 years.

And by way of summary, we're going to expand the consolidated test matrix to look at a larger number of conditions, a larger number of test facilities. We're going to use this quantified code to model accurately and engage what goes on in the other plants.

CHAIRMAN WALLIS: You have a lot on your plate.

MR. BAJOREK: There's a lot there, yes.

One of the things we are going to try to do in 2002 is automate the process. It's a lot of work and there's a lot of comparisons to data. If we do a good job on the first few tests, capturing the scripts to do the comparisons, setting up the methods to run these things in mass and do comparisons to mass, we may save -- we'll definitely save ourselves a lot of grief and agony further downstream.

MEMBER FORD: You've got the data scattered around the one to one correlation like that what is your matrix of success?

MR. BAJOREK: It's going to be in the several parameters that were defined for reflood rather than PCT. The matrix would be to have all those at a reasonably small bias and uncertainty. Now, reasonable, I think in the past people have basically looked at 5 or 10 percent in bias and uncertainly on the order of 30 to 50 percent. A lot of that just has to do with the scatter of the experimental data.

MEMBER FORD: I was about to say that surely that the scatter is obvious in the experimental data.

MR. BAJOREK: Right.

MEMBER FORD: But your model should be able to predict that step.

MR. BAJOREK: For the different condition, yes.

MEMBER FORD: Well, for the -- that scatter is presumably due to uncontrolled experiments, but you can quantify that, the degree of lack of controls. So can you not -- would you not -- your matrix of success be that you can bound your observed scatter? Not only in the experiments, but also in the reactor? I mean that's the uncertainty --

MR. BAJOREK: You would hope that if you define, let's say, bias and uncertainty in a model, when you apply that in the separate effects simulation, you also can show that you've bound or you

-- excuse me. You bound it in the separate effects tests and you are confident finding delta C in the whole scheme.

MEMBER FORD: The wider reasoning behind my question is that asking for the matrix of success if a licensee comes in with their own code, do you use your code? I mean, I know why you're touting your code, to be an informed reviewer, but at what point do you say this model is no good based on a matrix like that in comparison to the observational query. Does yours do better than he or --

MR. BAJOREK: That's what we're hoping.

MEMBER FORD: And if that happens, then do you say he can't use his code?

MR. BAJOREK: No, because I think what happens is if you do a good job on your code, you should have a relatively small uncertainty when it's propagated. If you did a poor job on the code, that should grow.

So, if you come in with a code that does not perform well against separate integral effects tests, the price you pay is a larger uncertainty of the whole scale application. My twist on that is if your code doesn't have the right sensitivity, I guess that's a question we have to look into.

CHAIRMAN WALLIS: Steve, we're going to see you after lunch?

MR. BAJOREK: Yes.

CHAIRMAN WALLIS: What I propose is that we break for lunch and we get back here by 1:00? Can you do that, have a quick lunch.

MR. BAJOREK: How much time do we have after lunch?

CHAIRMAN WALLIS: 2:30. We'll be back here at 1:00.

(Whereupon, at 12:33 p.m. the meeting was adjourned, to reconvene this afternoon.)

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

(1:08 p.m.)

MR. BAJOREK: This afternoon what we'd like to start doing then is looking at and reviewing some of the work that has been done over the past year on the experimental programs that we're relying on right now to solve some of the major thermal-hydraulic issues, and also to give us some additional data for the code development.

The ones that we're going to talk about, a couple of these we may move quickly because we've talked about these back in July, are:

APEX, work that has been going on there to address the pressurized thermal shock; work that has been going on at the PUMA facility, Purdue University.

The work that has been going on to take a look at critical flow, and we anticipate using the facility to take a look at the BWR boiling instability, the flow instability.

The rod bundle heat transfer program at Penn State.

ATLATS or the phase separation work that's being done also at Oregon State University.

I'm going to present some work that has been recently given to us by Vijay Dhir at UCLA looking at subcooled boiling.

And finally, we'll wrap up taking a look at the interfacial transport project that's being done at Purdue by Dr. Ishii, also Kajasoy at the University of Wisconsin, Madison.

But for the work that's been done in 2001 in APEX, APEX in late 2000, maybe a little bit earlier, had been modified to look much like a combustion engineering unit. It took advantage of the fact that the APEX facility in its original format for the AP600 had a 2 x 4 loop, the pumps were replaced. Excuse me. The can pumps were replaced in the APEX facility with loop seals and pumps so that it would look much like Palisades and Calvert Cliffs.

Most of the experimental work that has been going on in APEX over 2001 has been designed to take a look at PTS issues.

Now, we presented a lot of this information in July of this year when we also got to see a test at APEX. And I've got a couple of overheads to summarize the PTS work.

Most of the work that is going to be planned at APEX for 2002 is going to be directed towards the AP1000. Dr. Rais was recipient of a DOE MURE grant earlier this year. This gives him funding now to modify the APEX facility to replace the heater core, enlarge the pressurizer, change the core makeup tanks, add some additional instrumentation so that it looks much more like the AP1000. And that's the work that will be going on later in 2002.

Now the PTS work that was being done at Oregon State was the central part of three overall components to take a look at PTS. OSU, the APEX facilities, was used to develop the experimental database, look at downcomer mixing effects. This was accompanied with RELAP and REMIX calculations to try to gauge how quickly these plumes would dissipate in the downcomer. This was accompanied by a thermal-hydraulic uncertainty evaluation that was done at the University of Maryland.

CHAIRMAN WALLIS: Is that still being done?

MR. BAJOREK: It's finishing up right now.

Following the meeting in July/August time frame, Dave Bissette decided that they needed some additional tests to add to the ones that had been previously done. They started doing those in September/October time frame. As of October they were almost done. I think they still had a couple more to do.

CHAIRMAN WALLIS: Final report is in December?

MR. BAJOREK: December. Yes, the end of this year.

The work in the facility as it is right now, scaled as I mentioned to the CE plants, the work that had been done --

CHAIRMAN WALLIS: The question is whether or not this has been adequate to resolve the PTS -- they've done something to my mike? They took it away. Someone took it away. Oh. Yes.

MR. BAJOREK: I believe it is. Is Dave Bissette here? I think he's left. But --

CHAIRMAN WALLIS: There's a mike here.

MR. BOEHNERT: No, it's the table mike.

CHAIRMAN WALLIS: There's a mike here. So we're okay?

MR. BOEHNERT: Yes.

MR. BAJOREK: From my understanding, they're going to be able to wrap up the tests this year, issue the final report and I believe that is going to resolve the PTS issue, which leaves us for upcoming events.

The early part of the year will be occupied primarily with finishing the testing, writing the final report. Excuse me. Not the end of this year, that's going to be due the end of January. But starting the end of this year and into most of 2002 leading towards the end of the summer, the facility is going to be modified. The larger-diameter heater rods in the core are going to be replaced with smaller-diameter rods. They're going to put in a new data acquisition system.

The pressurizer in the AP1000 is substantially larger than it is in the AP600. That's being replaced. Likewise, the CMTs. CMTs are larger in the AP1000, they also have a different type of orifice to reduce the form loss from the CMT into the DVI line.

CHAIRMAN WALLIS: Do you know what the licensing schedule is likely to be for 1000?

MR. BAJOREK: Right now we're scheduled to issue an SER early next year. I'm not sure exactly --

CHAIRMAN WALLIS: So these tests will come after the SER has been issued?

MR. BAJOREK: I'm sorry. SER for phase 2 of the review. Phase 2 of the review is taking a look at the codes, for their adequacy, taking a look at the test and the analysis program. And we're going to be issuing our opinion on those, probably March or so of

2002.

Phase 3, Westinghouse decides to go ahead, they would be issuing their analysis, the finalized design and then going through the rest of the review. I think the SER for that would have to be sometime late in 2003/2004.

CHAIRMAN WALLIS: So your results will be timely enough input?

MR. BAJOREK: Yes. Yes. It seems very aggressive and ambitious, but they're hoping to do all of this modification to the facility and be able to begin hot down testing the end of next August. If that were the case, testing would begin later in 2002 and probably continue well on into 2003.

CHAIRMAN WALLIS: Now is the government doing some analytical work to figure out what key tests need to be run?

MR. BAJOREK: DOE asked us several months ago to comment on the text matrix. They made it clear it is their test. We gave them some recommendations based on previous tests that have been run in the AP600. The ones there that had been the most interesting from a licensing standpoint were the DVI line breaks, cold leg breaks, okay, where you had multiple failures and failures of the ADS-4 system.

Those are the ones that gave the minimum inventory in those tests. And we would presume that those tests would also generate the minimum inventory in the AP1000.

At the top of the list is the DVI line break. That one, by far and away, seemed to be generate the minimum inventory.

One of the things that I've been involved with over the summer and the last couple of months has been in a scaling analysis for the AP1000 in the test program. Part of our concerns stemmed with what will go on in the facility -- or more, the full scale plant during this ADS-4 period.

The DVI line break is clearly going to make entrainment in the upper plenum pool, entrainment in the hot leg, into the branch line much more severe than it was in the AP600. Going from AP600 to AP1000, that's a 73 percent increase in the core thermal power.

The vessel is the same diameter. The hot leg is the same diameter. So having this additional core power is going to greatly increase the superficial velocities during the ADS-4 period and also during the long-term cooling period. So we're looking at that.

We did make a recommendation that they add instrumentation to try to get the branch line quality--

CHAIRMAN WALLIS: That's the sort of thing I had in mind. If you'd thought about what are going to be the big differences that we need to worry about, therefore design the experiment so they focus on the right thing?

MR. BAJOREK: Yes. But those tests will start later in the year and I look forward to seeing some of those results.

PUMA is the integral test facility that represents the SBWR. It's located at Purdue University. It's an integral test facility that has a reactor pressure vessel, internal components to represent the core, downcomer, chimney and separator.

Most of what has been done over 2001 has been used using the the facility as a separate effects test. Now, this also stems from work that was noted in AP600, again during this ADS-4 blowdown period. Rather than critical flow at higher pressures being the most important break flow phenomena or range of conditions, during the ADS-4 we have critical flow at a relatively low pressure.

During the AP600 analysis using RELAP it was noted that one of the deficiencies in the code was the performance of its critical flow model during this lower pressure period. They've been gaining some additional information in the facility corresponding to these lower pressures, making use of some advanced instrumentation.

The long range intended use in 2002 is to start to look at the BWR flow instability problem.

CHAIRMAN WALLIS: You're saying that PUMA is going to be used to look at critical flow at low pressures?

MR. BAJOREK: It has been. It has been.

CHAIRMAN WALLIS: It has been?

MR. BAJOREK: And what I'd like to do now is Weidong Wang has a few overheads to describe that work.

MR. BOEHNERT: What's the issue with flow instability? Has it been looked at or -- are you going to talk about that?

MR. WANG: Yes.

CHAIRMAN WALLIS: I suppose that these valves are going to be tested anyway for ADS-4?

MR. BAJOREK: Not the ADS-4. They've tested the ADS 1 through 3.

CHAIRMAN WALLIS: Oh, it's too big to

test?

MR. BAJOREK: Right.

CHAIRMAN WALLIS: Blowdown from a valve isn't something you predicted, right?

MR. BAJOREK: Yes. The AP1000 you get a preset line size --

CHAIRMAN WALLIS: And it's like a straight pipe?

MR. BAJOREK: Yes.

MR. WANG: My name is Weidong Wong. I'd just like to give you a little overview about PUMA project.

I will basically deliver an overview of a PUMA project and also talk about critical flow and why we do that, and also inflow instability and the status for the plant for the coming year.

This PUMA facility is the only operational facility for the next generation SBWR in the United States. And the facility is a scientifically scaled from SBWR and it has extensive instrumentations, over 500, for flow void fraction. And Steve just went over all this. And I just give you a few pictures, cartoons that let you have a better idea about what it looks like.

This is a schematic of what this PUMA

facility. And they have a reactor vessel just very tall like a pen here, a pen. And this is containment, and it's a compression pool, basically it's a separate component and connected by pipes.

Just give you a few pictures so that you know what we are talking about.

And this is the size of the dry well containment. You do not have these pictures because I have difficulties because it's white -- black and white. And it's not real clear. And here inside is the vessel and the people are working here.

So you will see it's a pretty large facility.

And this is a control room, and there are people working there. And they have extensive instrumentations and they're all monitored by these computers or televisions, because we can see the bundle and the boiler fraction goes through core vessel or in the compression pool.

And Y which is for the critical flow, as Steve just mentioned about, actually we know critical flow from the light water reactor is important under low pressure, because either that they are all AP600, they have automatic deprivation systems.

And at a low pressure, basically

mechanical non-equilibrium for the liquid phase and the vapor phase, so the last thing is that can be large due to density ratio. And also thermal non-equilibrium can be large.

And for our code TRAC-M and RELAP5 we note basically it's assessed for the pressure above 500 psi.

And the shortcomings for the previous tests, first of all, we know it's not -- they do not have a detailed in-line measurement for the critical flow and also no systematic experiment to address the mechanical non-equilibrium and thermal non-equilibrium and the pressure effects.

And I tried to quickly go over some examples results and then give you the conclusion, because of time.

This is one of the example results. Pressure effect for the slip ratio. You will see from here the quality -- okay, this here different pressures. With the low pressure like for 30 psi with the experiment we have to go either from 30 psi to 150 psi. And you'll see this slip ratio can be very significant here.

CHAIRMAN WALLIS: This is for what? Flow through a nozzle or something?

MR. WANG: I have two plugs, actually. We plug both for nozzle and for the orifice. For this particular one, it is for the nozzle. And basically for the nozzle and for the orifice we saw the same trend.

And this can tell us, you know, for the the AP600 application -- first, we had difficulties with original critical flow model. Then later we developed some temporary or interim critical flow model. Use Henry Fausky, which is a homogenous type of flow model. And then here you will see, at a very low pressure, the slip ratio can be high.

MEMBER SCHROCK: Of course, your earlier experiment wouldn't be very good for flashing critical flow, would it?

MR. WANG: We have went through -- basically we tried to study this mechanical non-equilibrium, thermal non-equilibrium, and for chemical non-equilibrium we used air and water. And for the thermal non-equilibrium we used super-cooled water and the flow between we basically used saturated water and steam to make this experiment.

MEMBER SCHROCK: You misunderstand the intent of my question. As liquid is vaporizing it's adding more momentum mass to the vapor phase. That phenomenon doesn't occur to your water experiments. So the question, is the adequacy of information from air-water experiments in flashing steam flow experiments.

MR. WANG: Well, certainly, I'm not sure for the answer, but I think that we have a parametrical study basically for this -- our objective to get mechanical non-equilibrium and here, if we have a pressure, it's high enough.

CHAIRMAN WALLIS: Well, this slip ratio is very dependent on the flow regime. If you have flashing mixture which is breaking up into droplets because of flashing, it's very different from something like an annular. So you have to be pretty careful about duplication.

MR. WANG: This slip ratio actually is measured above the break point. It's not really at the choking plane And basically we've --

MEMBER SCHROCK: Measured where?

MR. WANG: Above the break point. This measured in -- we measured the void fraction and then we measured the quality. And these void fraction, we measured it by impedance meter, and this quality is computed by the inlet of this critical flow. And from this correlation we calculate -- from this equation we calculated the separation and tried to see under this low pressure condition this slip ratio of water relation or with slip ratio with this -- with pressure. And certainly this one can not really represented as the choking plane but it can tell you something about the slip ratio. It's important at this choking plane.

I'll give you another example for the test results. Subcooled water. And from here I have showed 150 psi for the orifice and a nozzle. And therefore we focus on one of this same pressure and we noticed that for the nozzle have a higher critical flow mass rate. And for the orifice -- for the orifice it is smaller.

CHAIRMAN WALLIS: What area is this based on? This is based on the total area of the orifice hole?

MR. WANG: Yes.

CHAIRMAN WALLIS: No vena contracta or something?

MR. WANG: Right, for the orifice, right.

CHAIRMAN WALLIS: You expect something like this from the contraction.

MR. WANG: Right. And here, we tried to -- basically tried to see what is important factors for this thermal or mechanical non-equilibrium and we concluded basically -- I say we, this project is done at Purdue University -- and we have concluded because say if you only have this orifice and the orifice, since it's short, and it doesn't have much time for the liquid to evaporate. And we expect some kind of higher critical mass flow rate, but we see here it's smaller. And we conclude that basically it's a mechanical non-equilibrium is more important than thermal non-equilibrium. This is all that we wanted to say here.

CHAIRMAN WALLIS: When you compare a nozzle with an orifice, how do you decide what's the effective flow area for the --

MR. WANG: We use the same flow area.

CHAIRMAN WALLIS: You don't have a contraction coefficient?

MR. WANG: Yes, we do not. We'll use that, but we expect, of course, for this orifice you have a higher loss, but here we focused on the orifice, first for the geometry basically for the -- for the nozzle, the lighter liquid -- lighter vapor have a high acceleration. This is also is not a explanation. But we really try to here to see what the effect of the thermal or mechanical non-equilibrium

MEMBER SCHROCK: I don't think you've got it right. Critical flow rate is going to depend so strongly on where the flashing begins. The orifice is not going to behave like a nozzle. I think you're seeking an answer to a question which is -- may be a reasonable question, but I don't think your approach is going to get you there.

MR. WANG: Okay. We'll feed it back this to Professor Ishii and we'll have more discussion and try to get back to you.

And these tests show you the examples related to this, because the code cannot really predict the data well. This is the RELAP5 prediction for this critical flood, 30 psi for the orifice case. And also we should check on here there's some bigger problems.

Just gave a summary of this program.

And for the critical flow we have basically 15 to 25 percent higher flow rate for the nozzle than orifice. And we notice a larger slip ratio with the lower upper-stream pressures.

MEMBER SCHROCK: In the Purdue work is their critical flow measurement using water?

MR. WANG: Basically we have -- the upper stream is controlled and we use a steady state.

MEMBER SCHROCK: Well, you've shown us data for air water systems. And I would argue, just in general, one should not expect to get critical flow phenomena where splashing water is based on air/water measurement. They're quite different systems.

CHAIRMAN WALLIS: Well, I think that the subcooled water test must be for water alone.

MR. WANG: Subcooled water, yes it's for water alone.

MEMBER SCHROCK: Water alone?

CHAIRMAN WALLIS: So what you're telling us is that the code's probably not going to give a good prediction for this ADS-4?

MR. WANG: Right. And we tried to get some data and see, in the future model development, we use this data to develop some better model or at least we have some data here and maybe for the critical flow models there's some adjustment we can use to improve the prediction.

CHAIRMAN WALLIS: Presumably, AP600 was licensed on the basis that the critical flow predictions were okay.

MR. WANG: We do have done some work to predict this -- improve the AP600 and --

CHAIRMAN WALLIS: After they built it, then they have to open ADS-4 on a running reactor?

MR. STAUDENMEIER: Yes.

CHAIRMAN WALLIS: I thought they were going to be full scale tests.

MR. STAUDENMEIER: For the valve

CHAIRMAN WALLIS: That's going to be the proof of the pudding.

MR. WANG: Thank you, Joe. And also actually, maybe the flow regime use is --

CHAIRMAN WALLIS: I'm suggesting that since this is critical flow, maybe Professor Schrock should see whatever reports are coming out of Purdue. Can you do that?

MR. WANG: Sure. Right now I have a draft report, so I will try to -- but right now we only have a draft report, so it's not --

CHAIRMAN WALLIS: So it's a useful time to review it.

MR. WANG: Right. Actually, I try to get it to you as soon as possible.

MS. UHLE: We'll give that to you.

MR. WANG: And the flow instability, it's planned for this year and next year. Why we do that is because we saw some flow instabilities for the operating PWRs and also for the AP600. Small break LOCA we noticed there's a lot of flow instability. And we would like to get some data to assess TRAC-M.

For advanced BWR based on natural circulation, for example, like SBWR. And this -- based on natural circulation pressure is more prone to instability, especially during start up because there's no forced flow which you have a -- if you have forced flow, you reduce a chance to have instability. And flow is determined by natural circulation and void fraction. And the power affected also by the fraction, as I've said, some feedback and there's some strong covering.

And for the largest scale experiment which takes data from simulated material is not available and the effect of void fraction, feedback and also time lag for this -- convection time lag is not studied -- that is our objective to try to --

CHAIRMAN WALLIS: PUMA isn't a nuclear facility, is it?

MR. WANG: It is not, but we try --

CHAIRMAN WALLIS: How do you do void reactivity power --

MR. WANG: We try to use some parametrical studies. For example, we can measure inside the core, we can measure the void fraction and from the void fraction, we can use our kinetics code to get some -- the power feedback on the zed power.

CHAIRMAN WALLIS: Can you program the power to reflect the void?

MR. WANG: Yes. And for time lag also we do similar trick. Say, for this use electrical rod. It's not nuclear power and the time lag, they will be different, but we will try to find out from the nuclear fuel and for the time lag how much, then we will try to control the electrical power to delay the --

CHAIRMAN WALLIS: So electrical power was a shorter time response --

MR. WANG: Right, it's much shorter.

CHAIRMAN WALLIS: -- so you could program in?

MR. WANG: Try to delay some certain amount so that it could match the feedback.

And this is basically our objective to try to obtain some instability data from this larger facility at the low pressure and also obtains experiment data for BWR and low point when reactivity you have a feedback. And evaluate TRAC-M for the ability to predict three different types of instabilities like density wave, flow excursion and the flashing-induced instabilities.

And also try to see the accuracy of prediction based on stability boundary and amplitude and frequency. And the ability to model effects of neutronics and thermal conduction time lag on instability.

And where we thought -- just a summary.

MEMBER LEITCH: How do you get low instability with power feedback. I thought it was the power feedback that basically led the PWR into an unstable situation?

MR. WANG: Basically in say we cover constant power. And if it's a perfect steady state it's fine. But if you have some perturbation for the inlet velocity, then instability can occur with certain geometries. For example, like density wave, if you have some inlet velocity perturbation, then the boiling lengths will be changed. If the boiling lengths has changed, then there's a pressure drop across the channel because a two-phase flow in a single phase pressure drop it's kind of automated and it contains. And that effect can propagate into the system if system just have like out of phase and you have density wave --

CHAIRMAN WALLIS: I think if you change the words a bit, if you said data on BWR transient flow response, but you could sort of vary something and then look at the transient response. It doesn't have to be unstable just strictly to produce a transient response which could then lead to instability when coupled. It doesn't have to be unstable for you to measure these kind of times in response.

MR. WANG: And basically I just revealed-- actually I have a summary --

CHAIRMAN WALLIS: Does this feed into a summary of regulatory response to say BWR, how it operates?

MS. UHLE: Yes.

MR. BAJOREK: It's part of the synergy program.

CHAIRMAN WALLIS: Part of the synergy program. We'll find out two years later whether or not we made the right decision on it.

MR. BAJOREK: According to General Electric they stay away from those regimes where they would get these instabilities.

CHAIRMAN WALLIS: According to General Electric there's no problem at all.

MR. WANG: Well, in summary, for the inflow instability -- actually we only can say the status -- right now we have done some analytical study and basically found out we have to reduce some payloads in the inlet in order to have some instability. And that is where we are, and we will start to do our experiment very soon.

CHAIRMAN WALLIS: Thank you. Stole your mic? I guess you'll have to speed up again.

MR. BAJOREK: Okay. The way I think I'm going to do that is by not spending a whole lot time on the rod bundle heat transfer project.

Joe talked this morning about the type of data that we hope to get out of the facility. Just by way of background, the facility itself is full height, very well instrumented, essentially a 7 x 7 bundle with the corner rods knocked out for a total of 45 rods.

The rods are protypical, not only in length but also in diameter.

An interesting feature about the rods. I'm sorry.

CHAIRMAN WALLIS: Well, I guess we've seen a lot about this before. The question's always been when we going to get some results?

MR. BAJOREK: The results we hope are going to be coming in later in spring, early summer this year.

As I mentioned, most of the work that had been done at Penn State this year has basically been in bundle construction, shaking down the facility, putting in supply tanks, putting in the DC power supplies. Their schedule right now is to begin testing in, I believe, April of this year. They're going to start with a battery of about 15 tests. Those will be reflood tests and then continue further on in the year looking at steam cooling tests and then tests where there would be steam and droplets injected at the bottom of the bundle.

CHAIRMAN WALLIS: My concern has been this is an expensive long term program that someone's going to cut the budget before it gives you any data at all.

MS. UHLE: We will be finishing our reflood data, the first phase at any rate, by the end of this calendar year.

MR. BAJOREK: It should be January.

MS. UHLE: Right.

CHAIRMAN WALLIS: It would be good, though, to start showing some results as soon as you can so that you can show that the program --

MR. BAJOREK: That's why I guess the reflood tests are going to be up there first. Try to get the most important information and then build things later on.

MS. UHLE: We're also in the discussion with Korea to extend the program with some of their grid spacer designs in a collaborative effort to extend the program to get even more data.

MEMBER SCHROCK: So does this imply that pressurization problems will have been resolved?

MS. UHLE: Yes.

MEMBER SCHROCK: Yes.

MR. BAJOREK: Okay. The next facility that I want to talk about is the ATLATS for the phase separation. But kind of as a lead-in to that and the problems that we're observing in the ATLATS, I want to put that in light with what we're seeing from the AP1000.

As I mentioned earlier, one of the big differences in the AP1000 compared to the AP600 is the very large increase in the total core power. We're going to see much large superficial velocities at anytime during the transients that we observe in the AP1000.

They've changed the resistance of the ADS-

4 line, greatly reducing the resistance. They've also reduced the resistance of the CMT. They made some other changes to the PRHR. But primarily with respect to entrainment processes, it's been the increase in core power that's really going to drive things.

As I mentioned, we have been doing some scaling evaluation from a top-down scaling perspective. Westinghouse doesn't have too bad of a story. Actually what they've done in the AP1000 by increasing the ADS-4 valve, they've made it look actually a little bit more like the SPES facility. So when you look at the scaling parameters early on, it's even better agreement with SPES, which they have used for code validation, than the AP600.

Later on it still looks very much like APEX in the OSU tests, not too far off. Critical period where we're having some heartburn showing from a top-down scaling perspective whether OSU is okay is during this ADS-4 period. Part of the question comes into what is the critical flow, as the pressure decreases, what's the quality that leaves the ADS-4 line?

One of the issues that is definitely going to be -- have to be taken up in the phase 3, however, are items that come from a bottom-up scaling. Well a bottom-up scaling looking at more localized processes in the core, steam generator, where else in the facility.

Where we see problems right now from the scaling leads us again to look at phase separation at the hot leg leading to the branch line.

These figures don't show up very well, but the situation we feel that is going to become even more important in the AP1000 than it was in the AP600 is this condition where we have a froth going up into the hot leg and we're entraining some fraction of that into the branch line and out. Now, this factors back to the safety of the system and in the analysis, because if we start to entrain large amounts of fluid, you get a larger two phase pressure drop and that delays the time at which you transition over -- excuse me, drop to a low enough pressure that your IRWST can begin to inject into the system. The question mark if that period probe is too prolonged and you have too high an entrainment, too high a boil off, you lead to some part of core uncover.

The second question that is arising from our bottom-up scaling is what goes on in this type of a scenario where it might be a DVI line break? You don't necessarily have a level pushed up into the hot leg, but now we have a high-quality froth above the core. The question is how much of that becomes entrained in the gas flow, eventually up and out the branch line.

Now, I think everybody remembers seeing some of the test results in the ATLATS facility back in July. And by way of background, the basic reason for having the ATLATS arose from some of the AP600 beyond design basis tests that had been run which showed that there was some core dryout when they started with a lower inventory in the vessel. The RELAP couldn't predict that.

We have a situation where we're showing hints of core uncover, the codes aren't predicting it and we know it was due, primarily, to not being able to predict entrainment in RELAP.

Now, we saw the facility, saw some of the some film clips and also saw how the facility behaved in July. And the meeting, unfortunately or fortunately, noted that, hey, there's some significant problems with the facility.

First and foremost, they're system-dependent oscillations. It seemed to make a very large difference depending on whether they have blocked off at the steam generator, have a line open from the steam generator back to the top of the upper plenum. Other comments that we have received at the meeting is that there was an inappropriate use of the previous data and existing correlations in their most recent model development.

The model that they were developing seemed to assume some type of an annular ring around the bottom of the branch line. This was the physical picture that was being used to develop a newer model for onset branch line quality.

And, Dr. Schrock, you have made a number of comments on the references that they were incorrect. Previous comments have not been incorporated. And that their use of this person's data and this person's correlation was at least very confusing, misleading and probably wrong.

CHAIRMAN WALLIS: This just doesn't just affect the entrainment, it affects behavior of ADS-4. In ADS-4, the choke flow, you sometimes see steam, sometimes you see it very wet or even a slug of liquid coming along, that changes the flow rate out of ADS-4 to the entrainment, and you can set up conciliatory behavior. You may need to be able to model conciliatory behavior, not just some average. I'm not sure they're doing that.

MR. BAJOREK: No. What I want to go over are the types of things that we've started to do since that meeting. I'm sorry.

MEMBER SCHROCK: No, finish what you were saying. I'm ready to comment further on the summary of what we learned from that meeting in July.

MR. BAJOREK: Okay.

MEMBER SCHROCK: Are you done?

MR. BAJOREK: Well, I was going to go into the things that we're going to be doing with the facility and the things that we're going to try to do --

MEMBER SCHROCK: In terms of the answering Dr. Wallis.

MR. BAJOREK: I'm sorry, I'm not sure what you're asking right now.

MEMBER SCHROCK: Well, what I'm trying to make a comment on is this summary of the things that you found to be significant problems in the old facility as a result of the meeting in July. There are, in my mind, important aspects of that that are not reflected in this statement. One of them is the fact that the code attempts to solve the problem by saying it knows what the flow regime is. When the flow regime is satisfied there's a potential for entrainment of liquid in the branch line. For that to be a reasonable proposition, you have to see that in the experiment, in fact, you get stratified flow. In that experiment you did not get stratified flow. You had a sloshing back and forth.

MR. BAJOREK: That's certainly correct.

MEMBER SCHROCK: No stratified flow evident, and therefore that's the number one question I think to be addressed, is your problem in running RELAP, TRAC -- whatever code it may be that you're trying to do the calculation with it --is the difficulty that you have the flow regime wrong or is the difficulty something about the model that you use if the flow regime is right?

MS. UHLE: Can I answer it?

MEMBER SCHROCK: So that you haven't addressed that issue, and I think that's step number one in coming to grips with how you're going to get something out of the OSU facility that will solve your problem.

MS. UHLE: Can I answer at this point, at least, do you mind?

MR. BAJOREK: Go ahead. Go ahead.

MS. UHLE: Okay. I think what was shown to you at the OSU facility was the goals of the facility was to look at essentially each flow regime, first starting with the horizontally stratified and going into the intermittent regimes as well. And what the movie that was shown to you was looking more at the intermittent. And we have data from --

MEMBER SCHROCK: Well, we weren't looking at a movie, we were watching what was happening in the facility.

MS. UHLE: Well, okay. I wasn't there. So you saw not a movie, but the real facility.

That was for an intermittent regime. We do run in horizontally stratified mode. And there was date taken for the horizontally stratified. I don't think that was communicated to you because it was Research's goal to develop phase separation models spanning all flow regime and horizonal pipes.

And the reason why we looked at -- or the first attempt was to see if we could, regardless of flow regime, come up with a correlation that just looked at, say, average level and superficial velocities was because of exactly what you're saying. That the code, if you took this model and you applied it across all different flow regimes, the answer you would get would be dependent on what code you're using and its prediction of flow regime. And so they tried to come up with factors such as average level superficial velocities regardless of flow regime to come up with your entrainment rate. And that didn't pan out.

So what will happen now is we will, unfortunately, have to rely on the fact that you know your flow regime and take the data and make sure that we are consistently determining the flow regime for the horizontally stratified case as well as the intermittent, the wavy.

MEMBER SCHROCK: The flow regime that we saw was -- the flow condition which is not described by the flow regime maps.

MR. BAJOREK: I think your basic question is okay, you have this condition in the facility. The flow regime maps and the code right now, and for at least the next several years, are static. They cannot track waves or track the development and change of one flow regime down a pipe. In the long run, we would hope we would hope that we would get that type of thing out of the Purdue or, actually more appropriate, the interfacial area transport being done out of University of Wisconsin.

For right now we're kind of stuck with regimes we have.

MS. UHLE: And they are looking at the fact that they were getting reflection from the steam generator side and getting rid of that to run the intermittent tests, you know, getting a flow regime that is not --

CHAIRMAN WALLIS: It maybe required that you ask them to develop a general correlation entrainment out of the branch but using a geometry that looks like AP1000. Because if you had a long pipe instead of just try to find a flow in the other branch, you might get something completely different at what you see. Maybe you ought to be focusing more on what actually happens in something which simulates AP1000, therefore results might be at least useful for analyzing AP1000. Don't claim this is some sort of scientific study of a branch pipe under other conditions.

MS. UHLE: But it's not just protypically AP1000. I mean, in some ways PWR or hot leg pressurizer. I mean, there are a few LODs down from the --

CHAIRMAN WALLIS: How many LODs do you actually get? Maybe you could analyze that, what will actually really happen, get a good correlation in terms of models of what really happened and not try to mix in almost for something else, with 1000 --

MS. UHLE: But in a reactor, I mean you're typically more fully developed. So the point of having a really long, horizontally stratified regime, there's no place in the reactor that you ever would be fully developed.

CHAIRMAN WALLIS: Right.

MS. UHLE: Right. So we're trying to identify the horizontally stratified in a sense that as horizontally stratified as you can get in a protypic reactor geometry. You know, that's the hard part.

MEMBER SCHROCK: Well, what we saw in Oregon was explained to us as the experiment that was used to produce conclusions about the containment problem and in fact the level shown as a level which is determined by reducing the flow rate until it ceased to have liquid entrainment.

MS. UHLE: And then they came back up in the other direction and it mismatched.

MEMBER SCHROCK: I didn't see any coming back up in the other direction. I asked about it and there was no answer at that time. Maybe those came after that question was asked; I'm not sure. But I think -- what I'm reading here doesn't convince me that you have a clear picture yet of sorting out what you're going to have to do to get useful information out of the OSU experiment.

CHAIRMAN WALLIS: I think that will probably be in the conclusion. Sometime we're going to summarize.

MR. BAJOREK: Well, let me go through and summarize the actions as we see them right now.

CHAIRMAN WALLIS: The pictures you're showing here of these double bumps -- that's not what happened?

MR. BAJOREK: No. No. No, that's --

CHAIRMAN WALLIS: That's a fantasy?

MR. BAJOREK: No. And I mean it certainly wasn't what we saw in the facility. But we think that the goal one on this --

CHAIRMAN WALLIS: It's an analyzable situation, but not relevant.

MR. BAJOREK: -- We think the first thing is the basic, we're going to try to better understand the system oscillations. The question is these oscillations as we see in ATLATS facility, do they also occur in the APEX facility, and how transferrable is the information that we're getting from ATLATS to the full-scale AP1000? The scaling that was done for

ATLATS, as well as for the OSU hot leg and branch line, were based upon having the right void fraction in the upper plenum, the right L over D between the upper plenum and the branch line, and the correct capital D to small d ratio between the branch line. It really did not look at anything on the length between the branch line and the steam generator, size and heights of the waves that might form in a pipe of diameter D.

What we would like to do is to try to understand that better to realize whether the waves as we see them at ATLATS are also going to occur in the larger scale facility. That would be a review of the scaling criteria.

Now, we did take Dr. Wallis' suggestion and asked them to run a series of tests in which they injected into the top and this figure shows what had been intended to be porous injectors to go into the core -- it doesn't show up very well at all. But it does have an auxiliary air port by which we an do injection into the top of the facility.

They ran those tests, they sent those to us earlier in the month. We haven't had a chance to go through those in great amount of detail, however my observation in taking a look and plotting the liquid levels, there still are a very high amount of oscillations. It does not appear as to whether that smoothed things out.

We've since gone back to them and asked us based on those oscillations what are some of your averaging procedures? Because we see a lumping of much of this data, some of which Jennifer noted was horizontal stratified. If we had those movies, I think we'd be able to see that. A lot of it was intermittent.

We think at this point we need to start segregating that data into information which was truly horizontal stratified and something that it is intermittent, wavy, what other type of flow regime that was apparent.

In future work we're going to request that they supply a CCD or some other recording of what that flow pattern was. Our expectation and understanding going into the meeting is that we were going to see a lot of horizonal stratified flow. We'd like to try to get that recorded in addition to the comments that they do have in the test reports. You have to dig to find them, but there is a visual observation on what that is. We'd like to start with that and segregate out the points and get them into their appropriate regimes right now.

CHAIRMAN WALLIS: Just want to make a point that joining of the hot leg and the vessel is accomplished by intersecting two cylinders leaving you with a sort of strange, sharp edge around that opening. That doesn't exist in the plant. It could be of some significance in the hydrodynamics that you're looking at here.

MR. BAJOREK: Okay. With respect to the references and their use of the data, we've also asked them to supply all copies of the references that they've been using in assimilating their report and plan to ask them to rewrite that section where they talked about their literature search.

Based on the information we got, we agree with you that it's confusing and misleading the way things have been lumped together. Some of the reports are difficult to get. We've asked that they supply them to us. We're going to do our own review.

We feel that the model development needs to be revised to be more regime-dependent. If we can some day lump everything together, that would be the simplest thing for the code application. But based on what we've seen, we should use the horizontal stratified data, keep that with models which are appropriate for horizontal stratified flow regimes.

This in your ring picture that seems to have been used in their development certainly didn't show up in the experiments. And what we would prefer is, rather than this figure over on the right which they assumed, go to a picture which a similar model had been devised by, I think it was Yanamoto, where the picture, physical picture of the fluid beneath the branch line is something that is forming more a conical or a pyramid shape. At least that physical picture looks -- corresponds much closer to what we --

CHAIRMAN WALLIS: But if you have a photo that looks like a sketch?

MR. BAJOREK: There is one in the report.

CHAIRMAN WALLIS: We found a situation where at least at the moment it looked like that picture.

MR. BAJOREK: In fact, it was interesting how they did it. They must have had a boroscope inside the pipe looking axially. And you can see almost a formation of a water spout.

CHAIRMAN WALLIS: Well, maybe it does for some regimes, but what we remember very much was large-scale oscillation of the whole pipe.

MR. BAJOREK: Yes. Yes. In fact, the

oscillation was between the branch line and the steam generator.

CHAIRMAN WALLIS: Okay. We have to move on to the next one.

MR. BAJOREK: Okay.

CHAIRMAN WALLIS: And then you can 15 minutes of summing up or so.

MR. BAJOREK: Okay. Subcooled boiling is work that is going on at UCLA. We had Professor Dhir come and present his results to us about 3 or 4 weeks ago.

Now, the work that's being done at UCLA is also in response to AP600 and AP1000, where there's a realization that most of the decay heat removal is now going to be done at lower pressure. We feel that the models for subcooled boiling would not be as good at the lower pressures as they were at higher pressures, typically where you would need them for small break LOCA.

The other question that's going to be answered by the UCLA work is the idea of heat flux splitting. How much of the energy and subcooled boiling goes into void generation versus sensible heating of the liquid? Right now whether it be in TRAC or RELAP, the models are largely ad hoc. Based on some limited test data to come up with the models, but nothing very mechanistic in the way its treatment of this heat flux split. So the objective of the UCLA work, very much like the Penn State for dispersal of film boiling, used advanced instrumentation and detailed facilities to get high-quality information by which we can develop these mechanistic models.

MEMBER SCHROCK: I don't know if you read the comments that I made in a recent report, but my recollection was in reviewing the -- TRAC's documentation back in 1987 was that they had heat transfer directly from the wall to the vapor, and cases where the wall was -- the flow regime map let it. So the transfer is nonphysical. It has to be to the liquid and then to the vapor.

So I think it would be useful if this could get sorted out in the way this was being described and state more clearly what this heat flux splitting is all about, and the way it's used in the code. It originated the subcooled boiling models and that got massaged and massaged and massaged and came out as a GE thing under Leahy's name. But I think that a lot of people have had confusion about what this heat flux splitting means. And I think most of those people have been people who work with codes, not the people with experience. It needs to be dealt with.

MR. BAJOREK: Well, I think there's the physical models, and I think what you're pointing to there's -- a lot of times the code -- you look at the hydrodynamics and you pick your flow regime on one set of conditions and then you take the wall temperatures and maybe some gross estimate of a void and say this is what's going on near the wall. But those physical pictures may not necessary correspond. You may predict a bubbly flow, who knows whether the bubbles are concentrated out in the fluid or close to the wall. Those selections have to be consistent --

MEMBER SCHROCK: For heat to be transferred from the wall to the vapor, you have to have a dry wall.

MS. UHLE: Right. But see, we have this problem --

MEMBER SCHROCK: In the physical work.

MS. UHLE: But in the problem of numerics if we're taking one second time steps, you can't do that because you would get way too much super heating of the liquid. And you can't get around that. So that's where some measure of realizing that you're in a numerical system in some way differs from reality, and that's why you have to rely on assessment.

MR. KELLY: May I make interjection? This is Joe Kelly from Research.

What Professor Schrock is alluding to goes way back, in forced convection flow they did things like void fraction volume --

MS. UHLE: Not necessarily. That's what I'm trying to say is the fact that if you're taking over a period of a time step of a second, you can't put all of the liquid, all of the heat flux into the liquid. And you know currently in our numeric systems or numeric schemes and we would have too much super heat the liquid and then the next time step you would get the interfacial heat transfer to the vapor. I mean, that's all I'm trying to say is that whether-- how we solve this problem we can talk about later, but why it was done in the past, it may sound weird to you but a lot of it is simply because we had to work in the numerics of the time. Now when we make the code more implicit, then we can get rid of those things. But the computer limitations in the past prevented that because we just didn't have enough memory or speed of the computers were too slow.

CHAIRMAN WALLIS: I think that again we're getting into too much detail. If we're going to review Professor Dhir's work, we're going to have to spend a whole afternoon.

MR. BAJOREK: Well, it would take quite a bit of time.

CHAIRMAN WALLIS: There's no way that we can get an overview of these programs beginning at that level. What I get from this is that there is a problem with predicting the amount of voids you get and the heat flux in subcooled boiling --

MR. BAJOREK: Yes.

CHAIRMAN WALLIS: And that probably sometime during the year we may need to look at this in more detail.

MR. BAJOREK: Yes. I think sometime in probably the spring would be the right time.

CHAIRMAN WALLIS: I don't recall actually having a presentation.

MS. UHLE: It came to the staff.

CHAIRMAN WALLIS: Well, maybe that's where we could contribute.

MR. BAJOREK: To summarize what he is working on. Breaking up the wall into several components using high speed visualizations. Two different test sections, one a rod bundle another a flat plate test section. Flat plate in order to give him things like nucleation site densities, motion of the bubbles, collapse rate of the bubbles as they leave the wall and then getting additional information from the rod bundle to augment that.

I've left in the package the types of measurements that are being obtained in the facility.

I'm going to jump to more conclusion, closer to the conclusions.

He's been successful at developing a model to predict the delta T at the onset of nucleate boiling in a subcooled flow, that seems to do a pretty decent job at predicting not only his own data, but a fairly substantial set of data that he also obtained in a literature search.

CHAIRMAN WALLIS: Your measure of success, this sort of a picture?

MR. BAJOREK: Well, this would be one of them. I mean, because he's trying to get the onset correct. He's also trying to get the right heat flux at the onset correct, simultaneously. Get the single phase heat transfer coefficients and also be able to get models for the bubble size and the rate of collapse of those bubbles. It basically gives you the condensation component of that split.

This shows the heat flux based on his

model to try to predict the heat flux, which is a contribution of a partial subset of those terms, and by in large it seems to do a successful job not only of his data but also on other sets of data.

CHAIRMAN WALLIS: Could you give us time to see what the state of the art was before he came along, and if he drew a picture like this based on whatever you were using before, is this an improvement?

MR. BAJOREK: Yes, it would. I mean, he's also done the comparisons to some previous models and you can see where the scatter is significantly larger.

MS. UHLE: Joe Kelly has a good paper on the subcooled boiling model if you'd like to see the current state of the art.

MR. BAJOREK: Okay. Now where he's going with this work now, he's gotten enough data on the flat plate test section. Most of the work that's going to be done in 2002 is to try to come up with a better term for this flux split, to get the additional terms in this heat flux contribution to the total heat flux during subcooled boiling, expand the data base, getting additional information for the rod bundle, increase the range of subcooling and look at some higher pressures. Right now everything is fairly close to atmospheric.

I think he can take the facility up to close to 3 or 5 bars. And he thinks that most of that work can be completed in 2002, which is why in the overall schedule we're looking at trying to implement those models later in 2002, but probably not in time to get into the Rev 0.1 release.

Okay. The last topic that we're going to have is looking at the interfacial transport, which has been done primarily at Purdue and University of Wisconsin. Jennifer's going to talk about that.

MR. BOEHNERT: Do we have these slides, Jennifer?

MS. UHLE: No, I'll get them. Because I thought that Steve had them, he thought I had them.

I've talked about this before at the ACRS meeting, so the objective of the interfacial area work is to get away from the static flow regime use in the code, the reason being is that for one thing, we need to use interfacial area in the code. It's a value that we have to have a closure relation for. It determines the interfacial heat transfer as well as the interfacial drag.

We currently model it using static flow regimes. I think everyone's aware of the fact that the flow regimes were developed in steady state situations, lots of air/water. At any rate, the transition criteria and the use of the static flow regime, it doesn't represent the actual physical processes of creation and the destruction of the interfacial area, so there's no time and length scale. And so if you change your flow rate; in some sense if you have an oscillation you instantaneously change your flow regime. That doesn't sound that bad, but if you consider the situation of annular flow and you increase your vapor flow rate so that it's beyond the point where you're entraining liquid drops, you can increase by several orders of magnitude the drag in the interfacial area of interfacial heat transfer so that it causes this oscillation and it can also cause some inaccurate answers.

So we're trying to develop a first order equation for the transport of the interfacial area; that is the objective. We realize --

MEMBER KRESS: If you have that, you no longer need flow regimes at all?

MS. UHLE: That is the goal, but before we can take out the flow regimes we have to have the data in the model covering all flow regimes in geometries prototypical of nuclear power plants. And that is the big effort. That's why we're trying to collaborate with France and Japan, and open this up for international collaboration, so it's a big scope. We are making progress ourselves but we realize that we had originally thought that Japan and France were going to provide the steam water. We're still working on that.

MEMBER KRESS: What is the position now?

MS. UHLE: I can go through where we currently are. I just want to point out, though, if we don't to the point where we actually do use this interfacial area transport equation for the flow regimes, this project is not useless by any stretch of the imagination because of the fact that we do use values for interfacial area.

We will be able to take these measured quantities of interfacial area and then compare them to the correlations that we use in the code currently to make sure that we are at least getting a prototypic value of interfacial area for the flow regime of interest.

So, again, if the modeling doesn't work out in the long run, the data is still useful.

CHAIRMAN WALLIS: This principle has a separate conservation equation --

MS. UHLE: Yes.

CHAIRMAN WALLIS: It's something you can could put into your TRAC now --

MS. UHLE: Yes, I did that. Yes, I did that. If you remember a couple of years ago where I put in the first or I put in the -- one group interfacial area equation. In other words, it was for bubbly flow. So by one group I mean that the vapor phase was all spherical, and therefore the drag coefficients were, again, first spherical configuration. And in --

CHAIRMAN WALLIS: So how long did it take you to do -- a long time?

MS. UHLE: Yes, it took me a week. It took me a week, and that includes modeling and comparing to the data, although I did call our numerics guru for a few challenges along the way.

CHAIRMAN WALLIS: You called this TRAC-M development.

MS. UHLE: Yes.

CHAIRMAN WALLIS: And the thing is if these guys are successful --

MS. UHLE: It'll go in easily.

CHAIRMAN WALLIS: -- in 2004 or something, put into TRAC as an implement?

MS. UHLE: Yes.

CHAIRMAN WALLIS: With an option or something.

MS. UHLE: Right. Now with the two group, it's going to take me more than a week. I was going to do it this year, but then I said I was demoted to assistant branch chief and they don't let me touch the code anymore. But I was planning on doing that to put in the two group equation. And there's a little bit more complexity with the two group equation, because you do have to solve a matrix.

CHAIRMAN WALLIS: All you have to do is delegate somebody younger and quicker.

MS. UHLE: I thought it was older and wiser.

PARTICIPANT: That's his answer why we hire lower grades --

CHAIRMAN WALLIS: I'm not sure we need to spend on this. It's going on it's processing --

MS. UHLE: It's going on. We're covering flow regimes. With respect to Professor Schrock's questions, we've covered bubbly flow -- sorry. For the co-current upflow we've covered all flow regimes up to annular. We're doing counter current flow. We've completed bubbly flow. Started to do co-current down flow for bubbly flow. Horizonal, we've covered all co-current regimes and we're starting to extend to other geometries, so we do have this database and comparing.

There are two group models that they've come up with, although it's not put in the code, they do compare to the data as they develop it.

In the future, we need to go to steam water for the source and sink term of the phase change. And, again, extending to just other flow regimes and geometries.

We're hoping to have the final model, you know, our ideal would be to have it in 2005 in the code and replace the static flow regime. It depends a lot on --

CHAIRMAN WALLIS: I hope that they are publishing results --

MS. UHLE: Yes. Yes. You haven't been reading *International Journal of Heat and Loss Transfer* then because, yes, we just published something.

CHAIRMAN WALLIS: You're jumping ahead with that accusation. You don't know what I've been reading.

MS. UHLE: Yes, we've been publishing.

Are we done except for the summary?

CHAIRMAN WALLIS: Well, I was hoping that we could talk about the papers

MS. UHLE: I can give you the papers, if you'd like.

MR. BOEHNERT: Yes, we'd like the papers.

CHAIRMAN WALLIS: I was trying to jot down as where we could interact with you in the future that would be profitable. My colleagues should come in on this. But I feel with the development of this consolidated code that what we should do is encourage you to keep up your enthusiasm for the activity but I think where we might contribute is in the documentation. Do you have draft documentation that we can look at and give you some input and avoid giving you surprises when we see it later on? Maybe we'll make the documentation better? I think in the other areas of Joe Kelly and company, doing work with their former knowledge of what they're doing than we are, I think that they go for it and we'd like to see the result. But then I think we should discuss what we need to do about each of the review of some of these other programs, USU, Penn State. But can we first look at other comments on the TRAC

MEMBER LEITCH: My question, I guess, or

comment is that I'm a little confused about what release means. Does that mean that it can only be used in certain circumstances? And if so what purposes? In other words, are there other grounds or to what extent -- seeing that we've talked about perhaps 8 or 10 applications and I guess what I think should be the outcome of the status of this by the end of next year. It would be useful to address these other applications. What will the status be? In other words, I guess we've seen a program that ends sometime at least in the next 13 months, but obviously the research effort is geared towards the targeted applications. But I'm just a little confused as to what will be value of using the TRAC code and the RELAP5 in these applications. I guess that's a reasonable question.

MS. UHLE: No, no. That's a very reasonable question, and it's a quick answer here. Is that by the end of 2002 we will be as good as the old codes for the targeted applications, so we can from then on rely on the TRAC code.

Now, the fact that RELAP5 is now the workhorse code mostly for the international community as well as for NRR, we foresee bringing that in-house and maintaining and it using it as a benchmarking tool as we continue the effort. So it's not like we'll be dumping RELAP5. But at that point we'll be able to use either code for, say, the PWR applications. We will, of course, think that TRAC will be better for the large break LOCA for the PWR.

We will be as good as TRAC-B used to be for the BWR applications. And we can do stability and 3-D kinetics for the BWR to replace RAMONA.

So, again, we'll be starting to focus and start this transition into relying on TRAC-M.

MEMBER LEITCH: The synergistic effects?

MS. UHLE: That will be done with TRAC-M.

MEMBER LEITCH: And the PBMR?

MS. UHLE: TRAC-M. Right. But we will by the next time we -- we say the next fall meeting, we will have the physical models in to do the PBMR. Hopefully, have identified data sources and at least have a few plots to show with respect to system behavior.

MEMBER LEITCH: I guess just to paraphrase, I think what I heard you saying is by the end of next year this is when it comes out, TRAC-M will be equal to or better than?

MS. UHLE: Yes.

MEMBER LEITCH: So you will make another

presentation to the committee?

MS. UHLE: Yes. That is the goal, yes.

CHAIRMAN WALLIS: So you will come to us towards the end of next year with a consolidation of the codes?

MS. UHLE: Yes.

CHAIRMAN WALLIS: You will then have consolidated the codes?

MS. UHLE: Yes.

CHAIRMAN WALLIS: Right so you won't have improved them much.

MS. UHLE: In some cases, for example, the level tracking we've improved. The large break calculations with Joe Kelly and Weidong Wang's reflood work, we would have improved. Hopefully we will have the phase separation stratified flow model in for use with the AP1000, we would have improved that.

The other improvements have been more user convenience , speed, robustness rather than physical models. And then at that point in time as we then go into more of a PIRT base developmental assessment effort and continue working more closely with the test programs, we would then focus on improving the physics. But the original charter of the consolidation and what the Commission had signed off on was to recover capabilities by the end of this period, and we feel will achieve that.

MEMBER SCHROCK: Do you have any indication that industry wants to start using it?

MS. UHLE: Not so much industry. Industry's interested in the graphical user interface because it works with RELAP5. And, of course, there's the strong use of RELAP5 in industry.

Shanlai Lu on the staff has for NRR's use has taken the TRAC-G and developed a pearl strip that allows us to take a TRAC-G input deck and convert it into what TRAC-M can run. So NRR would be using that and is a comparison for the future application of the TRAC-G submittal for the large break case.

The Naval Reactors is very interested in using the consolidated code because Betest and Capital are looking at, and in fact consolidating their analytical work as well.

And then, of course, the international user group is holding off, you know, waiting to see how it works. Most people are interested in the ability to recover RELAP5 functionality, and that is what we need to -- we will proving this in a month or so.

MEMBER FORD: I've got three comments.

Not being a fellow hydraulic person and not specific to the physics.

First is, you weren't clear about the qualification of the code especially when you're qualifying it against scattered databases. Presumably the code should be able to predict the uncertainty that you have.

The second question, and actually more a comment. The second question is what will the hierarchy be for the various codes when this TRAC-M code versus the licensee's code, what determines whether one is better than the other? Really a professional comment.

The third one is really also a comment. In the beginning that mission statement said safety margins and therefore presumably the next stage after TRAC-M is to incorporate it into materials degradation, and I'd be interested to hear about that. Is that your ultimate goal?

MS. UHLE: I'm Sorry.

MEMBER FORD: Well, aging phenomena of the materials.

MS. UHLE: I mean, our work with the materials interaction really is coming from providing thermal-hydraulic conditions to the division of engineering, and as well as working with the PRA branches where we provide, you know, based on the material degradation at these new thermal-hydraulic conditions, or maybe because of flow induced or flow accelerate corrosion you're going to have a higher failure -- or sorry. A higher break frequency, you know, that would go into the PRA. You know, that's more of our interaction.

MEMBER FORD: Well, there's a lot of material degradation issues when you have a synergy. Presumably that's all been passed along --

MS. UHLE: Typically the level of detail you need to couple thermal-hydraulics to something like flow accelerated corrosion is not going to come out of a system code, because our nodes are like this big. And you're looking at the boundary layer to look at, you know, the physical processes going on to do the flow accelerated corrosion. And more --

MEMBER FORD: I see.

MS. UHLE: That would be more a computational fluid dynamics linkage.

MEMBER FORD: Perhaps this phenomena is related to the core shroud.

MS. UHLE: Again, the idea of the thermal fatigue cycling, that again is looking more at large eddy simulation to get the frequency of the water coming up at a different temperature, going back down. That is not something a system code is ideally suited for. That would be more of a computation fluid dynamics application.

And we do have CFD technology in-house, and we are, again, hiring to increase that and that is something that we can think of as far as interacting with the division of engineering as these applications come up.

CHAIRMAN WALLIS: What I had in mind in this summary was we give you some input, and we speak again about more activities, say, in six months, and how we can interact in the next six months.

Tom, did you have -- I think we're talking about the TRAC-M --

MEMBER KRESS: Yes. TRAC-M. I did have a couple comments, but I'm not sure my comments are on how best to interact. My comments are more with respect to what Ms. Uhle was saying. I think you ought to view integral experiments as rough. I don't think you're going to predict experimental error. We're talking about two different things. Go look and see if your predictions fall within the boundaries of the experimental error. So my comment there is use separate effects testing to determine the uncertainties in your specific model. Think very hard about how to incorporate them in the code in a way that you get an uncertainty distribution in your final product. When you get to that point, you really have a code that is very useful.

My other comment I had is that I certainly like what I see and I encourage you to continue with this. If you are very successful it would solve a whole lot of these problems with flow regimes, how you transition from one to the other and how you deal with them on the code. I'd certainly like to hear more about that later.

CHAIRMAN WALLIS: Can we move on then to the separate programs?

The OSU program, I'm not sure -- any hope of bringing them around to our viewpoints? Why don't we try to figure out if there is some way in which we can interact. I don't want to be with you or them at the end of the program and have exactly the same comments we had when we visited.

MS. UHLE: As part of my action items that I have written down, it is to schedule some sort of test program review if I can interact with Paul to do that. Because, obviously, with your expertise it's of good value to us to learn.

CHAIRMAN WALLIS: Right.

MS. UHLE: And I mean I think our goals our consistent really, although sometimes it can be a combative interaction. You know, we want an accurate code, we want to be able to extend the code to other applications easily, we want to be able to understand uncertainty and calculate it so that this tool can be of use. You're looking at a whole lot of people that have put a lot of time in this program, and the idea of it not being useful, you know, we wouldn't get out of bed in the morning.

So I do think that our goals are consistent, and so I think further interaction with you on a more frequent basis can only benefit us.

CHAIRMAN WALLIS: Maybe there is a way in which OSU can come before this Committee before the report to the full assembly. There's no way.

MS. UHLE: That's --

MR. BAJOREK: No, that was for the ETS.

CHAIRMAN WALLIS: ETS. The other work is still going on?

MR. BAJOREK: The work is --

MS. UHLE: Oh, yes, we're getting a preliminary model for the --

CHAIRMAN WALLIS: Work on a useful interaction with OSU that we could have.

MS. UHLE: Yes. Okay.

CHAIRMAN WALLIS: Penn State, it seems they're still building the apparatus, they haven't gotten their results. I'm not sure we have anything we can --

MS. UHLE: I mean, they're doing shakedown testing now and characterizing like volumes and lost coefficients and things of that nature.

CHAIRMAN WALLIS: And PUMA oscillations, I don't think we have anything to get until they start doing something? We might, I think, contribute to the critical flow models.

MS. UHLE: Right. I have down to give you that critical flow report for Professor Schrock.

CHAIRMAN WALLIS: And maybe you can evolve at some time an actual presentation by them?

MS. UHLE: Yes.

CHAIRMAN WALLIS: And then on DEER, I think we really are due a presentation. It's been going on for some time, we have not had the detailed interaction, the kind of questions we'd love to ask and don't have time for today, so maybe we should schedule something for later. After the start of the year maybe.

MR. BOEHNERT: That'll be fine.

CHAIRMAN WALLIS: All right. Anything else on -- I didn't have anything immediate

MR. ROSENTHAL: You expressed an interest in some of the MOX work.

MS. UHLE: MOX work.

CHAIRMAN WALLIS: There is a fuel subcommittee of the ACRS.

MR. ROSENTHAL: Yes, I think that will be a better place for that and we could advise it --

CHAIRMAN WALLIS: It's all from a drop, it's neutronics.

MR. ROSENTHAL: We would do the neutronics and other MOX related issues about how you load the power and then the source.

CHAIRMAN WALLIS: But we'll do that with kind of a separate subcommittee on MOX. Maybe that's where it actually --

MS. UHLE: Okay. Yes, I have that down. Although we will give you some written information to respond to Professor Schrock's questions. Although it may not be answering all the questions that you've asked, that can be at a future time.

CHAIRMAN WALLIS: I think that the purpose of this meeting is for us to give some input for the main committee and the writing of the research --

MR. BOEHNERT: That's correct.

CHAIRMAN WALLIS: This does not require some letter or anything?

MR. BOEHNERT: No, it does not require a letter. It's fed into the work on the research --

MS. UHLE: I mean, one thing I do want to point out, because based on the feedback you give us annually is that we understand we need to tie in our test programs closer. It's not news to us to hear that.

In the past it's simply been how much time we have and the staff we had available. Now that we've been in this hiring mode and we've been bringing more expertise in-house, we're going to try to start to do that. It's a big focus for us.

CHAIRMAN WALLIS: I just want to be encouraging.

MS. UHLE: We realize that, it's not -- in fact, you know, Steve being the senior level scientist here is the perfect person to really lead that initiative, and he's been doing a great job in trying to tie in the model development work, the test programs more closer to the code development work that

Joe Kelly will be overseeing.

CHAIRMAN WALLIS: I hate to go back to the TRAC thing, but you know you have done a good job of consolidating these codes and at the end of this next year you're going to show that they'll at least do all the things the previous codes did, which is a bit like saying Amtraks going to run at least as fast as the steam trains used to run in the '30s. And what we're really looking forward to is that there's a high speed train or something that's really that much better. That's what we'd love to see.

MS. UHLE: Right.

CHAIRMAN WALLIS: The sooner that can get on the track, the better.

MS. UHLE: Yes, right. Again, we agree with that. Now, we've been doing what management has assigned us to do as far as the Commission policy being to do the consolidation first before we start the improvements. We also want to make improvements, probably faster than you do, because we're the ones doing the work and it frustrates us more than it probably frustrates you. So that is going to be our focus.

But we were tasked with this consolidation effort first, and that was the high priority.

CHAIRMAN WALLIS: Even if you have to smuggle the improvements along.

MS. UHLE: We've done that.

MR. ROSENTHAL: No, no. But we work in accordance with the operating plan, of course. But, no, in conjunction with the synergy work we have planned ATWS calculation and we can now do couples, 3-D, space time kinetics, really better ATWS calculations than we were able to do. And that'll be a shorter term product. But we do some benchmark.

So we're going to start seeing the benefits now.

MS. UHLE: Ready for your next victim? I think they're behind you.

CHAIRMAN WALLIS: I'm very glad that you have all these people now to work on these problems. It's good to see Joe Kelly back here. Go away with a good feeling.

MS. UHLE: I also went away with the concept that any PIRT that you are involved in we're supposed to ignore, because you are unduly prejudice, that's the number one lesson we learned today.

CHAIRMAN WALLIS: I think I'm utterly clean. I don't think I've ever been involved in a PIRT. Now nor have I ever been.

Do you have a few final remarks? Then we will close this part of the meeting.

I would like to take a break. I notice there are all the people waiting. We have caught up some time, so we'll try to keep on time, at least get out of here before 6:00.

We'll take a break.

(Whereupon, at 2:43 off the record until 2:02 p.m.)

CHAIRMAN WALLIS: No introduction, Mr. Henry, please begin.

MR. HENRY: Thank you, Mr. Chairman.

We're happy to be here today just to have the opportunity to present to you this new activity of using the MAAP5 containment code to replace the models for containment integrity at both Beaver Valley and Point Beach.

Before I get into talking about it, I thought perhaps you would like to hear from the two different sites of their motivation for going to a different code for containment integrity, that it has some differences which are site specific. So, maybe just a couple of minutes with each site.

I'd like to introduce Mike Testa from the Beaver Valley site and then he'll be followed by Harv

Hanneman from Point Beach.

MR. TESTA: My name is Mike Testa. I'm with Fist Energy, and we operate the Beaver Valley Power Plants, that's Beaver Valley 1 and 2. And I'm Project Manager for the Power Uprate that's being undertaken there. The power uprate that we're looking at for the Beaver Valley plants to increase the power in total by about 9.4 percent.

The MAAP code and the use of the MAAP code is an integral part of that, and I just want to give you, as Bob said, a minute or two perspective on the use of MAAP at Beaver Valley.

The Beaver Valley plants are three loop Westinghouse PWRs. The architect engineer was Stone and Webster. The containments were designed subatmospheric, that's the way they're currently operated. And we want to use the MAAP5 computer code, basically, to replace the existing design basis computer code LOCPIC. And using the MAAP5 code we want to, again, reanalyze the containment and move to an atmospheric containment.

Benefits for going to an atmospheric containment are that right now for personal access to the containment, it's in an oxygen deficient environment and the people that access the containment are required to wear supplemental breathing apparatus. And this will eliminate the need for that. That goes towards enhancing personnel safety on access to the containment.

The other thing this does for us is that with a move to atmospheric containment where we change the initial condition for the containment operating pressure, we're incorporating that into our best estimate LOCA analysis. And this will allow us to gain margin on peak clad temperature, so we'll be gaining a benefit in that respect also.

And, as I mentioned, this supports our power uprate initiative in that the power uprate is going to be based on the containment analyses that's done with MAAP5.

CHAIRMAN WALLIS: If you do not use MAAP5, are you not able to get this 9.4 percent power uprate? Is it critical?

MR. TESTA: Yes, it's critical in that, yes, we've done some studies with the existing code and with then we looked at MAAP5 and it affords us additional benefit in that we can increase the initial containment pressure and basically review or rerun the design basis spectrum of accidents and stay within our containment design pressure.

MR. BOEHNERT: You said you're going to a best estimate LOCA code.

MR. TESTA: We're going to use that, yes. Westinghouse best estimate LOCA.

MR. BOEHNERT: Westinghouse?

MR. TESTA: Yes.

MR. BOEHNERT: Have you checked with them?

MR. TESTA: Yes. Again, our plans for MAAP5 is that we're going to utilize it for the containment integrity evaluation. Again, we want to replace the LOCPIC code. And in doing this we're going to perform the analysis using MAAP5 code consistent with the current design basis requirements, and that we're going to analyze for LOCA, steam line break, different spectrum of breaks and look at the corresponding results, the response of the containment given pressure temperature and so forth.

And the last thing is that, again, the MAAP5 takes advantage of the latest experimental information. And what we want to do with MAAP5 is move or take the computer code in-house so that we can put our engineers in a position to be able to utilize the computer code and to make operating assessments. We've been working up to this point with Dr. Henry to develop the inputs and the parameter files, which is a benefit to our developing our in-house expertise.

MEMBER KRESS: What was your last code that you used before?

MR. TESTA: LOCPIC.

MEMBER KRESS: L-O-C?

MR. TESTA: Yes, L-0-C-P-I-C.

MR. BOEHNERT: When are you making these submittals?

MR. TESTA: We talked about that yesterday a little bit. There's going to be a topical submitted in January time frame for the MAAP5 code and we're looking at May for the Beaver Valley plant specific submittal. And in there will be the MAAP5 code, the analysis that was conducted, the results and also the supporting information on allowing us to move to an atmospheric containment.

MR. BOEHNERT: What about the LOCA code, when are you going to make these submittals?

MR. TESTA: Well, the LOCA code, that will follow. That will be later on in around September time frame. And we're basically putting in the building blocks for a power uprate submittal.

MEMBER KRESS: When you use MAAP5, and I don't know if this is for you or somebody else, do you use it differently? Do you use other sources also?

What do you use as the input?

MR. TESTA: The input of MAAP5 is going to be the Westinghouse mass and energy input.

MEMBER KRESS: Okay. So you use the mass and energy input?

MR. TESTA: Yes. Yes. Correct.

MEMBER KRESS: You'll only use the containment part in MAAP5?

MR. TESTA: Right.

MEMBER KRESS: Then the NRC would use this and they'd never have to check the containment?

MR. TESTA: Correct

MEMBER LEITCH: Does MAAP5 have the option of one region, or five regions?

MR. TESTA: Do you mean as far as analyzing or --

MEMBER LEITCH: Yes.

MR. TESTA: Yes. Right now the developed model for Beaver Valley is 17 nodes for both Beaver Valley 1 and 2. Basically the same model is broken down into 17 nodes and review that for large breaks, you know, which nodes or compartments they occur in and then we're evaluating the corresponding response within the given of the multi-node response capability of the code.

CHAIRMAN WALLIS: So how many nodes are in this MAAp5 code?

MR. TESTA: Seventeen.

CHAIRMAN WALLIS: How many in your present code?

MR. TESTA: One.

CHAIRMAN WALLIS: One?

MR. TESTA: Yes.

MEMBER LEITCH: So to get the results where the containment pressure is acceptable you not only are changing the code but you're increasing the number of regions analyzed.

MR. TESTA: Yes, that's part of what Dr. Henry's discussion will be is on the benefits or the need to incorporate multi-node model.

CHAIRMAN WALLIS: Thank you very much.

MR. TESTA: Thanks.

MR. HANNEMAN: Good afternoon. I'm Harv Hanneman. I work for Nuclear Management Company and the Power Uprate Project Manager for Point Beach Nuclear Plant.

A little background, Point Beach is a two unit site with two LOOP Westinghouse reactors, roughly 1500 megawatts thermal each. We have large dry atmospheric containments for both units. And our initial motivation for using the new MAAP5 methodology is to support containment integrity analysis for possible future power uprate of about 10 percent in reactor power. And we're in the planning phases of that project right now, and we saw the need to get additional margin for our peak pressure and also temperature in containment because of the 10 percent higher reactor power.

However, other benefits that we expect to achieve by the use of MAAP include the accommodating a pre-accident containment pressure of 3 psig. So that would be in our technical specifications in the range of pressures that would be allowed in containment initially.

Provides margin for some of the issues on containment fan cooler service water boiling, which came out of Generic Letter 96-06.

And also provides a plant specific main steam line break containment analysis for Point Beach.

Currently our licensing basis is an evaluation of a generic two LOOP Westinghouse analysis for containment, so going to the uprate, we thought we needed a plant specific analysis and we believe MAAP will give us the margin that we need.

CHAIRMAN WALLIS: Does MAAP give you a

margin for this service water boiling issue? Does it predict containment or something?

MR. HANNEMAN: We expect it to predict slightly lower peak temperatures early in the accident, and that's when boiling is an issue.

CHAIRMAN WALLIS: Coolant containment?

MR. HANNEMAN: Right. Right.

CHAIRMAN WALLIS: I understand now.

MR. HANNEMAN: So our application of MAAP would be to use MAAP5 for the containment integrity analysis for the plant. We would continue to use the Westinghouse methodology for calculating the mass and energy releases as an input for both LOCA and steam line break accidents. We currently use the Westinghouse COCO methodology for containment integrity, and we would replace that with the MAAP5.

This would allow us to take advantage of some of the latest experimental information that Bob Henry will be discussing here in a few moments. And it also provides us an opportunity to bring the containment integrity analysis in-house so our own engineering staff will be performing the plant specific calculations; that'll give us greater knowledge of that analysis in-house and also allow us to perform more timely responses to any operational emergent issues that come up with regard to containment response.

MEMBER LEITCH: Harv, do you, like Beaver Valley, also need to use MAAP5?

MR. HANNEMAN: We've done some initial analysis using the COCO methodology for both LOCA and steam line break. The LOCA peak pressure was slightly under our containment design pressure of 60 pounds, but the steam line break the pressure exceeded it at the uprated condition. So, that's why we feel we need this methodology to give us a little bit more margin.

MEMBER LEITCH: How many nodes are using?

MR. HANNEMAN: I'd have to defer to 10 -- 9.

MEMBER LEITCH: Nine.

MR. HANNEMAN: Nine volumetric nodes. And currently we have one also with the COCO.

MEMBER LEITCH: Just a quick aside to Mike, the people at Point Beach are talking about an initial pressure in pounds, do you have a similar number for Beaver Valley?

MR. TESTA: Yes, and for the move to atmospheric containment we're looking at developing an operating band of 12 to 16 pounds. One atmosphere for us is 14.3

MEMBER LEITCH: Thanks.

MR. HENRY: What I want to present for you today is really a work in progress, and we particularly wanted to get your feedback on the approach. As you can see, there's a couple of sites, that we really want to know how you feel about this and what has to be done in the future. And has been said by both of them, there will be a submittal to the staff sometime planned early next year and it'll be led by a submittal of the methodology itself for the staff to begin to review.

But in addition to your feedback on the methodology, as we go through this you'll see that there's a lot of experiments here and the experiments represent a level of understanding and the capabilities of the calculational tool. If there's some experiments that we haven't managed to cover here that you think would be very helpful in understanding the capabilities of the model, we also want to get that particular feedback and get it early on so that we can take advantage of the expertise on this committee.

Obviously, I don't have to tell you. Feel free to ask me any questions as we go through this. But let me also say early on that there's obviously a lot more material than we can cover in the time that's allotted. I apologize for that. We'll go as slow as you want to go, but we did want to bring to you the fact that we've worked very hard at trying to make sure that the model is comprehensive of these experiments and in a very simple manner.

So, the things that I'd like to cover for you today are the issues that are related to: nodalization; representation of the atmospheric motion, which is circulation within the atmosphere has a major influence on the rate of energy transfer from the containment atmosphere on a nodal basis to the wall.

Let me also say just up front the nodalization scheme and map is generalized. You can have as many as you want to define. Right now the code will allow you up into the range, of what, I think 26 or so. But usually it's a very highly compartmentalized containment that would need 26 nodes, but that's why there's different nodalization schemes for the different plants. As an example, two LOOP versus three LOOP gives us different compartments. Different geometry gives represented differently. But we'll talk about that as we go through this.

CHAIRMAN WALLIS: Circulation is going on within the nodes?

MR. HENRY: Within each individual node is where it's evaluated, yes.

CHAIRMAN WALLIS: Now, I don't quite understand that. So you have some sort of a model and it interconnects the hose between the nodes, but it also super imposes some kind of a circulation within each node?

MR. HENRY: We will get to that. And the place that's important, Graham, is that's what dictates what the local boundary layer is and therefore, the rate at which energy can be transferred to the wall.

In addition to this, this blow down and give you forced circulation flows, but then you also, obviously, have to comprehend natural circulation flows because there can be compartments that are isolated or there's later in time when the flows die down. Natural circulation dominates. That has to be a key part of it.

Another very essential part, which is nothing new to MAAP5, that's already in MAAP4, is the ability to have countercurrent natural circulation if you have heavy over light at an opening between the two so that they can exchange mass and energy, and that natural convection type phenomena.

Condensing heat transfer, of course, we look at the condensing on cold heat sinks. We looked very hard at the separate effects test, and that's where our understanding comes from. And we try to make that step to the containment analysis in a very structured logical manner without any kind of games. So our whole understanding comes from the separate effects tests.

And then lastly, the influence of water entrainment, and that's another place where the local circulation velocity is important because we can have water films on the walls, we could have water accumulate on the floor. If you have velocity which exceed the entrainment rate, then that material could be picked up and put into the atmosphere.

So from our perspective, as has already been discussed, we want to move to something from a design basis approach to something which is more realistic, and we hope to be very realistic of the containment response.

And the issues that we see that are involved in this are:

Certainly nodalization, because we want to represent the containment geometry;

The need to represent the displacement of noncondensible gases, and that's a major reason why multi-node differs from single node because you can displace air out of the region and, of course, displace noncondensible gas means that for certain conditions at short time frames the energy that's transferred to the wall can be much greater. If you have strictly a single node, then the partial pressure of the air is always the same;

We need to represent the potential for induced circulation, which means we solve the momentum equation in the gaseous atmosphere as this blowdown occurs;

And we want to represent the potential for stratification, so we look at these nodalization schemes. There's always a potential above the operating deck of having more than one more node. So if you have light gases, there is a potential that it can accumulate in the top of the dome.

MEMBER KRESS: In read in the material we received that there is no momentum equation in that. Did I get that wrong?

MR. HENRY: I think so. But like I say, there's a lot of momentum equations.

CHAIRMAN WALLIS: But they're not transient momentum, they're in a pseudo-steady state.

MR. HENRY: Correct. Transient in the sense that you give me the current conditions and I'll--

CHAIRMAN WALLIS: It really should be momentum DT in there.

MR. HENRY: Yes. Well, for a given cell the momentum in a cell -- in a node changes given the blowdown time. Circulation velocity is a function of time. You can ask me the question when we get to it. Maybe I'm misrepresenting or misconstruing what you're saying.

CHAIRMAN WALLIS: Well, I think your momentum equation does have a D by DT determinate. It just balances.

MR. HENRY: Okay. Well, we'll get to it. For the kind of nodalization schemes that we recommend, certainly it's to move away from the single node for reasons noted here, but you don't have to have tremendous number of nodes. You just have to represent the fact that the air can be moved to different locations, that you can have stratification, etcetera.

From our perspective, what it means to

have a realistic model, and I'm just going to discuss both of these points together in time, to save us some time.

We want to make sure that we represent all the systems and all the phenomenaology, that have a first order effect. And that's very straight forward. And we want to also represent those which clearly have a second order effect, which means that they impact things in the order of 10 percent.

MEMBER KRESS: When you say systems, you mean things like fan coolers and sprays?

MR. HENRY: Sprays, right. And, of course, the M&E coming out of the break and any special things. If we're looking at another plant, like Cook, the dynamics of the ice condenser and its melt and drainage, and etcetera.

And the things that relate to 10 percent that could be issues. Things like water entrainment may either influence things by order of a 100 percent or the order of 10 percent. As we'll see later on, both nodalization of water entrainment have a significant influence on this.

That's our real focus, to make sure that we cover all these phenomena, and when you get down things which relate to one percent, it's kind of hard to deduce what kind of influence they really have and then take the jump to full scale containment experiments and try to look for that particular effect.

CHAIRMAN WALLIS: The first order is 100 percent, and you can't have very many of those, can you?

MR. HENRY: Correct. Don't have too many. That's where order of magnitude comes in.

CHAIRMAN WALLIS: You said first order was 30 percent or something, then you could have three of them.

MR. HENRY: That looks like an argument I need to delegate to somebody who is younger and quicker, Graham.

Okay. First off, but what's the influence of nodalization? Because that's one of the aspects that you just heard that's important to these two sites. They currently are licensed with single node models and it gives them some difficulties when they take the current design basis in the M&Es and apply it to the model. So is it the limitation of the model or is it a limitation of the design? Well, the only way you can figure that out is to do something which has more than one node and look at the influence of it.

So what we did to just determine the influence of moving to a multi-node containment was take MAAP and let MAAP produce the M&E. And so this is not coming from a design basis M&E, but it's not meant to say this the plant response. All we want to look at here is what's the influence of single node verses multi-node for a large break LOCA response and for main steam line break response.

What we have then --

CHAIRMAN WALLIS: What you want to do with CFD is you keep applying the nodes until it makes no difference. Here you're showing there is a difference, but you don't show -- you keep on going to a 100 or 200 or 300 node --

MEMBER KRESS: I think there's a difference of what we're calling nodes. These nodes have specific boundaries --

CHAIRMAN WALLIS: A physical basis.

MEMBER KRESS: Yes, physical basis. Those ones you're talking about are kind of different. There's a difference, I think.

MR. HENRY: And you're also taking away any information I could use if you guys say come talk to us again. I mean, that was one of the things I want to do next time was show you how we progressed.

In any way case, we're going to get to that in a little bit. And unfortunately I didn't have it put in here, but try to look at the differences in example CVTR going from one to 4 nodes and then 6 nodes. Six nodes which are this way and 6 nodes which -- might be 2 this way and 4 this way and etcetera. What you really find is you're not very sensitive to that. What you're sensitive to is getting past one node so you can have air move throughout the containment.

If you have various rooms, then it's certainly to your benefit to make those nodes, because things could potentially be more concentrated in that room if there's not sufficient natural circulation.

CHAIRMAN WALLIS: You might say it's rather ridiculous if you take several rooms and mix up all the atmospheres and then saying that that's typical of everything that's going on. That's s very crude and probably inappropriate way to look at what's happening.

MEMBER KRESS: So it would depend on where your break -- which room your break occurs in?

MR. HENRY: Slightly. And as Tom said -- Mike said, excuse me. Your Tom, right?

CHAIRMAN WALLIS: But the room where the

break is very different from the rest of the containment?

MR. HENRY: I think Tom's point is we look at a break in each of the three different compartments for Beaver Valley, as an example. And that's part of the analysis. But it's not greatly different between them, but it is tenths of psi difference because you don't necessarily get the same condensing profile throughout the containment depending upon where the break is. Because even though the compartments you might think are equivalent, but they don't necessarily have the same entry area and existing area, etcetera.

Anyway, to the point of nodalization, this is a demonstrative calculation. This happens to be a Westinghouse two LOOP plant, and we divided this up into 5 nodes and also ran it with 1 node. So one of the nodes is, of course, the reactor cavity. The second node is the loop compartment which houses the two loops. The third node is the annular region which is outside the loop compartment. And then the operating deck, which is here, we put two nodes in. one above the operating deck, one here and one in the region above the spring line.

CHAIRMAN WALLIS: I have to ask why would you ever mix 1 and 5 in any kind of node?

MR. HENRY: I think the answer to that is originally when people did design basis calculations that was judged to be conservative, give you a higher peak pressure. And from a practical point of view it certainly makes sense that you would always have the same air pressure, pressure everywhere, so it limits the condensation rates.

CHAIRMAN WALLIS: So the big action role of this is the condensation on structures and that sort of thing?

MR. HENRY: Yes, the big actor is condensation on heat sinks. That's what really drives the bus on whether or not you live within your current design basis pressure differential. And some of that is shown in this slide. What we have here is really single node and multi-node, which is shown here when it says 5 node and 1 node.

So if I take these two, which says 5 node and 1 node, which is this solid line and this large dashed line here. This is MAAP4, and I apologize I didn't get that written on there, but that does not have things related to atmospheric circulation to water entraining. And as we walk through this you'll see some of those influences. Whereas there here are MAAP5, which is the design basis code that we're looking for for these two sites. And you'll notice that you can identify, this induced flow is equal to 1 means the induced flow from the break was included here.

But as you look at this for 5 node with MAAP4, this solid line, and 5 node with MAAP5 you see no difference. And it's true. Because the only real thing that made a difference here was going from 1 node to 5 node reduced the peak pressure substantially, roughly in atmosphere. And the whole reason is that in the local near the break you pushed air away and you got enhanced condensation during this short time frame of about 10 seconds. And that makes a difference.

And all we're doing here, it's the same code, it's the same physics. Obviously, we're just changing the number of nodes. So you can see even by including all these new models we're going to talk about from MAAP5 that didn't make any difference and it's strictly the single node going from 1 node to multi-node that made the difference.

CHAIRMAN WALLIS: This is pressure absolute?

MR. HENRY: Correct. This is pressure absolute here.

CHAIRMAN WALLIS: But it says G on the other side, right?

MR. HENRY: And here, as you can see, this is one atmosphere there. This is .5 and 1.5 times 10 to the fifth. This is absolute and SI units and we've put it in gauge over here.

For this particular plant, the design basis pressure is 60. But, again, that's for --

CHAIRMAN WALLIS: 60 psig?

MR. HENRY: 60 psig. And that's for design basis mass and energy increases, which are not in this calculation. That was not the intent here. The only intent was to illustrate the difference of going to multi-node.

And, as you might expect, for a large break LOCA this is just the temperatures in containment, again, in terms of Kelvin and Fahrenheit. There's really not much difference between the two codes.

CHAIRMAN WALLIS: Of which node?

MR. HENRY: Excuse me, Graham.

CHAIRMAN WALLIS: Which node? Temperature in the containment is different in different nodes.

MR. HENRY: Well, of course, this only has one node. This is the lower compartment, so this would be node number--

CHAIRMAN WALLIS: It's different nodes. Okay.

MR. HENRY: Right. This is 2. I think that's 4 and 3.

But in essence it says that there's not a big difference between them, and that's not surprising for a large break LOCA, because the blowdown itself puts so much moisture into the atmosphere.

So then we take the same analysis, again, just from a demonstrative point of view what does it mean for main steam line breaks, and that's a little different story then. But here we have these two are MAAP4, that have nothing after them. And these that say induced flow=1, this is MAAP5, which again is the code that we're talking about here.

CHAIRMAN WALLIS: I only see 3 curves.

MR. HENRY: You always take my punchline, Graham. This 1 node curve and this 1 node curve are on top of each other. It doesn't make any difference from a practical standpoint. And the reason this is different here is now, as we'll get to later on, what's influential in MAAP5 is induced circulation because a main steam line break goes on for a lot longer time. But if you say there's only 1 node available, then as a result of that you're doing this momentum equation into one huge node of the containment, and really it's a huge mass and it hardly stirs it all all and issues related to enhancing any local velocities or entrainment really go away. So that part really disappears in one node and they become the same calculation.

But when you go to 5 nodes now, of course, the blowdown is coming into one of those nodes, which is a much smaller region and also you, obviously, have higher heat transfer in that local. Because it is 5 nodes you're displacing air away from it and you have the potential for also reentraining moisture in the containment because a local velocity in that node can be above an entrainment criteria.

MEMBER KRESS: Does that act like a water spray?

MR. HENRY: Exactly.

MEMBER KRESS: Does the code then account for revaporization of the droplets?

MR. HENRY: They can allow the droplets to revaporization. But principally when you entrain something, you're entraining the film off the wall, so you entrain at the average temperature, which is T side on the outside and T wall. So you actually get some subcooling.

MEMBER KRESS: So you get some subcooling.

MR. HENRY: Which really is the major thing to do. But yes, Tom, it can revaporize. In fact, that's part of what you see with multi-node because it can get down to something in the bottom of the containment for local partial pressure is not so high and the droplet might be warmer and it can vaporize down there as it falls through that node.

MEMBER KRESS: Do you have a model for the rate of entrainment when the droplet --

MR. HENRY: At the rate of entrainment and we put that into -- it goes directly into the aerosol model where the deposition rate is depending upon the airborne density. So the airborne density --

MEMBER KRESS: So you exercise -- is this still the aerosol model that was built by --

MR. HENRY: Mike Epstein. Yes. So it becomes -- water is just part of the aerosol --

MEMBER KRESS: Part of the aerosol?

MR. HENRY: Right. But the only reason I wanted to make a point, is the aerosol can come from either entrainment or from cooling of steam, both of them get put into the aerosol mix.

And this, Graham, to go back again, we

have 4. The piece in the legend, the two curves are simultaneous on top of each other because here again, the circulation has no influence. It's so slow. And what we get out of that since it's also not entraining, you get temperatures which are typical of what you see in some of the main steam line breaks.

CHAIRMAN WALLIS: I don't know how we would apply it, the circulation model to those five different rooms and there's not going to be one big circulation pattern to these five rooms. It just does not plot.

MR. HENRY: I agree with that. Just from the concept if I assume that it applies, it says it's not going to make any difference anyway because I'm too big, you can't make me circulate fast enough. But if it does and it entrains, you can see what's gained on the peak temperature, so it's again substantial. So not only is the pressure lower than the 1 node system, the temperature is also lower.

CHAIRMAN WALLIS: These are all, of course, predictions?

MR. HENRY: These are calculations, right. We're going to get to comparing this with experiments.

I just hesitate saying predictions because it's really for a generitized system. It's a two LOOP plant, but it's different from Point Beach's containment model in terms of nodes and level of qualification.

And also then we just look at for main steam line break between MAAP4 for a 5 node model and MAAP5 for 5 node model. And this all becomes because MAAP4 knows nothing about atmospheric circulation, knows nothing about entrainment model. And all we're comparing here is the influence temperature of those particular models which is what we'll talk about today. And this has the multi-node in it, but it still isn't enough to really -- I'll get to CVTR. If I take this approach from MAAP5, which we thought was a quite good code when we started that comparison, it overstates the pressure in CVTR by something in the range of 10 psi and it overstates the temperature by-- I forget the actual number. Like 50/60 degrees Fahrenheit. This is the physics that we believe is controlling that.

So I mentioned MAAP is not a 1 mode model, so this isn't meant to -- this just shows you the various pieces of physics that are in the model, and some of these are severe accident related which Tom correctly asked us earlier what's being reviewed. And what's being reviewed is that the containment model as it applies to design basis accidents.

Now, the key actors in that for these two different plants, we obviously focused on the heat transfer to heat sinks, which is shown here. Tom, the aerosol model we just talked about, which is part of this.

The heat transfer to equipment, which is just all the steel and everything inside, whether it's handrails or ducting or whatever it may be.

Condensation on all the walls and on all the heat sinks. The metal, concrete, steel lined concrete, stainless steel line for fueling pools.

Fan coolers for Point Beach. And, of course, the sprays for both Beaver Valley and Point Beach.

And lastly, the flow from the primary system. This is not coming from MAAP now when we talk about design basis things. This is coming from Westinghouse design basis mass and energy release calculation for both large break LOCA and the main steam line break.

MEMBER KRESS: This allows the use of sprays and fan coolers. Is there a single criteria in the DBA that says you can't use the full capacity on those?

MR. HENRY: Correct.

MEMBER KRESS: That's probably --

MR. HENRY: There's a whole run matrix that's used by both of the sites depending on what their specific conditions are. They look at all the different kinds of single failures and look for the worst one in both sets of conditions.

And then also, some temperature is part of that, so that has another set of M&Es or way that you treat the previous M&Es, to mix or not mix them coming out of here.

And, again, we talked about uncertainties in the models, but there's also variations in the operating perimeters that have to be part of that DBA calculation. You have to look for the most limiting case of operating conditions.

MEMBER KRESS: I guess I would look for a net positive suction head located in those compartments that could prove affected.

MR. HENRY: Well, the analyses that you look for is the net positive suction head when you go into recirc there for sure, yes.

MEMBER KRESS: But you look at that?

MR. HENRY: Yes, that's part of the --

MEMBER KRESS: You don't --

MR. HENRY: Yes. And you look at it on a plant specific basis. Because even when you have two units at the same site, they don't necessarily have the same systems.

Okay. So this is the conceptual part then of the circulation, which is one of the key things that we think is missing in MAAP4 and it wouldn't buy you anything if you just looked at a one node anyway. But the concept that it has is that a blowdown into this gaseous region adds momentum to the atmosphere. Obviously, if we just had one node and we have momentum going in, where did it go?

CHAIRMAN WALLIS: These are different nodes in the sense that there's previous nodes or are these different nodes within a given compartment?

MR. HENRY: These are nodes in the same sense as the previous nodes.

CHAIRMAN WALLIS: So there are rooms? These are four different rooms?

MR. HENRY: They may be rooms or they may -- this node boundary may be drawn in the atmosphere, as an example.

CHAIRMAN WALLIS: I find this an extraordinary diagram. I mean, the idea that there are rotating cylinders in each one of these rooms.

Fantastic. And the idea that the incoming flow coming up like that rotates the cylinder on top of it is also fantastic. And the idea that nothing happens between them except interfacial shear is also fantastic.

I couldn't understand what you could possibly be showing. This is sort of a study of what it sees in liquid helium or something.

MEMBER KRESS: You just conserve momentum.

CHAIRMAN WALLIS: No, there's no momentum. It's on the angular momentum.

MEMBER KRESS: You've got momentum coming in to flow and you're going to put that all in the atmosphere until it circulates.

CHAIRMAN WALLIS: It doesn't happen that way, it's all angular.

MR. HENRY: Well the angular momentum is still momentum. Graham, this is merely now to describe the concept. Because the concept is it will -- let's first just think of a single node here as an example.

CHAIRMAN WALLIS: But that's not the way it works.

MR. HENRY: I know it isn't. I know it isn't. But from a single node point of view, it's easier to see what happened to the momentum that came in here. Where did it all go? Because in a single node you now have inflow, where did all that momentum go? We can have conservation mass and conservation of energy, where did the momentum go? It has to go in terms of somehow this fluid is circulating.

CHAIRMAN WALLIS: I thought you were going to say, the incoming flow in region 2 there actually set up some sort of a circulation around the jet which helped the heat transfer to the wall?

MR. HENRY: It certainly does that. This doesn't mean that this sits here and spins with either a sphere or cylinder, whichever you choose on that one. It only means -- it only gets down to this fundamental thing right here. Schematically what the code thinks of is I've got some velocity in this node which is different from the through flow velocity. I have circulation. And this is merely a way of representing that, but that momentum that gets added to that node says the only way I can satisfy my momentum balance is I've got to circulate faster.

CHAIRMAN WALLIS: I don't understand that.

MEMBER KRESS: Well, it bothered me when you said that those nodes could be virtual nodes in the middle of the air and still do that. Those are not really boundaries of rooms?

MR. HENRY: Certainly whenever it is a boundary of a room, you use the boundary of the room. But sooner or later you'll have to draw if you want to be able to investigate whether or not things can be stratified.

MEMBER KRESS: I see.

MR. HENRY: You have to eventually draw something up here, which is air. Otherwise you're always just going to have rooms and this will always be one node and you won't have any stratification potentially.

MEMBER KRESS: But you can't treat that in terms of momentum the same way --

MR. HENRY: Let's talk about it.

CHAIRMAN WALLIS: Let's go back to this other picture that you showed us. I don't understand it. You've got nodes and you've got flow between nodes, which is the usual thing.

MR. HENRY: And that's part of the calculation.

CHAIRMAN WALLIS: And within each node you have some sort of circulation as well? Is that the idea?

MR. HENRY: Yes. Because if you have flow coming in and you conserve mass and energy, so what you have going out of here is merely the through flow, then you won't be conserving momentum.

CHAIRMAN WALLIS: Well, that's because it forces on the wall.

MR. HENRY: No, even without that. You're just going to defuse or any momentum goes away. You don't satisfy it by the through flow alone.

CHAIRMAN WALLIS: You're really confusing me altogether.

MR. HENRY: Okay. That's tough to do. You're a hard guy to confuse.

CHAIRMAN WALLIS: No. I mean the momentum balance works out always. If you don't have -- if the momentum balance doesn't work out you've got forces of some sort. The idea that the linear momentum is balanced by angular momentum is a very strange concept. So something else is going on.

I think what you're saying is that the incoming flow in to that chamber stirs things up so the fact that this sort of -- some average velocity, which is low, is not characteristic of the real velocity seen by the wall. Isn't that what you're saying?

MR. HENRY: Well, in a sense yes. That's why I wanted to go back to just from a simple concept look at a single node. If I did an experiment with a single node. And I blew down into that. What would be the governing velocity of the through flow that comes out, if I make it one dimensional. Of course not.

MEMBER SCHROCK: The way you've run it, it doesn't look like there should be any shear between nodes in that picture.

MR. HENRY: Well, we're going to get to that.

MEMBER SCHROCK: You're both going in the same direction.

MR. HENRY: Right. Yes, they're going in the same direction, but they don't necessarily have to be going at the velocity.

Suppose I put a bunch of structure up in this node, as an example, Virgil. So this may be going at a much higher velocity than this. I still have to represent the fact that there could be momentum transfer across this arbitrary boundary that the nodalization has created. That's all it really means.

MEMBER SCHROCK: How you come by those velocities, you're going to show us.

MR. HENRY: Sure. I knew you'd guys would have tons of questions on this, and that's why we're here.

CHAIRMAN WALLIS: Well, I just understood, let's go to the next equation.

MR. HENRY: Okay. I would just say, before we leave this, Graham, you said it very well. All this is meant to merely say -- all this says is that a node has a property that we looked at as circulation. And that's merely the way of making sure that we do conserve momentum throughout these various nodes.

CHAIRMAN WALLIS: Circulation cannot conserve momentum.

MR. HENRY: Okay.

CHAIRMAN WALLIS: Circulation cannot conserve linear momentum.

MR. HENRY: Yes, you're right. It does not conserve linear momentum, but --

CHAIRMAN WALLIS: Spin those things up to the speed of light, and it won't conserve --

MR. HENRY: That's why I wanted to make sure we talked about the single node. Within a single node is an example there can't be any linear momentum. There's no out flow. The only thing you could have is something that goes back to that's going to spin it somehow or other. But you know from those kinds of experiments that you have a lot higher energy transfer at the wall, and that you get by the through flow velocity or the pressurization velocity.

CHAIRMAN WALLIS: What is this -- can we go to the first line here?

MR. HENRY: Sure.

CHAIRMAN WALLIS: What's going on here?

MR. HENRY: All this does is say that the way we look at this is the equation -- equating the impulse and the rate of change in that specific node. What's it's mass and what it's velocity.

CHAIRMAN WALLIS: "U" is a circulation velocity?

MR. HENRY: Yes.

CHAIRMAN WALLIS: So this is spinning?

MR. HENRY: It's a concept of there's something going on and whether it's one thing or spinning this way or whatever, it's not a through flow velocity.

MEMBER KRESS: It's not spinning. It's just falling circle. That's different than spinning.

MR. HENRY: Okay.

MEMBER KRESS: It's not angular momentum --

CHAIRMAN WALLIS: If you give something an impulse, it moves linearly, it doesn't --

MR. HENRY: Yes, it does.

MEMBER KRESS: Well, this is a linear motion. But I don't know what "F" is yet, that's what's bothering me.

MR. HENRY: We have three different forces that we look at, which is the force on the wall here, on the shear force on the adjacent node, which Virgil was asking about. So if you have a difference in the rate at which you have the circulation velocity and nodes, then that has its own influence. And then if you could have any kind of embedded structures that slow things down, they also have to be --

CHAIRMAN WALLIS: What is U_c in your figure?

MR. HENRY: U_c is the property in the node which is --

MEMBER KRESS: That's the result you're trying to calculate, right?

CHAIRMAN WALLIS: What is U_c in this figure?

MR. HENRY: U_c in the concept of the model is that in addition to through flow that this is also has --

CHAIRMAN WALLIS: Where is the U_c? I mean is it an average of some sort or is it on the wall, or in the middle. Where is U_c? I don't understand.

MR. HENRY: Okay. U_c when we look at the energy transfer to this wall right here, U_c is the velocity that's dictating what the boundary line --

CHAIRMAN WALLIS: U_c is the velocity along the wall?

MR. HENRY: U_c is the free stream velocity next to the wall.

MEMBER KRESS: It has to be some integral of the velocity in the whole mass --

MR. HENRY: It is, yes, right, Tom. And that's because it's coming from a momentum balance on each node.

When you have a through flow velocity and in each node you have a property called circulation. And whatever that velocity is, that's what determines the free stream velocity next to the wall, it also determines the velocity that could entrain anything that's collected in that node. Reentrain water, which is what --

CHAIRMAN WALLIS: What you would call a turbulence velocity or be about the same thing?

MR. HENRY: Yes.

CHAIRMAN WALLIS: It would be about the same thing?

MR. HENRY: Exactly the same thing.

CHAIRMAN WALLIS: It is the amount of stirring up of the nodes, a measure of the stirring up of the fluid in the node by incoming flow?

MR. HENRY: Exactly. Exactly. The only reason I pictured it this way is to try to break it down to the most simple thing. The code thinks I have a velocity here and so where does that go. Well, it's evaluating as if it is stirring or a turbulence velocity. It's not the through flow velocity to the next node.

CHAIRMAN WALLIS: Well, I think you're going to have to look at the details of this somehow, because, you know -- it may be a brilliant idea, but I'm having trouble understanding it especially treated like this. There's no way that incoming flow going straight up there is going to stop, swirling things around in the way you've drawn that.

If you had said there was a level of turbulence, a mixing or something, I think I might come closer to understanding what you mean.

MR. HENRY: But that's exactly what it is. But the code has to have some concept that you're loyal to and how it incorporates this information into -- the information flow of what you're actually calculating.

MEMBER KRESS: Did you mean for your Ms in that first equation -- second equation to be under the parentheses?

MR. HENRY: This is what the M is, this has the same units of force. This is kilograms per second and --

CHAIRMAN WALLIS: There's no way that in the way you've described U_c that that first equation you've got comes from a control volume analysis. It comes from some kind of a word picture of some kind. There's no way you can draw those Fs on a box and show me how the linear force produces angular momentum.

MR. HENRY: Graham, I completely agree with that. You won't be able to take this into something and say, gee look that's now angular momentum. But by the same token, when it -- when you hit all these structures, and I'm just trying to follow through what you've done, is you've created turbulence. So some way this thing has a velocity that's different than the through flow velocity.

CHAIRMAN WALLIS: I think that this is important and you're going to have to establish there some sort of a believable, mechanical basis for these U_cs in terms of physical phenomena.

MR. HENRY: Absolutely.

CHAIRMAN WALLIS: Because I'm not looking for something that's academic and terribly fancy --

MR. HENRY: I know.

CHAIRMAN WALLIS: But this seems to be fanciful.

MR. HENRY: We'll take that as an action item. When you see us the next time we'll go through how we get to that.

CHAIRMAN WALLIS: We have to sort of buy off on this. It may turn out to be a brilliant move in terms of a way out of the box, so you're going to have to represent something which is important physically.

MR. HENRY: That's a very good way of putting it. You have to find something that's consistent with this big thing that represents 17 nodes. What's going on. Right. What's going on.

MEMBER KRESS: And I think this is a reasonable concept if you have real boundaries. But I'm not sure when you stick these virtual boundaries in --

MEMBER SCHROCK: Yes, I'm having the same problem.

MEMBER KRESS: That's why I think I need to see this, the validation.

CHAIRMAN WALLIS: I'd like to see it also --

MEMBER KRESS: I think it can be done, really.

MR. HENRY: Well, we have some experiments here that focus on just that thing.

CHAIRMAN WALLIS: I think what you're saying is if you open this door and you open that door and there's a draft going through here, it stirs up the fluid in the corners as well. Is that the sort of thing you're saying?

MR. HENRY: Yes. And the rate at which it stirs it up is dependent upon the -- you can't get it from mass balance.

MEMBER SHACK: But you're saying that the stirring is related to the momentum? You don't get the stirring without some momentum.

MEMBER KRESS: He actually has another equation that calculates this momentum going out. He's got momentum coming in and going out. It's the difference between those that goes into the stirring up. It is sort of an integral -- it's an integral amount. It has to go somewhere.

CHAIRMAN WALLIS: Because you always get the forces on the wall.

MEMBER KRESS: I'm ignoring those.

CHAIRMAN WALLIS: You ought to give him a D for that.

MR. HENRY: I knew we were going to have a lot of questions on this. You guys are true to form. You're still younger and quicker than I am.

Graham, what actually happens here if I take all of this out of here, is I would expect this to be a jet which begins to entrain as it goes up through here. It entrains on the way up and it hits up here, and it spreads and it comes down. But all that ends up being, stirring of this atmosphere, and stirring eventually -- well, basically it hits this wall and you take momentum out of it and you start turning it angular momentum now.

The way the code has a concept of that, because you can't -- it's very difficult to put all this kind of structural detail in --

CHAIRMAN WALLIS: The code doesn't have any concept. You write the concepts.

MR. HENRY: Okay. You're right. The way my code has a concept --

CHAIRMAN WALLIS: No. The way you imagine it does. Let's get it clear: This is some kind of a Henry fantasy.

MR. HENRY: This goes way back. We've had a lot of fun with this over the years. So it's always been his fun, though.

Anyway, what is imagined for this then and the way it gets incorporated into the code is instead of trying to represent all this through detail, as an example, for jet flow etcetera, is to put this in something that says okay let's do the momentum balance on this and it will be interpreted as a velocity, which is turbulence, circulation and that velocity, that influence is what's used to determine the shear on the wall, the energy transfer to the wall and also it's ability to entrain.

So, that's why I put this up as a concept trying to put this into a large code that you could easily track through what it is or what its influence is and what are all the things related to slowing it down, whether the influence is out of entrained structures. But you eventually have to get to drawing boundaries in the air someplace or you won't have stratification.

CHAIRMAN WALLIS: It would be easier for

me if instead of calling it circulation you said there's a schematic of -- there's a mixing, mixing velocities which are produced by the flows or something like that. The idea of the circulation with these big cylinders rotating is something that I have trouble with. But if you said -- same as you got flow in a pipe where the transfer to the wall it's governed by the turbulence which it's sort of set up by the main flow and you just don't say it's a linear flow because then you wouldn't have transfer to the wall at all. Let us somehow model the turbulence. I think that's what you're trying to do.

MR. HENRY: That is what we're trying to do.

MEMBER KRESS: But tell me, how do you get the momentum out?

MR. HENRY: Well, see, I've got to get even with him next time. We have a slide that's nothing but words and he's going to say can't you draw me a simple picture of this.

Okay, Tom.

MEMBER KRESS: How do you get the momentum out there with a pressure difference in an area, a lost coefficient bobbing between the node?

MR. HENRY: Yes.

MEMBER KRESS: So really --

MR. HENRY: Since it is a pressure different --

MEMBER KRESS: You don't have any screening in the momentum code.

MR. HENRY: We have the pressure difference that says what is the flow rate that's leaving the node.

MEMBER KRESS: Okay.

MR. HENRY: And that's evaluated. And what it carries with it is whatever that turbulence velocity is.

MEMBER KRESS: It carries its node velocity with it?

MR. HENRY: Yes.

MEMBER KRESS: Computing the pressure difference?

MR. HENRY: The pressure difference --

MEMBER KRESS: Is this that lost coefficient?

MR. HENRY: Yes. Yes, there's a lost coefficient if it's just wide open, then there's basically no lost coefficient. But, you know, the real fundamental thing at least we've discovered here, and that's what I want to also verify to myself as we work through this is what was the insight here was if you didn't this, then you never got the right answer. If you did have it, it didn't make much difference how much detail you went to as long as you said someplace that momentum got observed and therefore we had turbulence velocity which was higher than just the through flow velocity.

MEMBER LEITCH: You've talked about this containment of pressure and temperature in macroscopic sense but then do you calculate pressure and temperature in each one of these virtual nodes, or which one --

MR. HENRY: Each node has its own pressure.

MEMBER LEITCH: And I guess my question then is that it seems to me to say in the LOCA, that's where the LOCA occurs, you would have a higher pressure and temperature.

MR. HENRY: It does.

MEMBER LEITCH: Does that become limiting?

MR. HENRY: Generally not because it's usually the saturation temperature corresponding to the pressure in the room, and that's what we figure with most plant's design basis already is. But certainly the pressure and the temperature in the break room are highest. We'll get to a little bit of that later on.

MEMBER LEITCH: Higher than the previous methodology that are indicated?

MR. HENRY: Lower. Lower pressure and some are lower temperature than previous methodologies. Because you get more condensation by displacing the air. Temperature is also mitigated because of all the moisture that gets entrained back into the atmosphere. So it's hard to ever have super heat, which is again what the experiments seek.

MEMBER KRESS: When you say dry runs is submerged pressure, what does this submerged mean here?

MR. HENRY: It could be things like grading, I-beams.

MEMBER KRESS: Submerged means it's just in there --

MR. HENRY: This is submerged in the air right here, as an example.

MEMBER KRESS: Okay. It didn't mean it was under liquid?

MR. HENRY: No. No, it's just submerged in whatever the local fluid is.

MEMBER KRESS: Submerged surfaces normally is a function of exposed surface area.

MR. HENRY: Right.

MEMBER KRESS: It depends on which direction the flow is going. Does the code recognize flow direction somehow and --

MR. HENRY: No. No, it just thinks it's--

MEMBER KRESS: It takes the polarity of whatever the structure is --

MR. HENRY: If you have a pipe that runs through the room, you know, it doesn't care whether it's horizontal or vertical it has this turbulence velocity that's used. We value how fast the it can slow itself down.

CHAIRMAN WALLIS: How do you get drag on submerged structures? It's the circulation velocity that's dragging on this structure or --

MR. HENRY: Yes.

MEMBER KRESS: Do you use some sort of friction lost coefficient or --

MR. HENRY: Just drag coefficient.

MEMBER KRESS: Do you have form losses in it?

MR. HENRY: Well, it take it -- basically, again, it comes down to if you put it in the code, once you have it in it doesn't matter a whole lot on details. But what we really use are just drag coefficients associated whether we think it's a cylinder or a square, or -- it's usually a pipe or some kind of I-beam or grading --

MEMBER KRESS: So there is some sort of consideration of flow direction versus the orientation

MR. HENRY: Yes. Again, yes. I did not answer your first question right. We're always assuming it's going across it, it's not going with. You asked me a question, I responded incorrectly.

CHAIRMAN WALLIS: I think you have something like a K epsilon here. You're saying that the turbulence level in these nodes is a source of energy to be fed in to increase the turbulence which is the flows and then there's various frictions and so on are dissipating turbulence. So you get some measure then of atypical mixing velocity within the node. I think that's the kind thing you're doing here?

MR. HENRY: Yes. As opposed to saying it's only the through flow velocity, which I'll come back to in a second.

One other aspect is the condensation occurs under natural convection conditions. In MAAP we use the analogy for between heat to mass transfer.

So the thermal boundary layer is the same as what we have for the natural circulation flow. Of course, under laminar conditions, the Nussel number for all gaseous flow -- excuse me, for single phase flow is proportion to one-fourth power and then turbulent flow we have the lower Reliegh numbers, the one-third power at the higher Reliegh number, about .4 power which comes out of standard textbooks.

And what we use for that, there's the maximum of all these, depending upon what your specific conditions are. The Reliegh number --

CHAIRMAN WALLIS: Your whole idea of having circulation velocity is that the stirring enhances the forced convection and produces the transfer to the wall.

MR. HENRY: Right.

CHAIRMAN WALLIS: Now you're bringing in pre-convection --

MR. HENRY: There are times where stirring velocity is so slow it has no real relevance. It eventually dies away, in other words. But there are times when this is the governing process of energy transfer to the wall.

CHAIRMAN WALLIS: So you have mixed natural convection and stirring convection --

velocity?

MR. HENRY: You could also put here --

CHAIRMAN WALLIS: Is this why you put the max in here, is that --

MR. HENRY: Yes. Because as the velocity dies away, then this natural convection will take over. So you have to have a consistent way of addressing that as well.

CHAIRMAN WALLIS: There could be a condition where the circulation would actually act in the opposite direction of the natural convection and--

MR. HENRY: Right.

CHAIRMAN WALLIS: -- the net result would be to reduce the heat transfer.

MR. HENRY: Right. That's one of these pieces right here. When we use this, as I'll show you in a second, which is just a straightforward saying this looks nothing more -- it doesn't know that there's a film on the wall, you just have natural convection driven by the temperature difference and, therefore, what's the hydrodynamic boundary layer, what's the mass transfer boundary layer. We find that we under predict the condensation rate when we go to specific experiments, separate effects experiments.

MEMBER KRESS: What do you use for L?

MR. HENRY: This is the height of the wall.

MEMBER KRESS: The height of the node if it's the virtual node.

MR. HENRY: Yes. It almost cancels itself out, as you know.

So what we have -- we'll get to, is it's strictly correlating factor that says okay what are our differences. And when we look at the data, the higher the mole fraction of steam, the worse we do in this straightforward representation of going from single phase -- the heat transfer analogy, applying this single phase gaseous representation to the condensing potential.

MEMBER SCHROCK: What do you do for condensation on your horizonal surfaces?

MR. HENRY: The condensation on horizonal surfaces, it's usually dictated by the conduction on the surface. That's water, it's very low, of course. And if it's vertically -- if we're on a ceiling which is facing downward, then we end up using this same thing for experiments that I'll get to later. Because, obviously, we have ceilings to the containment and --

MEMBER SCHROCK: These natural convection for mass transfer formulations are not appropriate then?

MR. HENRY: That's right. And that's why we go to the experiments. When we get to these ceiling, which are facing downward, that is the representation that they see in those particular experiments.

CHAIRMAN WALLIS: I guess your document explains this F_m so I can understand it?

MR. HENRY: This F_m is right here. This is the correlating parameter, this is merely a viscosity radiogram that says --

CHAIRMAN WALLIS: Oh, that's viscosity?

MR. HENRY: This is a viscosity ration, this is to say this N_u of the gas over N_u of the fluid. It merely gives us -- this is the most -- as we'll see in a second, this is the most effected parameter here that says the more steam you have, the worse this representation does.

CHAIRMAN WALLIS: What is N?

MR. HENRY: That is the mole fraction of steam.

CHAIRMAN WALLIS: So there's something in your documentation that justifies this equation somewhere?

MR. HENRY: This is strictly --

CHAIRMAN WALLIS: So it explains where it came from in your documentation? If we've got the code documentation, could we understand where that came from?

MR. HENRY: I hope so. This is really just a correlation for -- this is dimensionless obviously, and these which we -- the viscosity ratio because we have to cover all pressure levels here, the reason this is to the .8 is viscosity squared to the .4 power. And this is linear because all this is is the fact that if you have low density gases that are being condensed, they can collect in the boundary layer and they can impose the natural convection which is going on. And there's a ton of papers in literature that say this virtually cancels itself out, and it does.

MEMBER KRESS: What happens to things that condense on the ceiling and other horizontal surfaces?

MR. HENRY: Let me come back to that when we get to the experiments in a second, if you would. Because we're going to certainly come back to that.

I just wanted you to understand when we get to natural convection, there is an enhancement to the condensation rate to the natural convection side that, again, comes from separate effects.

In the interest of time, I won't spend a lot of time on this because we already talked about it. The mass energy releases comes from design basis calculations as they are applied to the containment models for both sites.

And I'm not going to spend a lot of time on this one either, because it really says much the same thing. We look at all these types of accidents and as a result we'd like to find all the experiments that we could find that are applicable to these kind to test the total capabilities of the containment model.

The fact that the design bases mass energy releases come from separate models and they get their input to the containment model. So in essence what MAAP is calculating for the core in the RCS is just thrown away. It's ignored.

There's a mass energy release time dependent mass energy that's coming into the containment. That's exactly what we do to benchmark the calculation against these major experiments of CVTR and HDR. We have the mass energy releases which are specified by the experimenters.

So I think I came to you guys once before and said would it be worthwhile to try to make sure that we preserve some of this very key data and put it in the codes and that's really what we're trying to do here as well. So it's preservation activity as well as a convenient way to benchmark the code on a continual basis.

Experiments that we currently have pulled together, and this again, as I said, this is one of the key places that we want to have your feedback, is separate effects.

We've used the Dehbi condensation experiments at MIT, the Anderson condensation experiments at Wisconsin, the Hitachi condensation experiments which related to a containment test but it gave us another separate set of tests that we could compare the condensation model against under natural convection. Uchida condensation experiments, Tagami. When we get to the spray experiments they just lightly touch on the nice thesis that was done in Canada by Kulic for both single droplet as well as spray header behavior. And for countercurrent natural circulation we used the salt water, the brine water tests done by Epstein and Kenton for countercurrent natural circulation where you have both heavy over light as well as heavy over light with a through flow induced as well to assess that set of conditions that could flood the natural circulation flow too.

So these are the separate effects tests that we've built up to date.

The large scale integral tests include small break, large break and main steam line like conditions for HDR. CVTR tests -- I should say the HDR tests are all international standard problems also. CVTR tests are steam into a containment. Steam came from an adjacent power plant.

And the containment standard problem tests were done at the Battelle Frankfurt facility. There are two different types of hooking up of that particular set of containment compartments.

By doing these, of course, we're also demonstrating the use of external M&Es, because that's what these are.

MEMBER KRESS: Have you checked into the Marveicken --

MR. HENRY: We have, and they're so dominated by the suppression pool.

MEMBER KRESS: They are, yes.

MR. HENRY: But, indeed, those are ones that we like to add to this whole thing but not so much for these guys.

That's a good point, Tom, because I wanted to -- there are a couple of other experiments here. One in particular is a CSDF test at Hanford. While it's ice condenser related, it certainly enables you to see what's this code going to do for the natural circulation flows that they put into those compartments. So that's also part of it, but not listed here.

And the separate effects, one is the experiments that we used heavily were the experiments performed by Dehbi at MIT. And this was interesting to us because you had a very long condensing length, even though this is maybe something like an inch to an inch and a half or so, but it had 3.5 meter condensing length that gave nice natural convection conditions to benchmark the model against. And they also, obviously, had air as the noncondensible gas and they put in light gas to see what the influence was with helium also.

MEMBER KRESS: The vials for the outer chamber heated or insulated --

MR. HENRY: Yes. These were insulated here so that the steam came from boiling water and the cold water was flowing through this copper condensing cylinder that they have here.

CHAIRMAN WALLIS: And you put in a certain amount of air so you have some noncondensibles?

MR. HENRY: Right. And in some case they have a set of experiments where they bled steam through the boiling water as well.

CHAIRMAN WALLIS: Now your code with nodes in it, now it really doesn't address the question of how do you predict the heat transfer coefficient in a geometry like this, does it?

MR. HENRY: Well, natural convection heat transfer coefficient that I just showed you, you could either benchmark it based upon the condensing coefficient on the wall just due to the natural circulation condition --

CHAIRMAN WALLIS: Are there correlations for a cylinder inside a cylinder or this kind of natural convection?

MR. HENRY: Well, I can show you exactly what --

CHAIRMAN WALLIS: You borrowed them from some other context, or something?

MR. HENRY: The size of the cylinder means that this almost looks like a flat plate in terms of what the -- vertical flat plate in terms of what the natural convection is on the outside.

CHAIRMAN WALLIS: But it's long, so you have flow --

MR. HENRY: You do the hand calculation, this steaming rate is nowhere near what it takes to flood the film. This steaming rate is very slow.

MEMBER SCHROCK: Does it count as turbulent film?

MR. HENRY: The following film?

MEMBER SCHROCK: Yes.

MR. HENRY: Yes. The turbulent film gets fit for the turbulence.

CHAIRMAN WALLIS: I didn't mean it that way in terms of that sort of flooding. I mean you're going to use some sort of Nussel numbers or something or obtained from a correlation like the ones you've showed us?

MR. HENRY: Yes. It comes directly from those correlations.

CHAIRMAN WALLIS: Assuming that this is the same as flat plate in an infinite environment?

MR. HENRY: Yes. That's the assumption.

And this is the data. I apologize, these are pretty small figures, but this is at a pressure 4.5 atmospheres. In essence, one atmosphere of air.

This is at a pressure of 3 atmospheres and

1.5 atmospheres. For a variety of air mass fractions this is the way the data was reported by the experimenter.

And then this plus the two on the next page have helium fractions, this being 1.7 and the others the 4 something and 8 something percent helium.

Now, this solid line right here is MAAP4, which is just those mass and energy, the analogy of heat to mass transfer applied to this set of steam conditions.

CHAIRMAN WALLIS: So what you're testing is is this F_m?

MR. HENRY: In essence F_m comes from these, Graham. That's the correlation that comes from the separate effects tests. What's the reason that this-- why don't these equations work, as an example. Well, you can see, certainly, as we have more and more steam in here, the difference between those equation by themselves and the data increases.

CHAIRMAN WALLIS: When you say comparison of MAAP with Dehbi's, the only thing that MAAP did was introduce this F_m.

MR. HENRY: Right, and that's a correlation that comes from this information.

CHAIRMAN WALLIS: The information itself

came from these data.

MR. HENRY: Right.

CHAIRMAN WALLIS: It ought to fit them, it was itself derived from the data.

MR. HENRY: My only point here is to show you this is all fit --

CHAIRMAN WALLIS: How well it does?

MR. HENRY: Yes, how well it does and the fact that you've known correlations from day one, right?

CHAIRMAN WALLIS: Yes.

MR. HENRY: This is a value of that F_m of 1. I'd like to get that as close to 1 as --

CHAIRMAN WALLIS: And what this does is it justifies that F_m is a reasonable way of modeling condensation.

MR. HENRY: Exactly. Exactly. It's nothing to say this is how well this does. But then I'm going to take this same thing to all the other experiments before I ever apply it --

CHAIRMAN WALLIS: Okay, so now I begin to understand. Because, you know, you send us our slides ahead of time, which was a very good idea.

MR. HENRY: Obviously. Well, it's always a good idea.

CHAIRMAN WALLIS: Well, I'll look at this and I say what has this got to do with containment. It's really a separate effects to get the condensation coefficient.

MR. HENRY: Yes. And I should if I look at this test, I should be able to go to other tests and do just as well. If I don't do as well, I'd better broaden these uncertainties --

CHAIRMAN WALLIS: So you're not modeling any of the circulations or --

MR. HENRY: No, no, no. In fact, this is really set up to be just natural convection is the dominant thing.

This is a value of 1. This is a value of that F_m of .5 and 1.5.

Well, certainly from the standpoint of moving through various pressures, it does a reasonable job of bounding the data so we can find out the role of uncertainties or this uncertainty, where this particular thing applies in a containment analysis. But before we do that, we obviously want to go to a bunch of other separate effects tests and see just how well does it do with those as well, different geometries.

This is the same calculation and the only difference here is that little term I say we put in, here's the influence of light gas accumulated in the boundary layer. And the only difference between here and here is that term, and this a hydrogen -- or excuse me, a helium accumulation of 1.7 percent.

CHAIRMAN WALLIS: This is the average heat transfer coefficient?

MR. HENRY: Yes, it is. That's all they measured in that test.

Now I should -- I mean, to give the author credit, he developed his own correlation for what that was. This effective -- of course, this really should just be heat transfer coefficient here. That's my fault. But in essence, he had his own correlation. Again, following in the structure of the code he wanted to put something and clearly understand how the code's using it. That's why we put in our own correlation for it here, because we know exactly how the information is getting transferred from node to node to node.

But here you can see the obvious influence. If you have a one node model, so we're always sitting at some kind of mass fraction down here someplace -- let's see, I should be more like in here. Here. As opposed to pushing air out so some nodes may be condensing here, but the break node is much more down here. That has a tremendous influence on the peak pressure that you would calculate.

CHAIRMAN WALLIS: So this noncondensible mass fraction appears as N in this F_m?

MR. HENRY: NFST, that's all one thing.

CHAIRMAN WALLIS: Your also influence as FST?

MR. HENRY: Well, NFST is the mole fraction of steam. NF is mole fraction and ST is steam.

CHAIRMAN WALLIS: So this is saying that F is one plus something that's proportional to mole fraction?

MR. HENRY: Yes.

CHAIRMAN WALLIS: And the Nussel number goes up when N goes up or does it go down?

MR. HENRY: The Nussel number goes up with increasing steam mole fraction. The more steam we have in there, the more -- the measured -- yes, this is N for mole fraction and F and ST is steam.

CHAIRMAN WALLIS: We're looking here at noncondensible mass fraction.

MR. HENRY: Right. Since it's only air and steam you could --

CHAIRMAN WALLIS: Use it the other way around?

MR. HENRY: Right. But this is of steam here. I could turn it around, but that's the way the experimenter reported his data and I always try to be faithful to what he represented as information.

MEMBER KRESS: Well, how did he extrapolate against the cube of MC delta P of the water, probably, you've got the area.

MR. HENRY: Measured wall temperature --

MEMBER KRESS: Measured wall temperature?

MR. HENRY: And the environment temperature.

I just want to make sure, this was no indictment of his correlation, but we put it in in our own way and we know how the code's going to use the information.

CHAIRMAN WALLIS: Well, this looks a little strange, but I guess we've got to go on.

Usually when you put in a little bit of air it has a big effect, and this looks as if it doesn't. As a matter of fact, it's rather a gentle effect of putting in air. You have to put in a lot of mass fraction.

MR. HENRY: Well, we're going to get to

that, Graham.

CHAIRMAN WALLIS: Because there's a zero, and you may never get to zero.

MR. HENRY: Right, never get to zero there. Right.

CHAIRMAN WALLIS: Because zero is way off.

MR. HENRY: Right. We're going to get to that.

MEMBER KRESS: I think that's the reason we didn't get it the first time.

CHAIRMAN WALLIS: That's why I didn't understand it because F_m seems to be linear and steam fraction they always kind of leap up when you get very close to 1.

MR. HENRY: Here's a couple -- Virgil asked me a question before about what happens with vertical -- with the flat systems, and in particular the ones that are important to us are the downward facing systems, which are the containment doom as well as all the floors of the compartments. And that's why we focus on Anderson's experiments because he had, indeed, measured things, which I'll show you.

Let's go to his configuration, which were interesting to us so that we could relate what he measured in downward facing systems.

So he ha something that looked like the top of the containment all the way down to the side. So this was heat flux zone 1 up through 14. That goes from vertically downward all the way up to -- excuse me. Horizonal facing downward to vertical. And it was a slice of the containment-like geometry.

MEMBER SCHROCK: Is the water running down or is it dripping off?

MR. HENRY: Both.

MEMBER SCHROCK: Both.

MR. HENRY: Both. And the net result of what he saw as we go from 1 to 14 as shown here for a particular test, so this is horizonal facing downward. This is cooling plate number one. But here's number 14, so this is the one that's vertical.

And what he saw from the practical point of view is that there's no difference in the energy transfer rate to the wall. And he had two different ways of doing it with a heat flux measurement and a containment energy balance here. And I need to get back into his thesis to make sure I understand what these -- the relative uncertainties of these are, but that will come.

And what we gleaned from this is for downward facing systems there's virtually no influence. And, as you know, of course, when you go to those natural convection kind of relationships, the length essentially cancels out of it anyway.

So, this is just a preview of how we're going to look at it, but at least this gave us -- and this is things that Anderson reported -- of various hot flow of temperatures and wall temperatures, this is what Anderson measured as the heat transfer coefficient. And this is what Dehbi's correlation, which I mentioned here the author had formulated himself, shows.

As the temperatures increase, which means the pressure has to increase. These are reasonably close. If anything, Anderson's tend to be higher than Dehbi's or even more energy -- higher heat flux, higher heat transfer coefficient than what we're doing, except at this very low one. So this will also dictate when we finally get to doing this detailed comparison what the uncertainty boundaries are that we think have to come from separate effects tests.

MEMBER SCHROCK: So do we know where these experiments, where the relationship between the resistance in the diffusion layer is compared to the conduction resistance in the film?

MR. HENRY: For most of these that relate to design basis type of energy transfer rates, there's hardly any resistance in the film. Resistance is on the gas side and/or on the concrete wall, and the film is a very small amount of the resistance. We struggled with that for a long time, Virgil, ourselves and we went to the trouble of making sure that we had this Laminar to turbulent film transition. We saw no influence of it, but I'm not surprising you with it I'm sure, you've seen it many times.

MEMBER SCHROCK: This funny shift from a lower value in Anderson to a higher value in Anderson as you go across these conditions which correspond to higher temperatures in the steam environment.

MR. HENRY: Let me tell you where this comes from. This comes out of Anderson's paper that he put into literature. This table was in there --

MEMBER SCHROCK: I haven't seen it. Where was that published?

MR. HENRY: I can get that to you. I'm trying to think. I think it was in *Nuclear Engineering and Design*. But I will get it for you.

MEMBER SCHROCK: And this thing you showed us --

MR. HENRY: I've got to get his thesis so I could understand where these numbers actually come from, because he's obviously averaged over some of these plates.

I'm sorry?

CHAIRMAN WALLIS: On the previous transparency something went by me. You've got plates at different orientations, is it related somehow to the picture in slide 24. What were the various plates here?

MR. HENRY: This is looking at a frontal view of his experiment. This is the side view. So these plates are individual plates that have their own cooling core so they can --

CHAIRMAN WALLIS: This is like a sort of two dimensional containment?

MR. HENRY: Yes. Steam comes in here and they measure condensation rates in each one of these plates under average conditions that are in that table.

CHAIRMAN WALLIS: And the orientation makes no difference?

MR. HENRY: The orientation makes very little difference.

CHAIRMAN WALLIS: This might indicate that it's some sort of a circulation locally that's been happening rather than --

MR. HENRY: It is spinning.

CHAIRMAN WALLIS: Yes. Is that what's happening or is it --

MR. HENRY: I really think that -- and, again, I want to get his thesis so I understand more than what's in that particular paper. But there's a couple of things that have been going on here.

Obviously, you have heavy over light. But if you collect enough water in this region, which is just horizonal facing downward, that by itself is going to fall away --

CHAIRMAN WALLIS: And it drips off the top.

MR. HENRY: Drips off and that certainly tears up any stable boundary layer. And what you eventually get to over here, which is vertical, this is also in excellent agreement with Dehbi's vertical experiments.

CHAIRMAN WALLIS: This is with a lot of noncondensibles.

MR. HENRY: Right, this is.

CHAIRMAN WALLIS: So it's nothing to do with Nussel's film, and the limiting thing is in the air spout.

MR. HENRY: Right. Right. And to that

effect, I should also mention they went to great trouble to make sure the condensing plates weren't limiting, for obvious reasons.

But I found this to be very helpful in going to these containment conditions.

CHAIRMAN WALLIS: When this happens, there's some global replacement variable, which is the same for horizonal and vertical it's dominating everything. Gravity doesn't really matter in that in that global picture.

MR. HENRY: That's right. It's probably a mixture of setting itself up this way as well as stuff coming down this way, and so gravity doesn't matter much.

But this also gives you the kind of information you need to say, the containment side, they're pretty complex geometry, how do I treat this thing. And fundamentally what we say and our logic is the length already canceled out anyway, so from a practical point of view, systems which are facing downward we treat with the same kind of heat transfer coefficient, effective heat transfer coefficient, because that's what these are all put in. HTC is heat transfer coefficient.

There are a couple of things we should get to, so I'm going to -- I do need to leave here not too long after 5:00, Mr. Chairman, if that's okay.

For the Hitachi experiments and for the Uchidas, I put -- this came, again, from the Hitachi paper that shows their measurement. And they had a geometry that was related strictly to suppression pool. But again they were measuring the effective heat transfer coefficient. Graham, the only reason I put this up here is, here's your steepness that you were looking for. So that's all there.

And this is not my line, this is Hitachi's. However, the way in which MAAP looks at Uchida, which is shown on that Hitachi slide is shown here. So, that representation I showed you with F_m etcetera as a function, and now this is the ratio of noncondensible gas to steam. Here's the steepness and this is the way that correlation looks. And this needs to eventually have those same uncertainty boundaries put on it, but this was all we could do is digitize the information that came out in the original Geneva paper, which was a real tiny figure.

I'm going to skip the next ones because I want to get to some of the integral tests, because our whole process is to try to build the understanding from separate effects tests and then test their capabilities when we get to integral experiments. So, if it's okay with you, I'm going to jump to the CVTR experiments.

So, this is CVTR, which is a decommissioned containment now. I'll wait until everybody gets this. And they had a line from an adjacent power plant that came into here and it discharged into this node.

Now, a couple of things here. This is a 12 node model and as we talked before, this is a generalized containment scheme, so historically these nodes got added later down here. That's why 11 and 12 are down in here. And 9 and 10 are embedded nodes that are inside of the -- that we represent the refueling cavity and I forget what else inside. It just doesn't show up on this figure.

CHAIRMAN WALLIS: There are structures and things in there that you don't show?

MR. HENRY: Right. And that's part of the problem is, it's hard to find a description of all those structures. But there is in the experimental report, there is a specification of what the heat sinks are and the uncertainties that they subscribe to their estimation of heat sinks. So we use that. And to some extent we have to do a little bit of guessing of where they are, but there's only a couple above the operating floor. This particular thing had a steam generator on it. There's a fair size structure up here.

MEMBER SCHROCK: When these guys do it for the plant specific, they have to make these choices?

MR. HENRY: Right. They have to go look at where they have rooms.

MEMBER SCHROCK: Yes.

MR. HENRY: And they certainly have to have something which says I want to make sure that I can see stratification if it would ever occur.

MEMBER SCHROCK: But you make a comment that it's hard to come by that information.

MR. HENRY: For CVTRs it's hard to come by that information, because it's a decommissioned containment.

MEMBER SCHROCK: Okay.

MR. HENRY: In fact, it's being torn down now.

The reason I wanted to make this point here, there is a generalized nodalization scheme. You could hook nodes together anyway you want, so node 4 can talk to node 11, there's no sequential problem associated with it.

The other thing I wanted to show you was the thermal couples that we will talk about here in comparison, there's a thermal couple 28 that sits out in this location, I think it's at elevation 370. Thermal couple 11 is right below the operating deck, so it's in this region between these two nodes. Thermal couple 7 is sitting at, and I think it's something like 297 or so, it's right here. And thermal couple 5 is here. And just so you know where they are inside the nodalization scheme.

And one of the things I need to show the staff and of course this committee in the future is suppose we started with one node, what would we get? If we had two nodes, what would do we get? If we have four, what do we get?

And also, I can tell you ahead of time, basically if I would have made this one node, two nodes, three nodes, four nodes, I'd get something very close to what you see now. But don't take my word for it. I owe that to you in the future.

We used these 12 nodes because we wanted to see what are all the axial temperatures and the influences on containment pressurization. So, we'll keep coming back and forth to this, I'm sure.

There were three tests; test number 3,

number 4, number 5. The only difference is test 3 had no sprays at all in it, so steam went into containment and then it just cooled down over a number of hours.

In test number 4 they turned the sprays on at about 210 seconds at half the capacity that the containment had.

And test number 5, exactly the same except full capacity of the sprays.

So this is the pressure that's measured for all these different gauges throughout the volume. And, of course, they're in very close agreement, which is expected. And this is only the first 400 seconds, this is the same set of measurements over the first hour.

Remember that thermal couple 28 that's up somewhere around 370 or so, that's this thermal couple TC28 and showing here both the temperatures in node 1 and node 2, and this is that measurement for zero to 400 seconds and zero to 4000 seconds.

The sprays come on just about right here. You see a little kink right there, and that's when it comes on. And so all this that you see here is all being driven by the spray cooling.

MEMBER KRESS: The break is when the steam quit going in?

MR. HENRY: Yes, that's exactly right, Tom, the steam -- the mass energy stopped right here.

Now, why we do this to begin with. We thought MAAP4 was a pretty good code, as it generalized nodalization and it had the kind of energy with the natural convection thing I showed you that didn't have F_m in it, and it allowed air to be pushed around containment. And we did the best job we could with MAAP4 and this CVTR test, we had a pressure that was up here. It over-predicted the pressure by about 7 psi, as I recall.

The best nodalization we could think of, all the heat sinks, everything else, the best thing that we could put in there. So that's what really got our attention. What are we missing? And the thing that we're missing is when we do mass and energy balances, as an example, we don't end up having any idea of what that turbulence, whatever that circulation is because we never were solving for it. That momentum just disappeared. So everything that was driving through the containment was all just due to through flow and what it had to have to pressurize the various other nodes to the same pressures. And it would push air either way. But no way could we get it down from up here to there. So that's where the whole concept got started: What does this mean? What are we missing in these nodes?

The other aspect is the temperature. We over-predicted this temperature by something like 50 or 60 degree Fahrenheit. So we obviously had some things that were really missing in both. What governs the peak pressure, what determines the temperature in these nodes.

And we also looked at the rest of the temperatures as we worked down into the containment. So this one is TC11 you see here, which is right below the operating deck by 4 or 5 feet. TC7, which is further down. And you can see with this one having a peak of something in the range of 230, it's not too much different than right above the operating deck.

When you get further down, this is hardly increasing at all. And that's still a challenge to us because this particular rise that you see right here is only because the system's pressurizing because the pressure is going up.

So what we do in order so that we have some kind of perspective of what's going on, these three lines right here are a heat sink that's a quarter inch -- assumed to be quarter inch thermal couple, which is a big thermal couple, just sitting in that node. So how much would we slow down this measurement if we had this generic thermal couple sitting in there, because we don't know what that thermal couple or RTD looks like, that wasn't in the report.

Now you can see, that slows it down a little bit, but still not as much as -- there's something else even going on that makes that lower region even cooler. But if we didn't have this turbulence circulation velocity, we would really overstate this temperature again, and this would also be overstated because the pressure's higher. This whole thing is coming about because the pressure is going up and it's just eV to the gamma as a constant.

You can see certainly after the sprays come on, we get quite bit agreement down low in the containment as well.

And then we go to the very bottom of the containment.

This is TC5 for the first 400 seconds, the first 4000 seconds. And now we get much better, at least understanding that this could be because of some thermal response to the thermal couple and maybe it's seeing some water dripping down from the wall.

This is again the average temperature up in node 2, but the key thing I wanted to mention to you the CVTR provides is it has detailed representations of the liner temperature in the break region.

So this is the side of the liner that faces the gas space. This is the side of the liner that faces the concrete. And in CVTR the liner does not contact the concrete, at least not at the beginning of the test. It's separated by 3/8th of an inch. It doesn't mean that it couldn't be pushed out during the test.

But this is our evaluation of the liner temperature that's facing the steam. And the reason this data that's shown here and the data shown here is exactly the same, and the whole reason is that we don't know where that measure was taken. We just know where its elevation is. We don't know azimuthally where it was in the test report. This is that particular heat sink, which is our break node, which was node number 2. And this is the node right beside it at the same elevation. So we show a little bit higher temperature, of course, in that node than we do here, but at least we can see it's certainly following the liner temperature quite well, which is one of the evaluations that these guys have to do. They have to evaluate the liner temperature during these design basis calculation.

So that's why this was particularly important to us. And this one I'm going to put in better context for you in the future, but this is, again, the liner temperature. We have nodes in the concrete, which can be fairly thick. This is our node, and this is what their temperature is, imbedded in the concrete. And, again, these two are exactly the same thing. It's just that they're two different nodes at the same elevation.

I will put this in a heat flux context for you, so you can really see this in terms of how much energy, what's the transient deposition of energy into the concrete, because that's really matters.

CHAIRMAN WALLIS: I don't quite understand the lines here. The data are the results.

MR. HENRY: Right.

CHAIRMAN WALLIS: And there's something called MAAP calculations -- which line is that? There are two solid lines --

MR. HENRY: This is best estimate or what I should be calling realistic just to get --

CHAIRMAN WALLIS: Realistic and pessimistic.

MR. HENRY: Okay.

CHAIRMAN WALLIS: And then the data is way down below there.

MR. HENRY: Right. This calculation right here is basically the same as what's up here, because this is the inside liner temperature. There's the liner, then there's a gap and there's the concrete.

CHAIRMAN WALLIS: And your thermal couple reading is way down there?

MR. HENRY: No, this thermal couple is sitting in the concrete.

CHAIRMAN WALLIS: And what's the other --

MR. HENRY: This is our first concrete node. The node can be -- so that's why I say in the future I'll put this into the heat flux rate.

CHAIRMAN WALLIS: Somewhere in between.

MR. HENRY: Right. I'll characterize the transient heat flux, which is more meaningful for you. I apologize for that.

But this, of course, is easier to represent. Okay. They have temperatures in the concrete, how well you're doing there.

So these CVTR tests are very important to us because that was the first clue we had there's something that's really missing in this process and what is it. Unfortunately right after that are the spinning cylinders.

There's a couple more here that I'll go through very quickly, again, Graham, with your permission, just because this is a particular containment configuration that gets put together two different ways for these two CASP experiments.

CHAIRMAN WALLIS: It's very easy. We do this for homework. I mean, you could just take one of the rooms with flow in one and up the other side and do some of that room calculation, you would show that these flows in and out set up separation cells in the room; they wouldn't be quite like your cell, but they would be straight up.

MR. HENRY: That's right.

CHAIRMAN WALLIS: And you could actually predict from some of CFD calculation what the role of heat transfer should be. That would be not too difficult a thing to do.

MR. HENRY: You got my attention. I'm sure we'll come see you again.

CHAIRMAN WALLIS: Students do this for homework.

MR. HENRY: I guess we'll have to find someone who's younger and quicker.

CHAIRMAN WALLIS: Well, the frequency here has CFD capability. It could do the same thing.

MR. HENRY: Sure.

CHAIRMAN WALLIS: That might be the more realistic thing you should put in the cylinder.

MEMBER KRESS: Well, the building in some ways you have to validate the concept he is trying to put across.

MR. HENRY: And we'll go look for some things.

I also apologize, I skipped over the first HDR experiment very quickly, which is a large break LOCA to get to CVTR, which is more meaningful. The reason I skipped over HDR, not that it doesn't mean a lot, it does. But as I showed you earlier on with 1 and 5 nodes, there was no benefit to looking at circulation or turbulence or anything else. MAAP4 did a good job with HDR. But the CVTR it stunk, so we wanted to get right to the heart of the issue. And the reason was we believe we were not correctly representing the potential for energy transfer of the break nodes.

I should also mention the way we do this calculation of turbulence, etcetera, we get about the right kind of circulation velocity that was observed in CVTR, which is a very difficult thing to measure. They did have some -- I think they had turbine driven, turbine flow meters sitting in the annulus. You get down to the bottom of the containment, their velocity is almost none existent. So it's only a couple of nodes that see this enhanced energy transfer rate.

Okay. The reason I want to touch briefly on these, D15 with CFP1 with this schematic -- which was again, now, Graham, this is their schematic, not mine.

CHAIRMAN WALLIS: I realize that. The actual thing looks quite different.

MR. HENRY: Right. This looks like it's a straight through thing, which is what its intent was, but when you get to the real thing -- I guess we already went past it.

CHAIRMAN WALLIS: Did they clear the special building for this test?

MR. HENRY: Well, this is a whole series of tests. This went on for a number of years.

CHAIRMAN WALLIS: -- a series of compartments.

MR. HENRY: So this says, for those of you that may not have seen this before, it's breaking your node out of room 6, and then it goes through room 4 to room 8, then up to room 7 and into and out of 4. These two roles are in line, and then into room 5 and into room 9. So it looks all straightforward there. But just so you appreciate the complexity of it, when you look at the configuration, room 9 which is shown here, includes all this annular region here, which is there also, as well as this big hole in the middle right here, which is this hole right there.

So here's room 6 and this is the break pipe coming into it. And room 6 then flows through room 4 here, the level of path, and this is room 4, that little tiny thing, but it is the full height. And goes into room 8. At room 8 goes up into room 7, which is right above it. And room 7 over to room 5. Here's 8 to 7. And back through 4 into room 5 and then up to 9.

So it's a very complicated structure, but it at least gives you an idea -- gives you a test of how well you're doing representing the pressure distribution in this particular test.

I also wanted to mention to you that there are the two experiments; the test configuration of course comes from the test report. The mass energy releases and their uncertainties are characterized in the individual test reports. So we used this.

The additional information is, and this got us in touch with Teja Kantzleiter who was the key experimenter on this a long time ago. He was kind enough to send an email that defined the inner surfaces of the outer concrete walls, the thing that defined room 9, to have a 1 millimeter surface coating, that the inner walls had half a millimeter surface coating on both sides. So all those floors and ceilings, and that had a thermal conductivity of about .3 watts per meter degree K. And that the concrete itself, of course, is density to specific heat, and thermal conductivity to the best they could figure out was about 2 watts per meter degree Kelvin.

So the information that you have in front of you -- and again to try and get the most information to you, these are fairly small figures -- but this represents the transient pressurization for the most realistic behavior in containment. And where this says optimistic and pessimistic, the pessimistic also has in it their maximum mass and energy release. The maximum you can get from that uncertain analysis. And the optimistic has the minimum here, whereas this which is realistic is using what they thought was their best estimate of how fast this came in the containment. And, of course, these are measured with -

- it's a two-phase flow and they're estimating it from a momentum measured on the drag disk.

So this is the pressure in containment. Temperatures for the three different things, again, in these rooms close to the break. And this one is the break room.

And this looks like a real mess here, like a bunch of spaghetti, but what's shown, again, is this generic thermal couple. So if we look at the solid line, which is right here, as an example, that's the most realistic representation, and this is that generic thermal couple that we respond, and then right above it here.

So, again, I don't know what their thermal couples look like. I don't know if they're close to any structure, etcetera, but at least we can see that something is -- it's roughly a quarter of an inch piece of structure holding the thermal couple in place -- is one of the reasons these things could lag and then the temperature could stay up. Because out in here there is basically no motion going on. It's just radiation, the environment, and natural circulation. The blowdown's all over with back in here. Obviously the blowdown is over with right there.

And this because we had -- we had talked before about measuring the pressure differences throughout the containment, these are now compartment-to-compartment pressure differences. So now from room 4 to room 7 this is the measured pressure difference. Again, this is in terms of Pascals, of course, but it's negative because of just the direction of the flow. But this is from room 7 to room 8.

CHAIRMAN WALLIS: Well, the Pascal is pretty small.

MR. HENRY: Right, these are fairly small pressure differences.

This is 10 to the 4th here that we're looking at --

CHAIRMAN WALLIS: Oh, there is a 10 to the 4th.

MR. HENRY: Right. That's still not a big pressure difference.

And the other point I wanted to make to you, this one that was measured to be zero because there are things we still want to make sure that the code comprehends but does not comprehend -- as I said it's a work in progress -- this is room 4 to room 5. And that's that small little room where the holes are in line. So in essence we get streaming flow directly from 5 through 4 into 7.

CHAIRMAN WALLIS: And you don't deal with streaming flow very well?

MR. HENRY: Right now the way the code thinks of it, it goes from room 4 to 5 -- whichever way this is -- and mixes and then it goes out. So it needs a delta P to get out, that's why this is here. But in essence it says there's no reason for me to stop here.

MEMBER SCHROCK: Momentum never began and got quieted down.

MR. HENRY: Right, and that is linear momentum.

The only reason I wanted to show you that, is there's another test, again, schematically now, it's the same set of rooms but they're hooked together differently. So now the break is into that little room 4. Then it goes up and goes out those two holes, which in 4 it was streaming through this way. Now it goes out both ways into room 7, then into 8, into 5, up into 9 and eventually comes around into room 6.

So this shows you, it's the same set of figures now, but it's going into this little room here 4, so here's the pipe that's going into it, right there. So it's this little square, but it is the full height and this is a hatch on top which after the experiment was over we detected there was a leak path here from this break room into 9. So, again, we include that in the representation. But as it goes into that room, then it goes out sideways right here into room 7 and 5, whichever way it was. One of them went down into, I think, 8. Yes. And 5 went upwards into 9 here and eventually came around and filled 6 from down below.

CHAIRMAN WALLIS: So if you have one of these horseshoe shaped rooms, or whatever, I don't know how you describe it.

MR. HENRY: Yes. Half a ring.

CHAIRMAN WALLIS: Half a ring around it, you have the same circulation velocity in all parts of it in your model?

MR. HENRY: Yes, there's the same turbulence velocity in each node.

CHAIRMAN WALLIS: It's a first approximation, right?

MR. HENRY: Right. And the key thing here is --

CHAIRMAN WALLIS: I think a realistic model would actually say we'll model the annular ring as one thing and the cap as another. Two nodes instead of one. Even though it's one room, but it's so different.

MR. HENRY: Yes.

CHAIRMAN WALLIS: You're not going to do that?

MR. HENRY: Yes, I can. I can.

And since you've made that point, I should also tell you that clearly it represents this part of this room 9, there's 2 nodes out here, 2 more nodes up here --

CHAIRMAN WALLIS: There's all various nodes that --

MR. HENRY: In the calculation, yes. But I thought your point was in these also, because these -- here's where the half thing is. You could certainly do that.

One reason I thought this was also helpful is the first was the linear progression through the nodes. This is more like parallel flow paths.

And this is, again, the best estimate and most realistic for pressurization in the break room 4, and this is over the first 50 seconds, this is over the first 1000 seconds. It didn't come through very well, but that's 10 to the third here.

And then this is pressure difference from room 4 to room 9 and the first 2.5 seconds. And the pressure history of room 9 over the first 50 seconds, which is one of the nodes in the outside region.

This is -- 7 to 8 there's an example, which is break room into the next largest room out. Excuse me. 7 is the next largest room out and then 8 is the room it goes down into.

CHAIRMAN WALLIS: When you've got this concrete -- this insulating concrete wall -- doesn't the thermal resistance of the insulation actually end up dominating rather than the condensation side?

MR. HENRY: The only thing it's insulating is the paint. The paint matters --

CHAIRMAN WALLIS: I thought they said they had some coating on this.

MR. HENRY: Well, that's the coating, so it's like an epoxy coating. And that epoxy coating is only on the walls which are going outside, and it's there to be a sealant.

CHAIRMAN WALLIS: But it is a significant heat transfer, isn't it?

MR. HENRY: Yes, it is. And it is prior to calculation.

We don't have to go through this detail to compare. We just want to make that you can see that it's doing a reasonable job on compartment-to-compartment pressure history, transient pressurization as well.

When we look at all these things, whatever those various things that are happening in a point of time, whether it's natural convection, forced convection, etcetera, the uncertainty boundaries you have for each of those models that came from separate effects tests were the same in all cases. So you're not tooting one of those parameters for a specific test, and different tests.

The part which gets into uncertainties gets to a short set of propriety slides, so I don't know if we need to -- we can be out of here probably about 15 minutes.

CHAIRMAN WALLIS: Well, you said this was work in progress, so we're not -- you don't have to give an evaluation of the MAAP in its final form.

MR. HENRY: No.

CHAIRMAN WALLIS: This is just to let us know that you're doing it and get the feedback.

MR. HENRY: Get the feedback; I certainly got plenty of that, and I appreciate it. And if there's any experiments that you think that we should have in this mix that we have overlooked --

CHAIRMAN WALLIS: When will this come up

in its final form?

MR. HENRY: We have a deadline to submit to the staff in January, which is next year.

CHAIRMAN WALLIS: Fairly soon?

MR. HENRY: Fairly soon, yes. And then the staff has heard from us twice on this; once in June and yesterday to keep them updated on our approach by the experiments. We want to make sure that when it comes to the technical basis that we're looking at things that you guys think, that they're the driving force -- here's how I understand it -- it must be doing, what the containment must be doing.

CHAIRMAN WALLIS: So you expect to come to us again fairly soon with a finished product?

MEMBER KRESS: The staff review.

CHAIRMAN WALLIS: Or the staff has to decide you want to do that.

MR. HENRY: Right.

CHAIRMAN WALLIS: They may not want you to see us at all.

MR. HENRY: That's between you guys and the staff. Certainly if you want us to come talk about it, we're at your disposal to talk about it.

CHAIRMAN WALLIS: Well, it looks like a considerable step forward in the modeling of containment. Up there on the right-hand page dovetails with industry.

MR. HENRY: Appreciate that.

MEMBER KRESS: Of course, the staff, they plan, I guess, I don't know, access, too.

CHAIRMAN WALLIS: And I think also, since this seems to be key for you, maybe that can be used for these outbreaks.

MR. HENRY: Yes, we're particularly keen on making sure that once we have a model that goes with the experiments, that all that knowledge that's associated with the experiments gets transferred into their --

CHAIRMAN WALLIS: I think the outbreaks are going to come to ACRS anyway.

MR. HENRY: Right.

MEMBER KRESS: Do you view this as saying that in old code and this is a way to utilize that margin by getting rid of some of those conservatisms?

MR. HENRY: That's exactly right, Tom. But one of the ways that we would say that is that the top suppliers do the right things for the right reasons.

CHAIRMAN WALLIS: This margin isn't the real margin, it's a margin of something in theoretical equations, because you didn't know what was going on, you had to have a -- when you know more you don't need such a big margin. You probably mustn't get the impression that they're somehow producing a safety margin by producing an uncertainty which enables us to make a better decision.

MEMBER KRESS: It's a some kind of level of safety. I think you are reducing the margin, because we're going to uprate the power and we're going to put more stuff in, we are reducing the margin. This just tells you you've got enough margin there that you can do that.

CHAIRMAN WALLIS: I don't suppose you can tell us what you mean by margin?

MEMBER KRESS: The difference between the pressure and the design limit. The actual pressure you get for design limit.

CHAIRMAN WALLIS: Actual pressure, not just pressure.

So do you move on to the staff, then, or do you want to say a little bit about this?

MR. HENRY: Instead of passing out the proprietary slides, let me just say what's really in, because that how we treat the uncertainties, what we will eventually bring back to you. And what's inherent in the process is that we believe that the way you get closure is that you test against -- you develop your uncertainties with separate effects tests, and you work to these large scale tests for closure. And by closure we're looking for the realistic and the pessimistic and optimistic and we try to stay away from conservative, because sometimes we don't know what that is, given the attribute that you're investigating. And we look to see if we can bracket the data, not bound the data.

And once we're able to bracket the data, we feel we have a 100 percent and 10 percent kind of understanding of what's driving the bus and all these analyses and also in the experiments. That's really what's in the proprietary part of how we establish that closure.

CHAIRMAN WALLIS: Is this congruent with the CSAU?

MR. HENRY: Well, unfortunately I was part of that once upon a time. It is consistent with that, but -- and I was part of it when it was for direct containment heating. And the only thing that's different here from that is I tried to simplify it in my own mind to fewer steps. But I also established closure back to, say, you think you got this model, are you able to bracket the data with the model, given that uncertainties that comes from something else to allow you to understand the detailed physics with the processes that you're working or you just globally bound it? And we would prefer to be able to bracket it. These guys take it in-house, we want them to have something that the engineers know where it all came from.

CHAIRMAN WALLIS: You talk about the 95 percentile dosage, or just bracket?

MR. HENRY: We prefer to deal with just bracket, but you certainly could take this to a distribution. If you can do it with just bracketing, well again the uncertainty bounds for individual physics come from things like we saw with separate effects tests. If you could live with that, you shouldn't have to do anymore. If you want to look at a distribution, you got to go back to those and define the distribution and you put it into a Monte Carlo kind of approach at a plant.

CHAIRMAN WALLIS: Do you have separate effects tests of these circulation velocities?

MR. HENRY: You know I'm going to go look for them. For the CFD calculations of flow into a closed node, right?

MEMBER KRESS: This CVTR --

MR. HENRY: If I knew, I could find that book called two phased flow, but I don't remember those being in there.

MEMBER KRESS: Your CVTR containment model, are there virtual boundaries in it as well as --

MR. HENRY: Yes. I'm sorry, Tom. I meant to make that point when we were there.

MEMBER KRESS: Yes. I thought that was the one test where you really had --

MR. HENRY: I appreciate that. I'm going to show you a couple. When we get to the HDR there is a virtual boundary, but there's so many nodes, so many rooms in the containment that you need to represent all these -- or least virtually at least half of these. And there's a boundary up here because at E11.2 there was stratification.

When we get to CVTR, which I'm glad you made that point, because those virtual boundaries are here, here, here, here and this is treated as a virtual boundary because I can't find out what the grating was as you walk down. They're not very specific about it and all the pictures are above the operating deck. But this definitely is. These are boundaries here and that is.

These are not -- obviously as you can see, these aren't annular rings, these are just slices through the containment. So, this is half of a cylinder and this is half of a cylinder here.

MEMBER KRESS: How does MAAP deal with creating stratification when you got light gas and a heavy gas.

MR. HENRY: You can accumulate gas in the node just because it eventually gets transferred up and you slow down the condensation and slow down, therefore, the energy transfer rate. Or you could have it come in as it does in HDR at this kind of location and it has a plume model that evaluates its ability to mix if all this is really just relatively quiescent system. Mix and rise to the top, but if it's not completely mixed by the time it gets to the top, it accumulates. And those virtual boundaries, and even when we get to the plume model, that entrainment rate goes back to the Recue Spalding entrainment model, and then the kind of entrainment coefficient that we use is defined by their model is 0.1, which is basically what they say to look at real tiny gas-to-gas. But if you go look at volcanoes it's roughly 0.1. It's the best estimate of the entrainment rate of surrounding material.

Now we have bounding values on the other side of it that are pessimistic and optimistic, whichever the influence of the specific attribute that you're looking at.

CHAIRMAN WALLIS: At 0.1 it's like you're mixing when you get a plume that produces --

MR. HENRY: That's exactly what it is. That's where it all came from. If we got something that's an extremely powerful jet what's it doing.

MEMBER KRESS: In these containments, both of them have sprays?

MR. HENRY: Yes.

MEMBER KRESS: If those are working all of this gets overwhelmed by the sprays. The sprays do everything. So this is only if the sprays are assumed not to work?

MR. HENRY: No, the sprays don't always do everything. But they eventually get into plant specific analysis. But main steam line breaks, the sprays do part of it but it's still pressurizing. The only thing that turns around eventually is the M&E stops.

MEMBER KRESS: What is the time for the sprays?

MR. HENRY: The typical time for sprays is anywhere from 45 seconds to a minute. But for main steam line breaks, the M&E may last for 100, 200 seconds.

MEMBER KRESS: -- the time that you get into the recirculating mode.

MR. HENRY: Well, we're still in the injection mode, but it's still the sprays are not necessarily turning the pressure around, they're just slowing down its rate of pressurization. But the spray momentum is also part of this whole thing here.

CHAIRMAN WALLIS: I understand some folks have to go to the airport.

MR. HENRY: I appreciate that, Mr. Chairman.

CHAIRMAN WALLIS: I don't want you to go to the airport with too much momentum.

MR. HENRY: I'm going to spin out of here. I apologize, but we do have to leave because I do want to -- I will touch base with Rich, but the people from the sites will be here also.

CHAIRMAN WALLIS: The people from the sites are going to be here? I thought they were going to leave first.

MR. HENRY: Excuse me. Tom Beach has to

leave.

Thank you for all your consideration.

CHAIRMAN WALLIS: That was very interesting presentation and interaction.

MR. HENRY: I enjoyed it.

MR. LOBEL: My name is Richard Lobel, I'm with the Plant Systems Branch in NRR I didn't come prepared to make a presentation because the submittal hasn't been made. There was question about how we were going to proceed with the review, and we had a short preliminary meeting this morning to talk about that.

The review will be done in conjunction with Research. In fact the Office of Research will do most of the review because they, we felt, had the expertise and the others and also had the resources to do this. We wanted to make sure that we could do a very thorough complete review of this, and the expertise that's available in Research helps us do this.

We will do contained calculations. We will ask probably both licensees for the input to their specific calculation in one form or another, whatever is convenient for them and for us to use. When we do an audit, that's usually how we work things out. We have a conference call and ask them to submit it in whatever form is convenient for the people here who are going to be doing the calculations.

We also will be doing a little more of the study of the uncertainties in the containment experimental data. A lot of work has already been done by Research, and that was another reason for getting the Office of Research involved in what normally would be just an NRR review. Because they have a lot of expertise from work they've done in the development of the contained code and comparing with experimental data. And since Bob Henry didn't go into it very much because a lot of that was the proprietary part, but his method depends a lot on the use of experimental data in the calculation of procedure itself. And so we wanted to look in more detail at the experimental uncertainty, too.

We haven't thought about it in a whole lot more detail than that yet. We plan to do an aggressive review when we get the submittal.

The plant specific submittals aren't due until May. We're going to try to get the plant specific information before the submittals are made if that's possible so that we can start doing the calculations earlier and identify the significant issues as soon as we can.

That's about it.

MEMBER LEITCH: Have you used the MAAP5 before? In other words, are we looking at -- there's two things we talked about was basically the change from MAAP4 to MAAP5 and also the nodal concept. Has the change from 4 to 5 been reviewed previously?

MR. LOBEL: No, I don't think we even have MAAP5 in-house yet. They will be submitting that at the same time. I understand from talking to Bob Henry just before this session started that they will be giving us a copy of that at the same time they make the submittal.

MEMBER LEITCH: I see.

CHAIRMAN WALLIS: When you say a copy, do you mean a copy of the -- the modern copy of the code or you mean the documentation?

MR. LOBEL: No, the documentation. Documentation.

CHAIRMAN WALLIS: Do you actually a running copy of the code in electronic form?

MR. LOBEL: We may, and we may use that, but we'll probably -- the plan is now to concentrate more on using contained and comparing with their analysis and let them run --

CHAIRMAN WALLIS: with other codes the policy has been to endeavor to get an electronic copy of the source code so you can run it.

MR. LOBEL: Well, we may do that and, you know, we're certainly interested in your recommendations and suggestions.

CHAIRMAN WALLIS: Well, we definitely thought it was a good idea.

MEMBER KRESS: Well, this may be an exception. MAAP I think belongs to EPRI and it's not the licensee's code. It's not their privy to even give it to the staff I don't think --

MR. LOBEL: But on the other hand, if we really wanted that and considered that part of the review, the licensees would have to try to accommodate that as part of the review.

Let me say, a lot of this isn't going to be a detailed review of MAAP. What we're going to try to do more is review the method, because MAAP is a lot more than just the containment. And what we were going to try to do is -- the thinking is in NRR that there's a couple of different options for the review of MAAP that's still being talked about, as I understand, in the office. And what we would do is what we've been calling option one, which is look at the models that are pertinent to the containment and see that they're reasonable but concentrate mostly on doing an independent analysis and a review of the methods that are used in this procedure, which is a lot more than just the code. It's their use of uncertainty and experimental results. You saw that a little of that from the pictures he was showing.

So, it's not going -- the plan was not going to have this be much of a review of MAAP itself except the specific containment models that are involved and to concentrate mostly on audit calculations and correlations, and that.

MEMBER KRESS: I presume this is a changed licensing basis. Does that open the door for all other PWRs to come in and do the same thing?

MR. LOBEL: It could, it depends on the results of the review. What we've been asked to do now is just to do the review of a general report and then two plant specific analyses. But there was talk at the June meeting about maybe having them come in with a topic report that applied to more than just the two plants. There wasn't any talk of that yesterday, so I don't know what they're planning to do for that.

The broader the review is now, the easier it will be on us in the future. We won't have to keep going through this for four loop plants and ice condensers and what else it may apply to.

CHAIRMAN WALLIS: Have you reviewed their momentum equation formulation?

MR. LOBEL: No.

CHAIRMAN WALLIS: You've heard the discussion here?

MR. LOBEL: Yes.

CHAIRMAN WALLIS: It would be unfortunate if we had a code which seemed to work in comparison with data but which had somewhat bizarre interpretations of momentum balances.

MR. BOEHNERT: Extraordinary.

CHAIRMAN WALLIS: Yes. I'm sure Bob Henry is smart enough to fix that up, but what appeared here looked very strange. Maybe we're just being stupid. It just looked very strange. We don't want to get into a situation where something seems to work but the theoretical basis justification doesn't really stand up.

MR. LOBEL: Well, I think that we all can agree that the phenomena is there certainly --

CHAIRMAN WALLIS: For other reasons than the way that the math is actually sort of encoded in the momentum equation. Maybe that the phenomena going on which caused it are well represented by the way things come together. And then maybe then it's up to the person to bring together the documentation to give a technically believable justification then for what they do.

MR. LOBEL: The philosophy we've used in other reviews is to try to not get in a position that you were just talking about where the code may be predicting data but have something in it that isn't physically real.

CHAIRMAN WALLIS: That's the last thing ACRS wants to have to fight regarding the --

MR. LOBEL: I guess we've already answered this a little bit, I thought it would be worthwhile for them to come and give you a presentation because this is so new. It's a completely different approach than what's in the standard review plan now for the most part. We didn't have any plans to ask them to come back again, but it sounds like to hear from them after a point where we've gotten into the review ourselves, so maybe a presentation on what they've done and what we think of it after a round of questions.

CHAIRMAN WALLIS: Then you've got the submittal. They're going to have much more detail about the technical basis because, again, what we've seen so far doesn't really explain it well enough.

MR. LOBEL: Yes. I'll share that with you if you want to see the submittal when it comes in.

CHAIRMAN WALLIS: Does the committee have some other points at this time?

So what we need is just to -- the full committee meeting we need an oral presentation --

MR. BOEHNERT: We make a subcommittee report, or you're scheduled to make a report.

CHAIRMAN WALLIS: -- progress and that we have some questions.

MR. BOEHNERT: Yes.

CHAIRMAN WALLIS: And I don't think we need much time with the full committee.

MR. BOEHNERT: I think we've got a half hour scheduled.

CHAIRMAN WALLIS: All right. We're going to make it on time unless Professor Schrock has a lot of questions.

MEMBER SCHROCK: No, I'm going to my taxi.

MEMBER KRESS: Now we know how to fix it so Virgil doesn't have any comments.

CHAIRMAN WALLIS: Any reason why I should not recess -- okay. I close the meeting, is that okay.

MR. BOEHNERT: That's fine.

(Whereupon, at 5:27 p.m. the meeting was adjourned.)

CERTIFICATE

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

Name of Proceeding: ACRS Thermal-Hydraulic Phenomena Subcommittee

Docket Number: (Not Applicable)

Location: Rockville, Maryland

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.

Debra Wilensky

Debra Wilensky
Official Reporter
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