Official Transcript of Proceedings

NUCLEAR REGULATORY COMMISSION

Title:	Advisory Committee on Reactor Safeguards Thermal-Hydraulic Phenomena Subcommittee
Docket Number:	(not applicable)
Location:	Rockville, Maryland
Date:	Wednesday, June 26, 2002

Work Order No.: NRC-453 Pages 1-341

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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

(ACRS)

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THERMAL-HYDRAULIC PHENOMENA

SUBCOMMITTEE MEETING

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WEDNESDAY

JUNE 26, 2002

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ROCKVILLE, MARYLAND

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The Subcommittee met at the Nuclear Regulatory Commission, Two White Flint North, Room T2B3, 11545 Rockville Pike, at 8:30 a.m., Graham B. Wallis, Chairman, presiding.

SUBCOMMITTEE MEMBERS:

GRAHAM B. WALLIS, Chairman

VICTOR H. RANSON, ACRS Member

SANJOY BANERJEE, ACRS Consultant

VIRGIL SCHROCK, ACRS Consultant

STAFF PRESENT:

PAUL BOEHNERT

ALSO PRESENT:

STEVE BAJOREK - RES

DAVID DIAMOND - BNL

JAMES HAN - RES

JOSEPH M. KELLY - RES

CHRIS MURRAY - RES

RALPH MYER - RES

FRANK ODAR - RES

JACK ROSENTHOL - RES

HAROLD SCOTT - RES

AKIRA TOKUHIN - RES

HAROLD VANDERMOLEN - RES

QIAO WU - Oregon State University

C-O-N-T-E-N-T-S

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

8:30 a.m.

CHAIRMAN WALLIS: This is the Subcommittee on Thermal-Hydraulic Phenomena. I am Graham Wallis, Chairman of the Subcommittee. The other ACRS member in attendance is Victor Ransom. The ACRS consultants in attendance are Sanjoy Banerjee and Virgil Schrock. For today's meeting the Subcommittee with review portions of the Office of Nuclear Regulatory Research's Thermal-Hydraulic Research Program.

Specific topics to be discussed include: The Phase Separation Test Program being conducted in the Air/Water Test Loop for Advanced Thermal-Hydraulic Studies Experimental Facility located at Oregon State University; and the status of the TRAC-M Code consolidation and documentation effort; and the Rod Bundle Heat Transfer test program being conducted at the Pennsylvania State University. The Subcommittee will also review the proposed Resolution of Generic Safety Issue 185, Control of Reactivity following small break, loss of coolant accidents in pressurized water reactors.

The Subcommittee will gather information, analyze relevant issues and facts and formulate proposed positions and actions as appropriate for deliberation by the full committee. Mr. Paul Bennett is the cognizant ACRS Staff Engineer for this meeting.

The rules for participation in today's meeting have been announced as part of the notice of this meeting previously published in the Federal Register on June 11, 2002. A transcript of this meeting will be kept. At the present moment it is being recorded and the transcript will be made available as stated in the Federal Register Notice. It is requested that speakers first identify themselves and speak with sufficient clarity and volume so that they can be readily heard. We have received no written comments or requests for time to make oral statements from members of the public.

We are now eager to proceed with meeting. I will call upon Steve Bajorek from the NRC's Office of Nuclear Regulatory Research to begin.

DR. BAJOREK: Thank you very much and good morning. My name is Steve Bajorek from the Office of Research. In the past we have typically talked about the test programs all at once, looking at several programs all within the same morning, all within the same day.

Today what we would like to start doing is focus on each of these test programs one at a time.

Today we will take a look at the ATLATS facility, the test program that's being conducted at Oregon State University. In future meetings we hope to focus on some of the other test programs that are being done to develop models for TRAC-M and to resolve other important issues for research namely being the subcoolant boiling project at UCLA.

I think we will be looking at that next month and in the future the rod bundle heat transfer program. Like we started some work at Penn State, we've received some test results. That's why we want to talk briefly about that this afternoon in anticipation of a future meeting.

What we would like to accomplish this morning is to update you on efforts over the last year to develop improved models, refine the test data, to look at entrainment in a horizontal pipe where we have an upward oriented branch line. Some of this committee was at Oregon State last year. We looked at the test apparatus. We had a number of comments on that. We are going to try to resolve some of those today.

In addition I would like to outline some of our other confirmatory work that we're looking at now. We anticipate starting in the fall and through the early parts of 2003. These are also very much related to the problem that we will be looking at with ATLATS.

In the format that we hope to get out of these meetings is we want to get your feedback. What do you think of the models? What do you think of data? What are your suggestions for refining the program and coming up with models that would be of more value when we put them into a code like TRAC-M or RBHT.

Now the air/water test loop for advanced thermal hydraulic studies also know ATLATS was constructed in about 1999 and was intended as a facility to look generically of the problem of phase separation when you have a horizontal pipe with a branch line.

In general the problems of most importance for small break where you have a relatively small orifice connected to a large pipe and the question is how much of one or the other phase is swept along with either the gas or the liquid depending on whether that branch line is at the top, the bottom or at the side of the pipe.

One thing I would venture to state is that the interest has been probably been directed most at the bottom oriented break because that's of the most critical importance for a small break accident. At least in evaluation models that's the orientation you would get your highest peak cladding temperature because it drains system to the lowest level.

However in the AP600 design certification it was noted that correlations and models and relap had a very difficult time trying to predict the phase separation for this upward oriented branch. Therefore the initial test that were run in the ATLATS facility focused in on this upward facing branch and was trying to get test data and improved models for getting the carry-over into the branch line which would represent the 80 as for system in the AP600.

This is of critical importance to an advanced plant because high rates of liquid carry-over into this line one depletes inventory from the primary system but also increases the two-phase pressure drop through the ADS and lengthens the duration of time that you would have to wait before the IRWST would come on and bring additional coolant into the system. So most of the work has been directed towards that.

Now most recently the same issues have come back again in the design certification for the AP1000. As we reviewed the test programs and we looked at scaling for tests that were used to benchmark the Westinghouse codes for AP1000 two things stood out. First it was very difficult one even to do the scaling analysis from a bottom up point of view because of the lack of information for phase separation for this upward oriented branch and the complications that we see by having a branch line in the horizontal pipe. Typical flow pattern maps like Titel Ducler (PH) don't adequately represent what we've observed in facility.

But making use of what we could at that time a couple of things were very clear. The higher superficial velocities that we would expect in AP1000, 75, 76 percent uprating in power compared to AP600 would give us onset at much lower levels of water in the horizontal pipe. We would expect that the entrainment to occur over longer periods of time and to have higher entrainment rates in to the branch line of ADS-four system. This is very important for the AP1000 because of the uprated power for operating much closer to the conditions where we may get some core uncovering.

Secondly, one of the parameters that's important for sizing and scaling of the branch line is the ratio of the branch line diameter (d) to the hot leg diameter (D). One of the motivations for ATLATS with respect to AP600 is that ratio was significantly larger with that which had been typically investigated in other facilities. Most of the previous work had looked at branch lines to main pipe diameters on the order of 0.1. In AP600 that ratio is 0.34. If I remember correctly for AP1000 it's 0.46.

So if we had trouble in AP600 justifying these correlations for branch line entrainment and carryover the situation becomes a little bit worse for AP1000 because we're even further away from the initial database.

The other issue that was identified in the AP1000 scaling was the upper plenum pool entrainment and carryover, a very related phenomena. We also found that it was improperly scaled and that the test data didn't adequately cover the range of conditions for the AP1000. The reason I bring this up is we're looking at ATLATS as being a test facility to help us look at that problem in the future. As Dr. Wu will show everyone in a little bit, the ATLATS facility takes the vessel scaled one to one with APEX, the hot leg scaled one to one with APEX and then the branch line and does a good representation of the vessel to hot leg to ADS format.

We think we may be able to use ATLATS because it's low pressure, low temperature, air/water to look at some phenomena for pool entrainment with some proper modifications of the facility to help us address this.

CHAIRMAN WALLIS: It's actually more important because the most that entrainment from the hot leg can do by itself is empty the hot leg. But if you get a carryover from the vessel and you emptying the vessel as well.

DR. BAJOREK: In some regards it comes to a matter of timing. Early in the transient maybe right after ADS-1, 2, 3 or early in the ADS-4 the situation we would look at and where Dr. Wu's investigation would help is getting entrainment when we have very high levels over in the hot leg. Well, but at that point you're really not worried about uncovering the core. You're worried about getting to that condition. So how quickly things are entrained and how they are entrained are very important.

The ATLATS should help us get at entrainment which we've noted occurs due to intermittency in this part of the hot leg and due to a coherent plug that forms when the levels are high moving back and forth between the steam generator inlet plenum and the branch line. Most of your entrainment is localized over in this region. (Indicating.)

CHAIRMAN WALLIS: Wholly coherent plug because there's no steam flow in that steam generator side.

DR. BAJOREK: Yes, this side is effectively plugged and at least there's a high --

CHAIRMAN WALLIS: Fluid is being pumped to the right and there's nowhere to go.

DR. BAJOREK: I think what we saw in the facility we get momentum in part of this plug. It goes up. It comes back. It covers the plug up and back. It's trading off kinetic energy and potential energy in this plug.

CHAIRMAN WALLIS: It's very different.

DR. SCHROCK: And the two-phase slug as you've described passing the ADS-4 involves an entrainment process that has nothing to do with the level for incipient entrainment with the quiescent stratified surface present. So the point I'd like to make at this stage is that there should be a lesson learned here about how the models and the codes are examined when they are applied to a new situation.

It goes back to AP600. We didn't ask hard enough questions there about the physics of what's happening in this hot leg. It was simply presumed that that correlations that attempted to take care of this kind of thing and had been developed in the past would serve reasonably for this purpose. The fact is they don't.

There should have been in the AP600 a program to get experimental data that would answer these questions. It didn't happen then under DOE's sponsorship and it didn't happen again under Westinghouse sponsorship for AP1000.

It's really a little ridiculous that NRC should be paying for this when the beneficiary of the activity is making these absurd arguments over and over again that everything fits nicely and we have creditable calculations. Indeed they do not exist.

CHAIRMAN WALLIS: How do you use Tidel Dougler (PH) for right-hand side of your picture with this? There's no flow or the flow is solitary.

DR. BAJOREK: I think the lesson is you shouldn't. It may apply over a year and maybe the misconception that started with AP600 is that you can just go ahead and use that. It's a horizontal pipe. Missed maybe in that initial review is that the phenomena were just completely different. It may work if you had your classic large break where they was co-current all the way through the steam generator. You still have some questions. But this is completely different from what that had been.

DR. BANERJEE: If I recall in the AP600, we presumed as I was involved in the scaling that the flow in the ADS-4 system was homogeneously equilibrium even upper bound.

DR. BAJOREK: Yes.

DR. BANERJEE: That at least got the inventory down fastest and the pressure down slower. In fact it will improve because of the phase separation here. So I think we did a bounding scaling calculation there. I don't know how good that was.

DR. SCHROCK: I wasn't criticizing the scaling.

MR. BOEHNERT: Maybe we should. The examination of applicability of correlations that are in the codes.

DR. BAJOREK: There are two avenues to the scaling and we did follow your methodology for what we would call a top-bottom scaling approach. It globalizes many of the things that you may miss if you start looking at the details. Where this started to show up is when we did the so-called bottom-up scaling approach. When you start looking at flow patterns and entrainment and you need those correlations for specific phenomena, that's where things like this start to crop up and you can see the uncertainty.

CHAIRMAN WALLIS: I see in oscillations there's a system effect that this plug goes up ADS-4 and gets ADS-4 involved and there's a drop and then it changes this vapor flow rate. Maybe if we don't model this part right you won't model the oscillations right.

DR. BAJOREK: That's true.

DR. SCHROCK: Let me make a comment at this point that maybe you could address as the presentations go on. The problem I generally had or have seen in the past with a lot of this work on separate effects, correlations and what not, there's never any plan on how you are going to actually incorporate that into the code.

In other words, are the parameters you're actually using in the correlation available in the code? How are they going to fit in the framework of the code? If you can't put it into the code or leave it to somebody else, then there are always gaps between say the investigation and the actual application in the end.

We have fallen down on that point I think repeatedly so that it's very necessary to benchmark it against what you have at the current time and also develop a framework for how it's actually going to be utilized in that code. In fact, that should be done first. Then the experiment should be planned.

DR. BAJOREK: After Dr. Wu's done, I will talk a little bit about that. Yes, one of the things we are trying to do in the branch is as we do the test programs, begin to make those modifications to the code, do the validation on the models then so that you know if they work or they don't work or what needs to be refined while the test program is active and not do so a couple of years hence after you may have even torn down the facility or the people have disappeared. We're attempting to do that.

From our meeting last year, there were several issues raised. One had been around based on Dr. Schrock's comments in looking at some of the early literature project reports that we had. They weren't focused on the problem at hand which was looking at the upward facing branch line. It was difficult to segregate out what we really hoped to gain from this series of tests because it was mixed in with work for side-oriented branches and down-oriented branches.

In addition, how some of the works were referenced and described were confusing. So we asked that this part of the report be revised. We've gone through there. We've reformatted it. We think we've made it more accurate at this point.

MR. BOEHNERT: I'd like to make another comment along those lines that you might want to think about. It's along the lines I think what Virgil was talking about. This is oriented only towards the top rig. Now there are bottom rig problems where you have vapor pull-through which is the analogous situation with the entrainment phenomena. Side branches were a combination of the two.

If this is to be generally useful to the NRC I would think you would want to address all of these because the same model or type of model is used in the codes for each one of these processes. So it would seem to be good to have a program which is going to address maybe tomorrow it might be a bottom break you might want to look, an instrumental line break or something. So you would improve those as well.

DR. BAJOREK: The long term scope of the project as I mentioned at the facility was to look at other orientations. It's more the concerns with advanced plants in the upperward phasing branches deriving those tests be first. But in the long term plan, it would be to look at these other orientations. Right now we wanted to try to focus on that.

Two of the other issues that stem from last year's meeting is that one the flow plans were very highly oscillatory, intermittent. Nowhere near the classic horizontal stratified smooth interface that has been the starting point for many of the models for onset and for entrainment.

As we noted previously the typical Titel Duclar (PH) flow patterns descriptions based on co-current flow really don't help out the type of oscillations that we see in the ATLATS facility which we expect in APEX and to an extent in the AP Advanced Plant Designs.

Likewise the model development that last year was preliminary, it was based on again assumptions that the flow was going to be horizontal stratified. We learned that it's not really the case and that the flow was significantly more oscillatory. So in changing the direction of the program since last year we've asked OSU to go back, redo the literature survey, make sure that the database was directed more towards the problem at hand, and that the modeling efforts should try to capture more of the correct physics for the situation. Don't necessarily tie ourselves to the idea that this is a horizontal stratified flow. Assume there is intermittency, some type of an oscillating plug in this hot leg that we have to incorporate into the model.

In addition we had data from the pipes being capped off at the end, return lines from that side, no steam generator, with steam generator. We said let's focus on the data that's closest to the physical situation of the entrance which was where we had the steam generator and an inlet plenum essentially blocked off from the rest of the loop but we would have the situation where we induce these oscillations and have the steam generator at least interacting with that part of the leg.

So we've asked them to go back and do that. But we didn't change the overall objection however which is to develop models for a relap or a TRAC-M that can be used with the primary quantities that the code predicts. We need to try to get at things that the code can use and not try to track a plug going in the hot leg because the code numerics are not set up to do that but rather come up with models then you would get the global integrated mass flow out the branch line or the ADS system over dozens of seconds, maybe hundreds of seconds. That's the time scale that's of importance to AP600 and AP1000.

We don't hope to try to capture the individual blips and spikes that occur from the oscillations but rather get something that can capture the integrated effect of this over a much longer period of time.

CHAIRMAN WALLIS: So you're really developing a correlation or a method for predicting what happens in AP600 and AP1000. One shouldn't then take the resulting correlation and apply to a lone type without a steam generator.

DR. BAJOREK: No, I don't think so.

CHAIRMAN WALLIS: So you are not providing a general tool for a code with any kind of branch like this. You are doing something very specific for the particular geometry of AP600 and AP1000.

DR. BAJOREK: One of the things that Dr. Wu is going to describe is in his model he's going to come up with a method for looking at the wave length in the pipe relative to that pipe diameter. He's found that this model does an adequate job of not only the ATLATS data but also for the situations where you would have co-current flow in the pipe.

Now extending the model beyond AP600 and

AP1000, I think still is going to need some additional model development. We should probably look at some of the earlier data where we didn't have the steam generator.

MR. BOEHNERT: How do you reach a conclusion concerning cocurrent flow with that experimental facility? Are you doing some new experiments?

DR. BAJOREK: No, some of the older experiments had it either capped off or in some of them it was open over on that other side. So we would have to look more closely at those types of experiments. But, no, you're right. At this point we are looking at this model as being something that's applicable when you have a steam generator on that side and you have intermittency in that region between the branch line and the steam generator.

MR. BOEHNERT: One of the things that you haven't mentioned that I had commented on while we were at the facility is the fact that the construction gives rise to these relevantly sharp corners that the flow has to pass through. Going out of the vessel into the hot leg you have a cylindrical surface which intersects another cylindrical surface with a different access orientation. The edge is sharp.

It's not a smooth pattern as in the reactor.

I'm not so sure but what that plays a role in that development of these slugs. To the extent that it's a typical then the information that you'll get from the experiments involving any kind of intermittent flow seems to me remains suspect as to whether this would still be suitable for the nicely rounded entrance that you have in the reactor geometry.

DR. BAJOREK: I can't say that we've really taken a harder look at that. One of the things that we did do and Dr. Wu will show you some of the results as we did look at tests where the injection was different into the vessels rather than bubbling everything through the porous columns. It was injected toward the top at least trying to affect that interface and how things got into the hot leg. I think it will show that there wasn't a whole lot of differing.

Now that still doesn't get at whether the sharp edge was affecting there but it's a little bit of an indication of how things were coming from this vessel into this hot leg which were a bit robust. But no, we really haven't address that.

MR. BOEHNERT: You're still using or

attempting to use the level for incipient pull-through in the correlation scheme as I read these documents. One of the things that I wanted to point out that we hadn't discussed before is that the technique for observing that in the previous experiments differed from one to another.

For example, KFK used an acoustical method to indicate incipient pull-through. Whereas in our experiments, it was observed visually. There was a noticeable difference, a coherent difference between the two. I don't find this mentioned in the documentation that I've read on this so far. It may be a relatively minor point but it's something that ought to be examined anyway.

DR. BAJOREK: Okay. I'll look into that.

DR. SCHROCK: On the literature survey and the material that was provided I noticed that missing was whatever was in the codes that you are trying to fix. It would have been very interesting to have benchmarked what the correlations are predicting relative to what the codes are currently predicting to know more where the inadequacies are. I like to suggest that you do that.

DR. BAJOREK: I hope we can do that once we get this model and we can start doing a cross comparison in the codes.

DR. SCHROCK: The secondary advantage of that of course is that it would make the people more familiar with what is in the codes and then they might have a better idea of how to fix this.

DR. BAJOREK: It's the Rothe correlation that I don't think you have that on some of that.

DR. WU: This is Qiao Wu from Oregon State University. It was Dr. Schrock's correlation inside and we spend three months trying to moderate the user -- five and try to address it the problem you mentioned.

DR. SCHROCK: It would be nice if the result was in the report.

DR. WU: Yes.

CHAIRMAN WALLIS: I think whatever Westinghouse uses, they can tweak it. When they tweek it, they can simply we have more entrainment and the hot leg level just goes down a bit. That's no big deal because you're not emptying the vessel.

DR. BAJOREK: They are supposed to come back with a report where they are going to be looking at those types of things. The end of July is when we're expecting that. We have one where they compared two codes, one without an entrainment model in the hot leg and the other one I believe is Dr. Schrock's.

You get significantly more entrainment in that. You see a big difference in the codes but we still need the sensitivity I think to other models and then something that says whether those models are bounded or at least reasonably represented by hopefully this type of experimental data. With that I'm going to turn it over to Dr. Wu who is going to through the models and the latest work.

CHAIRMAN WALLIS: Thank you very much.

DR. BAJOREK: Thank you.

CHAIRMAN WALLIS: And you want a final word before lunch?

DR. BAJOREK: Yes, I have about three overheads to talk about the integral tests that we are planning for, the APEX facility and just a couple of brief comments on actually --

CHAIRMAN WALLIS: Could I ask the transcriber are you ready for the transcript now or you still setting up?

COURT REPORTER: I'm still setting up.

DR. WU: Good morning. My name is Qiao Wu, Assistant Professor at Oregon State University. My presentation today is "Phase Separation at an Upward Oriented Vertical Branch in a Horizontal Pipe."

The focus will be mostly about the progress after the last year or two July 16 ACRS meeting at Oregon State University.

The progress covers four parts. The first is on a database review update. The second one will be the facility introduction and just a review a little bit. We will deal with some instrumentation, evaluation according to the ACRS suggestion. Next I'm going to talk about the entrainment onset study and the entrainment rate study and also the experiment and modeling approach.

So the database review update was in response to ACRS's comments. Originally the database largely included the side branch vertical main pipe, all these different mechanisms mixed together. It's too large compared with what we are studying right now on the upward horizontal branch. So we narrowed it down and put more efforts on the description.

Of the tester facility, it tests the test conditions, instrumentation, model development of each investigation. Also it did a cross comparison. The cross comparisons were mainly just for the correlations since each correlation presumably matches their own data. So that's also our --

We didn't mention the measurement the

difference which was constantly consistent because of the shift of ten different investigations. That was Dr. Schrock's investigation and we're going to look into that and add it to the summary and the comparison.

The database currently included these six investigations. The first one was originated by Dr. Zuber. It doesn't have an experiment and modeling efforts but it does have a scaling analysis and also recommended a correlation for the scaling analysis.

Afterward Crowley did an entrainment onset experiment that it tested the data. I only found one point in the platform in a graphic form. It's not tabulated for the entrainment. Also the KFK has extensive work. They had two reports, one by Reimann and the other one Smoglie, her thesis. The model appeared in Smoglie thesis so their testor were correlative for air/water and a main pipe diameter which is much greater than the branch diameter.

In Berkeley under the leadership of Dr. Schrock, they did an air-water and steam-water test. Again the branch sizes are very small. It's 3.96 millimeter for the steam-water tests. For the air-water test it goes to about 2 centimeter.

In 1989 Maciaszek and Micaelli (CEA) did steam-water test but they presented it in a graph form and a non-dimensional form so it's very hard to get a meaningful result for the cross comparison. They recommended that their own entrainment onset correlation and entrainment rate of correlation. Finally JAERI did an air-water test and they have heir own entrainment onset of correlation. So this is six investigations that were covered by the database review.

DR. SCHROCK: You two of the five here that involve steam-water as opposed to air-water simulation. In the experiments that we did we found that the air-water and steam-water data agreed quite well when the break was in the steam volume. But when it was submerged, it appeared that there was a viscosity liquid, viscosity effect.

As far as I know that is still an unresolved issue. It seems to me that we're the only ones that have reported that in this context. Although in studies of draining of liquids from tanks and low gravity, Catton (PH) had also suggested a liquid viscosity effect for that problem.

If you are going to extend this eventually to look at the submerged breaks I think that's an issue that ought be reexamine because it seemed to be a clear effect in our experiments. It’s a mystery to me as to why it was not observed in the French experiments for example.

DR. WU: Yes sir.

DR. SCHROCK: Maybe it’s because they didn’t do air-water I guess is the answer.

DR. WU: The action is we’re documenting the data for the steam-water only the KFK data is well documented. You can find the CIGGRF -- for the rate of pressure but --

DR. SCHROCK: But there’s a specific recommendation in our report that it was an unresoluted issue that needed to be looked at. Another one was that we thought we saw a roll for the liquid axial velocity past the break which of course modifies the flow pattern for the liquid being sucked in to the break in the steam volume. That one I think in my mind remains unsolved.

In this particular application it appears there is no steam flow past the break. All of the steam going into the hot leg goes out the break. But in other applications and certainly in all the experiments there was a flow past the break that has an influence whether it’s small or large depends on the circumstances.

DR. BANERJEE: Can I ask you a couple of questions please? When you have ADS-4 initiation what's the pressure and what are the temperatures in the system?

DR. WU: The full pressure automatic. My understanding of the pressurization is at full pressure you open it to pressurize the system.

DR. BAJOREK: This is Steve Bajorek from Research. At the initiation of ADS-4 you're up at a pressure that corresponds to T-Hot. I think you're around 1100 or 1200 psi. This rapidly vents down to atmospheric. I think you spend most of the transient around 50 or 60 psi towards the end of it. So it is a transient.

DR. BANERJEE: And the physical properties, how much do they vary in terms of surface tensions and densities compared to air-water tests?

DR. WU: There's a range. Density for air-water you are talking about the -- process.

DR. BANERJEE: Sort of take a few points along it and give us an idea.

DR. WU: Say maybe 200 to about 800. Initially maybe 200 the density ratio and you create it to vapor and then finally it goes up until about 800. Thus the increase to 800.

DR. BANERJEE: So 50 or 60 psi is where you spend most of your time and your air-water experiments are at similar conditions.

DR. WU: Our full capacity is 110 psi. That’s our compressor.

DR. BANERJEE: And what about the surface tension? How does that simulate? It would be nice to see a comparison of thermal-physical properties between what you are doing in terms of a simulation with air-water and the range of conditions that you would get with the steam-water system to understand at least what the variables in this process are and what the relevant nondimensional numbers might be.

DR. WU: I agree but when we did the facility design basically according to the original Zuber scaling approach --

DR. BANERJEE: Who’s scaling approach?

DR. WU: Dr. Zuber. It’s basically a Froude number similarity in the hot leg. To my understanding it’s basically the pressure of the fluid in or fluid condition in the hot leg. Under the entrainment, the process mostly for the previous research found it to be like a Bernoulli effect. In entrainment you create the foam free surface and didn’t consider the surface tension there. When we designed our test facility and designed our test matrix we didn't factor that viscosity and surface tension effects into it.

DR. BANERJEE: Surely entrainment must involve something other than the Froude number.

DR. WU: Yes.

DR. BANERJEE: So what is the logic behind not having a weather number or some sort of capillary number or something there?

DR. WU: It's like -- It's the stability if you consider both the gravity and the surface tension as the stable force then you have to consider surface tension effect. There is a previous study by Dr. Schrock and Dr. Zuber suggesting and also the CEA correlation and they all throughout the surface tension effect that only using the gravity effect as the stable force examines the way we didn't test it so we decided we didn't need to go further to investigate the surface tension effect when we design our experiment.

DR. BANERJEE: Is there not surface tension effect?

DR. WU: I can't say.

DR. BANERJEE: Very similar problems occur in chemical plants.

DR. WU: Yes.

DR. BANERJEE: Onset of entrainment is very governed by surface tension what goes into the emergency relief pipes.

DR. WU: Yes. I'm aware of that.

DR. BANERJEE: So it's strange that surface tension doesn't play a role here. If you have air-water and steam-water surface tensions more or less the same you would get similar phenomena. But if you are trying to capture something where there's a wide variation in surface tension because of the wide variation in pressure and temperature, one would surely want to inform the correlation about surface tension effects.

DR. WU: I think I'm going to look into that to see what's around here for the prototype condition. But for my air-water test facility, I don't think I can vary the surface tension. It's a room temperature and atmospheric.

DR. BANERJEE: It's fairly sure that there's no surfactant effects here at all?

DR. WU: I'm not sure because we didn't put the time on that so we didn't look into the surface tension effect on this entrainment effect. That's later. Next please.

CHAIRMAN WALLIS: While we're looking at the figure there, the database, I looked at the diagrams that you had of these various facilities in one of your papers or some other that came through. It looked to me as though the Crowley-Rothe experiment had a side break. It looked as if the KFK figure 2.3.4 showed a slight on the side and on the top. But the Rothe figure shows it on the bottom of pipe.

DR. WU: Because these investigations are not only for the upward branch.

CHAIRMAN WALLIS: If you are only going to compare with upward oriented options.

DR. WU: Yes.

CHAIRMAN WALLIS: So it seems very confusing having these diagrams of Crowley-Rothe and KFK and Schrock which showed something else. But you are only going to select data where they had the pipe orientation like your pipe orientation.

DR. WU: The ways we copied their figures into the view. Since they did several combinations like a vertical side and vertical up and the bottom, they only show the one system plotted. Otherwise we need to replot it.

CHAIRMAN WALLIS: But it is clear then that you only selected the data from an upward oriented branch.

DR. WU: Yes, that's in the report.

CHAIRMAN WALLIS: It's misleading if you just look at the figures that you have there. For JAERI I couldn't figure out the break at all.

DR. WU: They actually concatenated their branch.

CHAIRMAN WALLIS: I think it's important because I think we realize from your experiments that the system matters. It's not just the break orientation I worry about looking at those figures. I look how long was the pipe and what was at the end of it and was there a chance of slugs forming and all that. It's hard to tell. Maybe your final report needs a better description of what these other people actually did.

DR. SCHROCK: Those past experiments we were charged with getting data for entrainment from a stratified upstream region. So the experiments are designed to produce a stratified upstream region. That takes some doing. It doesn't just happen all that easily.

The description of the experiment involves a lot of detail which is swept under the rug in your description of the background. If the literature survey has a purpose it's to understand all the information that can be gleaned from that assortment of experiments which have differences, maybe important differences, and you're not going to find any important differences if you don't pick up the rocks and look for them. When you read your report it doesn't look like you picked up very many rocks.

DR. WU: I'll put more effort on the comparison of the tester facility to modify it.

MEMBER RANSOM: One thing maybe you can clear up for me. From all the diagrams and everything it looks like the steam generator is voided and you would have steam flowing up. Unless there is no heat sink there maybe somebody could help me on why there is no through flow.

DR. WU: It's so the loop's -- The cold leg is looped back into the reactor vessel.

MEMBER RANSOM: You still have the possibility of condensation in the steam generator I would assume.

DR. WU: That compared with the ADS-4 line beneath. We need to connect it up some more.

MEMBER RANSOM: So is secondary side voided under these conditions?

DR. WU: Yes, it's a nonreserved

condition. It’s the --

DR. BANERJEE: Until it clears the loop seal if I remember now.

MEMBER RANSOM: But normally you would think you would have steam going up, down and condensing in the tube.

DR. BANERJEE: Yes, it doesn’t act as a heat sink under these conditions.

CHAIRMAN WALLIS: Heat source, isn’t it.

DR. BANERJEE: It’s a heat source in fact.

MEMBER RANSOM: To the secondary side.

DR. BANERJEE: Yes, if there is --

MEMBER RANSOM: Okay.

DR. BANERJEE: I’m just thinking back five or six years now. Joe is sitting there. He knows this stuff. What’s happens there exactly?

DR. BAJOREK: This is Steve Bajorek. At this point, the steam generator, ADS-4, has become largely ineffective. It is full of water. It’s a heat source but because there is no loop seal in the AP600 or the AP1000 and there’s enough water in the cold legs and the down comer you don’t have much flow going through there. So after I think it’s the ADS 1,2,3 initiate and you’re getting to this ADS-4 the steam generators are pretty much out of the picture.

MEMBER RANSOM: Okay.

DR. WU: It further has a volume cushion to affect this oscillation. Without that the steam generator is totally ineffective.

CHAIRMAN WALLIS: To follow up on my question, are you going to show us what Crowley and Rothe did so that we can get an idea of how relevant it is to your work? Or are you just going to take some data points and put them on a graph? I don't understand what these various people did from looking at your report.

DR. WU: Do you mean the correlation wise or experiment wise?

CHAIRMAN WALLIS: What did they do physically? What was the Crowley-Rothe experiment physically for example? I don't understand enough about it from your report to know how to judge how it fits into your work.

DR. WU: I didn't plan to go through that.

CHAIRMAN WALLIS: You're not going to show us a picture of their experiment.

DR. WU: No. I tried to present what we did and I thought that it was --

CHAIRMAN WALLIS: At least Dr. Schrock knows what he did.

DR. SCHROCK: Almost. (Laughter.)

CHAIRMAN WALLIS: This is a test to recall.

DR. SCHROCK: It's almost 20 years ago.

DR. BANERJEE: See if you remember.

CHAIRMAN WALLIS: I ought to remember the Crowley-Rothe.

DR. BANERJEE: I guess the more general question is are you going to be using data selected for upward facing takes or whatever from any of these two shore up your correlation.

DR. WU: We are going to use this one, Berkeley data and the KFK data. They are well documented. For the CEA and JAERI their data was published in nondimensional form and we couldn't reprocess it. We sent a letter to JAERI, Yonomoto, and he said he doesn't have motivation to send us the raw data. So we only have these two data sets available.

DR. BANERJEE: But do they also have upward facing?

DR. WU: Yes.

DR. BANERJEE: JAERI and the French.

DR. WU: Yes.

DR. BANERJEE: They do?

DR. WU: They have their experiment there. They are also proposing that the correlation and we compared with their correlation instead of their data because we couldn't get their data.

DR. BANERJEE: Because the ratios are closer to your conditions than some of the other data.

DR. WU: That's right. So we assumed their correlation fits their own data so we compared with their correlation because we couldn't get at their data.

DR. BAJOREK: Dr. Wu, I think the point that we really want to make is that out of dozens of studies that have looked at offtakes relatively few have taken a look at the upward facing branch. These are the ones that we think are closest to what you're looking at. However in no case do they have a steam generator or something on the other side of this pipe that would induce this oscillating plug that we see in the ATLATS facility.

Now the unique differences from each one of those, you really have to dig into the test report. JAERI had the fixture that could be rotated around and go through a separate orifice to get out. Maciaszek I think built a little weir on the end of his pipe to try to force a level. I think in the Berkeley experiments I think the pipe is open so that all the flow left. I'm not sure about the other two.

DR. SCHROCK: The liquid was pumped out of the vertical section at the end to maintain the level.

DR. BAJOREK: While these help to give us some data none of them are really representative of what goes on in the advanced plan. So we use these as a point of reference.

DR. SCHROCK: That's what I meant when I said the experiments were charged with an objective of understanding entrainment from a stratified surface. The experiments had to be done in a way that they produced a stratified surface. Those circumstances don't happen to correspond to what's happening in AP600 and AP1000. So the approach of using those experiments as I said a year ago doesn't make very much sense.

CHAIRMAN WALLIS: Well it might apply over a range. It might apply over the range where the level is low in entrainment onset and haven't developed the slug.

DR. SCHROCK: It may not be irrelevant. There might be a time when it is significant but they haven't found what that is.

CHAIRMAN WALLIS: But you can't just

blindly take the data and try to correlate it.

DR. SCHROCK: Yes. But what they did find is a situation which seems to be clearly very different from the physics of those experiments. So continuing to try to make the code regurgitate an answer based on the physics of the stratified upstream region in my mind is just asking for trouble.

CHAIRMAN WALLIS: Well, I noticed that the correlation developed in the ATLATS data better than anybody else's so maybe that's what we should focus on.

DR. WU: Thank you. We also tried to fit in Dr. Schrock and KFK data because they represented two kinds of extreme conditions. We hoped that this correlation will develop not just only for the ATLATS testing facility. That was our initial objective. We tried to figure out some mechanism behind it. Next please.

So the summary of these available investigations, publications was that Dr. Bajorek already summarized it. The ratio of the branch size versus the main pipe size for the prototypic conditions advanced plan is in the region of 0.3, 0.4. AP1000 is 0.47 actually going up. For the testing facility previous investigations mostly using small force, small branch.

CHAIRMAN WALLIS: So one could really conclude that you are in a different world altogether.

DR. WU: That's what is my intent.

CHAIRMAN WALLIS: You might learn a little bit from the thoughts that the other people had.

DR. WU: Yes.

CHAIRMAN WALLIS: But physically you're in a different world.

DR. WU: Yes, and we tried to figure out what the difference is. In the modern development especially for the entrainment the onset model we tried --

CHAIRMAN WALLIS: Something like a point source model might work for a very small little d/D but it's not going to work well for prototypic condition. Point sink I mean it would be.

DR. WU: The CEA test has a relatively larger branch so their correlation actually for larger branch using the lower -- correlation later I'm going to mention.

CHAIRMAN WALLIS: Actually I think Steve Bajorek the d/D was 0.46 or something.

DR. WU: 0.47, 0.48.

CHAIRMAN WALLIS: It's off your scale

even. It's even worse.

DR. WU: This is originally AP600.

MEMBER RANSOM: This red circle is the typical conditions you're saying.

DR. WU: Yes, originally I put about there 0.33 and I remember when we published the paper it said that's the proprietary information so I'm positive of those red dots.

DR. SCHROCK: Is that AP600 or AP1000 or both?

DR. WU: AP600, the 0.33.

DR. SCHROCK: And where is AP1000?

DR. WU: 0.47. Next please.

CHAIRMAN WALLIS: -- the L to steam generator over D.

DR. WU: It's for the branches.

CHAIRMAN WALLIS: I know.

DR. SCHROCK: A thought occurred to me that when you look at that process it's not unlike a manometer effect and a model which attacks it from that point of view might give you a better result than you are going to get by trying to put it in terms of entrainment from quiescent interface. It's the fraction of the time that a slug covers up the break and it's sucking in a lot of liquid then. Some other fraction of the time there's very little liquid getting entrainment.

DR. WU: Exactly. We tried to use the average parameters.

DR. SCHROCK: But your modeling doesn't do anything at all with the periodicity of that motion.

DR. WU: No, we used the average height, the average for all tests, the average void fraction in the downstream side.

CHAIRMAN WALLIS: This is meters per second you are showing here.

DR. WU: Yes.

CHAIRMAN WALLIS: D/fg is in meters per second.

DR. WU: Yes.

CHAIRMAN WALLIS: So KFK did experiments in the range of centimeters per second gas flow.

DR. WU: Yes.

CHAIRMAN WALLIS: That's not even a breeze.

DR. WU: You have a very big horizontal branch so it's a 200 and 0.6 millimeters.

CHAIRMAN WALLIS: But in Europe in AP600 it's much higher.

DR. WU: Yes. AP600 in this range.

CHAIRMAN WALLIS: And there's higher pressure so it will be squared.

DR. BAJOREK: Yes, for AP1000 during the ADS 1,2, 3 and 4 you are actually up with superficial gas velocities 10 sometimes 20 meters per second. You are very high. However if you compare that to ROSA, SPES and APEX in terms of a Froude number the agreement isn't all that bad. You're within about 50 percent.

DR. WU: This figure shows --

CHAIRMAN WALLIS: Excuse me. Once you get that sort of velocity I think you have to worry about other mechanisms of entrainment as Sanjoy mentioned. That's a range of velocity that you worry about ripping off droplets where surface tension is the restoring force and not gravity.

DR. BAJOREK: We looked at that in the scaling. When you start to do it with that it's almost like an annularized type of flow. You can see that yes they are very far apart in AP1000 and typical test velocities.

DR. SCHROCK: Am I wrong or isn't this JF the JF in the horizontal pipe?

DR. WU: JF is here.

DR. SCHROCK: The horizontal pipe, yes.

DR. WU: This is the horizontal pipe.

DR. SCHROCK: So JF would be zero in the AP600 and AP1000.

CHAIRMAN WALLIS: You never get to it on a long scale. It would be zero if there were entrainment and carryover.

DR. BANERJEE: But the average of zero. It would be very live locally.

DR. WU: It depends on the entrainment rate.

DR. SCHROCK: Yes.

DR. WU: So like Dr. Schrock mentioned their superb investigation is very focused to the stratified case as to their objective. They did a very wide range test and the data was well documented with the steam-water and air-water.

This figure doesn't necessarily show in the hot leg fluid region for all these -- far reaching map. We would like to say that we want to extend our JG to higher range I think recover the WAVY range and it also goes to the slug plug range. That's our objective to extend our database to a wider range and the inner scaling vicinity. However we couldn't get it to the annular flow for the bigger hot leg that we have.

Next please. So a summary of the previous two figures for the advanced plants as the branch size versus the hot leg size for AP1000 it's 0.47 and the ratio for AP600 is 0.34. These parameters are significant to the equation and those of previous investigations. So that's our motivation. We would like to run a test with a lot larger branch. Also the gas superficial velocity range in the previous tests is much lower than that in the advanced plant generally is greater than one meter per second. So we want to extend our database to higher JG range.

Also in the previous experiment investigations were applicable to co-current stratified flow. Traditional flow regime map in our test we showed was not adequate for this investigation because they showed that in that length that map we used just to represent where we are going to get our data.

DR. BANERJEE: This reminds me of what they call slop catchers in the oil-gas business where they have these horizontal pipes and vertical pipes which are almost the same size. They could go in one direction and the gas goes up. That's how they separated it out. There's a lot of data on that type of a situation.

See what happens is that you fill up the horizontal pipe with liquid and the gas gets thrown off. It's like a separator. Their velocities must be fairly similar. So leaving aside the nuclear literature you might look at the oil-gas.

DR. WU: Okay, I'll do that.

DR. BANERJEE: Slop catchers they're called.

DR. WU: They're called slop catchers.

DR. BANERJEE: You should talk to a guy named Jeff Hewitt about that.

MR. BOEHNERT: He's expensive.

DR. BANERJEE: He's expensive but he knows a lot about it.

DR. WU: Okay, I will work on that and see if I can find some data. For the correlation approach, the investigation focused mostly on two parts. First there is the onset of entrainment. All of them did as basically the Froude numbers are correlated to the onset height of this gas chamber height.

CHAIRMAN WALLIS: Which is d?

DR. WU: D is the branch size.

CHAIRMAN WALLIS: Branch size.

DR. WU: The lower case d is the branch

size. The upper case D is the horizontal pipe size.

CHAIRMAN WALLIS: So upper case D doesn't come into this.

DR. WU: No. None of the correlation has the --

CHAIRMAN WALLIS: It's just a pool.

DR. WU: Just a pool. Then when the liquid level is above the entrainment the onset level all of them using the actual gas spacing height versus the entrainment onset height and correlates the branch quality with this parameter and did a reasonably good job. We are going to follow this same approach because in the model unit to accurately predict this entrainment onset level and afterwards the liquid entrainment rate. So the logic would be the same. We don't do anything different. Next please.

CHAIRMAN WALLIS: So the argument is that once the liquid is lifted up against gravity it has to be entrained once you get it lifted up enough. It's gone. You don't worry about it.

DR. WU: If the level is above it you have to be entrained sooner or later.

CHAIRMAN WALLIS: A mechanism of the break up of the liquid is irrelevant.

DR. SCHROCK: I'm disappointed that I'm so lacking in ability to persuade you but I just don't think h_b has any relevance to how much liquid gets entrained out of those slugs.

DR. BANERJEE: We should go back to the previous slide.

DR. WU: Please you mean the attribute of the entrainment onset now.

DR. SCHROCK: The physics of that are so different from what's happening then.

CHAIRMAN WALLIS: He's reviewing the database. He's telling you what he did.

DR. SCHROCK: No, he's telling me he's going to continue to do it in the correlation that he's going to propose.

CHAIRMAN WALLIS: He's just saying this is what exists. What he has to propose you have to let him get that far. He hasn't proposed anything yet so maybe we should let him go ahead and see what happens.

DR. SCHROCK: I thought he just told us that he's going to do that.

CHAIRMAN WALLIS: This is what the other guys did.

DR. BANERJEE: The Froude number density ratio.

MEMBER RANSOM: Dr. Wu.

DR. WU: Yes.

MEMBER RANSOM: Didn't you just say that you were going to stick with this same framework?

DR. WU: No, a similar approach but the correlation is different.

MEMBER RANSOM: How about the parameters, h over h_b?

DR. WU: This one cannot correlate our qualities. So we have some other parameters like on the branch size and on the main pipe size.

CHAIRMAN WALLIS: Okay so you are going to do something different. I think we need to move on and see what you did.

DR. WU: I didn't make it clear. I followed the logic but we didn't follow this approach because we couldn't correlate it. We found this data cannot correlate by h over h_b. We added an energy balance equation for that later.

DR. SCHROCK: I'm glad Graham is able to distinguish this. I couldn't.

CHAIRMAN WALLIS: I didn't say I could distinguish anything. He's talking about what other people did and I want to go and find out what he did.

DR. WU: The summary of the cross comparison of these entrainment onset correlations you'll see the Smoglie correlation and the Berkeley correlation by Dr. Schrock are very similar for the small branch size.

CHAIRMAN WALLIS: I don't understand the ordinate there.

DR. WU: This is for the Froude number like we defined on the previous pages the gas velocity branch under the Smoglie.

CHAIRMAN WALLIS: I see. This is really a Froude number. What you call a Froude number V^2/gd.

DR. WU: Yes.

CHAIRMAN WALLIS: That number is really (V^2/gd) of Δ -- I understand. The whole thing is a Froude number.

DR. WU: Yes.

CHAIRMAN WALLIS: Now I understand. The Froude number must Δp multiplied by gd.

DR. BANERJEE: Can you go back?

DR. WU: Go back please.

DR. BANERJEE: The length scale there is d for the Froude number so it's --

DR. WU: It's a branch of.

DR. BANERJEE: It's a very strange Froude number because normally it would the height of the liquid that would enter into the Froude number. There is not a Froude number which has any physical significance that I can see.

DR. WU: That's based on --

DR. BANERJEE: The number is based on the speed of a gravity wave versus the speed of the flow right.

DR. WU: In fact there's a rate enforced there rating it this way. Actually this parameter basically, if we go back to the derivation of what Smoglie is actually you say the Bernoulli effect on the interface and they just rearranged the form to this kind of Froude number form. That wasn't there.

DR. BANERJEE: So the original Froude number involved the liquid level and then they had a novel thing and then it simplified into this form somehow.

DR. WU: Yes, they arranged it to this nondimensional form. It was originally the form of the analysis investigations so everybody followed the convention. Next please.

CHAIRMAN WALLIS: It's a funny plot you have here. Froude number.

DR. WU: It's like the gas velocity.

CHAIRMAN WALLIS: But you put it over six orders of manager. The range of data must be a very small part of the graph.

DR. WU: Now we select the plot of these. The data ranges was in this range.

CHAIRMAN WALLIS: Very small.

DR. WU: I just extended this to 10 because when I put h_b/d you go to one that is too small. I had do to two decades.

DR. SCHROCK: For the downflow case there was an experiment done at high pressure at INEL using hardware out of the Loft experiment. It was a very complex discharge geometry but it showed the extreme range of conditions that you get compared to the other experiments.

In order to put that on the same graph paper you had to have two or three decades, I've forgotten. But it's way off from the other data because it was at high pressure. In your report you say that the Schrock and Smoglie data don't agree but here is your graph that by all normal NRC standards of agreement is exemplary.

DR. WU: I think I need to make a comment here. This is entrainment onset. Basically your data and KFK data agree. For the entrainment to read along, KFK data has very high quantity 95 percent to 1.

DR. SCHROCK: So it was the entrainment rate.

DR. WU: That's two sets of data does not agree. But the summary of this figure, the KFK and the Berkeley correlation for a relatively small break group very well. Maciaszek correlation is for a relatively larger break has a different return. And the original -- correlation that's irrelevant because it's a pipe over a pool of water. But that gives the foundation as to how we correlated the data is the Froude number because it doesn't have the confinement. With the confinement, I changed to the Maciaszek correlation for a larger break and for KFK and Berkeley correlation for smaller break. Please.

MEMBER RANSOM: Before we go on what is the definition of the Froude number that you are using there. The codes don't seem to be defined in the report.

DR. WU: Go back please. One more.

MEMBER RANSOM: Now right there. What is the Froude number?

DR. WU: It's the V_{g3}/gd.

DR. SCHROCK: You're calling that the Froude number.

DR. WU: Yes. Without the density

modification.

CHAIRMAN WALLIS: What I said is the Froude number should include the density ratio as part of its definition. The restoring force is Δp gd.

DR. BAJOREK: I've seen it used a lot of different ways.

DR. WU: Go back.

DR. BAJOREK: The Froude number with the V/dg but the modified Froude including all of the density you're talking about there.

DR. WU: This is a single phase of fluid mechanics. You are usually using this one.

CHAIRMAN WALLIS: There is no Froude number if there's no Δp. The mechanism is gd Δp versus p V^2. That's what comes out of your dimensional analysis of the equations. That whole thing is a Froude number not the separate factor.

DR. WU: This whole set.

CHAIRMAN WALLIS: That's what comes out of our dimensional analysis of the equation.

DR. WU: I grabbed that one from a single phase of fluid mechanics book. They treated this as a Froude number squared.

DR. BANERJEE: And V is the velocity in the off-take, right?

DR. WU: Yes.

CHAIRMAN WALLIS: So the uptake is minute and I think this cannot be very good, can it, because Vg would be immense.

DR. WU: It would be -- to there.

DR. BANERJEE: That's the ultimate limit.

DR. WU: Can not be good to --

CHAIRMAN WALLIS: It's very strange that the surface knows what the small part diameter is.

DR. BANERJEE: It's seems that it's way up there.

CHAIRMAN WALLIS: It doesn't make an sense.

DR. BAJOREK: Now wait a second. Dr Wu, doesn't that come from treating it as a point source following the streamline and conserving mass in the pipe?

DR. WU: Yes.

DR. BAJOREK: That formulation originated by taking a look at the large quiescent pool with the pipe sitting above it.

DR. WU: Yes.

CHAIRMAN WALLIS: It actually it does.

DR. BAJOREK: So you need that area for continuity because he didn't know the size of this very conical region. Which is why in the early figures last week well why do we have what looks to be concentric ring. That was the initial formulation. Now engineers being engineers had taken that and applied it now to a pipe or a T type of geometry. But the formula or that format has remained.

DR. BANERJEE: I think Smoglie in her thesis did this. If I recall.

DR. BAJOREK: Yes.

DR. BANERJEE: At a point sink.

CHAIRMAN WALLIS: You cited Bharathan too.

DR. WU: Yes, those actually just did a nondimensional analysis. Smoglie did a point source with this streamline cursory interface. So we produced the result by adding a mirror source on the other side to preserve there is no flow goes through the interface.

Next please. For the entrainment rate correlation the KFK data is in this range. It’s 95 percent to 1 so they correlated their data in this curve. Berkeley, Dr. Schrock’s data, actually falls very low in this range to the 85 to 90 percent. So it’s covered well the range of quality. They correlated their data with this curve. The Yonomoto and Maciaszek had a relative larger break size and I just changed these two parameters to see how the correlations work. It goes on a curve like this. So it's spread three different ways.

CHAIRMAN WALLIS: It's about as big a spread as you could ever find.

DR. WU: Yes. So we think there are some other parameters other than h over h_b like Dr. Schrock mentioned. So we have to pick up that one to really group these together.

CHAIRMAN WALLIS: So maybe quality isn't the right variable. It is a mass flow rate ratio. Maybe that's not the right variable.

DR. BANERJEE: Quality is defined as homogenous quality.

DR. WU: That the flow quality.

DR. BANERJEE: Is it flow quality?

DR. WU: Yes, flow quality.

CHAIRMAN WALLIS: Qualities of flow.

DR. WU: So the gas mass flow rate over the total mass flow rate.

CHAIRMAN WALLIS: I'm always doubtful of that when you do air-water. Air has such a low density that I don't think the mass flow rate ratio is the appropriate quality to use.

DR. WU: We used the kinetic energy like

half of mv^2 and later forcefully convert it to the quality case. The mechanism is basically a kinetic case.

CHAIRMAN WALLIS: If you were entraining in for instance the Schrock's model of a slug coming by you just take off a great chunk of liquid and then you have all gas and a chunk of liquid. If the gas had no density at all, you would still be taking off the same amount of liquid. So it's not clear that the mass flow rate of the gas --

DR. WU: That density ratio has to be in the --

DR. BANERJEE: H by H_b equal to one means that the level is always at the onset of entrainment.

DR. WU: Yes. It's like the level by entrainment is going to go up. Go up to somewhere no entrainment occurred. That's the onset of entrainment.

DR. BANERJEE: I'm trying to understand the physics of this. H by H_b equal to one for the mass data --

CHAIRMAN WALLIS: That point there.

DR. WU: Should be here. You have only single phase gas goes through the branch. You don't have liquid there.

DR. BANERJEE: Right but you have the d by D equal to 3.0 goes to one at 0.2.

DR. WU: That's their correlation's program. They cannot go beyond so they stopped here. Their correlation --

DR. BANERJEE: From a physical viewpoint you are getting a quality of 0.3 at the onset.

DR. WU: No, this is the branch size. The nature of main pipe to the branch size has nothing to do with the level.

DR. BANERJEE: H by $H_{b.}$ If I look at your graph I mean your picture h is the level of the gas that is measured downwards during entrainment.

DR. WU: Yes.

DR. BANERJEE: H_b is at the onset of entrainment.

DR. WU: Yes.

DR. BANERJEE: So h by h_b equal to one is the level at the onset of entrainment.

DR. WU: Exactly.

DR. BANERJEE: So you are getting in fact that data shows that you get a wide -- If it was true that d by D 3.4 should be one. The quality should be one, right, at the point entrainment stops?

DR. WU: Yes.

DR. BANERJEE: But the other one is showing that it's one at 0.3 or something. That data.

DR. WU: By the work.

DR. BANERJEE: Is it that data or is it --

DR. WU: No this is a correlation. I used as our real parameter. The real size ratio of our hot leg to the branch and we applied their correlation. It goes here and it cannot go further because it's already reached one.

PARTICIPANT: Obviously there are other things going on.

DR. WU: So that correlation cannot be applied.

DR. BANERJEE: Even the other one shows that very rapidly you get this high and low quality.

DR. WU: So in entrainment the most we have is this right here.

CHAIRMAN WALLIS: I guess we can conclude that this old work is no good and move along.

DR. SCHROCK: Yes, right.

CHAIRMAN WALLIS: See what you did.

DR. WU: Let's see the test. The summaries are scattered.

CHAIRMAN WALLIS: It's more than a scatter. There is a vast disagreement.

DR. SCHROCK: The flow in the branch line is a critical flow so there is a good deal of flashing going on between the entrance to the branch line and the point where the flow is critical. It's always a little bit of a surprise to me that it has no impact on these determinations of how much enters the branch line and especially comparing air-water versus steam-water.

CHAIRMAN WALLIS: There's a feedback in the real reactor this critical flow at the valve.

DR. SCHROCK: And these experiments that there's critical flow out that path too.

CHAIRMAN WALLIS: That determines the back pressure and that puts to some extent what goes out this branch line because if you have a slug of water going up there that changes the whole flow rate and the pressure and everything else in the system because of the critical flow. There's force.

DR. BANERJEE: All the experiments showed exactly what you are saying which is that if you have a slug of water going through that one thing. Which is why we have the difficulties with this. That was pretty good actually.

MEMBER RANSOM: Act of desperation.

DR. BANERJEE: Yes.

MEMBER RANSOM: Just a little clarification, Dr. Schrock. All of the data is for that kind of case where the break line is choked?

DR. SCHROCK: There's a small fraction of the data that gets at such low upstream pressures that you don't get critical flow. But the majority of the data is critical flow in the branch line.

MEMBER RANSOM: Is that true in these experiments also?

DR. SCHROCK: Yes. They have about three or four atmosphere upstream pressure so they get critical.

DR. WU: So our test mostly here -- for the vicinity. This is the vessel. Inside it has seven spargers stainless steel porous rods. The air injection through these pipes goes through and sparge it out of these spargers. The water goes through this side in that and sheer off these bubbles from a pool boil (PH) simulating a pool boil (PH) in the inside of the vessel.

The hot leg is made of PVC. We actually welded the PVC by ourselves at a significantly reduced cost. We bought a welder for $400 so that we can make as many Ts as we want versus the casted acrylic. It's like a fraction of the cost.

MEMBER RANSOM: Do you have a scaled drawing? How close are those tops of those porous rods to the off-take line?

DR. WU: It’s scaled to the fuel rods or the top --

MEMBER RANSOM: So they are almost up to the -- Why did you choose that geometry versus just using a porous plate?

DR. WU: At the beginning we had a kind of ambition. We put several valves there. We said if we shut down the center we might generate some kinds of profile and see what’s the effect of that. We were trying to say we can’t have control over this part and that was the origin of that kind of thinking.

MEMBER RANSOM: This way you have seven plumes that obviously you’re going to get more flow out the top of the rod than you will out the lower parts just because of hydrostatic head effects.

DR. WU: It’s kind of low GIG so it -- because the vessel is fairly large so it has pool boil (PH) characteristics. You don’t see these kinds of vapor column coming out of the surface. In fact Dr. Wallis and Dr. Schrock were there and you didn’t see this shooting out gas for the surface because there was low GIG. Please.

So we had another problem about two d upstream and the two d downstream to measure the hot leg level. It's a kind of conductivity probe. Two types of tests were performed. The first one is the entrainment onset tests. Basically we filled the water above the hot leg elevation and then opened the gas valve and blow the entrained liquid out until it settled. No more entrainment. That's our entrainment onset level.

Then we tried to go upward. We fixed the gas flow rate and increase in liquid level to somewhere where it started to entrain. Each time you have to overshoot it so you can't observe the entrainment. Then finally it settled down so basically the approach is the same. We didn't see much difference.

I said that the type of test this is steady state entrainment. We inject fixed gas flow rate and liquid flow rate so we know the quality out of the branch. Now we go back to measure the level, downstream and --

CHAIRMAN WALLIS: H becomes the thing you measure to force the entrainment.

DR. WU: Yes. So we force the entrainment. We go back from the quality to find the edge. But in reality in the code you need the edge to really predict the entrainment so it's a backward --

CHAIRMAN WALLIS: You need to tell us how you measure h.

DR. WU: Let's go to the next page and I will show that. We correlate half ring type conductivity probe. It's a flush mounted side of this PVC material originally and sandwich it into two flanges. This tube or electrode that we're pulling out and then the level will be reflected in the nonlinear form. This is the output voltage. This is the actual height inside that.

CHAIRMAN WALLIS: What's the principle?

DR. WU: It's the conductivity.

CHAIRMAN WALLIS: Resistance. Conductivity.

DR. WU: Yes, resistance.

CHAIRMAN WALLIS: So you have to monitor the conductivity of the liquid all the time.

DR. WU: We actually ran this at 80 kilohertz so it's like an impedance probe. It has capacitancy factor. So it's an alternated current. So each time we ran the test we calibrated. After that we calibrated to avoid --

DR. SCHROCK: You have two rings and they are separated by a couple of feet.

DR. WU: No, it’s one half ring. It’s two electrodes to make this switch.

DR. SCHROCK: You have two flanges on this instrumentation. It’s only the one that closest to the test vessel that has the impedance probe.

DR. WU: Both sides have it.

DR. SCHROCK: Both sides. That’s what I said. So you have two different locations. It isn’t clear to me yet how the data from those sensors ends up giving you a height. How do you translate the experimental data from that instrumentation into the height of liquid?

DR. WU: Using the voltage output the impedance between this because the water level changes between these two electrodes. It’s reflected of the water level because there are changes of conductivity.

DR. SCHROCK: As I visualize this slushing going on, you have one thing at one set of sensors and another thing at the other one at any given instance. They’re both time dependent. So it isn’t obvious to me how you extract a single height.

DR. WU: No, it’s not a single height. It’s two heights.

DR. SCHROCK: Two heights.

DR. WU: We use the downstream -- because it will make you observe the slug appearance down in the steam generator section. The average height of the downstream is greater than the upstream side. It's different.

DR. SCHROCK: But the entrance to the branch line is between the two. What sense does it make to attribute it to one or the other?

DR. WU: Because right under the branch here, it's hard to instrumentate there to get the level. Also you see that there's a conical shape of liquid always being put there. So we are trying to either use the upstream or downstream level to build our model. We found actually and later I will show the movie entrainment mostly coming from the slug backward from the steam generator side being pulled out.

CHAIRMAN WALLIS: You measure the vessel level too.

DR. WU: I measured the vessel --

CHAIRMAN WALLIS: I would think the low flow rates, the vessel level and the hot leg are all the same. Everything's pretty horizontal.

DR. WU: It's not the same.

CHAIRMAN WALLIS: It's not the same.

DR. WU: We found like Dr. Schrock mentioned from a vertical vessel is like a cylinder to the side branch. There is also an issue of surface separation. The mixture level in the steady entrainment test in the vessel is higher than in the hot leg level.

CHAIRMAN WALLIS: The mixture level that is.

DR. WU: Yes, the mixture level is higher.

CHAIRMAN WALLIS: I keep thinking of how the code is going to work. Is the code going to predict these levels?

DR. WU: To my understanding the code actually says in the mixture level and the hot leg level is the same. When we used the -- five to model our facility we found that the level is the same. So I think maybe the work sponsored by NRC right now is performing in Milwaukee, Wisconsin they have some kind of -- about the form of vessel to the side branches as phase separation. Maybe that can give us some input to modify the code a little bit.

CHAIRMAN WALLIS: So it's at this separation process --

DR. BAJOREK: The results that you have seen, the idea that the mixture level was higher in the vessel than it is in the hot leg, is consistent with Cojasoy's (PH) work in Milwaukee which shows that most of what occurs in that hot leg is set up early in the hot legs. It's not a development all the way down the hot leg. But it occurs near the inlet nozzle and whether it's intermittent or stratified occurs over on that side.

CHAIRMAN WALLIS: The bubbly mixture in the vessel then you have a stratified from the hot leg you're going to have flow into the hot leg from the top and back out the bottom. There's going to be something that happens near that through the hot leg that has to be analyzed by itself somehow.

MEMBER RANSOM: You have to remember that the codes have a one dimensional view of the pipe. So there's only a center line. This is a limitation. And you have the void fraction which would tell you the level in the pipe but nevertheless the off-take from the vessel is a point. It's not a distributed area like you would have in a real situation. So you have to realize that the models have these limitations and it has to fit in that framework.

While I'm at it, would there be any value in actually feeding the gas flow from the upper part of the vessel and so you do have a stratified level in the vessel itself? I know some of the other experiments like Creare were run that way. So that you did not bother bubbling the vapor or the gas through the liquid and creating a rather messy mixture level in the vessel in effect. I realize the real situation is different than that. Then at least you can interpret the experiments more easily.

DR. BAJOREK: Go to your next one.

DR. WU: Next please. Also in response to NRC's comments that figure is actually later.

DR. BAJOREK: Okay.

DR. WU: We did some blow down from the top port. That's not from the side.

DR. BANERJEE: And there is a separated level. It's not churned up mixture coming back and forth.

DR. WU: Yes. There is a working condition where there is no slug.

DR. BANERJEE: But there is a level that is not full of bubbles and things, the liquid, in this.

DR. WU: Generally in this, no. You don't see much air bubbles being changed in the hot leg liquid.

DR. BANERJEE: You can see this visually.

DR. WU: We have two movies that show it. It's quite clear separation.

CHAIRMAN WALLIS: Do you have an analysis of what the hot leg level should be based on some analysis of how it goes from this bubbly mixture to stratified? Do you have an analysis of what happens at the junction between the vessel and the hot leg?

DR. WU: No.

CHAIRMAN WALLIS: That would seem to be important.

DR. WU: I treat the level out of that range as the parameter I would be interested. Otherwise I will follow the logic and say I'm going to use the vessel mixture level to correlate the entrainment rate. That really makes this model just work for an ATLATS facility.

CHAIRMAN WALLIS: Froude has to do something to predict that level on that hot leg.

DR. WU: Yes.

CHAIRMAN WALLIS: You don't have an analysis of what's going on. You were just going to use some blind general method.

DR. WU: I agree with you. I actually played with that and decided to correlate it but that's not our task order.

CHAIRMAN WALLIS: If h_L is the thing that matters in all this work you ought to be able to predict it somehow.

DR. WU: I can go back to all the data. We have all the data. We can look into that.

DR. SCHROCK: In the previous slide you showed what I guess is really a calibration curve.

DR. WU: Yes.

DR. SCHROCK: That is an excellent fit of the data to a curve based on a liquid level which is measured optically or some other way.

DR. WU: It's measure by DP.

CHAIRMAN WALLIS: If it's a flat interface.

DR. SCHROCK: Just a flat interface.

DR. WU: Yes.

MEMBER RANSOM: But then when you go to the use of the instrument with the kind of flow that actually exists there, you get this extreme scatter.

DR. WU: Yes.

DR. SCHROCK: Sorry. I'm still unclear on how one interprets an h_L from this kind of scatter.

DR. WU: Let me finish this one.

DR. SCHROCK: Okay.

DR. WU: What's the response to this. It

was last year ACS meeting mentioned our data sampling rate is like 1 Hz because we had like 20 channels and then we need to run 20 minutes. So we couldn't afford to getting a higher data acquisition read. Dr. Schrock and Dr. Wallis said you have oscillation like one second. You have a data sample in a rate of one second and maybe you're missing something.

So that required us to evaluate the data acquisition reading effect especially for the slugging effect. For the entrainment onset level when the level doesn't have a slug it doesn't have that much scattering. We have reported to Dr. Bajorek. These are used when slug appears. When slug appears you have these scattering, the physical scattering to this flow. It's not as a measurement scattering.

So my conclusion is the scattering due to actual liquid level fluctuations because the slug. So the rate about this is the downstream steam generator site. You can clearly see this scatter getting worse because of the slug here going back and forth.

CHAIRMAN WALLIS: Tell me what's being plotted here.

DR. WU: This is standard deviation of the liquid level over the liquid level and this is the average of the liquid level.

CHAIRMAN WALLIS: What is the sigma?

DR. WU: Sigma is a standard deviation of the liquid level. This is a fluctuation term.

CHAIRMAN WALLIS: The standard deviation of 0.4 would be pretty significant.

DR. WU: Because the new created slug coming up you have this --

DR. BANERJEE: Do you have a time plot?

DR. WU: We have -- time clock?

DR. BANERJEE: Time plot of the level.

DR. WU: Yes, I didn't put it in the presentation. Yes, for the 50 Hz, we're --

DR. BANERJEE: I mean it's not scattered. It's actually a wave or something.

DR. WU: Yes. It's a wave. So when you average it, then you see the standard deviation.

CHAIRMAN WALLIS: I'm not concerned about averaging because maybe the entrainment comes from the top of the waves and the bottom is irrelevant.

DR. WU: For the code you can only see the average. So that's our approach. We only put this log of frequency on the inside. I don't think it's compatible with our approach for this.

CHAIRMAN WALLIS: But clearly if you had a level which was smooth and you had a wave on it which went up to the branch pipe then you get some entrainment.

DR. WU: Yes.

CHAIRMAN WALLIS: And just using the average wouldn't reflect the physics.

MEMBER RANSOM: Is h_L the average?

DR. WU: The average in the liquid level.

MEMBER RANSOM: And average on the other side too? Sigma divided by average.

DR. WU: Yes.

MEMBER RANSOM: So each one is the average.

DR. WU: This is the average and this is the standard.

MEMBER RANSOM: And each data point is an experiment?

DR. WU: It's an experiment for the comparison, the open symbol and the solid symbol is the one Hz and the 50 Hz sampling read. You have a similar scattering. The 50 Hz doesn't make it better. So what we think is the one Hz is kind of slow but since we have at least a four minute duration so that average behavior is about the same. So we caught some slugs there.

DR. BANERJEE: But this is under different conditions or is this at one condition? I'm a little confused by this.

DR. WU: These are different conditions.

DR. BANERJEE: So many different conditions.

DR. WU: Yes, this is the average level. Each point is equivalent to four minutes average.

CHAIRMAN WALLIS: How big is the pipe?

DR. WU: The pipe is 6 inches ID.

CHAIRMAN WALLIS: Sixty or six?

DR. WU: Six inch.

CHAIRMAN WALLIS: So if we have an average of four and we have a sigma of range of 0.3 does that mean that the pipe is sometimes full of liquid? It probably does.

DR. WU: Yes. That's right because of the coming outpour. The slug.

DR. SCHROCK: But the data seem to show that you have a higher average level next to the steam generator than you have next to the reactor vessel. Isn't that puzzling?

DR. WU: I think it should be like that and this is what we expected the difference between the stratified and what we have right now. Since you only have this branch going there you have liquid momentum and gas momentum stop there and turn around.

So you have to have a certain kind of gravity here to stop the liquid.

DR. SCHROCK: So it's pushed some liquid up in the cold leg and on average it holds it up there as other stuff is exiting through the break and upstream the average level is lower.

DR. WU: Yes, amazingly we found the -- five actually has a momentum balanced model if we turn that some and the -- action with your entrainment correlation can actually predict this difference.

DR. SCHROCK: But it just aggravates the situation further in trying to use h_L as a parameter that has significance for the quality in the break flow. I mean h_L where. You have two different h_L neither one of which are at the entrance to the branch line.

DR. WU: That's a very good question which bothers us. We tried the upstream. We tried the average. We tried the downstream. We found the downstream correlated very well because we think the downstream washing the slug back is closer to the branch and the entrainment mostly happens governed by the downstream height. That's later in the correlation development.

DR. BANERJEE: I'm still puzzled by the

slide on the right. Each of those points is an experiment of four minutes duration.

DR. WU: Yes.

DR. BANERJEE: And what has been varied in these experiments?

DR. WU: We ran the tests like that way. Usually it is a fixed gas flow rate and inject a liquid for example four gallons per minute for five to six minutes. We increased the liquid again so it was step up so they never were changed. We went through that process. Then we go back to run the second series by changing the gas flow rate and repeating the change of liquid flow rate.

DR. BANERJEE: So let's take the average liquid level up the steam generator side as being four. The points which are in this vertical line if you have the same experiment exactly, the same gas flow rate, the same liquid flow rate, do those four minute duration segments actually change the standard deviation then? Sigma by h_L for exactly the same experiments, does it change? So let's say that h_L is fixed at four.

DR. WU: When slug appears this is scattered. It can be here. It can be there. There's no systematic shift that way as the gas flow rate --

DR. BANERJEE: No, I’m saying doing the same experiments do you get a scatter if I asked you to plot --

DR. WU: Yes.

DR. BANERJEE: So the four minute duration is not a sufficiently long average. Is that it? If you took an average for a long period of time. I’m missing something.

DR. WU: The average could be the same but the standard deviation may change a little bit.

DR. BANERJEE: Well, it’s changing by how much? I mean does it go from say 0.15 to 0.35, the standard deviation?

DR. WU: It’s 10 percent to 34 percent.

DR. BANERJEE: So for the same experiment you will get different standard deviations in this four minute segment.

DR. WU: Let me see. It’s the same experiment.

DR. BANERJEE: That’s what I’m asking. Is it that?

CHAIRMAN WALLIS: I think it’s different experiments.

DR. BANERJEE: That’s the question I’m asking you.

DR. WU: It's a different experiment but each time it changes.

DR. BANERJEE: So what is changing? If h_L is four -- Let's fix it at four h_L.

DR. WU: You cannot fix it. Since you change the fixed gas flow rate, you change the liquid flow rate.

CHAIRMAN WALLIS: It's a combination of liquids and gases.

DR. WU: So then you will have changes in the liquid average.

MR. KELLY: Excuse me. This is Joe Kelly from Research. Correct if I'm wrong, Dr. Wu, but I think sitting on this side I think I'm hearing it differently. I think what he has as a standard deviation is really the wave height. So each point is from one experiment, one combination of superficial gas and vapor velocity. So averaged over four minutes there's a height in the hot leg at this two different elevations.

What you see in the figure there's a difference in that average height between whether you're upstream or downstream of the T. What you also see if for that four minute duration the difference in the standard deviation of the measurements which were taken at either a one Hz or 50 Hz sampling rate. So you're seeing in effect the wave height processed through a standard deviation.

DR. BANERJEE: So each of those points represents a different combination of gas and liquid flow rates. That's not clear. What happens if you repeat the same experiment do you get a different sigma by h_L?

DR. WU: I expect the same average level and standard deviation.

DR. BAJOREK: That's a good question.

DR. WU: I don't know. If you repeated the standard deviation or not but the average level is the same because --

DR. BANERJEE: I know. But I'm asking you whether you get sigma by h_L the same.

DR. WU: I can look into that. I think it should be the same according your logic.

CHAIRMAN WALLIS: Not too different.

DR. BANERJEE: Within experimental error.

CHAIRMAN WALLIS: It depends on the frequency of the slugs. If the frequency of the slugs is eight minutes then --

DR. BANERJEE: Then it's wrong. The reason I'm asking you this question is whether there is very low frequency sloshing though I imaging four minutes is long enough.

CHAIRMAN WALLIS: You really need to look at a trace of height versus --

DR. WU: I can do that. I will look into that by just moving average and moving standard deviation to see when it converges.

CHAIRMAN WALLIS: I guess we can spend a lot of time on this figure but I don't think it's going to be used for anything else. We might as well move on. It's not going to be used for developing a model. It's just evidence that there's a lot of waviness going on. Now we are due to take a break at 10:30 a.m. and you're going to start a new part of your presentation and we're an hour late.

I think the reason it's late is because we've asked all the questions which was anticipating what you were going to say later. So probably you've answered the questions I hope which we won't have to ask later. So we can go faster if we take a break now.

DR. WU: Take break now.

CHAIRMAN WALLIS: I'd like to come back at 10:40 a.m. It's actually going to be a 11 or 12 minute break. We will start at 20 minutes before the hour. Give you a break. Off the record.

(Whereupon, the foregoing matter went off the record at 10:30 a.m. and went back on the record at 10:40 a.m.)

CHAIRMAN WALLIS: Back on the record. We'll continue with the presentation by Dr. Wu. Since we asked so many questions earlier maybe we can move along quicker and try to catch up so we can get lunch probably around 12:30 p.m. if we're lucky.

DR. WU: Thank you. This is Qiao Wu. First I'm going to talk about Entrainment Onset Study. So starting from the experiment the addition part that's in response to the ACS suggestion. Last year when Dr. Schrock and Dr. Wallis saw the interface and what the interface effect on the entrainment onset correlation. They suggested if we could inject gas on the air from the top.

So we did that but the gas flow rate was not that high compared with the main pipe injection because we put a hose to the top and we put a T inside so the air is not directly blown to the surface. It's blown to the side in such a way to suppress a little bit of an interface.

Next please. We found it does have for this figure it's h_b over the main pipe diameter and I correlated it with our new correlation as later I'm going to introduce. I found that the open symbol is the data obtained from the air blow injection from the top. It doesn't have an effect but consider the scattering of the data we could not differentiate it. So the conclusion here is it's not significant. It does have a little bit of an effect on that.

CHAIRMAN WALLIS: But it does tend to be bigger. The h_b tends to be bigger when you have top air injection.

DR. WU: Yes.

CHAIRMAN WALLIS: So that means that it's a more stable situation with the top air injection which would make sense when you're not shaking interface so much.

DR. WU: Yes.

CHAIRMAN WALLIS: But the deviation is getting worse as you go down to lower values perhaps so maybe if you are really worried about low values of h_b and I don't know what the range of your incident predicting, you might have to worry about the error just looking at the trend of the data points there. If you take those circles they're on a straight line pretty well if you extended that down. We would have a much bigger deviation towards the origin.

DR. BANERJEE: Maybe you should do a run at 0.2.

CHAIRMAN WALLIS: 0.2, yes.

DR. BANERJEE: h_b by d. See what happens.

CHAIRMAN WALLIS: That's a pretty full pipe, isn't it?

DR. WU: Yes, it has some difficulty to range from the top. I remember the run. I will see if we can get to this range here.

DR. SCHROCK: You only have five data points guiding you.

DR. BANERJEE: With a distinct trend though. You could put a line through them and probably go through all of those circles.

DR. BANERJEE: Different correlation.

DR. WU: Here you worry about going further here. It's a stretching out.

CHAIRMAN WALLIS: I don't know if I should worry or not but I think there would be a much bigger deviation if you went to lower h.

DR. WU: Yes.

CHAIRMAN WALLIS: Because when the pipes are almost full a little wave makes a big difference.

MEMBER RANSOM: Is the correlation developed based on the data or the air injection from below?

DR. WU: Below. It won’t stay that way actually.

CHAIRMAN WALLIS: Yes, let’s find out what this correlation is.

DR. WU: Okay. Let’s go to the next page. So the entrainment onset correlation developed and was actually based on Maciaszek’s work. And Maciaszek’s work was based on the Bharathan’s work for the lower parameter voiding. So that work basically assumes you have a gas, air goes into the branch, and at the interface there is a maximum velocity position and that maximum velocity for potential flow. So maximum velocity position actually corresponded to the lower pressures so your entrainment should occur here.

Based on these kinds of physical argument the first equation is the continuity equation. Just to say here is a cylinder with the diameter of the break. It is represented by the wave spacing. The gas goes into the cylinder from the periphery so the correlation actually says it’s like a pipe on the -- and the liquid that goes the gas goes into this break. Based on this argument the height of this wave height should be equal to the kinetic energy because of the infinite side the velocity is zero based on the potential flow.

That option is one option. Later we are going to modify it. We think our pipe is one dimensional and you go to infinite you still have a velocity. So we will modify this term a little bit.

DR. BANERJEE: One is the gas --

DR. WU: At this position.

DR. BANERJEE: Subscript 1 is gas.

DR. WU: Yes, gas.

DR. BANERJEE: And V_1 is the velocity in the horizontal leg.

DR. WU: Horizontal leg and this entrance position.

DR. BANERJEE: It's the average velocity.

DR. WU: It's the average velocity. They didn't consider the local velocity.

DR. SCHROCK: Just to the 2-d model, it imagines that there's a source on both sides.

DR. WU: This original approach for the interface stability didn't consider that.

DR. SCHROCK: I'm just contrasting it to the situation that you had in your experiment. You have gas flow from the right and gas flow from the left in this model. It's a 2-d model.

DR. WU: Yes.

DR. SCHROCK: And in the experiment you have gas flow from one side and not from the other side.

DR. WU: Yes.

MEMBER RANSOM: In fact that wasn't quite so clear. This model is actually an axisymmetric model about that center line that he has there.

CHAIRMAN WALLIS: I think it's 2-d in a plane, isn't it?

DR. WU: Yes, 2-d in a plane.

CHAIRMAN WALLIS: Which is you're multiplying V_1 by the area of that cylinder.

MEMBER RANSOM: So in other words he gets the velocity solution for the gas from the 2-d --

CHAIRMAN WALLIS: The velocity from the top of the wave is very different from the velocity at the side.

DR. WU: So this correlation didn't consider the confinement of the side wall of the pipe. It didn't consider the flow from one direction like Dr. Schrock.

DR. BANERJEE: What is ρ_z there? Can you walk us through the equations so that we can understand?

CHAIRMAN WALLIS: That's going to take us forever.

DR. WU: $\rho_1 v_1$ is the gas for the velocity at this cross section. And the number is the diameter of this wave ring right under the break. The third -- is the wave height h_b so it's basically this term is the surface error of the cylinder under the break. So modified by that's the gas flow rate that goes through the --

DR. BANERJEE: And what is ρ_2 or ρ_z ?

DR. WU: That should be 3.

DR. BANERJEE: Is that one?

DR. WU: That should be 3.

DR. BANERJEE: Is that for the gas?

DR. WU: Yes.

DR. BANERJEE: It's a continuity equation you have there.

DR. WU: Yes, it's continuity.

DR. BANERJEE: It should be $\rho_1 v_{3.}$

DR. SCHROCK: Rho(3).

DR. WU: This should be ρ_3.

DR. BANERJEE: But ρ_3 is what?

DR. WU: In that location.

DR. SCHROCK: It's potential flow so I guess it's the same as ρ_1.

DR. BANERJEE: It's the same as ρ_1, right?

DR. WU: Well it could be different.

DR. BANERJEE: How?

DR. WU: Because if you just have a break here the choking conditions are different. It can be as big as -0.6 for a choking condition.

DR. BANERJEE: So the pressure is different?

DR. WU: From here to here it could be if you have a high gas flow rate to the choking condition. (Indicating.)

DR. SCHROCK: Isn't a potential flow solutions?

DR. WU: This one didn't reach there yet. This one doesn't have that --

DR. BANERJEE: So let's call it ρ_3 but still the density of the gas.

DR. WU: Yes, this is mass flow rate. Forget about this one. Just say the mass goes through this branch. So the -- equation is the third -- change from infinite to this wave bump here is the kinetic energy converted the potential energy. That's the third edge.

CHAIRMAN WALLIS: And the reason that it's a depression is that you get a sination (PH) pressure again in the middle.

DR. SCHROCK: Yes.

DR. BANERJEE: But shouldn't it be ½ $\rho_1 v_1^2$ - ½ $\rho_0 v_0^2$.

DR. SCHROCK: That's zero.

DR. WU: It's a huge pool. That's infinite. This original approach, that's what we are going to modify. It's not the pipe. So it's physics like this. I want to follow the physics.

DR. BANERJEE: And what's the next one?

DR. WU: The next is you'll --

DR. BANERJEE: I got that. The next one?

DR. WU: It's a combination of these two equations. You get an automatic in there. It's just to replace the $\rho_1 v_1$ with the gas flow rate. You have an automatic --

CHAIRMAN WALLIS: Where does this magic Δ as 1/3 h_b come from?

DR. WU: That was Bharathan's work and you. What it did --

MR. BAJOREK: We found the paper too.

DR. WU: This is basically the difference of the third -- with respect to h_b.

CHAIRMAN WALLIS: So it differentiated something. Did it derived of a third?

DR. WU: When this goes to infinite --

CHAIRMAN WALLIS: You said that there's a maximum value of that thing as a function of delta. Once it goes over the top it gets sucked up.

DR. WU: Yes.

CHAIRMAN WALLIS: So there is a mathematics to it.

DR. WU: There is a mathematics it's just that --

CHAIRMAN WALLIS: It differentiates that.

DR. WU: It differentiates the third -- with respect to h_b. That derivative goes to infinity. That meaning a little bit of change of this h_b the wave bump is going to hit the top.

DR. BANERJEE: But is $d\Delta d\ h_b$ equal to infinity?

DR. WU: $d\Delta d$, yes. That's right. Exactly. When the derivative of this $d\Delta d$ over $d\ h_b$ goes to infinity leading to this --

CHAIRMAN WALLIS: There's another way to look at it. When w_g is big enough there are no more solutions for Δ. So it's the maximum value of the lefthand side gives you the maximum value of the right-hand side.

DR. BANERJEE: But $d\ h_b$ the Δ is equal to zero.

CHAIRMAN WALLIS: It’s a maximum. So it’s not that absurd, is it?

DR. WU: Thank you.

DR. BANERJEE: Let me ask you a question. When you have a hole like that, don’t you get a vortex, a swirl flow occurring in the air? I mean this is what happens in a bath tub. Or is this different from a bath tub?

CHAIRMAN WALLIS: Not if it didn’t have fortes of the infinity.

DR. WU: No.

DR. SCHROCK: But on bottom breaks you do see the --

DR. WU: This is an average approach.

DR. BANERJEE: But you get a very different pattern. You get a cyclone otherwise.

DR. WU: If you use the average approach you don’t see the cyclone. So you treat this as an average velocity.

DR. BAJOREK: Qiao, you see a little bit of that in some of your films.

DR. WU: Yes.

DR. BAJOREK: If you watch the wisps there’s a vortical motion to it.

CHAIRMAN WALLIS: The voracity has to come from somewhere.

DR. BANERJEE: Yes, but they were option close.

CHAIRMAN WALLIS: So I think we will accept that you are doing some math and you can get that equation down there.

DR. WU: Yes.

CHAIRMAN WALLIS: You have to say something about lambda so you say it's approximately d.

DR. WU: That was Maciaszek's approach. For lambda, you go to d and then he got his correlation. There's one -- power. So they replaced lambda with d. This parameter is theoretically 0.7 and his experiment result is 0.88.

CHAIRMAN WALLIS: Now is the experiment for a pool in a pipe or is it a pipe in a pool?

DR. WU: No, it's a pipe with a branch on the pipe.

CHAIRMAN WALLIS: It's a branch.

DR. WU: So all of these combine to part of this adjustable parameter.

CHAIRMAN WALLIS: So if you just took a vacuum cleaner and lower it down on top of a pool of water this is what you could also do as another experiment.

DR. WU: That was the results of experiment.

MEMBER RANSOM: There's one limitation you have to have in mind. The gas flow field is for a flat interface. The interface never deforms. So that as the bump forms in reality you get increased velocity across the bump and it will grow even more rapidly.

CHAIRMAN WALLIS: That's in his math here.

MEMBER RANSOM: No.

CHAIRMAN WALLIS: Yes it is. It's in the potential flow -- He hasn't used potential flow here.

MEMBER RANSOM: Yes, he has.

CHAIRMAN WALLIS: He's using $h_b - \Delta$.

MEMBER RANSOM: He uses it to get v1 and the $\Delta\rho$ relationship.

CHAIRMAN WALLIS: I don't think so. Anyway we can argue about this forever. I don't think he ever used potential flow in this model here.

DR. BANERJEE: You don't need it for the equations you've written.

MEMBER RANSOM: -- infinity to replace Δ --

DR. WU: For this one, no. You have to

bear in mind that here is the number over d when physics is if this surface is far from this opening you don't expect this low pressure on the high pressure low pressure is -- section. The high velocity still located right under the branch. It could be go this way (Indicating) is going further away so we think if we modify this number we perhaps can get a better case.

In fact later we found a -- if this point of source number is proportional to h_b. And if we put h_b here with parameter and move that h_b here then we have 1/5 power here. That's the correlation of KFK and also Dr. Schrock's correlation. So that means it's a two asymptotic condition. When you go to d it is the surface very close to the break or the break is fairly big. And when it's going away or the break is very small, then it's going to another asymtotic solution as proportion to h. So this kind of -- motivated us to say let's see can we find the number. Next page please. Let's go back.

DR. SCHROCK: Before you do that, I want to remind you of my problem with the fact that the flow is all coming from one direction in your experiment, in your practical problem but this idealization has it coming equally from both sides.

DR. WU: Yes, that's the wall approach. The second modification will be here (indicating) where the subtract of the v_0 --

DR. SCHROCK: Are you eventually going to account for that?

DR. WU: Yes, we have factor count on that in the correlation.

DR. SCHROCK: I'll wait.

DR. WU: Next please. The Maciaszek correlation for the relatively larger d branch size to the main pipe size. It correlated our data reasonably well. However for Berkeley data and the KFK data it doesn't work. So Maciaszek's correlation seems only applicable for the larger break side.

On the other hand, the Smoglie and the Schrock correlation correlated this solid assembly for the Berkeley data and these open symbols for KFK data. They group very well. The correlation of Berkeley and the KFK for this small break side however it missed our data. So that supported our argument that these two correlations may be too ptotic (PH) condition.

There are two approaches for me to do. One is I guess superimpose this two correlations, put a parameter before the two correlations. That depends on the d over d, the branch size. Now I just feed it.

I tried that and it was very successful. But I was not satisfied. I think I can do better than that.

The second one was more mechanistic approach but it's not rigorous. Let's go to the next page please. So what I did I think the velocity distribution along the interface can be found to form a potential for approach. So I want to see where this bump's right location is. I expect for the potential for the approach when the interface approach to the branch then the number should be equal to d. Then the way it's going away should approach to the point source.

CHAIRMAN WALLIS: I'm sorry. I have to ask. Have anyone ever seen this ring wave?

DR. WU: No.

CHAIRMAN WALLIS: So it's very interesting. All these theories based on something no one has ever seen.

DR. SCHROCK: More than that we did see that the liquid is sucked up as one more or less symmetric bump. Just before the first time --

DR. BANERJEE: Is there a little cyclone?

DR. SCHROCK: There is in the liquid for the downflow but we couldn't see any such thing in the upflow case.

CHAIRMAN WALLIS: Any voracity when it's dragged in and stretched is more visible. But it's not really a very significant part of the mechanics.

DR. WU: In fact this bump was artificial. I think it just say we'll predict the location of this lower pressure reading.

CHAIRMAN WALLIS: I think someone would do this experiment. It's simply taking the vacuum cleaner and bringing it down on the pool and seeing if you get this ring. It's very simple to do.

DR. BANERJEE: Let's do it.

CHAIRMAN WALLIS: Just go home and do it.

DR. WU: To see the ring. I did. I couldn't see it because of the instability where a break at one location. It's just coming from somewhere weak point and then lashing in. So you can not see a ring coming out uniformly homogeneously coming up. That's what my argument is. So if you consider this v_d effect.

DR. BANERJEE: So what happens? You see also some weird stuff.

DR. WU: It's like Dr. Schrock said. When that entrainment outcome forms somewhere there's a common chunk or cone shape and going upward like that. Or it has some dispersion.

CHAIRMAN WALLIS: That's because you don't get that stagnation point in the bottom. You have a point then the velocity continues to increase and you get lower and lower pressure. You have one wave then you no longer get that stagnation region in the bottom.

DR. WU: So you go and form here then this side is ceded. So what are we did -- Next slide.

DR. SCHROCK: I've never seen this solution for this distributed source sink combination in terms of the 2-d velocity profile. That would be interesting to see. I'm puzzled by how you select that source sink geometry to get simulation of the flow into that branch.

DR. WU: What I did is --

DR. SCHROCK: You have flow into that wall at that source but flow out of the wall in that source flow into the sink in the bottom.

DR. WU: What I did is this is a distributed source for each finite element I treated it as a point source. That potential flow is -- you can superimpose all these together. That's your integration. Together the velocity long term of this point. What I did with mirror source is exactly one to -- this boundary condition without the crossing flow. So that's the wall. You don't have the gas go through --

DR. SCHROCK: That's fine but I'm talking about the flow pattern in the vicinity of the corner on the branch line. It looks to me like it's coming out of those surfaces and converging on the axis of z somehow.

DR. WU: So you mean it's going this way? (Indicating.) It's merging to that.

DR. SCHROCK: I could put it more simply. I would just like to see the velocity profile that predicted by that potential flow solution.

DR. WU: Next please.

CHAIRMAN WALLIS: I think you ought to show that in your report.

DR. WU: You mean this one? This is the velocity profile on the interface.

CHAIRMAN WALLIS: On the interface. But I mean on the top of the pipe or the corner. It's not going to match the pipe very well.

DR. SCHROCK: The 2-d velocity distribution in the view that was shown in the previous slide is what I'm asking for.

DR. WU: Okay.

CHAIRMAN WALLIS: I think one could accept that if the model is reasonable this is not too far from --

MEMBER RANSOM: The one problem you have though this is potential flow and that won't actually exist. There will actually be a flow separation zone right in the center under this pipe which basically is a recirculation zone that quite changes the flow field. Outside of that region is probably fairly decent.

CHAIRMAN WALLIS: I think for the initiation the first picking up liquid is not so bad. But once you get a significant wave it's quite different.

MEMBER RANSOM: Even for a flat interface you still get this separation zone. Flow doesn't like to turn 90 degree angles actually.

CHAIRMAN WALLIS: Okay, so it doesn't like to recover pressure to the sination (PH) point. Okay, I think we're going to have to move on or we'll never get out of here.

DR. WU: So we focused on the velocity at the interface. Then the maximum point you see of the distance of moving away from the center the maximum velocity location is moving away from this. That's what we expected. I just have taken the position of this maximum line and the product with respect --

CHAIRMAN WALLIS: This giving you your lambda.

DR. WU: That's giving me my lambda. So the number you see clear as they were approaching to this dotted line formed this maximum clearly say it's when the surface approach to the break you can never go into the branch so the maximum should be lambda equal to d. That's Maciaszek's approach.

Asymptotically (PH) if you go that way then the surface goes far away from the break. The surface sees the break as a point source. So it's merges into asymptotic (PH) solution. This is the dashed line. It's easy together with other integration. So lambda is proportional to the distance, the h_b. We think this is the reason why the KFK and the Berkeley correlations -- this power dependence. For the Maciaszek, it's the correlation that uses lambda equal to d.

What I can do is if I can get the exact function of this curve, I put into the number there and I can expect the correlation function better than previous investigations. However since we cannot get analytical solution out of that integration so we first tried just -- this curve. Last year when I presented this I used the exponential function, that created a lot of trouble. This year we just simplified it to a linear. Next please.

CHAIRMAN WALLIS: I would have been lazy and just sketched in a curve and fit it in with the simplest math I could. So you've done a really simple function.

DR. WU: Yes. It's a linear function. And this modification is the velocity crest and the faraway velocity. It's not zero. We put this here and simplify.

CHAIRMAN WALLIS: So this is an achievement about bringing in this other parameter. You have brought together the data from these wide-ranging experiments. Big d over d ratio.

DR. WU: Yes. And as the capital D and lower case d so it's all the branch size effects considered. Then we can collapse all the data originally scattered.

DR. BANERJEE: But is k the same for all it?

DR. WU: K is about 1. It's 1.02 and a is 0.22.

DR. BANERJEE: But why does this work because the physics is all wrong, right?

CHAIRMAN WALLIS: No, this is only on entrainment. We haven't gotten to slug. This is just the glassy interface and then it leaps up into the hole.

DR. BANERJEE: So you are sucking from both sides.

CHAIRMAN WALLIS: Yes.

DR. BANERJEE: But not from the sides.

CHAIRMAN WALLIS: Not from the steam generator. You're not sucking really from the steam generator, are you?

DR. WU: This is a modification. We have only one side coming from one side this is v_1, the exact velocity in the hot leg. This is crested --

CHAIRMAN WALLIS: So you do a bunch from coming from one side.

DR. BANERJEE: He's taken not v equals --

DR. WU: If it's a two side I think this factor should have a two or something like that.

CHAIRMAN WALLIS: So you have to do something about that.

DR. WU: Yes. I think that's the case.

DR. BANERJEE: So what is this? You lost me.

DR. WU: Originally this equation I

presented --

DR. BANERJEE: I remember v_1 is equal is zero to begin with.

DR. WU: Because faraway, it's a plane.

DR. BANERJEE: So now you have put v_1 equal to some finite value.

DR. WU: Exactly.

DR. BANERJEE: But how does that -- You phrase that into that equation as h_b over d.

DR. WU: Yes.

CHAIRMAN WALLIS: When you do the math, that's what happens. The continuity.

DR. WU: Yes, the continuity equation. Also consider the asymptotic (PH) condition. You see when h_b equal to d the pipe is completely dry. You need infinite gas for rate to entrain.

CHAIRMAN WALLIS: There's nothing to entrain.

DR. WU: So your velocity has to go. That's one of the thinking there too. Both CEA Maciaszek correlation and the KFK-Berkeley correlation were right for their beta test conditions.

CHAIRMAN WALLIS: Even Bharathan was probably not too far wrong. He was the early worker.

DR. WU: The KFK data like Dr. Schrock

mentioned it had a weird wall there and they blew the excess into the vertical pipe drain and it's quite a different mechanism. It had scattering and registered on level with different gas velocity or with the same gas velocity but registered different level. So if we kick that out and use Berkeley's data and our data I think I'm very satisfied. In reality we should consider their data but this is just to show how well when these experiments technically improves. Next please.

That closes our entrainment onset correlation and now we're going to entrainment rate studies. That's a little bit different. Next please.

For the entrainment study the experiment after last year's ACS meeting we focused this -- The entrainment is just a steady entrainment that goes through separator and -- and steady. With the steam generator there's a gas volume there and of course that's --

CHAIRMAN WALLIS: I think we had some concerns about the compressibility of the gas since you have a transient going on. Actually as the slug moves around it compresses the gas in the steam generator.

DR. WU: Yes.

CHAIRMAN WALLIS: So the compliance of that makes a difference.

DR. WU: One of the reasons I think is -- Let's go to the next page. So again like last year, Dr. Wallis and Dr. Schrock said this side is the steam generator side. This side is the vessel side. We showed three kinds of patterns on the steam generator side. One is the oscillator rate. It has a slug back and forth from the steam generator lower head to the branch. The certain region we equate a transition region because that slug is not persistent and the frequency is unpredicted occasionally have. Finally if the qualities are really high you get a relatively stratified wavy condition without touching the top. So these are the three different flow patterns.

DR. SCHROCK: And the top one is much more oscillatory than the others.

CHAIRMAN WALLIS: Now we have a video. It doesn't look like potential flow. (Laughter.)

DR. BANERJEE: Also pretty frothy, you know.

DR. WU: Here is clear liquid. This part is very frothy.

DR. BAJOREK: Dr. Wu, do you have the film on here where it slowed down? Have we been able to play that one?

DR. WU: I think he didn't grab into the computer. Do you have a CD that can play? I have a few files inside that has a large movie. It's a high speed movie so you see it clearly. We have to close it.

DR. BAJOREK: In the high speed visualizations I think you can start to see a little bit of voracity. If you watch very closely it almost gives the appearance that when that slug is going up into the branch line there is flow coming from two sides as if it has shock around and it's coming up behind the slug. So it may not be so far fetched to look at this slug as having a velocity field on both sides of it.

DR. WU: I have it in my computer. Because of the CD I gave to him seems it doesn't work on the computer. He actually tried very hard yesterday afternoon to put my powerpoint here. I have my movie in my computer and later I will show you the high speed one.

CHAIRMAN WALLIS: Well the entrainment out from the slug you have a model that predicts the velocity of that liquid going up the branch pipes.

DR. WU: No.

CHAIRMAN WALLIS: You just have a correlation or something.

DR. WU: I have a correlation. Because of the entrainment coming from the downstream side, it makes sense because of the average level is closer to the branch so it will pull from the later side. So I used an average level of gas space height downstream to correlate. I found it's reasonably well. Let's go to the next slide.

Before I go to the modeling last time I plotted this into the flow region map because the ACS has some members that suggested that doesn't apply because the traditional fluid region map is for co-current situation. The downstream of the branch is virtually average gas flow rate is zero. The liquid that goes -- It's not a good representation.

This time we tried Dr. Bajorek and I over 15 kinds of combinations to how to represent it. Eventually I could predict it. We found this is the best figure. Quality versus the Δh is the height difference downstream and the upstream level difference. Then this is divided by the upstream liquid level height. I can do it divided by downstream. So it correlated some --

CHAIRMAN WALLIS: So when the quality is

zero we have no gas flow at all and yet the Δh is 0.6.

DR. WU: That's the problem --

CHAIRMAN WALLIS: Then why is up there?

DR. WU: We wanted it coming down because when the -- complete failure you shouldn't see any difference. Now we tried the hydraulic jump case. The hydraulic jump case is keep on going here and then it goes to infinite because it doesn't say the upper wall of the pipe, the hydraulic jump correlation. So right now we are trying to -- To these bracket symbol is stratified wavy. This should be the steam generator side.

DR. SCHROCK: Using this Δh for the two different locations seems like it's the problem that depends on the chance location that the designer built into the equipment if he shows a different spacing for the instrumentation you get a different result.

DR. WU: We tried the Froude number in the main pipe for both gas and liquid. We tried the mass flow rate for in the branch here. Things don't really represent this kind of slugs.

DR. SCHROCK: You don't have any data for any spacing other than the one that you have in the experiment.

CHAIRMAN WALLIS: Are you going to use it for something?

DR. WU: No.

CHAIRMAN WALLIS: You're not going to use it for anything.

DR. WU: This one is just to say the difference of --

CHAIRMAN WALLIS: But you're not going to use it in any correlation.

DR. WU: No. Dr. Bajorek is thinking about for the AP1000, the -- for we can predict when the oscillation occurs. That would be nice because the facility is subjected to these kinds of water impact. We are trying and that's not our task goal for the entrainment.

DR. BANERJEE: ΔH is the difference in height between the two sides?

DR. WU: Yes.

DR. BANERJEE: And what is the hrx there?

DR. WU: It's the gas chamber height offstream on the reactor vessel side.

CHAIRMAN WALLIS: I thought Δh would be held up by the stagnation pressure of the gas or something.

DR. WU: That's right.

CHAIRMAN WALLIS: Not by x. What is x?

DR. WU: This is the momentum of liquid flow and the gas flow. So I used the combination and the single of them but it doesn't occur -- Then I found a quality case action is greater than this. It's just 1, 2 -- transition ranging, this is a -- ranging at a lower liquid quality.

DR. BAJOREK: Dr. Wallis, I think the point is that what we are trying to do here is to come up with a different scheme to predict when we're getting these oscillations so that if we see them in something like an ATLATS or an APEX we can come up with a way of predicting whether they occur then in a large pipe such as AP600 or AP1000. We've looked at several different ways of looking at it.

I think by in large with the idea that if you have a level and the wave is large enough to strike the top of the pipe you have a change from the stratified regime to intermit tenancy or some type of an oscillating plug. It's work in progress at this point. We see some trends but nothing at this point that would really give us nice sharp boundaries so that we could use the parameters of either an equilibrium level in the pipe and a superficial velocity to try to estimate using code parameters what type of a pattern we're seeing.

CHAIRMAN WALLIS: I just don't like X as a variable because it gives us no flow at all with an X. It's obvious that momentum has to come into it physically. So it has to have something related to the flow rate. So an X by itself can't be the right parameters.

DR. BAJOREK: It's showing the trend with everything but not something we can really hang our hats on.

DR. WU: The model actually entrainment of the real model development based on our approach is when X should be the actual gas chamber height is smaller than the entrainment onset then you have a entrainment. Then you have to predicate entrainment rate. One of the basic options is the kinetic energy of the gas should overcome the gravity head at this part is for the -- and also the entrainment onset condition. The excess of that goes to the liquid kinetic energy entrained into the branch. That's basic option.

DR. BANERJEE: What is this? Of energy balance?

DR. WU: It's not balance. It's just to say that the gas kinetic energy over the entrainment onset condition should go to the liquid --

CHAIRMAN WALLIS: It's a kind of Bernoulli equation for flow side by side. If you take the right-hand side and put it on the lefthand side you have the Bernoulli equation on both sides.

DR. BANERJEE: Stream 2 containing liquid --

CHAIRMAN WALLIS: Equal pressures at the interface. It's parallel stream with equal pressures. I think that's what you're doing.

DR. BANERJEE: Right. And $C_{\triangle}$ that's the potential energy.

CHAIRMAN WALLIS: C should be one.

DR. WU: C is because you take a certain gas flow rate to entrainment. You have to go over a curve to start the entrainment. So that represented the entrainment onset condition. You have to go through that curve and then start to entrain because it's not any gas velocity you can transfer the kinetic energy to liquid entrainment. That's easy because of that consideration.

DR. BANERJEE: It's sort of a Bernoulli equation, I guess.

CHAIRMAN WALLIS: The next one is more mysterious.

DR. WU: The next one is actually just a

modification continuity equation modification. I replaced the velocity with the mass flow rate by the water fraction. There's no mystery.

CHAIRMAN WALLIS: Alpha 3 is a void --

DR. WU: If you move this to here and that's actually this rho v right?

CHAIRMAN WALLIS: Okay.

DR. WU: So you square root it that's because the -- velocity has a square and you have a density -- Later we will show that. Here I would like to say how we decided this same parameter because here I already moved it to the gas side. What I said is this h if it -- the gas flow rate, if it approaches to the entrainment onset level height, the determination inside should be zero because you don't have liquid flow. So that actually plug our entrainment onset condition into this equation. I got asymptotically (PH) they said it should be expressed like this way. So the h should be --

The other option here is alpha 3. There are several approaches. Yonomoto has shown for alpha 3 there's a conical shape of liquid and then put a -- to correlate an alpha 3. What we did here --

CHAIRMAN WALLIS: Let's go back here. This bottom equation is strange. What's in the square root there should be the same as your onset of entrainment correlation for predicting w_g^3.

DR. WU: Yes.

CHAIRMAN WALLIS: So you just take your onset of entrainment correlation and use C^1. I think that's what you are doing.

DR. WU: Yes.

CHAIRMAN WALLIS: But it's misleading at the bottom because that w_g^3 --

DR. WU: You do have this under a real condition. I got it -- under the h_b condition.

CHAIRMAN WALLIS: And the w_g^3 is under the h_b condition too.

DR. WU: Yes.

CHAIRMAN WALLIS: That what's confusing.

DR. SCHROCK: Could you point out where on the design where station 1 and station 3 are located.

DR. WU: Station one is in the horizontal pipe. Station three is in the branches.

DR. SCHROCK: Where on the horizontal pipe?

DR. WU: It's at the inflow.

DR. SCHROCK: So it's implicit in the model. It's symmetric half of the gas flow comes from each side.

DR. WU: For our case we just did it from the one side. From this side. (Indicating.)

DR. SCHROCK: The picture shows it both sides. Are you sure you're doing it on one side?

DR. WU: Yes, because that comes through here when you use the continuity equation. We use the two arrow. If you use the one side arrow that's just the one side. But the figures are right there. (Tape stops.) Same from both sides.

DR. SCHROCK: That also is splitting the flows in the station 3. Station 3 is at the mouth of the break.

DR. WU: Yes.

DR. SCHROCK: So a3 is half of that or all of that?

DR. WU: A3 is the -- rate. It's the arrow of this pipe. A1 is one side of the arrow of this pipe. It's one. If both sides are moving the lines here should be two A1.

DR. SCHROCK: Well, it's misleading at least.

DR. WU: That figure.

DR. SCHROCK: Maybe not right at the worst. I'm not sure which it is.

CHAIRMAN WALLIS: At least we see what

he's doing.

DR. WU: Let's go to the next please. So since we have w_f^3 then the quality is supposed to be like that (indicating) and the w_f^3 and the w_g^3 should equal to the whole thing (Tape stopped) putting into the same -- There is nothing strange here. It's just the entrainment onset correlation.

So this correlation is a function of the main pipe size, the onset level and the actual level and the branch size is also a function of the density ratio. The thing I need to mention is the -- on the accurate estimation of h_b that means we have to use the h_b we developed. If we use the h_b of some other then you make trouble with working under the different conditions. Next page.

If we see this data this is the calculated level because that's the experiment that we measured and this is the measured level. The Berkeley data, -- use the bracket symbol is the KFK data.

CHAIRMAN WALLIS: Excuse me. This is a model based on stratified flow type of entrainment rather than the slug oscillating entrainment.

DR. WU: Exactly. The average level.

CHAIRMAN WALLIS: This is only for the regime where you don't have slugs, is that right?

DR. WU: This data was gotten from slug too.

DR. BANERJEE: You use the average --

DR. WU: We used the average.

CHAIRMAN WALLIS: Your model says nothing about slugs. Your model has the liquid and gas flowing together as in Bernoulli equation.

DR. WU: That was the reason I said the code cannot see the slug. It can only see the average.

CHAIRMAN WALLIS: The model is based on co-current flow of liquid and gas using the Bernoulli type equation.

DR. WU: Yes.

CHAIRMAN WALLIS: And it’s not modeling any slugs.

DR. WU: No.

CHAIRMAN WALLIS: But the data here actually includes the slugs.

DR. WU: Yes.

CHAIRMAN WALLIS: Does it correlate the slugs better than the --

DR. WU: No.

CHAIRMAN WALLIS: Which of the slugs?

DR. WU: Slug is coming out there

(indicating.).

CHAIRMAN WALLIS: Okay.

DR. WU: It doesn't correlate that well here for the slug but -- everything together. You see the KFK data correlation different upon Dr. Schrock's Berkeley data. You cannot mix them together. But this correlation is doing some work about that to group all the data together. If we see in the JAERI, the Yonomoto correlation it seems they can do similar work but shifted.

CHAIRMAN WALLIS: This is funny. You're not predicting entrainment rate. You're predicting h over d.

DR. WU: Using the quality.

DR. BANERJEE: The quality which you know you're working back towards h over d must have been.

DR. WU: Yes. Because that's the reverse way when we were doing the experiment.

DR. BANERJEE: This is the process --

DR. WU: Yes, we get the quality working back to the level.

DR. BANERJEE: This h is what h?

DR. WU: It's actually the downstream but if all the data KFK and the Berkeley because there is no difference. They had a very stratified. Berkeley data is right under the branch their window really.

DR. SCHROCK: I don't think that's true. There are three windows, one at the break, one upstream and one downstream. You could in fact see a difference between the levels upstream and downstream. That's not a part of the correlation, the data that was presented, but it was evident in the data.

I'm still troubled with this (Tape stopped) seeing of different things here. In our experiments both the gas and the liquid flowed through the apparatus (Tape stopped) just at the end. Then we're looking for a branch flow which begins to entrain the liquid. The through flow never stops when you begin taking if off the branch line.

But you have a model here which is different conceptually from the flow pattern that existed in our experiment and the KFK experiment. Putting it all together and it comes out looking like one shoe fits all. It's troubling to me. I don't understand the definitions of the terms in the equation. I don't understand presenting engineering data where the variables in the equation are not clearly identified in terms of the diagram that the equation purports to represent.

CHAIRMAN WALLIS: So you have to begin at the beginning. What do each of these things mean? Where do you imagine them in that diagram? Then make the argument from there and there has to be some continuity in the flow patterns that are put together in one correlation. I don't think you can just take any assortment of flows from both directions, flows from one direction, through flow, with the linkage off the branch line and whatever and expect that they are all going to make sense when you amalgamate into some kind of a correlation. Why don't you tell us what h is?

DR. WU: Let's start from here if I can. Remember when we talked about the third action (PH) we say that the velocity under the crest that is responsible from there to entrain liquid out of the wave. So that actually is supposed to be the velocity under the interface of where is the maximum.

CHAIRMAN WALLIS: But then which is the h from your experiment that you used?

DR. WU: The h in our experiment is the downstream case.

CHAIRMAN WALLIS: Downstream of --

DR. WU: It's on each end or at the side because it's closer to the break.

CHAIRMAN WALLIS: Of course over there

(Indicating) there isn't any v_f^3 or v_g^1.

DR. WU: No.

CHAIRMAN WALLIS: So first thing you're doing is particular. You're taking the velocity as oxygen coming in and going out but then the h you are using is in the dead leg where there isn't any flow. Is that right?

DR. WU: Yes. The way the -- wash back because that level is actually closer to the branch so that entrainment would occur at the level where it's closer to it.

CHAIRMAN WALLIS: So the h you put in virtual plot and then you compare data is the steam generator side h.

DR. WU: Yes.

CHAIRMAN WALLIS: Which sometimes is the whole pipe, isn't it? Sometimes it's d.

DR. WU: No. Well on average.

CHAIRMAN WALLIS: On average sometimes it goes back and forth.

DR. BANERJEE: Why don't you redraw this figure because it's very confusing? I mean it's hard to follow what you are doing.

CHAIRMAN WALLIS: You mean draw it to look like the reality.

DR. BANERJEE: Yes.

CHAIRMAN WALLIS: That's the object of writing equations. If you don't draw a figure you could say well this is Bernoulli equation and can't figure out where all the terms come from.

DR. SCHROCK: When you grade the homework what do you do with the paper that gives equations with a lot of subscripts and a diagram that doesn't show the quantities that are in the equation? I don't spend too much time with that myself.

CHAIRMAN WALLIS: The students say you are unfair.

DR. SCHROCK: It may be.

DR. BANERJEE: But I think you should explain plane 3 and plane 1. Because if you understand you plane 1 is to the right-hand side here, right?

DR. WU: Yes.

DR. BANERJEE: Then plane 3 is crossing the entrance to the pipe facing vertically, right?

DR. WU: Yes.

DR. BANERJEE: Now that first equation is really the crux of everything. Everything else follows from that.

DR. WU: Exactly.

DR. BANERJEE: So one must justify writing that equation in terms of some form of a balance. You can't just pull it out of your hat.

DR. WU: Yes.

DR. BANERJEE: It's not clear. If you wrote it like a Bernoulli equation you still need to put down all the terms and say which ones you are going to neglect and stuff like that. At the moment there seems like there is Bernoulli term which would be $\rho_1 v_1 \; d_{g3}^2$ that you are getting rid of as I see it.

CHAIRMAN WALLIS: But you can't deduce it from the figure.

DR. BANERJEE: What?

CHAIRMAN WALLIS: I think you have to do it by hand waving. I can't imagine a figure --

DR. BANERJEE: Then you can throw out something.

CHAIRMAN WALLIS: His argument was a qualitative one in saying here is the ρv^2 which is available to b gravity and therefore it provides another ρv^2. That was a qualitative argument.

DR. BANERJEE: But you then have to say okay I'm going to neglect the gas phase going into that pipe or something. It has to be done properly.

CHAIRMAN WALLIS: I think your assignment for after lunch is to come back with a figure which shows how Bernoulli's equation for a particular shape of interface is related to that equation you have there. That is the problem we have is. We don't see how it's related to any kind of a flow situation or an interface geometry.

DR. WU: I'll try.

CHAIRMAN WALLIS: Do you think you can do it?

DR. WU: I don't think I can do the reverse one because this is the political argument to say the existence of the kinetic energy of the gas partially goes into the liquid because the air pointed to it and partially goes to the gas itself too because it's still moving.

I don't know what the factor is there I put proportional constant is there so that's my argument there. The equation of kinetic energy has to be for the gas kinetic energy.

CHAIRMAN WALLIS: Vg_1 is the velocity in the main pipe.

DR. WU: Yes.

CHAIRMAN WALLIS: So if I had the original experiment model you had with the pool with an infinite space there would be no Vg_1. But I could still get a Vf_3.

DR. WU: All this is compiled --

CHAIRMAN WALLIS: This wouldn't work though for --

DR. WU: For an infinite --

CHAIRMAN WALLIS: For the picture you drew.

DR. BANERJEE: Yes, you neglected Vg_3, rule 3 Vg_3 and maybe it makes no difference. But I think you should do as an energy analysis.

CHAIRMAN WALLIS: I think it's much bigger than Vg_1.

DR. BANERJEE: I would be greater than Vg_1.

DR. WU: If there is gas going out here, it still carries some kinetic energy but part of it is being transferred to liquid and I don't know what it is.

CHAIRMAN WALLIS: I think if you had a Vg_3 in that equation then you could justify Bernoulli's equation because they both come from sedation (PH) conditions in this parallel flow and I think you could justify that equation. But there's no way you could justify with a Vg_1 with Bernoulli.

DR. WU: The reason why I did this is

because of the original entrainment onset is based on this. That's the blow over surface velocity certainly -- start to entrain. So if I put it this side as zero, that's the entrainment onset.

CHAIRMAN WALLIS: So your initial model is based on the Vg_3 lifting up.

DR. WU: I can work it that to the --

CHAIRMAN WALLIS: If you put a Vg_3 in there you could justify that equation on the basis of 2 parallel Bernoulli equations.

DR. WU: I tried. So then here it doesn't work.

CHAIRMAN WALLIS: But Vg_3 is inside the square root.

DR. WU: Because of the continuity equation.

CHAIRMAN WALLIS: Yes.

DR. BANERJEE: But Vg_3 is related to Vg_1.

DR. WU: Yes.

CHAIRMAN WALLIS: I think you should have Vg_3 there and then it will work out and you can satisfy everybody with a picture. You will still get the same equation.

DR. WU: But here will be entirely different.

DR. BANERJEE: Use the ratio h_b to whatever.

CHAIRMAN WALLIS: I think the way out of your problem is to use Vg_3 in that first equation and justify it by drawing a picture and I think if I were hired as a consultant I could tell you how to do. (Laughter.)

DR. WU: Dr. Wallis, I did that. Down here would be --

(Discussion off microphone.)

CHAIRMAN WALLIS: Okay, we have to move along.

MEMBER RANSOM: There's something about this that bothers me though. In the picture you've shown of the periodic slide moving back and forth and entrainment occurs each time the slug filled up the pipe, entrained up the pipe, there's no relationship to the model. Again it's like getting the right answer for the wrong reason.

So I think you really have to address some of these other problems I would think when you observe it. If this model does fit in the end and it does satisfactorily explain once you take into account the real physics of what is going on in the process then it would be okay. But I don't see that I guess.

CHAIRMAN WALLIS: Well, I think he's telling us that the data points which agrees most with the theory are the ones where you have stratified flow. You go to the next figure. I think you told us that the points that are most on the line are the ones where you had a stratified flow, right?

DR. WU: Yes.

CHAIRMAN WALLIS: Next one. So you said that the red points on the right there are the high points. Those are the slug points. And the theory does best on the red points up there which are stratified. Is that right?

DR. WU: Yes.

CHAIRMAN WALLIS: So it's just some luck that it was more or less stratified but it was best for regime which is most like the math.

DR. WU: Yes. I have made an explanation for that.

(Discussion off microphone.)

DR. WU: The ways like you mentioned when slug come actually entrainment starts and when slug is down then there's no entrainment. We tried to average it with the average level represented in this process. Definitely you can average the level but the nature between the quality is not -- superimposed on the level.

You cannot just say that's the average of that. So my dilemma is that to develop a correlation without conducting these slug authenticated frequencies that's the code not permitting these parameters just to say the average. We do that. This is the choice I have. Unless if you give me the permission to really put the slug -- inside. I think I can do much better job.

DR. BANERJEE: But you would have to calculate that frequency as part of the problem. It probably can be done but you would have to --

CHAIRMAN WALLIS: It would be system dependent.

DR. BANERJEE: It would depend on the whole system. Because you can imagine if you have now the ADS valve or something that the back pressure which is set up and also some feedback. So if as Vic says the slant is oscillating back and forth that frequency might depend on a whole lot of system parameters that are really quite different from yours.

DR. SCHROCK: You also have to have a model which more or less ad hoc and it seems to work and it's satisfying because it seems to work. It leaves you with the problem of just finding its scalability to the reactor geometry. How are you going to deal with that?

DR. WU: My argument is because the KFK data and the Berkeley data is very different geometry. And also for Dr. Schrock's data it has air-water and steam-water data. If the correlation can do a reasonable good job for all this data I guess that's how you evaluate and check on the scaling capability.

DR. BANERJEE: But in the case that let's say h/d is of the order of 0.1 or 0.2 or whatever there then your slug data is run by a factor of 2 or more.

DR. WU: Yes.

DR. BANERJEE: That's evident.

DR. SCHROCK: In the scaling argument I think you brush over to --

DR. WU: If you go through the other correlation maybe you will start to appreciate this work. The other correlation -- Next page please.

CHAIRMAN WALLIS: I think the more fundamental question here is that you take your flow rates and then you measure h and compare with a calculated h.

DR. WU: Yes.

CHAIRMAN WALLIS: What the code is going

to do is you're going to calculate some h's and predict flow rates.

DR. WU: Yes.

CHAIRMAN WALLIS: It's not clear to me that when you ask the code to that it's going to do a good job of predicting flow rate.

DR. WU: In fact this correlation is using -- calculated X and we actually tried to go back because our quality expressed in our experiment and downward measure to h. So it's better doing it that way than what we are doing. We need interaction to find that h but for the real case using h is straight forward explanation.

CHAIRMAN WALLIS: But nobody is trying to predict the flow out the break out the ADS line.

DR. WU: Yes.

CHAIRMAN WALLIS: I think you have to ask how good a job it does of that using whatever method you eventually come up with.

MEMBER RANSOM: One other aspect that Dr. Banerjee brought up a while back and Dr. Schrock again with the scaling issue is that clearly under the stratified case the mechanism is one of entrainment where droplets are formed and entrained in the outflow so you think there should be some Weber number effects in this. It would be more satisfying if that were included or shown to be small.

DR. BANERJEE: You know I don't know because if I recall the data what was happening was that you were getting these slugs and liquid coming out of the -- I'm trying to remember the data of ROSA or whatever those facilities were. It was chugging along. So there was obviously quite a lot importance to these slugs and they must be somehow dependent on the dynamics of the system and the lines.

In the regime where you don't have slugs your model seems to be more dependable. Where you have slugs it may still be dependable but you have work a little harder to do it.

CHAIRMAN WALLIS: Whatever the model is it's better than anything else. It's better than these guys here.

DR. SCHROCK: But now in turn you have to have the data to fit of the justifying the scaling problem on the basis that you've compared all of these available things. All of them have upstream pipes that are on the order of six inches in diameter, every one. What's varied is the branch line diameter. There's nothing in the database that tells you anything about the physics of this sort of entrainment out of the big pipe.

CHAIRMAN WALLIS: But your Weber number is going to be much higher.

DR. SCHROCK: Right.

CHAIRMAN WALLIS: Surface tension is lower. Velocity is higher versus density's higher Weber numbers. Much higher.

DR. SCHROCK: I think you do need a reasonably physically based methodology the strengthen the scaling arguments that you have to provide in the end I don't see how you get them out of this especially with these differences one directional flow, two directional flow, through-put of the gas in some of the experiments, not in others. But then all of the data being pulled together.

CHAIRMAN WALLIS: All we need is a perception that relates this work to the real problem.

DR. SCHROCK: Yes.

CHAIRMAN WALLIS: Up front. It shows what the differences could well be in the real system. But you've obviously made some progress. Your model whatever it is is much better than these other ones here.

DR. WU: Yes, it's simple model. It's just excessive kinetic energy of the gas probably goes to liquid. Dr. Banerjee's comments about the system dependent oscillation, we did some identifiable calculations and they did not find caused actually caused some of this oscillation. It's actually -- we didn't present this part because it's not part of our work yet to want to save this code. How we can use our correlation later with the code.

Then the code is duly time step very short actually predicted this two Hz to four Hz oscillation down stream side. So obviously they pick up instantaneous height and -- entrainment. The downside of the cushion of the gas compressor benefit didn't find it or could pick up. Before our experiment we had to rely on the average measurement. We could not get an instantaneous --

CHAIRMAN WALLIS: I think the frequency in the experiment, It was just an experiment, is related to the transient time of the slug in the ADS line.

DR. WU: Yes. So maybe the quality of the entrainment model if it's correct then it can match this oscillation by the code itself.

DR. BANERJEE: You're able to measure the frequency.

DR. WU: Sure. We did the step analysis.

DR. BANERJEE: It's just that the way you are doing your experiment, you find the average entrainment rate because you run it essentially to steady state. Right?

DR. WU: Yes sir.

DR. BANERJEE: But if you wanted to you could actually find all the rate velocity because it's coming out into the chamber, right?

DR. WU: Yes.

DR. BANERJEE: Out of a relatively short pipe. If I remember your diagram.

DR. WU: Yes. The oscillation for downstream and the quality there's a real time -- I assume it's also oscillating.

DR. BANERJEE: But you don't look at the quality in that particular pipe?

DR. WU: Because it's not the average actually here.

DR. BANERJEE: But if you look at that, then find the oscillation.

DR. WU: So you mean measure the instantaneous gas line the liquid flow rate through the liquid that flows in the branch.

DR. BANERJEE: I'm not even saying measure the instantaneous gas. You could probably measure the liquid flow rate without too much difficulty coming out as a function of time.

DR. WU: If you use -- catch time it's going to settle down and we use a --

DR. BANERJEE: Think about it.

DR. WU: Think about how you can get an instant --

CHAIRMAN WALLIS: It's not trivial. You have actually a hydrostatic head in that pipe and when it's full of liquid you have a higher pressure so it has to come from somewhere. So you have to compress your gas in reactor vessel.

DR. WU: So you mean the injection.

CHAIRMAN WALLIS: It depends upon the compliance of that --

DR. BANERJEE: It's a system effect.

CHAIRMAN WALLIS: I think we should move away from this. We're not going to get anywhere with it today because there are many parts of the system that are affect it.

DR. BANERJEE: But the whole systems affects that.

CHAIRMAN WALLIS: Of course. I think we ought to finish up with your presentation of entrainment rate model. We'll go to lunch and then your assignment is to come back with a believable duration of Bernoulli's equation.

DR. BANERJEE: Then you can write that equation as a transient model.

CHAIRMAN WALLIS: So can we move on?

DR. WU: Next page please. This shows you we put all the available data in the originally different equations to make that. That gives you more appreciated to the work we did.

DR. BANERJEE: How does h over h_d one there?

DR. WU: That's the only correlation is like that. Although we don't mix it because that's unfair to do the comparison.

DR. SCHROCK: Historically people got started this data with the coordinates flipped from the general practice if you want the independent variable on X axis and dependent on the ordinate. This is the other way so it's not as though h over h_d is dependent on quality. It's quite the opposite. So getting numbers up there --

CHAIRMAN WALLIS: That's a really good experiment.

DR. SCHROCK: What?

CHAIRMAN WALLIS: They fixed the quality and they made it h.

DR. WU: In the variable it's evaluating to the correlation they presented it another way so we followed that. I will change it back.

CHAIRMAN WALLIS: Would they just be h_d because you're using the h downstream near the down generator side?

DR. WU: No we used the upstream side as getting worse because the level is lower so the gas h is even worse.

CHAIRMAN WALLIS: Which is h then? Which h are you using?

DR. WU: This is the same on the steam generator side downstream side.

CHAIRMAN WALLIS: It's just getting higher.

DR. WU: Actually it's the gas chamber. So the level is higher and h is smaller.

CHAIRMAN WALLIS: Smaller.

DR. BANERJEE: I guess this is a very confusing slide because what you've put as measurements right. The red dots are measurements. And the h_d you're using there is actually predicted by some correlation.

DR. WU: Yes.

DR. BANERJEE: Not your measured h_d.

Because if you measure h_d, h over h_d can never be more than one. So it's not a measured value of h over h_d. h_d is a correlation.

DR. SCHROCK: That's right. And the question is is it the same for all these data points?

DR. BANERJEE: I don't want you plotted.

CHAIRMAN WALLIS: So maybe we can just move on. There's not much to this figure.

DR. BAJOREK: Now wait a minute.

CHAIRMAN WALLIS: I don't think he's talking about this figure anymore. Let's move on.

DR. WU: Now the summary database improvement, the new database focused solely on the previous investigation of liquid entrainment in upward or vertical branch of the horizontal pipe. We do have all the branches's orientation data and we digitized it in case later as we needed that we could sort it out for each mechanism as Dr. Schrock mentioned. They have to be categorized into different mechanisms.

The second entrainment onset experiment the injection from the vessel top did not have much effect on entrainment onset correlation but it does show a systematic shift on kinetic differentiated results. Under the data sample rate for the liquid level probe was appropriate for the duration to an error greater than four minutes.

Next please. In the entrainment onset model development, it simplified the correlation from the -- and considered gas velocity effect in the main pipe. The new model agreed with available test data of different geometry, scale and Floude properties within 20 percent of the -- It's a linear scale. If you go back to the other presentations that are public, they use log scale and this is a linear scale. They thought the log scale could be even better --

In terms of rate we tested -- with a steam generator. They found oscillatory and transition and stratified way to flow downstream there. We tried to predict the onset but that's extra work though.

CHAIRMAN WALLIS: You don't change anything downstream. You just took your scaled pipe and scaled steam generator and say what happens if we make that pipe twice as long or anything.

DR. WU: No, we did for entrainment onset we did the -- variation but we didn't vary on the -- it's the scaled to --

DR. BANERJEE: It's scaled to APEX right? It's not scaled to AP600 in any sense.

DR. WU: It's the same on the APEX.

DR. BANERJEE: But based on some idea of

scaling.

DR. WU: Yes, so the Froude number that Dr. Zuber originally proposed that was under the boil fraction in the vessel and all this. And entrainment rate model development --

CHAIRMAN WALLIS: The simplest thing to do is to close off the pipe and throw away the steam generator and close the end of the pipe and see if it makes any difference. But you haven't done anything like that. You take off the bend and then everything and left it.

DR. WU: I did it.

CHAIRMAN WALLIS: You did it?

DR. WU: I did it a year before. That's the year I presented all these combinations.

CHAIRMAN WALLIS: What happens then? Does that give us correlation or something different? There is nothing in your theory that says that this is steam generated. Nothing in your theory that says what's downstream of the vessel or the break. There is nothing that says what's on the steam generator side.

DR. WU: At that time -- distorted the scaling. There were a thousand totally different --

CHAIRMAN WALLIS: It would interesting

though to see if that data from the distorted scale fits your theory or not. Because your theory says nothing about what's on the right-hand side or the steam generator side.

DR. WU: I will look in that.

DR. BANERJEE: The slug frequency will change because the compliances will change.

DR. WU: Yes.

DR. BANERJEE: And you find the slug frequency changes?

DR. WU: Yes.

CHAIRMAN WALLIS: There would be no slugs if you closed the pipe. They have nowhere to go. The sloshing. But it wouldn't be up and down in the steam generator.

DR. WU: No. That's because of the cushion space and we don't have the cushion space I'm sure. The proposed model based on the kinetic energy balance approach basically is the excess of kinetic energy over the entrainment onset condition partly goes to liquid. That's my basic option there. The option is not a rigorous derivation.

DR. SCHROCK: You're defining the excess as the total kinetic energy of the gas upstream.

CHAIRMAN WALLIS: B_1.

DR. SCHROCK: B_1.

DR. WU: Yes.

DR. SCHROCK: And as Dr. Wallis pointed out, B_3 is in fact going to be higher so there's really a deficit of kinetic energy not an excess.

CHAIRMAN WALLIS: B_3 is really much higher. I think something has to be done about that model.

DR. BANERJEE: You have to write it properly.

DR. WU: Okay. I'll think about it over lunch and see what I can do. Under the mechanism, the model predicted a trend of different data sets with reasonable accuracy improved compared with other correlations. However it has higher scattering under the slug oscillation.

CHAIRMAN WALLIS: Is it your job to consider whether or not the model is adequate for representing pressurized water reactors? Is that part of your job description?

PARTICIPANT: That's us.

CHAIRMAN WALLIS: That's you. So we need a whole new presentation that says the vapor numbers and the mechanisms described here are appropriate and everything. System effects are like that.

DR. BAJOREK: One of the next things that we are going to have to do with this is to take this and this scaling which is one for one with paybacks and show why this model, these models and these effects would not present in something like the AP1000. The l over d between the branch line and the steam generator was preserved in your facility and also in APEX so that link is still in there in the data but we have a larger diameter of course which the AP1000. It is not clear that you are necessarily going to get the same flow patterns developing in the AP1000 and the ATLATS type facility. We still have to make that branch.

CHAIRMAN WALLIS: So that’s another story we’re going to hear some other day.

DR. BAJOREK: Yes.

DR. BANERJEE: But APEX if I recall was the most poorly scaled from the viewpoint of the scaling study we did. We found that it wasn’t, the heights were not correct. It was a disaster in some cases. I have to go back and look at it. But scaling it to APEX is not really telling you too much about AP600 or AP1000. If these tests give you some information which is more general, you can also show that you can predict what happened in ROSA in terms of this or whatever it was called.

DR. BAJOREK: ROSA and SPES were the other two.

DR. BANERJEE: Yes, SPES was maybe also a little atypical because the lines were too small.

DR. BAJOREK: It's good for the early parts --

DR. BANERJEE: Yes.

DR. BAJOREK: Not really where this one lies.

DR. BANERJEE: ROSA was good. That was really very good. So it predicted what was happening in ROSA then you got something a little bit more general. You have to structure some more.

DR. BAJOREK: That's a good point because those diameters were larger than ROSA so there might be something to gain there.

CHAIRMAN WALLIS: Are we ready for your presentation, Steve?

DR. BAJOREK: Yes.

CHAIRMAN WALLIS: Thank you very much. That was very interesting. I think we have to have lunch and then come back and say how we respond because our job here is to add value to the work in some way. So I think we should probably do it after lunch or do you want us to do it before we go to lunch?

MR. ROSENTHOL: I would like Steve to get his comments out while it's still fresh in our heads. Having done that we can squeeze out this afternoon's work by maybe an hour where I'd like to not squeeze the rest of the afternoon by more than an hour. So if we could let Steve speak, go to lunch, put your thoughts together, discuss these issues for no more than an hour and then --

CHAIRMAN WALLIS: It might be briefer I think based on what we heard today hear certain things that we found acceptable, certain things we think need to be improved or fixed.

MR. ROSENTHOL: So we'll just pick up a half hour.

CHAIRMAN WALLIS: I think this might just be a list of five or six things. I think that in the long run the consultants on this committee may want to go back, read the reports and see what the basis of some of this work is that we did come across today and give you some critique. That won't happen today. Of course you have a lot of our response from the transcript that we don't need to repeat. Okay, Steve. Why don't you go ahead?

DR. BAJOREK: What I wanted to talk briefly on and this will take me about 10 or 15 minutes is where we go from here. What are our plans over the next six to a year? It’s very clear that there’s a whole heck of a lot of things that are going on in this data.

We see that the model development and analysis of this data continue to try to come up with better models as we saw the models that Dr. Wu has come with seem to do a better job for things which are horizontal stratified, where the slugging, the basis is at least weak at this point. But we see this as a significant step in the right direction.

It seems to do a better job than other correlations that we might want to apply for this horizontal stratified branch line type of geometry. Whether it’s luck or not, the model and correlations seem to do a good job not only for the ATLATS data but for other data sets. We don’t understand exactly how at this point but it seems though this is an improvement.

CHAIRMAN WALLIS: Did you compare it with APEX data?

DR. BAJOREK: No, not yet.

CHAIRMAN WALLIS: Isn’t there other data

from SPES or something else? Is there any other data beside APEX?

DR. BANERJEE: There is other data from ROSA and others.

DR. BAJOREK: You would have to get that out of the ADS. I think it would be here.

DR. BANERJEE: We had everything in detail. We had all the data.

DR. BAJOREK: ROSA however I think would be the most interesting one because of the larger diameter. That would be closer to AP600, AP1000.

CHAIRMAN WALLIS: This is going to be coordinated with OSU.

DR. BAJOREK: No.

CHAIRMAN WALLIS: I think the problem I would have would be if all this work stops and you're left with the correlation in its present form. Then you start doing comparison with ROSA and SPES and you say gee whiz it doesn't work. Maybe that could be fed into the APEX work, ATLATS work now so that Dr. Wu is also thinking about this other source of data sets. We don't understand exactly how at this point, but it seems that this is an improvement.

DR. BAJOREK: We're not going to completely give up on that. There are some question marks in the model development, what would happen if we made the diameter larger, what would happen if we changed the length between the branch pipe and the steam generators or we change the slugging frequency. The model is still applicable to that. There's a lot of things that we could look at. But we think there's a couple of things that might be of higher priority. I'll talk about that in just a second.

We are at the point now where OSU has developed a RELAP model. We think that the next step is to take that model, turn this into a TRAC-M model at this point think of TRAC-M and RELAP as being almost one and the same, put these models into TRAC-M, try to test these out to see if we can predicate the types of things that we saw in ATLATS, put those models and use those for simulations of APEX to see are we getting the desired effect from these models or are they washed out.

CHAIRMAN WALLIS: So you haven't tried yet to use his theory in RELAP to predict the break flow.

DR. BAJOREK: No. Not yet.

CHAIRMAN WALLIS: It's conceivable that a solution might not converge or something.

DR. BAJOREK: It may not.

CHAIRMAN WALLIS: We don't know.

DR. BAJOREK: The first time we put it in the code we'd expect it not to work.

CHAIRMAN WALLIS: When you try and do it backwards because he put in flow rates and then measured the height. If you try and predict the flow rate, you might find you get multiple values or something.

DR. BAJOREK: Everything he has in that model I think we can relate to what I call those primary variables, those things which occur in the main pipe to predict that quality. That's the way that it has to be for the code. But that's the next step. It is to put that in there. It's a near term need. We're supporting NRR in their Phase III evaluation for the AP1000. We'd like to use these models to help us make some of those decisions.

Now, the work that has been done in ATLATS also is in conjunction and compatible with where we see the work at OSU going next. That is to make more use of the APEX facility to perform some confirmatory tests that are directed towards AP1000. As I think you may have heard in the past, Jose Reyes was awarded a NERI grant to take the facility, modify it by increasing the core power, change the branch line size, change the ADS to make it look a lot more like the AP1000.

DOE is sponsoring a series of tests. We're watching what they're doing. We've worked with DOE. We've talked with Professor Reyes about what tests they would perform and what we see as being the open questions in AP1000 as to the carry over and performance of the ADS and giving them our two cents worth on what types of tests they should run.

They've gone out and formed their test series. We've augmented tests around that. We've planned a test series that would assume that the DOE-NERI tests have been run. Theirs are oriented more for design basis tests. That gives us the liberty to look at things which are directed more towards model development, code validation, and some beyond design basis. At the beginning --

DR. SCHROCK: These NERI tests will be done in this next fiscal year.

DR. BAJOREK: They're scheduled to start in October of this year. So most of those would be done starting in October and about a one year calendar. When we started the meeting, we talked about the entrainment processes and the effect, the advanced plans. We can group them to two different areas.

What Dr. Wu has been looking at has been the entrainment up here in the hot leg where slugging occurs when that h/d was very large, where we had that level up very close to the top of the pipe. But very important in the most critical accident scenarios in the AP1000 is the entrainment that occurs from the upper plenum pool and how that liquid gets carried over to the ADS during the very late stages of ADS for a blow down and throughout the IRWST injection.

Predictions made with RELAP and with Westinghouse codes shows that this minimum level above the top of the core is pretty small. Predicting how entrainment occurs in this region is also a bit of a black art. As we look at correlations and models which were available, we saw some very good work that had been done by Ishii and Kataoka back in the mid-80s. They made use of mainly information from the chemical industry for large pots and how much liquid was carried over. But the question that really wasn't asked when we presented that was how applicable was that to an AP advanced plant vessel. What happens with the de-entrainment?

So, in planning these test matrixes, we'll see them broken into two different categories. This is the ones that DOE is tentatively planning. These would look at DVI line breaks which are the most critical accident of interest for the AP1000, a couple of smaller breaks, and another one to characterize the natural circulation through the system, some no reserve tests which are examining the transient effects of the ADS-4 system as it blows down.

But it would also look at some what we're calling steady state entrainment tests to try to get at this type of an entrainment process so that we can take models that are perhaps similar to the Ishii-Kataoka entrainment model which says you get very high entrainment as the level is very close to the top of the pot but decreases in an inverse cubic fashion as that level gets lower and lower. We don't know whether for a plant it's going to be up here or down here. (Indicating.)

CHAIRMAN WALLIS: I have questions about that because it's still going to be carried to the hole there.

DR. BAJOREK: That's right, yes.

CHAIRMAN WALLIS: This is not just the dimension of the hole that matters.

DR. BAJOREK: Well, this E_{fg} which is the entrain flux to the total gas flux is how much of that liquid is actually carried up and out.

CHAIRMAN WALLIS: Yes, but I would think that this high attenuation with -- is the spitting of the drops. They spit out and then they fall back again. The stuff that's spat out from the far side isn't going to get to the hot leg by any means.

DR. BAJOREK: Well, the idea is to try to predict how much gets up into the hot leg.

CHAIRMAN WALLIS: Yes, but how they got from one side. So you have to be careful just blindly using an Ishii.

DR. BANERJEE: Are these low pressures also one sees quite a lot of fluctuation in the level? I don't now with these very low powers whether you get these slugs or not. You see eruptions and the level goes up and then down. I don't know what you see. Have there been experiments that have shown the behavior of the surface at low pressures?

DR. BAJOREK: I'm not aware of any. But that's one of the areas that we're at least thinking about at this point and planning ahead, that perhaps the next use of the ATLATS facility might be to try to get at this problem. In the APEX facility of course with the instrumentation and it's a heated facility, steam-water, it's more difficult to make changes.

ATLATS was scaled including the vessel one to one to APEX. It would give us a relatively easy means of putting in an upper core plate, something that looks like internals, taking them in, taking them out, doing tests in a more rapid fashion in order to try to evaluate how much of that liquid is actually entrained out of the vessel and then into the hot leg.

CHAIRMAN WALLIS: Then runs back from the hot leg into the vessel in the bottom. I wonder if your codes can handle that.

DR. BAJOREK: Are you referring to the horizontal CCFL?

CHAIRMAN WALLIS: I say that the droplets are entrained from the vessel and spat up. Some of the trajectory will to lead to them settling out in the hot leg and they run back in the stratified layer of liquid into the vessel. I'm not sure the codes can handle that several region model.

DR. BAJOREK: At this point, we would probably doubt they could handle that. In terms of developing the data to either validate what's in there or develop new models, we think we can get some of that out of the confirmatory tests in APEX but with the ATLATS facility given that it's easy to make changes and perform visualizations. We see using that facility to try to attack that problem in the future.

CHAIRMAN WALLIS: So the conservative thing to do would probably to be to assume if it's entrained in the vessel then it all goes out the ADS-4.

DR. BAJOREK: Right.

CHAIRMAN WALLIS: That may not fit at all when you compare with experimental data.

DR. BAJOREK: Thinking about the codes that I've been used to using, my expectation with most of that droplets that were entrained would in fact go out the ADS. Accounting for the de-entrainment either on the upper plenum structures or in that transition of the hot leg is not --

CHAIRMAN WALLIS: They're entrained on the far side of the vessel from the outlet.

DR. BAJOREK: I'm not sure the code would try to de-entrain them.

CHAIRMAN WALLIS: Well, the code wouldn't. Physically they probably --

DR. BAJOREK: Physically they would. Dallman and Kirchner ran some experiments back in the early 80s. They showed that after about two or three rows of guide tubes and stuff in the way 90 plus percent was de-entrained. That's from the far side.

CHAIRMAN WALLIS: Well, what's the time

schedule here? I thought AP1000 was going to come to the ACRS for approval pretty quickly. Here we're going to have a whole set of experiments which haven't even started yet trying to answer questions.

DR. BAJOREK: Well, these are answering Research's questions to confirm the behavior of AP1000, helping NRR come to their conclusions, but also to develop models for our codes. In order for us to get TRAC-M in order to confidently evaluate something like an AP1000, we need some of this data as well.

CHAIRMAN WALLIS: How about AP1000 approval? Can we wait for the results?

DR. BAJOREK: If I were a licensing plant, I would want this and feel I needed this data now. The strategy that is being pursued right now between Westinghouse and NRR -- Well, they're hoping to get an SER written I think by March 2003. They may see some of this data but certainly not enough time that you would want to sit and think about it and make some sense out of it.

I believe that their argument is going to be that if you bound it reasonably high there's still plenty of water in the system. Now, that might be an argument that would work for licensing. Is it safe?

But it's not one that would be very satisfying to say that you have the right models and you can actually predict it reasonably well. I'm not sure how successful that will be, their approach in the licensing. That remains to be seen I guess.

DR. BANERJEE: In the AP600, there was a period I think maybe quite briefly when the core did uncover if I remember or very close to.

DR. BAJOREK: It uncovered in a couple of the no reserve tests. But those were pretty extreme in terms of what the transient was. For AP1000 the simulations that have been run shows that it's close. There is a level that's maintained which causes us a lot of uncertainty that if this curve were a little bit steeper or less steep that might be enough to start getting a heat up in the core. We still think that there's plenty of water in the AP1000 and that you're not going to have a deep uncovery. But is it --

DR. BANERJEE: It was sort of a balance between what was going out of the ADS systems. When the IRWST came on, there was a time that I remember it went down and then it recovers. It drops really fast.

DR. BAJOREK: It's a race because you're at high pressure. The IRWST can't come on. During that period you're doing a lot of entrainment and a lot of flashing.

DR. BANERJEE: Right.

DR. BAJOREK: That's why this and what Dr. Wu has been looking at is very important. If there's sufficient entrainment and that pressure drop in the ADS is sufficiently large that the period of time is larger than what we expect, then IRWST isn't going to come on in time and that level will drop in the core.

DR. BANERJEE: It's crucially dependent on how much liquid goes out of the ADS system.

DR. BAJOREK: Yes. That's it. So looking at our plans right now, we're looking at continuing work at ATLATS, looking at the data, potentially doing tests to look at other entrainment mechanisms, but also using APEX now in its new configuration for AP1000 to primarily look at entrainment from the upper plenum and scenarios that really examine this ADS-4 system and how much liquid gets tossed out during that blow down period.

MR. BOEHNERT: When would these tests start, Steve?

DR. BAJOREK: They're scheduled to start in October of this year.

MR. BOEHNERT: Okay.

DR. BAJOREK: I know that they're actively modifying the facility right now. They'll hopefully get everything ready at that time. First would be some facility characterization tests to make sure the pressure drops, things have changed. We have asked Dr. Reyes to look at a schedule that benefits everybody. If you set it up for a DVI line break, we'll do some of the NERI tests and put ours in there --

MR. BOEHNERT: Yes. You're running concurrently with the NERI tests.

DR. BAJOREK: Yes. That way everybody gets a bit of cost saving.

MR. BOEHNERT: So is yours supposed to conclude in about the same time, a year?

DR. BAJOREK: About that. Because it is a DOE project, we're the second tier on this.

MR. BOEHNERT: Yes. They have first dibs.

DR. BAJOREK: So some of ours waits towards the end. As we get farther into the project, some of our tests are going to be at the end so that DOE can get theirs.

CHAIRMAN WALLIS: So what questions is NERI trying to answer?

DR. BAJOREK: They're looking more at

confirming the overall safety performance of the plant. They're looking at design basis tests and if you run a DVI-like case or a small break, the argument or what has been told to the staff is true. You don't have core uncovering. There's lots of water. There's plenty of margin.

Most of our tests are looking at is there a cliff. If you fail one other thing or if you do something small suddenly you get a big uncovery. So we're starting from the DOE matrix, making changes to that, making it more severe, and adding on some of these tests which are using it in almost a separate effects fashion to help us come up with the models for this upper plenum entrainment.

MEMBER RANSOM: Steve, you mentioned the simulations have been done for AP1000. Are those with TRAC-M?

DR. BAJOREK: No. Right now those have been done with RELAP.

MEMBER RANSOM: Do you know what model is used in TRAC-M for this entrainment type phenomenon?

DR. BAJOREK: Yes. That was the Berkeley model, coefficient to the 5.7 and 1.8.

MEMBER RANSOM: So it would have the same problems that RELAP5 has I guess.

DR. BAJOREK: Yes. That's all I have.

CHAIRMAN WALLIS: Thank you very much.

DR. BAJOREK: Okay. Thank you.

CHAIRMAN WALLIS: Now it is 12:30 p.m. I think we're all due for a break. Can we take 45 minutes for lunch? Would that be okay with the Committee? That's 45 minutes for lunch. So we come back at 1:15 p.m.

Then I hope we would spend about five minutes maybe just summing up what we heard, a reaction to this morning which will be I think preliminary because I certainly would like to dig a little deeper into the reports. And then we can move on with the rest of the program. For the point of the ACRS schedule, the most pressing thing is actually the resolution of GSI-185 because we're going to be asked to write a letter on that.

MR. BOEHNERT: Not until the September meeting.

CHAIRMAN WALLIS: That's September. September is not very far away.

MR. BOEHNERT: No.

CHAIRMAN WALLIS: Whereas we're not going to be asked to write a letter I understand except in so much as what we heard today affects what we say about the research program report, the big fat research report. We are not writing a separate letter based on what we heard this morning.

DR. BAJOREK: Joe, I'm not sure about what you have on the TRAC-M work. I know when I talk about the rod bundle heat transfer, I'm not going to need a whole half hour. I think I can contract --

CHAIRMAN WALLIS: I think our main concern with the TRAC-M development is why hasn't this baby been born.

MR. ROSENTHOL: It's very big.

(Laughter.)

PARTICIPANT: I think what might work out well is we're scheduled to do 185 at 2:00 p.m. Let's do that. Steve and Frank and Joe will just talk a little bit faster. We'll make up the time.

CHAIRMAN WALLIS: If we have to start at 2:15 p.m., there's no big problem with that. Okay. So we will take a break for 45 minutes and come back. Thank you very much. Off the record.

(Whereupon, at 12:33 p.m., the above-entitled matter recessed to reconvene at 1:18 p.m. the same day.)

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

1:18 p.m.

CHAIRMAN WALLIS: On the record. What I wanted to do is very briefly go over how we should respond to what we heard this morning. Since we're not writing an ACRS letter, we could presumably provide useful feedback. We provided a lot of comments which are on the transcript. I think the best way to give thoughtful feedback is probably in written form.

We all have comments on the modelling and the appropriateness and the various equations and so on. It seems to me that's best done by the consultants and the members who are here writing individual critiques which can then be passed on to the staff and OSU. Unless there are some points which must be made orally now, then I propose that's what we do. Sanjoy was saying that we each write a written critique of what we heard this morning in a form that is most helpful for the staff and to Dr. Wu rather than trying to say it all orally now unless there's something you want to raise.

DR. BANERJEE: When do you need them? Will we do them together, Graham, or what?

CHAIRMAN WALLIS: Well, I was just going

to take your comments and pass them on unless they're so -- that they're edited. I think that's the most effective thing that we can do rather than trying to summarize it orally now unless there are some points which you want to make which you didn't make this morning. Virgil, is there anything --

DR. SCHROCK: In terms of timing, we have a holiday next week so we don't need this --

CHAIRMAN WALLIS: That's right. I would say the middle of July or something. Don't forget it though. The sooner the better.

DR. BANERJEE: I'll do it.

CHAIRMAN WALLIS: I think this is just to summarize. Probably none of us would say that everything is so complete and solid that we don't need to do anymore work unless someone disagrees with that conclusion. That's my feeling from what I heard this morning. Does the Staff wish to say anything more?

DR. BAJOREK: No. I think that about covers it.

CHAIRMAN WALLIS: Dr. Wu is here, so if we individually want to meet him on the break or something if we have anything which would help right away, feel free to talk with him. Let's go on with the original agenda. Joe Kelly. It's always a great pleasure to hear from Joe especially if he has some technical achievements to tell us.

MR. KELLY: I need a microphone.

MR. BOEHNERT: Over here, Joe.

MR. KELLY: Thank you. Okay. I'll be talking about the TRAC-M code consolidation and development, just a quick status. When we started, we had basically five objectives. They were to modernize the architecture, to affect the code consolidation, to prove ease of use, accuracy, and numerics.

Improving the accuracy and numerics were basically going to be future activities. We've done very little on that to date. The ease of use is mainly being addressed through the development of a graphical user interface. I won't be talking about that today.

Our first efforts were in modernizing the architecture and that was to make it possible to make it do the development we felt we needed to do now and also in the future. That has been completed. But of course as we go through time and the occasion offers itself, we will continue to make some improvements.

CHAIRMAN WALLIS: Does that mean that something -- Maybe I'm on the second bullet. Does that mean the new code could behave like a RELAP5 if you asked it to or have you lost something which means that you could never go back and be like RELAP5? It's always going to be itself, a new breed.

MR. KELLY: Well, it will behave a lot like RELAP5. Let me go ahead because that's what I'm going to talk about some. Code consolidation affecting this has really been the major activity over the last few years. The initial objective was simply to recover the modelling capabilities of the predecessor codes.

Ramona is basically coupled from hydraulics and reactor kinetics. TRAC-P was PWR large break LOCA. TRAC-B was boiling water reactor of what was in transients. RELAP5's primary mission was small break LOCA for pressurized water reactors.

So what we wanted to do was to be able to have the same modelling capabilities, not the same physical models but be able to handle those types of transients. As we went along it was decided that we need to retain the investment that we had in legacy input models, basically and especially models for RELAP5. That's important really for two reasons. One is to keep our user base because most of the existing input decks out there are the electronic models. The other reason is to aid our own assessment.

The success metric that we gave ourselves was that the simulation fidelity must be equal to or better than each of the predecessor codes for their targeted application. What that means is for example if we're doing a PWR small break, say either a separate effects assessment or an integral effects assessment, we're going to compare TRAC-M versus RELAP5 for that test and do a code to code to data comparison. TRAC-M has to be at least as good as RELAP5 or we have not met the success metric. When Dr. Wallis asked why we haven't delivered this product yet, this really is the answer and trying to retain the investment in the legacy input models.

CHAIRMAN WALLIS: Now, does that mean that you have RELAP5 here and you have TRAC-M there? Compare them. What does it mean that you can say TRAC-M behave like RELAP5 and give me a prediction? Now, behave like TRAC-M and give me a RELAP quotation. So that it essentially still can do RELAP5 if you want it to.

MR. KELLY: No.

CHAIRMAN WALLIS: You cannot make it entirely RELAP5.

MR. KELLY: No. I'll try to go through the process in the next couple of slides to explain what we meant by consolidation. We didn't simply take three or four codes and glue them together because that would defeat the purpose of having one code that we have to maintain and learn and be able to do development in.

To process the legacy input models, this is the process. The large boxes represent SNAP which is the acronym for the graphic we use in interface and the TRAC-M code. For the moment, let's just focus on the bottom box. Traditionally an input deck is an asking text file. We still talk about cards and ASCII columns and so on.

This is read by the TRAC code. So you read the input. You process it. You initialize it. Then you can do the calculation and dump a graphics file which can be done in X and Y plottings using what we always call XMGR. When we incorporated the TRAC-B component models into TRAC-M and what I mean now is things like jet pumps or the fuel channel, they were built on top of pre-existing TRAC components.

For example, the jet pump is build on top of a T. So it's the T that has been specialized to be able to work as a jet pump. That's the way TRAC-B was developed anyway. So now if you want to model a boiling water reactor, you can use the TRAC-P or basically TRAC-M input and describe it all that way. We also included the -- from TRAC-B to read their input and in effect convert that to TRAC-M and go through the same path.

Now when you have five input decks, that was a little bit more complicated. The reason is there are just some fairly fundamental philosophical differences in the way things are modeled. They sound really simple.

For example, if you take a pipe and TRAC and you divide it into a certain number of nodes, where you obviously have junctions where you compute the momentum equations internally to the pipe, the TRAC also assumes that it has those junctions on the outside of the pipe. When you have five for a pipe model, you only get the internal junctions. Then when you go to hook that pipe up to other things you either have to use single junctions or branch components or valves which also work as a single junction. So there were some fundamental differences in the way the components hooked together.

We couldn't just simply read in a RELAP5 input deck the way we can a TRAC-B. But we had already started the graphical user interface. The first thing we did to that was make it work for

RELAP5. That was done because of our existing user base. So you can read in and ask the input deck for RELAP5 into what's called the RELAP5 model editor. That can display it. Then you can go in and edit each different component and change the input.

This is the new part. The RELAP5 model editor can now export what's known as a RELAP5 TPR file where TPR is an acronym for TRAC portable restart. That's something we're going to be using and you'll hear more and more in the future. It's a platform independent binary file that contains all of the geometry and fluid condition information to describe each of the components in this RELAP5 model.

Then inside of TRAC-M, we build on this part which will map the RELAP5 components to TRAC components. So typically a RELAP5 pipe will then also be mapped to a TRAC pipe. Then whatever it connects to will have to come to a new TRAC component which we've created called the single junction component. It's very much like what's in RELAP5. So we had to add some components to TRAC just so we could do this mapping.

This is in red which means it's an ongoing effort. But it's almost finished. It's somewhere in the order of 90 to 95 percent complete. We can actually take a RELAP5 input deck like a stripped down version of the typical PWR problem, read it in from an ASCII input file, input it to TRAC using the TPR file, map it to TRAC components, and execute it.

So basically all of the 1-D -- components and the heat structures have now been mapped. The part that we haven't finished is the control system. That's well underway. It should be finished in a little bit more than a month.

Now, once you've done this, you now have the RELAP5 components represented within the TRAC database structures. Then we have something called the TRAC TPR file which is the TRAC version of that. That provides the communication between SNAP and TRAC. So now we can take this back, read it into SNAP, and it will display it as the TRAC components. You can go in and edit it and so forth and then come back and run it.

This also is almost complete. It's probably more like 80 percent complete. The TRAC-M post-processing some of that's been done. This is more the visualization and making it easy to work your way through a transient calculation and see what's going on.

PARTICIPANT: (Away from microphone.) What does TPR mean again?

MR. KELLY: TRAC portable restart. It's one of these things. I won't spend anymore time on it because it's not that important to this. But what I did forget to say is RELAP5 is basically a 1-D code. It can simulate multi-dimensional by these cross flow junctions.

When you do this translation of 1-D components into TRAC even if RELAP5 is a modeling of reactor vessel with a down-comer and a core when it's mapped to TRAC components, you're not all of a sudden going to get a three-dimensional vessel model because there's not enough information built into the RELAP5 geometry to do that. So one-dimensional core channels with cross flow junctions and RELAP are going to be mapped to one-dimensional flow channels in TRAC with these single junction serving as cross flow.

What we're doing in the latter part of this year and this is going to be part of the SNAP development is building in Wizards to SNAP. When you read in the RELAP5 input deck and you're going to do this conversion process, you can tag certain one of the channels as what's your PWR core. You tag these channels and say I want this to be a 3-D vessel with so many radial rings, so many -- sectors. It will help automate that process so the user doesn't have to do it from scratch.

The other thing I wanted to say before I take this slide off is at the present in TRAC we still have the capability of reading these types of input decks, doing initialization, as well as all this component mapping. What we want to do in the future and this is how we're going to work going forward is move all of that up to the graphical user's interface.

So the TRAC itself will be streamlined. It will only be a computational colonel. So it will have numerics and the physical models but not all this input-output stuff. It just makes the code much larger and harder to work with.

So where are we? This is what we're doing in calendar year 2000. The project started in October 1997. These colors look great on this. They look great on my computer screen but they don't work to well here. So most of the effort over the past year has been on this line implementing RELAP5 functionality. What we mean here is not the physical models from RELAP but the components that we needed, for example, this single junction, in order to be able to use a RELAP5 input deck and then the component mapping. Again, this is about 90 percent complete.

The main path is along this way. (Indicating.) Once we're able to do the component mapping, we're going to start doing the development of simulations. Actually we've already started doing some of this. This again is going to be code to code to data comparisons. If you're doing a simulation for a boiling water reactor, you're going to run it with TRAC-M, TRAC-B, and then compare both of those to the data and use some sort of quantitative metric for each case.

If it's an axial profile void fraction at a steady state, a simple RMS is fine. But when it's a transient, it's going to have to be more complicated. How do you judge quality over the course of a transient? Each person that is doing an assessment is going to have to come up with the key variables they want to compare and what kind of quantitative metric they're going to use. That's something we're going to work at improving as we go along.

So this is the path when we get the developmental assessment done. We're planning on having that done by the end of this calendar year. We'll do the initial alpha release of the consolidated code. Alpha in this context means it's an internal release. So it will be used by our immediate contractors and anyone at the NRC.

There are two develop activities going on which I'll touch on briefly later. That's rod bundle interfacial drag and an interim reflood model.

MR. BOEHNERT: Virgil, do you want to use your microphone? She can't hear you.

DR. SCHROCK: What does that mean, the Ramona in parenthesis there?

MR. KELLY: Okay. Ramona was a 3-D kinetics --

DR. SCHROCK: I know Ramona much better than I do PARCS.

MR. KELLY: Okay. PARCS is the 3-D reactor kinetics module that we use in TRAC-M. So it couples the TRAC-M.

DR. SCHROCK: Is it derived from Ramona or is it something separate?

MR. KELLY: No. It's completely separate. It's the 3-D reactor kinetics module which was developed at Purdue University primarily by Tom Downer and his students. That's something that we can put on the agenda and have him come and give you a presentation on sometime.

DR. SCHROCK: What do you mean by what's

written in that box?

MR. KELLY: What's here is that we are now able to do a coupled thermal-hydraulics 3-D reactor kinetic simulation whether it's for a main steamline break or some type of boiling water reactoring stability by coupling TRAC-M with PARCS. That's done. It works. We've actually showed some results here in the past.

DR. SCHROCK: What you said didn't make any use of Ramona.

MR. KELLY: No. It's to replace the functionality of Ramona like this is going to replace TRAC-B and this is going to replace RELAP5.

DR. SCHROCK: Right.

MR. KELLY: So, we're moving towards having just one code that we maintain and improve.

CHAIRMAN WALLIS: When you prove that your physiology is equal or better than that of predecessor codes, doesn't this mean an enormous amount of comparison for a whole host of situations?

MR. KELLY: Well, that's what we're doing here.

CHAIRMAN WALLIS: Probably it's not going to work the first time.

MR. KELLY: You're exactly right. We're

not going to be able to do as much developmental assessment as we'd like. We're mainly just resource limited. What we've done is take the assessment matrixes of both TRACs and RELAP5 and taken a selection of test cases from all of those.

CHAIRMAN WALLIS: Did you show us that last time? You showed us more detail of those.

MR. KELLY: Yes. This is something that Steve is overseeing and again something we can come back to you on and show you in more detail. We would just prefer to have some of the assessments done when we come and show them.

So 2002 is not the end of it. We have an alpha release. But we've done the developmental assessment. The next step is to go through that assessment, identify the code deficiencies and there are going to be some where we need to improve either the physical models or the numerics. The numerics in this standpoint are from robustness and also computational efficiency. When you do that, repeat the developmental assessment, check to make sure the assessments are okay if not, go back through this loop.

CHAIRMAN WALLIS: This is all done internally. There's no public comment period where you put out a preliminary version and people outside NRC run it and come back with experiences.

MR. KELLY: Well, some of the assessments are going to be done by our contractors.

CHAIRMAN WALLIS: Okay.

MR. KELLY: Whether you call them internal or external.

MR. ROSENTHOL: Joe, and we're going to do a beta version to the CAMP members.

MR. KELLY: The beta version to the CAMP members will be in spring 2003. Then we'll start getting feedback from them. This will be the first official code release at the end of 2003.

CHAIRMAN WALLIS: That's the beta version.

MR. KELLY: The beta version will go to the CAMP members. That will be in spring 2003 at the spring CAMP meeting.

CHAIRMAN WALLIS: Okay. So there will be other people working on it.

MR. KELLY: Yes. That's one of the reasons we spent so much time trying to retain these legacy input models. It was so we could keep our user base so we can keep getting feedback from them.

DR. SCHROCK: These are sort of like heritage tomatoes.

(Laughter.)

MR. KELLY: And then also during this period of time as Frank Odar will tell you, we'll be updating the documentation. To make this feedback group work one of the things we're doing is spending some time automating the assessment process, making it easier to run if you will in a batch mode or a large number of assessments and get the plots out and do some of the quantitative metrics. That is one of Chris Murray of our group's many activities.

I mentioned that there were two ongoing development efforts. These were necessitated because of code deficiencies that became all too apparent. The first one was rod bundle interfacial drag. Tony Ulses of the staff went to do the Peach Bottom Turbine Trip. This is coupled neutronics thermal-hydraulics. He couldn't predict the steady state profile in a boiling water reactor accurately enough for the kinetics to work properly and proceed with the benchmark.

So kind of on his own, he bootlegged the TRAC-B interfacial drag and heat transfer correlations into the code only for the channel components in the BWR core. It worked so well that we took a step back and said obviously the TRAC and physical models aren't good enough for rod bundles. Let's go ahead and start with the TRAC-B models. So we're implementing them now. They're going to be applied only to the CHAN component which is the BWR fuel assemblies and to the 3-D vessel core region.

DR. BANERJEE: What's different about this model compared to the other model that was there?

MR. KELLY: There's two things. It's basically interfacial drag but also interfacial heat transfer for this application to subcool boil. Interfacial friction models in TRAC, we're talking about bubbly flow, bubbly slug, TRAC-B was primarily for large break LOCA. So they never paid a whole lot of attention to steady state profiles in the reactor core because --

DR. BANERJEE: Do you have drift flux model built in as well or what?

MR. KELLY: No. It's pure fluid.

DR. BANERJEE: It's pure fluid.

MR. KELLY: So you need an interfacial drag correlation. Now, TRAC-B derives an interfacial drag coefficient from a drift flux model. So it's a little bit more accurate for rod bundle type geometries. Basically you're saying that the bubble sizes that TRAC picked for slug flow were wrong. Then also the subcool boiling model wasn't as good. It was basically the people that worked on TRAC-B spent more time worrying about it because it was something that was important to them.

DR. SCHROCK: This 3-D vessel core region, I don't know what you get there that gives you what you need for the calculation in a PWR. I mean, transfer, mass, minimum energy exchanges are not in the BWR CHAN.

MR. KELLY: That's true. Interfacial drag in a lateral direction is something that is basically a black hole that people have not spent much time worrying about. It's done with different ways in all of the codes. It's something we need to pay more attention to.

DR. SCHROCK: I agree with that, but how one gets a 3-D calculation in the vessel core region is what I'm questioning.

MR. KELLY: Well, from my standpoint it's 3-D but only 3-D in the sense of very large regions. It's not 3-D in a CFD kind of sense. Here our computational volumes are quite often half a meter long. They may have between 1 and 10,000 fuel rods. So they're very large chunks of a reactor vessel. And 3-D momentum fluxes and so on in the core are really not terribly important. It's more just a radial and axial distribution avoid caused by the different power generations.

CHAIRMAN WALLIS: If you get them wrong, you can get some unstable behavior. You just permit them to exchange between parallel channel.

MR. KELLY: Yes. As we've seen in some of the AP600 calculations we did earlier.

DR. SCHROCK: I looked at some of that documentation that came on these CDs that were distributed. I gather from what you told me at lunch that it's not for TRAC-M, but it's documentation for TRAC code. Did I get that right?

MR. KELLY: Well, I probably didn't make it quite right. It's the TRAC-M version minus the BWR models. Remember, we just took TRAC-P and changed the architecture. We didn't change the answer. That was one of the things for better or for worse we tried to do was keep the answer the same as we updated.

DR. SCHROCK: I guess where I'm coming from is that I've been critical of the TRAC being represented as a full thermal-hydraulics 3-D computation. Those words, maybe not in that exact order, are used to describe what the code is. I don't believe it. I don't think you're going to have that in TRAC-M.

All I'm concerned with in this comment is that I think it's been oversold. I don't want to see you continue to oversell it as something that it really is not with arguments that some things are less important than other things and so forth. It's not a "3-D computation" in my view. If I'm wrong then I'd like to see how it's justified.

MR. KELLY: Well, I don't want to spend too much time here.

DR. SCHROCK: No I agree.

MR. KELLY: But you're certainly right in some senses. In other senses from a different view point, it does use a cylindrical core system. It does have a momentum flux tenser. It doesn't make the assumptions that a sub-channel analysis does that some of the momentum flux terms are negligible and leave them out. It has them there. But the finite differences are taken over very large control problems. It certainly doesn't resolve any kind of small scale 3-D flows and not turbulence, et cetera. That's not there at all. This because shear stress is not there.

DR. SCHROCK: But if you couple it to a nodal ertronics (PH) code, you need more detail then that void distribution. So how do you get that?

MR. KELLY: For the Peach Bottom Turban Trip, Tony Ulses --

MR. ROSENTHOL: He isn't here. I think we used three to five rings, I don't remember, and a dozen or 16 axial elevations.

MR. KELLY: I think Tom said something like 36 or 35 fuel assemblies and as Jack was saying something like 16 axial levels.

CHAIRMAN WALLIS: Many more axial than --

MR. ROSENTHOL: Yes. 16 times 3 free radial rings.

MR. KELLY: I mean, 35 independent fuel symbols.

MR. ROSENTHOL: Yes.

CHAIRMAN WALLIS: Joe, Jack I think promised that you guys would be so quick that we would catch up time. My experience is that Joe always speaks with three times as long as its allowed. It seems to be what's going to happen.

MR. KELLY: Okay. Well, I'm actually almost finished. I said all the important stuff. So I'll just go ahead and hurry up and finish.

MR. ROSENTHOL: Let me chime in then if I may. That is that there's an ISP-46 which is this

International Standard Problem on this Peach Bottom Turbine Trip where we have real data for real core, admittedly the Turbine Trip is not as challenging a transient or an accident as a LOCA but it's a real one that took place. We're hinting that -- distributions and kinetics went on very well as a function in time. So to whatever degree, it's a good thing. It's encouraging.

MR. KELLY: And I would propose that we have Tony come back and present the details on that.

CHAIRMAN WALLIS: It would be wonderful someday to have a success story like that. There's a lot of stories. We get all this overview of what's going wrong. It's good to see some real results.

MR. KELLY: And you'll be seeing more and more of that as the assessment goes on.

DR. BANERJEE: But that was after the -- I mean, you had to adjust the hood to make it work. Right? It wasn't --

MR. KELLY: The adjustment as far as I know because I didn't do the work was implementing the TRAC-B interfacial drag. That was an adjustment. That was just for the steady state.

CHAIRMAN WALLIS: It wasn't tuning it to the --

MR. KELLY: No knobs. Right.

DR. BANERJEE: It would be nice if it was just the cold that did it.

MR. KELLY: I agree. But that's why we made the decision to implement those models because our philosophy before was do all the assessments, see where there are deficiencies, and then change the models. But we decided to go ahead and make this change now because of that.

We started looking at some of the TRAC-P reflood. They updated the reflood model six or seven years ago. They really never did much assessment on it just because the focus changed. When we started looking at it, it turned out it wasn't very good at all. In fact, it had unacceptably large oscillations and was highly conservative when you looked at separate effects tests.

So we're basically redoing the reflood model and putting in a much more simple interim model that we're going to use for the AP1000. Likewise, later this fall, you'll see that effort and I'll see the assessment of it.

DR. BANERJEE: How was it highly conservative? Was it pre-cooling or what?

MR. KELLY: We're talking about separate

effects tests, things like FLECHT-SEASET. So they're one-dimensional. The oscillations are like vapor explosions. They just throw all of the water out of the bundle. In FLECHT-SEASET, the upper plenum works as a phase separator. So once you throw the water up there, it's gone. One inch per second reflood tests documented more like a tenth of a second which means it takes forever to flood the core.

All right. So that's everything that we're doing now. This is what the future looks like as best we can forecast it now. I really only want to say a few things about this. Once again the colors don't quite come out.

What we're working on now, and this is supposed to be blue, are these areas. (Indicating.) Those will go into the first release at the end of calendar year 2002, the alpha release. What I really want to say on this is you'll have planned development or planned PARC releases on one year intervals. But we plan always try to stop the development about six months before a code release so that we have some time to go through the developmental assessment and make sure we've made things better and not worse.

The only other thing I want to say is along the top block, the development assessment is what we're doing right now for the metric of success in code consolidation. From that point on, we're going to be doing PIRT based assessments. What I mean here is for each of the applications whether it's BWR transients, PWR small break, PWR large break, go through for the highly ranked phenomenon and you'll find separate effects tests for the right conditions, the right geometries, et cetera, and then also integral effects tests for each of those applications. Only after we do the assessment, then we'll find the model improvements we need to make. That's what's really going to drive the program in the future.

The next to the last slide is the incorporation of the experimental results. We presently have four experimental programs: a subcool boiling at low pressure at UCLA; a phase separation at T's at Oregon State which you heard about this morning; a rod bundle heat transfer program at Penn State University which Steve is going to talk about after me; and the interfacial area transport at Purdue and University of Wisconsin.

As the code consolidation approaches closure, this has become more and more one of my principal jobs. It's working on this together with Steve Bajorek. As Professor Ransom said, in the past there's always been this separation between experimentalists and code developers. Quite often that led to models that didn't work in codes or improperably understood things.

So Steve and I are going to be working very closely with the experimenters and actually doing some of the model development in-house in an effort to try to make the codes better and make these experiments fit the code needs. For the future, this program is basically over. It will be by the end of this year. All of these are starting to reach maturity with the exception of this one which is more an exploratory research program.

So in the future, it will be the code assessment results or new applications such as AP1000 which will drive the initiation of future experimental programs. We'd like to keep our experimental programs about the same level.

DR. BANERJEE: Are you going to tell us at some point more details about the reflood heat transfer and stuff?

MR. KELLY: Yes.

DR. BANERJEE: How are you getting rid of these vapor explosions? I guess they happen naturally. Right?

MR. KELLY: Well, there are those. But what happens when the TRAC-P reflood model is not natural ones?

DR. BANERJEE: I see.

MR. KELLY: Let me try to sum it up in a few words. The development work that was done took a very academic-type experiment that was performed by Professor Ishii at Purdue and the development work was very well intentioned. It tried to do things from a very I don't want to say academic again. But in effect, they correlated things in terms of parameters that are ill-suited for inclusion in a numerical framework.

Briefly, they broke the region ahead of the quench front and included seven different regimes; smooth, inverted, angular, wavy, et cetera. The length of each of these regimes is a function of a capellary (PH) number which is the velocity of a liquid jet at the quench front. If you look at any of code calculations, liquid verocity is especially in quasi-static wiggle. So all these lengths did crazy things. Your wall heat transfer just went nuts.

DR. BANERJEE: I remember that RELAP5 was giving big problems in the low pressure reflood of the AP600.

MR. KELLY: We actually didn't do reflood framing. It was just sitting still, yes.

DR. BANERJEE: Yes. Sitting still it was having problems. How did you get rid of that?

MR. KELLY: Well, there were a number of problems with relapse from the AP600. The worst ones were momentum fluxes that were referred to earlier. That was basically just the way the numerical scheme tried to do momentum fluxes.

DR. BANERJEE: That doesn't happen in TRAC.

MR. KELLY: We haven't seen those kinds of momentum flux loops, no. But if you get to low enough pressure, yes things are going to oscillate. As we do correlation development on model replacement by selection of the models, it always will help if you have a thought as to what's going to work well in the numerical framework.

I'll give you one quick example. It's the CHAN nuclear boiling correlation in which there was much -- were used. It does things in terms of the Martinelli-Nelson parameter which makes a lot of sense. If you're doing a steady state boiling experiment, you can always get the quality from an energy balance. That's very nice.

In a code calculation especially under almost zero flow conditions, what's the quality? It all of a sudden has no meaning. If you're a stratified layer and you have vapor above it, what are the qualities? You have the liquid come up and the quality is almost zero. So if you have X over 1 minus X and those are varying between zero and one, you've made a great amplifier of noise.

But if on the other hand you can do that as a function of void fraction, then you're much better off because void fraction takes some amount of time to change. That's one of the reasons for doing this work. Instead of having a static flow regime map where you can cross a regime boundary, a J_E J_F plot, instead you're solving a transport equation for the interfacial area.

If you go from one regime to another, you have to evolve in either time or space. So that's part of the rationale for doing this. It's to try to work to get rid of some of the unphysical oscillations.

DR. SCHROCK: This interfacial area of transport seems to have gotten a pretty firm hold. It seems to me without a clear consensus in the technical community that it has a fundamental basis. I don't know a fundamental basis for it. Are you at all concerned that you're getting into something built into the code that won't survive or are you confident it will survive?

MR. KELLY: Well, you'll notice that I refer to it as an exploratory research program. I have it down to the 2005-2007 timeframe. The reasons for that are that it needs to mature more. If you look at some of the two phased CFD work that's going on, they use something equivalent to interfacial area transport.

What you mean is that in order to get an interfacial area like bubble sizes, you model the processes that destroy and create bubbles. If you model those sub-processes whether it's turbulent break up for bubbles, Webber (PH) number driven break up, or more importantly for us bubble coalescence, larger bubbles over taking smaller bubbles, et cetera, if you could model those processes well enough, then you could model the physics behind what causes the flow regime changes.

At present and Jennifer Uhle was here probably a year or so ago and showed a calculation in TRAC-M. It's a side version. She had implemented it. It handled bubbly -- transition very well for those particular conditions. But to go from something more like slug to churn turbulent to annular, we're not there yet. Those are just the normal flow regimes in a vertical pipe.

You have to consider a rod bundle. You have to consider the hardware that's in a type of reactor. What happens when you go around a corner to the interface? There's a lot more work that has to be done before we could consider it.

MR. ROSENTHOL: Can I try to follow up? At the beginning of the meeting Steve Bajorek said in a discussion with Dr. Wallis, we went from these overview presentations into more of an indepth mode with the Subcommittee. So we tried to do that with subcool boiling today. In the fall, Steve will touch on it more.

We'd like to spend a fair amount of time on rod bundle heat transfer. In fact, it would be good to have a meeting there. Then maybe six months from now or so we would get Ishii and company in. Then we could take a good day to go over this thing. That would probably be a better way to answer your question.

CHAIRMAN WALLIS: Yes.

DR. SCHROCK: Well, I'd just like to

mention that my concern at the outset of this research program which I guess is in it's second five year period or something like that was that there wasn't a sufficiently broad view taken on what the root cause problems are in these codes. Specifically, I talked about the fact that structure the code has the premise that you have flow regimes that can characterize in some relatively simple way and that they have abrupt starts and stops or you use some numerical blending of them to bridge transitions.

In any case, the physics of the transition in flow regimes is simply absent there. I think that's one of the ongoing difficulties that the code has. I think in the OSU problem that's very clear. That is at the root of the problem.

What I'm concerned about it that we go on and on and on with the same approach to doing it, and we're putting more band-aids on difficult aspects of the problem. But we're not really getting at the fact that the structure of the code itself ought to be re-examined in the sense that you have this flow regime dependance. Then the flow regime map itself is overly simplistic. I think there is a major problem there, and it's not getting any attention yet at the research level that I see.

CHAIRMAN WALLIS: I think by now we've read the summary slide.

MR. KELLY: Okay.

CHAIRMAN WALLIS: So what I'm really looking forward to is seeing that this works.

MR. KELLY: And all through the next year of finding out where it doesn't work.

CHAIRMAN WALLIS: That's right.

MR. KELLY: Professor Schrock's comments are --

MEMBER RANSOM: There are a couple comments. One is along with Virgil's comment. Really a pilot code demonstration of this interfacial area transport modeling would be quite helpful I would think in trying to decide what potential it offers. I think that would be a good first step to take. It does offer some benefits I believe. Although it will also introduce some new problems.

The other comment that I had in attempting to write a paper on uncertainty in code calculations with regard to a risk-informed regulation has to do with common failures. One of the advantages that the NRC has had over the years although some people might argue a disadvantage is they had more than one code development. As a result of that they were actually able to discover things that are wrong with one of the versions of the code.

One thing that you seem to be headed for is one single giant code which of course could have an error in it which would mean that all your calculations are in error. So I think there does need to be some thought put into what is the check and balance system here to avoid that sort of problem.

MR. KELLY: Obviously we're doing this for resource reasons. We have to make the best use of our resources that we can. One good code would be better than two poor codes. But we have to make it a good code.

Now, what we can do for a check is there is one other large ongoing code development in the reactor safety area. It's the Katar (PH) Code in France. We have the right to use that code. It's just that we'll have to have users trained to use that as well. So we could do our code comparisons against that.

DR. BANERJEE: Another thing. I agree completely about the interfacial area business. I think it would be worth bringing that meeting up as much as possible because the flow interfacial area is a vector. Even in a 1-D code, there's area normal to the flow, area parallel to the flow.

Secondly, the interfacial area for momentum transfer is not the same as interfacial area for scaler transfer in some sense as we chemical engineers know extremely well. They're not even the same thing almost because it depends on the renewal frequencies. So before we go ten years down the road with this, it would be good to have this peer reviewed or at least reviewed by this Committee as quickly as possible.

MR. KELLY: I agree.

DR. BANERJEE: It's already five years down the road.

MR. KELLY: Most of what's being done to date is gather experimental data that can be used one way or another to improve whatever kind of model. Even if we went that way, we would still gain something.

DR. BANERJEE: Right. But even then there's a question of what is that data. Data, for example, if you were looking at heat and mass transfer, there's a different type of data than if you were just looking at momentum transfer. This is actually a point that we can discuss in great depth. It's been going on for a long time, this business.

MR. KELLY: Yes.

MR. ROSENTHOL: Why don't we try to do that this winter?

CHAIRMAN WALLIS: We are working through these programs. I would expect to see the -- first. Is that right?

MR. ROSENTHOL: Yes. And I'd like you to see the --

CHAIRMAN WALLIS: We haven't seen them for a long time. We also haven't seen Ishii for a long time.

DR. BAJOREK: We would anticipate next month looking at the subcool boiling, spending a day. I'll talk about the rod bundle. Maybe we could spend a day on that in October or November. Then follow it with Ishii in maybe January.

MR. ODAR: I'm Frank Odar. I heard that you have a favorite topic of discussion, TRAC documentation.

CHAIRMAN WALLIS: If I'm not mistaken, I think about 40 years or so ago we were both in Malibu, New Hampshire.

MR. ODAR: Oh, yes. We were. It's nice to see you again. I'm going to address the status of the documentation first, what we have now, and also the future plans.

This is the list of the documentation we have applicable to TRAC-M, not TRAC-P, not TRAC-B, but TRAC-M. That's the modernized version. The first two documents address version 3.0 where the modernization effort was completed and the code was converted to Fortran 90. The Theory Manual and that NUREG/CR document represents the old correlations and old methods. So it's nothing from that point of view it's not really new. But what is new is that the code has been modernized.

DR. SCHROCK: Is this a part of that set of CDs that we received?

MR. ODAR: Right. You received that CD.

DR. SCHROCK: I did spend many hours trying to dig threw it. But it's first of all difficult to do that on the screen.

MR. ODAR: Right.

DR. SCHROCK: You can't look back and forth or at least I can't.

MR. ODAR: I can't either.

DR. SCHROCK: I found sections of what was listed in the contents were not present on the CD that I got. Is it incomplete?

MR. ODAR: I better send you a hard copy.

DR. SCHROCK: Specifically I was looking for the models of correlations. I couldn't find them on the CD.

MR. ODAR: They were presented in different sections.

MEMBER RANSOM: Dr. Schrock, along that line, you didn't put any bookmarks on those CDs, so they're extremely difficult to use. The pages are numbers sequentially on the CD but in the table of contents of course you have them numbered by section. So there's no way from a bookmark or an index to find what exists on the CD. I would almost refuse to read that without that. I think you have to correct that.

MR. ODAR: All right. I will do that. I personally read the hard copy because of the changes on the screen.

MEMBER RANSOM: The problem with hard copies is it's a stack of paper.

MR. ODAR: It's about four inches thick. We do have the User's Manual corresponding to that. Actually the version 3.0 remember that it's applicable for PWRs. So therefore, it's almost like the old TRAC is more modernized.

The third manual, the Developmental Assessment Manual is about halfway modernized. It's the F77 code. Before we did the F90 conversion and the complete modernization first we made the step of F77. That means removal of some -- that uses -- So the code became almost platform independent. That shows the developmental assessment of the code. We think that the results are going to be the same as for the Fortran 90 version.

We do have also a Programmer's Manual for the same version which is version 3.0 that the programmers can use. The other three documents there really pertain to a little bit later versions. One of the documents that we prepared is an Assessment of Modernization and Integration of BWR Components and Spacial Kinetics in TRAC-M. That's a much later version. It's version 3690.

Surprisingly certain we have found quite a bit of errors in that assessment work. But those errors were found and corrected. It turns out that the modernization effort was successful because when we compared a good number of tests next to each other the results were quite accurate.

We repeated also the developmental assessment cases. There's the third report. They were also reasonable. By that I mean, the results that we would obtain from the assessment of version

5.5 were about the same as you would obtain from version 3690. So we have done I think -- story and we've also shown an inter application of spacial kinetics for the BWRKs. That turned out to be quite reasonable too. The bottom tests were connected. We have them in combination of all those two or three things.

The next document is the assessment document which is reflood. Assessment of the reflood model is used today in TRAC. We found that quite inadequate. It was expelling too much water. One of the main reasons that I found was that flow regime problems were not applicable. Because the problem was expelling more water than needed, the results were conservative.

The last document is the quality assurance document which we are applying to NRC Thermal Hydraulic Codes. It shows the type of documentation needed and also the type of review and independent assessment that's needed basically.

MEMBER RANSOM: I have one other comment. About a year ago I received a number of these in the mail while I happened to be working in a company that was doing some work on TRAC-M. I ran into the developers and said I got a manual on TRAC-M. They said that's not TRAC-M. It's TRAC-P. In other words, all of this documentation is about past versions of the code. It didn't seem to be current with what they're working on. Do you have any comment on that?

MR. ODAR: Yes. TRAC-M in the sense that it's the early TRAC-M versions require that the modernization is completed and for our qualities to change to convert it to Fortran 90 -- independent. So these documents you have benchmarked that -- version. I think it's true -- some of that information from the other. Particularly on the Theory Manual you can get similar information from older documents.

MEMBER RANSOM: Well, in these CDs that we have, will we eventually be getting current information on what the current TRAC-M formulation is or is it the older TRAC-P?

MR. ODAR: You will get the most current information. But remember that we don't intend to change the physics too much in the near term. Therefore, the correlations are going to be about the same. For example the facial shear model, that could be different. But the rest of the correlations would remain the same until further improvements are made.

MEMBER RANSOM: Let me ask the Chairman. For this next meeting, we will be reviewing these or will be reviewing the current formulations?

CHAIRMAN WALLIS: I don't know. You have to ask the Staff.

MR. ODAR: The next slide shows the planned documentation for different versions which are forthcoming. The alpha version of the code will come out at the end of the year. We need to produce a new User Guideline because we have lots of BWR components added. There is also interface with the parts code which is a spacial kinetics code. The User Guidelines changed quite a bit. The Theory Manual will be expanded to include again the BWR components. Some of the framework may not be in at that time.

Next spring we'll have expanded user guidelines including the -- capability. The Theory Manual which is probably close to draft one and draft two whatever the improvements that are made during those three months will be added.

CHAIRMAN WALLIS: Is this Theory Manual written for external consumption? Let's say a flow mechanisist from Cambridge University who knows nothing about nuclear reactors but knows a great deal about flow mechanics. Would he pick it up and read it and believe it as a good professional document? Is that the audience or is it the audience that's already to imbued with the assumptions and short cuts and methods which have been used in this field in the past?

MR. ODAR: This is a difficult question to answer. But I can tell the truth. The audience is the TRAC developers.

CHAIRMAN WALLIS: That's the problem. It's this inbreeding.

MR. ODAR: That's the problem. And we realize it. But there are lots of shortcuts written in TRACese.

CHAIRMAN WALLIS: One thing that the ACRS has tried to get across is that there's a public out there and there's a professional public. If everything is written in TRACese, then you have a hard time convincing an independent professional public who are really quite knowledgeable that this whole thing is a good structure.

MR. ODAR: I agree. We have changed the structure quite a bit. It has come a long way from the old TRACese documentation. But I realize that we do have much more work to do to explain the fundamentals, the physics of the correlations that are used. Hopefully, we'll provide it at the very end in 2003 when all development work is completed.

CHAIRMAN WALLIS: Well, let's look at the correlation. How deeply do you go into the correlation? Do you just say this is a correlation from so-and-so or do you look at the database behind the correlation and the range of parameters of which it should be valid and compare that with range of parameters in the applications? Is this part of the documentation?

MR. ODAR: This is partially adhered to, but not all correlations have the same basis, in other words, the same kind of analysis of a particular applicable of the range and validity. Do you remember these correlations were selected about 20 years ago?

CHAIRMAN WALLIS: I believe so.

MR. ODAR: You realize that there ought to be some improvements in selection correlation. I guess what I’m saying is as the time goes on we intend to make the improvements and the -- document really spells out what kind of detail we need in this documentation. That includes full interim equations applicable for the studies, scaleability studies, and everything should be included in the reactor analysis.

MEMBER RANSOM: Along this line, are there any peer review efforts that are ongoing in the TRAC-M development? One example would be years ago under

Favic and Tong, they put together a blue ribbon committee that I think met once or twice a year. They really took the developers to task in terms of what they were doing.

MR. ODAR: Right.

MEMBER RANSOM: Actually a lot of benefit came out of that. I think some kind of peer review process really would be beneficial unless we are the peer review.

MR. ODAR: You are all a part of the peer review.

DR. BANERJEE: Some of us belong to other committees.

MR. ODAR: In the process, we do have extensive peer review.

MEMBER RANSOM: You do?

MR. ODAR: Extensive peer review at every stage on the documentation. The documentation includes a requirements document --

MEMBER RANSOM: I'm thinking more of the development. You may be the wrong person to ask this. I think Joe Kelly would be the better one.

MR. ODAR: -- which is development. The final document is a development document. Because all of the engineering equations applicable to the capability equations ought to be answered in the requirements documents. I could tell you what is to be coded. All limitations ought to be right there. We're going to apply that. This is much later of course. We have a long ways to catch up.

CHAIRMAN WALLIS: I suspect then there are still areas where you take formulation of a momentum equation for a junction or something. There's still some question about how valid the formulation is.

MR. ODAR: That's true.

CHAIRMAN WALLIS: The last thing you want is to go through this great big structure and come to ACRS and we say we don't believe equation 1196.

MR. ODAR: Well, I think there have always been questions equations.

CHAIRMAN WALLIS: That's because no one is taking the time to work it out properly.

MR. ODAR: Well, it is also a very difficult question. It's a combination of a momentum equation and the mechanical --

(Discussion away from the microphones.)

DR. BANERJEE: One is a vector and one is a scaler.

CHAIRMAN WALLIS: That's right. We've seen that before.

MR. ODAR: Well, I think you know it's true.

CHAIRMAN WALLIS: Yes. I've seen some of that.

MR. ROSENTHOL: Steve tells me that his presentation is about five minutes.

CHAIRMAN WALLIS: We better go on because we're going to be here all night.

MR. BOEHNERT: Just as a response to Virgil. Take a look at that first list that Frank provided. There are NUREG/CR numbers. If you guys want paper copies, let me know and I'll work with research to get the paper copies of those documents if you need them.

MEMBER RANSOM: Are they working on CDS with bookmarks?

MR. ROSENTHOL: I can't answer that. Now we are.

DR. SCHROCK: I'd like for us to look at what he has there, not make us discover. Was it just my error in finding those correlations or were they really not there?

MR. ODAR: Well, it's not --

DR. SCHROCK: I'll just leave you with that thought.

MR. ODAR: It's not a separate --

DR. SCHROCK: I don't want to discover it again myself.

MR. ODAR: It was supposed to be including that.

DR. BAJOREK: Okay. I will be brief because I know we're over the time allotted here. There's a package coming around. You can refer to it and see some of these for yourself.

We heard the comment earlier that we really want to see some real results. With the rod bundle heat transfer program, we've been waiting for this as well for the last couple of years. In previous times when we've talked to you about the rod bundle program, we would say it's still being put together. It's still in pieces in the lab.

Last month Joe Kelly, Gene Rhee and myself went up to Penn State to witness one of the first tests that had been done. Our purpose was to take a look at the facility, review the initial results, and try to just make an overall estimate as whether the data that we're seeing is consistent with each other, whether there are any problems in the measurements, and whether the data appears to be consistent with what we had requested.

The RBHT just forgot to have an overview for the facility itself. It's a full height bundle. It's a seven by seven assembly of electrically heated rods. The difference between it and previous reflood tests is that this has a complete set of instrumentation, meaning when we want to look at grid spacer effect. FEBA did a nice job of taking a look at that.

In the rod bundle heat transfer tests, there will be thermocouples on the grids. There will be fluid temperatures. There is a very detailed array of DP cells. There are thermocouple regs by which to get the steam temperatures. So we can get the whole package of information on reflood and not have to sift through FLECHT and FLECHT-SEASET and FEBA and G2 to get bits and pieces.

In the next several figures, it shows some of the examples of the data that's coming out. I just want to make a few comments. One is it appears to us that the results are consistent. When we look at what's coming out of the test results and compare it to what we would expect from FLECHT and previous tests, we're seeing those things. It's a relatively long transient for in this case a one inch per second test. That's good because this is intended to help us with the model development.

We looked at the steam probe behaviors. The steam probes were a concern in FLECHT because they quenched up at around 900 degrees Fahrenheit which would put us up in here. (Indicating.) In the Penn State bundle the traversing probes are giving meaningful measurements to at least 600 degrees and in some cases lower than that. It means we can get meaningful vapor temperatures relatively close to the quench front where we didn't always have that previously.

A couple of comments on the results. You can see it in this figure where it shows an axial temperature distribution. There are also steam temperatures and grid temperatures. The grids are a first order effect. They truly dominate what is going on in the bundle. That's somewhat expected in a Y in the facility make up there are windows up and down this facility so that they can use a laser camera and digitally image the droplet field. They focus this on the inside of the bundle.

We found and it's amazing that you could run a reflood test and within minutes of completing the test, you get a droplet distribution. Now, the test matrix moves the camera around in some cases so that we see the effects above the grid and below the grid. We're getting droplet information and a distribution that makes sense. The size tends to match up.

DR. BANERJEE: These even look normal.

DR. BAJOREK: Yes.

DR. BANERJEE: Amazing.

DR. BAJOREK: And the size is very typical of what we had expected from previous experiments. This is a bit confusing but the software that goes along with the camera will give you those distributions as a function of time. Now, in this case, there are enough droplets in this particular test to get a nice smooth curve. This gives us a way to look at different periods of the test and seeing how potentially the droplet distribution may change as the quench front moves in the bundle.

The traversing steam probes. First by way of the bundle itself it's a relatively uniform planer rod for a temperature profile, meaning the housing is not having a very strong effect on the interior rods as we would hope. We are seeing with the steam probes a gradient in the steam profile as we move from the center of the bundle closer to the housing.

We're also able to pick up what I refer to as the subchannel effects where you see higher temperatures when you have the steam probe immediately between two rods as opposed to somewhere out further in the subchannel of the flow. So we're getting what we think is a fairly detailed picture of the vapor temperature distribution across the bundle.

DR. SCHROCK: I don't understand. If this is on the distribution and time, how do you see that?

DR. BAJOREK: This shows two different tests. The only difference is where the traversing steam probes were positioned. This one was in the subchannel, the middle of four rods. For this upper curve, the steam probe was immediately between two rods. So we're able to see the difference in steam temperatures in where the bulk mixing part of the fluid is versus where it is between the rods. As we go from the center of the bundle out to those rows closer to the housing, we would see this pair drop in temperature as you would expect.

MEMBER RANSOM: Are those the droplets hitting the probe?

DR. BAJOREK: Probably.

MEMBER RANSOM: A shoot up in temperature and then down. Although that's kind of a long time.

DR. BAJOREK: That's a fairly long time

and there's a lot of droplets.

MEMBER RANSOM: What explains the scilitory (PH) (PH) nature of the temperature measurements?

DR. BAJOREK: There are some oscillations in the -- itself.

MEMBER RANSOM: Pardon?

DR. BANERJEE: It's quite regular.

MEMBER RANSOM: Are they slugs or drops? I mean, they're actually bubbles I guess because they're higher temperatures so it must be steam and then go down to the liquid temperature.

DR. BAJOREK: These are bare thermocouples so they do occasionally get wet. There's a lot of liquid. I think when they do get wet there is a time period by which it takes to --

MEMBER RANSOM: Just one quick clarification. You have percent numbers on the droplets. Are they all normalized to 100 percent?

DR. BAJOREK: They are eventually, yes.

MEMBER RANSOM: Okay.

DR. BAJOREK: I guess our point at this point testing is moving along. They've been able to run on the order of seven or eight valid tests at this point. We're taking a look at the data as it's being produced. We're using this to modify the test matrix so these things make sense. The long run moving the camera around to positions of interest.

They're going to do the initial phase of reflood tests consisting of a group of 33. They should have those done in about September. Our proposal to the Committee is that potentially October, maybe November would be an appropriate time to spend a day looking at the reflood test program where we could go through in detail and show you the bundle, show you the results, show you the trends. We probably won't be at the point of developing models at that point, but explaining what's going on.

We think it would be worth having that meeting at Penn State so you could see a test, look at the instrumentation, see how it's done, and look at the facility that's been put together. It's an impressive facility and I think represents a very strong commitment on the part of research to continue advanced model development.

DR. SCHROCK: Could we have the benefit of some documentation on the instrumentation in advance of that meeting?

DR. BAJOREK: Yes.

DR. SCHROCK: I think that would be

helpful.

DR. BAJOREK: It's not bookmarked.

MR. ROSENTHOL: We'll give you what we have.

DR. BAJOREK: This is all on paper at this point.

DR. SCHROCK: We've seen some. But I don't know that it's enough.

CHAIRMAN WALLIS: This is near Philadelphia?

DR. BAJOREK: State College, Pennsylvania.

CHAIRMAN WALLIS: Where's State College? It's out in the boonies somewhere?

DR. BAJOREK: Yes. It's across the state. It's easier to get to than New Hampshire.

(Laughter.)

MEMBER RANSOM: Quick question. Is somebody thinking about how you're going to use this information to improve the TRAC code?

DR. BAJOREK: Yes. Really the entire test matrix which was designed by Joe Kelly is set up in such a way that we get information to develop mechanistic models for dispersed or off the heat transfer, interfacial drag, and inverted annular flow; those areas of the code where we have particular question marks.

MEMBER RANSOM: Precursory cooling out of that?

DR. BAJOREK: In what way?

MEMBER RANSOM: If you have reliable models that calculating precursory cooling for the bundles, I think that's what you had earlier.

DR. BAJOREK: Do we have them now?

MEMBER RANSOM: You will have them.

DR. BAJOREK: We will have them. We need to have them, yes.

CHAIRMAN WALLIS: So, thank you.

MEMBER RANSOM: I'm still puzzled. It's not a reasonable question.

DR. BAJOREK: What? I'm not sure whether your question is whether we have good precursory cooling models now or that's our intention to use this data to develop them.

MEMBER RANSOM: Are there new models of precursory cooling which I think has been a problem for these reflow reductions?

DR. BAJOREK: We would hope to be able to develop them out of this data because we're going to have a much better handle on the development of axial steam temperatures.

MEMBER RANSOM: That's basically part of Joe Kelly's contribution then.

DR. BAJOREK: Yes.

MEMBER RANSOM: Okay.

CHAIRMAN WALLIS: Anything else?

DR. BAJOREK: Okay. Thank you.

CHAIRMAN WALLIS: Thank you. Can we move on before we take a break? Then we'll take a break somewhere after the next presentation or maybe after Marino diMarzo's presentation depending on how things go. We are changing gears completely.

(Discussion away from the microphones.)

CHAIRMAN WALLIS: This is a new play all together.

MR. SCOTT: Yes. In fact you probably thought about alpha all morning, the void fraction creation. This afternoon we're going to be thinking about beta, the reactivity parameter.

I'm going to start off here and give you an overview of the Generic Safety Issue. Then next Dr. DiMarzo who is actually a part time NRC employee in addition to being from Maryland is going to talk a little bit about thermal-hydraulics. Then it sounded like you wanted to take a break. Then Dr. Diamond has a longer presentation in which they've used the PARCS code to make some calculations.

This slide is just for the benefit of people that are going to read a transcript later. So let me put up a diagram of a steam generator coolant and just talk a little bit. This event starts with a small break LOCA. For example, a two inch break is about the right size. That's about 20 square centimeters.

CHAIRMAN WALLIS: Does it matter where it is?

MR. SCOTT: It doesn't seem to. Well, it may matter where it is. Some scenarios would not result in boron dilution and some would. We didn't look into exactly which scenarios resulted in this.

MEMBER RANSOM: It looks like it would be pretty important whether liquid is leaving out the break or steam is leaving out the break. If it's steam leaving, no boron is leaving. If liquid is leaving, boron is leaving.

MR. SCOTT: Well, but the steam has to go over the candy cane and then condense here to put unborated water down where this green is. (Indicating.)

MEMBER RANSOM: No. The question is what leaves the system? Where is the break?

MR. SCOTT: I guess I don't know for sure in which calculation that BNW did where the break was.

MEMBER RANSOM: I know in the documentation I couldn't find out where the break was.

MR. SCOTT: Well, we don't say exactly where the break is.

MEMBER RANSOM: What do they assume, the liquid leaves or paper leaves? The importance of this question is you're trying to figure out if boron is leaving the system I think.

MR. SCOTT: No. What we're trying to find out --

MEMBER RANSOM: As well as dilution. I understand that.

MR. SCOTT: How much water that does not have boron in it can accumulate in the steam generator.

CHAIRMAN WALLIS: This thing is looking like a still.

MEMBER RANSOM: Yes. My point is that the boron inventory is also important if it all stays in the core.

DR. DIMARZO: Right. The scenario which is depicted here is a situation that occurs late in the transient where basically the inventor is being reduced.

MEMBER RANSOM: By liquid draining out.

DR. DIAMOND: By liquid draining out. You have lost most of the liquid at this point. All you have is liquid in the core because you have to clear the hot leg in order to have an installation process if you wish or -- at BCN type process. So it is necessary for that hot leg to be substantially empty.

If you have 20 percent collapse liquid leveling in the hot leg, you are bound to have now and then presumption of natural circulation. That would basically foul up the deborate water that you have accumulated because it would put borated water on top of it and it's mixing. So it's a very tight scenario in order to generate a slug of this magnitude.

That inventory cannot be too high otherwise you start getting two-phase natural circulation. It doesn't have to be too low otherwise your transitions is a severe accident. That's a very narrow bend. It has to be maintained long enough, that interval of --

MEMBER RANSOM: So you gather the worst case is a small break, like two inches in the liquid at some point. So you're losing boron as well as liquid.

DR. DIMARZO: You have some HBI capability.

MR. SCOTT: And it also has to be early in the core life because at the end of it's cycle the boron concentration normally is down.

MEMBER RANSOM: Right.

MR. SCOTT: So therefore if you could get a little bit of boron in, you'll be okay. Dr. Schrock, ten years ago when we did the CSAU with RELAP, I don't think this issue ever came up. We did assume only one HBI pump. We did BCN phase. I don't recall that there was any discussion. A little bit later I'll mention some of these scenarios and say about what time people brought them up if you want to talk about that.

At some point the natural circulation had stopped. You developed this as I showed unborated water in the tubes and in these legs. Just before the circulation is done, this may move up and then come back down. (Indicating.) We're assuming that the pumps don't start. Once you've refilled the system, the natural circulation starts again and now this slug of unborated water moves into the core. That's the assumption. You also get this same scenario in a steam generator plant. I don't know if I have a figure of that or not.

Also I should point out that the vent valves in the BNW reactor as long as there's no natural circulation flow that they are doing some mixing. There is some mixing going on in the core itself.

DR. BANERJEE: Are you going to show us a little diagram where there's vent valves?

MR. SCOTT: I don't have one with me. I can dig one out later.

DR. BANERJEE: Yes. It would be useful because there's a lot of appeal to the vent valves here.

MR. SCOTT: And as I recall from the write up it depends where the level is. If the level is up either at the vent valve level or higher, they may be more or less affected. But this was one of the things that BNW did later on to show that the transient is more benign as they get more effectiveness from the vent valves.

DR. BANERJEE: So it would be worth seeing the geometry of this.

MR. ROSENTHOL: But it isn't quintessential to the larger that we will be explaining to hopefully resolve the issue.

DR. BANERJEE: Right.

DR. DIMARZO: There's a lot of stuff in the BNW report that we don't pretty much subscribe as you'll see later on.

MR. SCOTT: Okay. I'm just going to summarize here what Brookhaven has found now with several calculations. We did go above prompt-critical. We did reach its power of 80 percent. I think the only thing that we changed was 37 calories per gram. So the total was about 50. BNW in their report calculated about 90.

That's part of the reason this scenario came onto the scene. Harold Vander Molen was asked to prioritize it because 90 or 100 calorie per gram is in the range where we think now about fuel damage for irradiated fuel. With this level of 37 calories per gram, we do not expect any fuel damage.

DR. SCHROCK: You don't give any information on the length of time this reactivity gain required.

MR. SCOTT: Dr. Diamond will cover all that.

CHAIRMAN WALLIS: It doesn't last very long at all.

MR. SCOTT: It's five or ten seconds. I'm sorry. The pulse itself?

DR. SCHROCK: The rise in reactivity to $1.02 happens over some period of time during which a lot of negative feedback occurs as well.

MR. SCOTT: Yes. But that would be real short.

DR. SCHROCK: But one of the things as I read the material sent to us, it occurred to me that there's never mentioned that maybe something like the SL1 accident could occur in a BNW system to this boron dilution. If the reactivity could be inserted rapidly enough, I don't think it could. You know what I'm referring to.

MR. SCOTT: Yes.

DR. SCHROCK: SL1 blew its lid essentially because it was half full when it received a prompt reactivity dose in a matter milliseconds.

MR. SCOTT: There is another aspect of the scenario which is you run the pumps. You pump the primary pumps.

DR. SCHROCK: That's what I'm getting at. When you use that --

MR. SCOTT: At which point --

(Inaudible.)

DR. SCHROCK: Reactivity insertion passed enough to give you that kind of phenomenon. If so, it should be considered.

DR. DIMARZO: Okay. The history of this particular issue. The BNW owners group essentially claim that pump restart would be a problem. So they decided to take the pump out. That's fuzzy at this point. But the idea would be that if you enter BCM or if you have the probability of generating such a slug your variant would be prevented from turning the pump on. That's where they are.

That's okay. But we are not happy with just leaving it at that. So we have concocted a slug that would be pumped. We passed this information to Brookhaven. They are going to run that calculation just to see what that would entail.

DR. SCHROCK: That's something to be done in the future.

DR. DIMARZO: That's something to be done between now and September.

DR. SCHROCK: So would it be looking at the possibility of an SL1 type?

DR. DIAMOND: It's still in the orders of magnitude slower now.

DR. SCHROCK: I think it's too slow.

DR. DIAMOND: Right. We're in a different regime here.

MR. SCOTT: I'm going to now mention some other scenarios just to give you some perspective. You may have heard these before or not. Let me just this put up. (Indicating.) Let me say this is a 22, the first one up here. The second one is the french. Let's say this one here is the french. This one is the GSI-185. Let's say this is the Swedish one here. (Indicating.) This is just pictorially.

What I can do now if you want to or maybe you want to save time, I can go down and describe a little bit about these. Would you like me to skip that?

CHAIRMAN WALLIS: Do they help us understand GSI-185?

MR. SCOTT: Not particularly.

CHAIRMAN WALLIS: Maybe we should just move right on then.

MR. SCOTT: All of them result in unborated water which eventually goes into the --

CHAIRMAN WALLIS: But we're trying to resolve GSI-185.

CHAIRMAN WALLIS: Right. Let me now jump into the process that we used in trying to do this. This little diamond here is acceptable. (Indicating.)

If we assume and calculate some ex-vessel mixing, do the neutronics, if we get a small pulse, no fuel damage, then we can show issue closure. We expect to go down this path. We've been going down this path.

If in fact at this point you got a high value of fuel enthalpy or fuel high temperatures then you could proceed over here and say let's see if we can get more mixing in the vessel between the down cover inlet and the core inlet which would make the pulse be broader or slower. Then you could do some calculations. Other people are doing this in Europe. Then you could come around here and get this one. (Indicating.)

Now I'm going to put up a graph that's from the report that you saw. When Dr. Diamond gets up here, he'll give you more details about this scenario and some other scenarios. Here's one note that I had. When Dr. Vander Molen did the prioritization he got two times ten to the minus five per reactor year for this GSI-185. That's the level that we're talking about as why it was considered to be worth further study.

CHAIRMAN WALLIS: Now, you're not showing here the reactor power.

MR. SCOTT: No. This is the reactivity

and the boron concentration.

CHAIRMAN WALLIS: The reactor power is a more dramatic figure presumably.

MR. SCOTT: So in this case now this blue line and on your handout it's black and white but it has circles is an input guide to the code. The code doesn't calculate that. We didn't take RELAP and calculate the whole scenario. We just have a core that has the PARCS and the kinetics and you drive it with the RELAP boundary conditions. Correct?

DR. DIAMOND: Sort of. Something like that.

MR. SCOTT: Okay.

DR. SCHROCK: So it's a burst of boiling that causes that precipitous drop in reactivity.

MR. SCOTT: This would be the Doppler comes on and takes you down. You still have positive reactivity so you can get another pulse. At this point I guess you're getting heating and it can take you down. You'll describe this.

DR. DIAMOND: Yes.

CHAIRMAN WALLIS: Boron concentration doesn't look like a slug. You're saying this is some kind of an average or something.

MR. SCOTT: Well, it's going to diffuse as it starts in because it’s mixed as it starts out before the pump. It has to come in through the pump, come down a leg, come down again. We didn’t assume any further mixing in the down-comer. So it doesn’t have a sharp edge on it and in the tail of it.

DR. BANERJEE: Why is it mixed in the pump?

DR. DIMARZO: Let me go and explain what’s going on here. This particular curve I think is --

MR. SCOTT: Very benign.

DR. DIMARZO: Benign and is the original claimed curve by the owners group. This was based on some -- mixing that was happening between the steam generator and the core inlet.

DR. BANERJEE: The by-process.

DR. DIMARZO: No. Basically the slug was coming from a steam generator, coming out the cold leg, going through the pump, and then flowing into the down-comer, mixing in the down-comer, mixing in the low head and then entering the core. What you get there is the core. So initially the slug was characterized as pretty shot. By the time you went through all these geometries, it was pretty diffused. That’s what they claim.

When you see the slide of the approach

that we have taken, we have taken no credit for any mixing happening into the down-comer or into the lower head of the vessel which is a pretty substantial amount of mixing. But the problem is that in order to know how much that is you would have to do a full blown calculation and scale it to plant. That's a route that we did not decide to take. What we did was just simply consider the movement of the slug from the steam generator to the entrance of the down-comer which basically involved the steam generator, the cold leg, the pump, and the remaining part of the cold leg to the vessel.

MR. SCOTT: You also have the borated ECCSs coming in and mixing with that as it flows.

DR. DIMARZO: Yes. But we didn't take credit for that either.

MR. SCOTT: Also we should say that this curve assumes that it's symmetric at the core inlet. In other words, the left half of the core is exactly the same as the right half of the core. Some of these experiments have shown that if you're just getting one LOOP to start up that has the unborated water it wouldn't be symmetrical. But we didn't try to make any assumptions about that.

The velocity into the core is around two percent of rated core flow. The size of this slug is 1000 cubic feet or about 28 cubic meters. We think that there are several scenarios where you may have less cubic meters than some other scenarios. So you can figure out here knowing the densities and the volumes it runs for about 100 seconds given these velocities. As I said, Dr. Diamond will tell you some more about the intricacies about the red curve.

Okay. Now I think I'm going to go to my last slide. Closing the issue. Let me say first a few words about my personal bullet here. It's additional calculation that we want to do. As we said earlier, we've already assumed natural circulation. But we will do a calculation where we've assumed pump bumping.

This was considered in the prioritization report. So for completeness, we need to do both these calculations. But we did the one first just to see where we were at. If we get a large pulse, then we'll show that these emergency operating procedures that say leave the pump off, that should be continued. The other possibility is that even with this scenario the fuel enthalpy will be such that it could be interpreted as giving fuel damage no worse then that from the rod ejection accident. In that case if they make the mistake of starting the pumps, it wouldn't be the end of the world.

Warren Lyon from NRR is the person that's been following this issue for a number of years. In fact, this scenario and I skipped over that before was identified in '91 or '92. So the scenario has been around a long time. It just didn't come up for a relook until a couple of years ago.

If we're done here with these items, then in September we can come to the full committee. Then the process is that we prepare a closeout memo to EDO assuming that there's no action that we're going to recommend to NRR. Okay. Thank you.

CHAIRMAN WALLIS: I thought you were recommending for the work. Maybe I missed this.

MR. SCOTT: We're going to do one more calculation.

CHAIRMAN WALLIS: That's all. I thought it was more than that. Maybe I got the wrong impression from what I read.

MR. ROSENTHOL: If I could make a couple of comments. Dr. Wallis, you were absolutely right. We're trying to sell GSI-185 and not all boron dilution events. So your earlier comment was right on and very important.

In the course of preparing to go we thought to the full committee, now we decided to go to the subcommittee first, we were briefing our office director. He said what happens if they turn the pumps on. We said that's a different scenario. He said for completeness, you really ought to understand that one also with its commensurate frequency.

Then there was a discussion about how many other scenarios we should do. We said no, this is GSI-185. Boron dilution at cold shut down or something else is some other GSI. So it was only really the recognition and preparation from meeting with you that we recognize that for completeness we really ought to do the assumed operators don't follow their emergency procedures and turn the pumps on. That's the stuff of the additional work.

I want to make one other comment because I think that Marino would be too modest. That is that a lot of people around the world are doing a lot of thermal-hydraulic calculations looking at the distribution of boron water in the system as a function of time. He's the one who said wait a minute, let me come up with some sort of bounding slug, and let's take advantage of this new physics tool that we have to do 3-D space kinetics. If we can show that the results for the pessimistic slug are okay, then we don't have to get into all the detail.

So at the very same time we're trying to resolve this reasonable narrow generic issue. There are people around the world doing lots of fancy calcs which is good stuff but maybe more applicable to other scenarios.

CHAIRMAN WALLIS: Maybe I misread, I thought I read a tough action plan which calls for more experiments and OSU, those sorts of things.

MR. SCOTT: Let me tell you about that. Those other tasks in there all had a prerequisite that said if --

DR. DIMARZO: No. He's referring to a correct one. As you will see, I have two slides. It's not going to be much. But basically I have formulated a simple model to characterize the mixing ex-vessel, in other words, from the steam generator to the vessel.

Then I add some LOOP data, some Maryland data which were repeatable and reliable at least to me at that point. I used that to validate against. That's inbreeding. So at that particular point I said maybe we ought to run a couple of tests blind and check whether the same model is able to predicate that in a blind fashion. In other words, it would be me running the calculation data and they are doing the experiment and then match.

Unfortunately AP1000 came on the scene at that point. We lost the window of opportunity to run those tests in OSU. So the only validation we have is data from Maryland basically which were obtained three or four years before. That basically is the base for the validation on the model which I'll describe to you all if you want right away.

CHAIRMAN WALLIS: Maybe I just didn't spend enough time reading it. I got the impression that you folks were not closing the issue, but you were asking for more work. They've had a task action plan that specified all this work to be done. That is not the case. You're actually proposing to close the issue with what you know now.

DR. DIMARZO: Yes. If we put through the pump a slug and we don't get anything dramatic, there is no point in trying to finagle the thermal-hydraulic to get the same answer.

CHAIRMAN WALLIS: Okay. We never fanagle thermal-hydraulic.

(Laughter.)

DR. DIMARZO: Okay. What I'm talking

about here is reported in a paper that came out on engineering and design. I have a few copies of it.

MR. BOEHNERT: They got copies.

DR. DIMARZO: So the idea here is to characterize the mixing that occurs between the steam generator and the entrance of the down-comer. The geometry that you are looking at is the steam generator, the steam generator upper plenum, the cold leg in the suction portion leading to the pump, the cold leg in the discharged section. That's it.

So basically this is nothing strange. I took something that is old and very well known. I went to Levenspiel back there. I said there are two possibilities.

CHAIRMAN WALLIS: Other OSU work.

DR. DIMARZO: Yes. There are two possibilities here. Either we have volumes that are completely mixed or there are volumes that are completely unmixed. So either we go to a plug flow or we go to a backmixed flow.

CHAIRMAN WALLIS: I suspect that plug flow is the worst condition.

DR. DIMARZO: Plug flow is the worst condition.

DR. BANERJEE: You are saying some

components --

DR. DIMARZO: Some components will be one way. Some components will be the other way. The reason why I elected to do that is that if you do anything modeling wise more complex then the question becomes how much mixing do you allow in a component. That's subjected to scaling problem. I didn't want to touch that.

So I said we have two volumes which I believe are fully mixed. One is the pump because basically you have veins in there. Even at fixed flow you have enough turbulence generated that which would cause mixing in that volume. Then you have the steam generator of the plenum which is also subjected to mixing because you feed it from all the tubes which basically are like little jets in that particular volume.

I made the assumption that those two volumes were completely mixed and everything else was completely unmixed. It was just a transfer.

DR. BANERJEE: Including the down-comer.

DR. DIMARZO: The down-comer I didn't touch. This is fed directly to the core.

CHAIRMAN WALLIS: This is ex-vessel.

DR. DIMARZO: The ex-vessel is fed into

the core. There is no down-comer. There is no lower head which has a substantial amount of mixing. So it was a very conservative position. It wasn't a top hat type thing. But it was next to that, the most conservative thing that you could do.

CHAIRMAN WALLIS: Is it conservative to assume backmixed volumes say in the steam generator at that plan?

DR. DIMARZO: Right.

CHAIRMAN WALLIS: And maybe it's not perfectly --

DR. DIMARZO: Right. So I had the test in Maryland that was conceived like this. The slug was filling the steam generator, the steam generator upper plenum, and was somewhere in the leg filling to the pump. So when that slug moved, the front of the slug would go to the pump only and the back of the slug would go to the steam generator upper plenum and to the pump.

CHAIRMAN WALLIS: But it's already a pure water slug, so it doesn't really matter.

DR. DIMARZO: No. Two interfaces. In other words, in the middle I have this water which has two interfaces; the front and the back. The front goes through the pump only. The back has to go through the steam generator upper plenum and through the pump.

CHAIRMAN WALLIS: Because there's borated water following it. Is that right?

DR. DIMARZO: Right. In our case, it wasn't borated. It was a temperature type situation.

CHAIRMAN WALLIS: Okay.

DR. BANERJEE: But in this case the slug is just pure water with borated water in front. Do you have any at the back?

DR. DIMARZO: Yes. Again, you have the same situation that you have at the front at the back.

DR. BANERJEE: Can you put that diagram up?

CHAIRMAN WALLIS: How does it get to the back?

DR. DIMARZO: No. We constructed a slug which was based on temperature in the Maryland facility.

DR. BANERJEE: Yes. I know what you did.

DR. DIMARZO: Okay. So basically we had salt and temperature. So the temperature is the tracer. The salt is such that it enables you to keep stuff where you want it initially.

DR. BANERJEE: Yes. Just to clarify the

geometry.

DR. DIMARZO: Okay.

MR. SCOTT: I'm looking for the --

CHAIRMAN WALLIS: The back of the slide is much less important.

MEMBER RANSOM: The event's over by the back of the slide probably.

CHAIRMAN WALLIS: So you've put borated water on top of the green.

DR. DIMARZO: Okay. So in the Maryland test, the green stops right here at the beginning and you have water which is cold back up here again. (Indicating.)

CHAIRMAN WALLIS: But in the real thing you have borated water way up --

DR. DIMARZO: In this scenario when you go in natural circulation what happens is this, you reseal the system. All these things are moved up here. (Indicating.) When finally the water fills up completely, the system natural circulation can resume. In other words, your generate your slug and it looks like this. (Indicating.)

DR. BANERJEE: But in natural circulation or just --

DR. DIMARZO: No. In order to generate a

slug, you are in BCM. There is no natural circulation. You're boiling the core. You're condensing the --

CHAIRMAN WALLIS: The slug doesn't move until you but in enough borated water from ECCS.

DR. DIMARZO: When you put the water in from ECCS, you basically push it up in the steam generator and you put borated water on the other side. At some point they meet on top and that's a condition required for single phase natural circulation which is what we're talking about. At that point the slug starts to move.

DR. BANERJEE: You're not talking about starting the pumps.

DR. DIMARZO: That's what the assumption is if they take the pumps out. If you imagine to start the pump, then basically you keep filling the pump at this point. They don't need to do all this business. You just turn on the pump. What happens is that now you pump water on top of the candy cane which joins the slug as it's being pumped out and the process happens similarly.

CHAIRMAN WALLIS: The worst thing you could do presumably is to bump the pump, put the slug into the reactor, and then turn it off.

DR. DIMARZO: And then stop. Right. Okay.

CHAIRMAN WALLIS: Because you get scared.

DR. DIMARZO: Right. What we did at Maryland was this. We had data on the front of the slug and data on the back of the slug. I had an assumption that says the two mixing volume are those two. I know those two volumes. Basically what I did is to generate a curve based on those two volumes and along that theory. That is the validation shown here which is pretty good. The Tao, τ, is the slug transient time. It's the ration between the volume of the slug.

DR. BANERJEE: It's space time.

DR. DIMARZO: Yes. Basically it's how long it takes for the slug to go through one section if it's totally unmixed. So the formulation is very simple because it's not relying on the dispersion factor which is what makes it very amenable to calculation. Clearly you can use any type of input in the function C of lambda, $C(\lambda)$ that you want and basically you get your output that way.

So I took this approach and I applied it to the initial condition that was supplied by the BNW owner group. That is what Diamond will refer to in his presentation here as the second case or third case. Right?

DR. DIAMOND: The second case.

DR. DIMARZO: The second case. So you'll have a comparison between this artificially mixed thing that the owners group came up with and this type of curve fed directly into the core. That's what you're going to see.

The next thing would be to pump. So what happens? First of all the slug that was proposed by the BNW owner group is a 22.3 meter cubed slug. The maximum amount of water that you can physically store there unmixed is 28 meters cubed. So I took 28 meters cubed and pumped.

CHAIRMAN WALLIS: So the only place it's mixing in your model is in the pump.

DR. DIMARZO: And in the steam generator upper plenum.

CHAIRMAN WALLIS: Upper plenum.

DR. DIMARZO: If it was mixing only in the pump, the back of the slug would look identical to the front.

DR. BANERJEE: He's made a very simple reactor model.

CHAIRMAN WALLIS: But the back of the slug doesn't come in until after you get to the right hand side.

DR. DIMARZO: Yes. After one transient time, the back starts to show up.

CHAIRMAN WALLIS: Right. I'm trying to think why you have such a gradual increase in the beginning there.

DR. DIMARZO: Because the first one is going through the pump which is a mixing volume.

CHAIRMAN WALLIS: It can only mix with the boron which is left in the pump. It only flushes it out.

DR. DIMARZO: Yes. Basically what it means is that the slug comes in and mixes with whatever is in there and comes out. That's the model.

DR. BANERJEE: What's the volume of the pump relative to the volume of the --

DR. DIMARZO: The volume of the pump is, let's see in that particular calculation --

DR. BANERJEE: Compared to the volume in the pipe.

DR. DIMARZO: Yes. That's the characteristic N that you're talking about.

CHAIRMAN WALLIS: Yes. Or the volume of the 1000 cubic feet.

DR. DIMARZO: No. In Maryland, it's not 1000 cubic feet. But the transient time for the pump is the volume of the slug divided by the volume of the pump is about seven.

CHAIRMAN WALLIS: Seven.

DR. BANERJEE: Transient time through the pump.

DR. DIMARZO: Transient time is very small because that transient --

DR. BANERJEE: This is natural circulation.

DR. DIMARZO: No. The point is this. In this model it doesn't matter how fast it goes. The model is formulated in terms of just one dimensional type. The time is essentially scaled by the flow rate as in the transient time. So you don't really need to know that.

To put this in perspective, the owners group made 22.3 meters cubed have a transient time of 110 seconds. If you take the transient time that they should have taken at steady state natural circulation, it would have gone through in 77 seconds. They took and increased that time, in other words, they made the flow slightly slower because they said this is going to be the start up of natural circulation.

What we are doing as a bounding slug as I said to you before is 28 meters cubed and it goes through in ten seconds as opposed to 110 seconds. So we basically bound that by volume and we bound that by flow rate pretty substantially. It’s 11 times faster.

CHAIRMAN WALLIS: Why is it so much faster?

DR. DIMARZO: Because now we are pumping.

CHAIRMAN WALLIS: Now you have the pump running.

DR. DIMARZO: Yes. I mean, in the bounding slug.

CHAIRMAN WALLIS: I thought you weren’t running the pump.

DR. DIMARZO: Let me put it this way. We have two cases; one that you see today which is natural circulation and the transient time is 110 seconds. The one that we will do is pumped 28 meters cubed so it’s a slightly larger slug going through in ten seconds.

CHAIRMAN WALLIS: Yes.

DR. DIMARZO: Okay.

DR. BANERJEE: What difference does that backmixing in the pump do for you? Does it help at all?

DR. DIMARZO: Yes because the front of the slug is what is most important here. To put it through that volume softens it up enough to make a difference. If you put through the vessel a square wave that is completely different.

DR. BANERJEE: Will you get the Doppler feedback?

DR. DIMARZO: I don’t know exactly what the neutronic impact of that is. That’s very important what you do at that slug.

DR. BANERJEE: That’s the smoothing.

DR. DIMARZO: That’s the smoothing there. Right. The tail it doesn’t really matter what you do in a way. It’s there for completion. Now obviously you could start doing over-speculation of what mixing should occur in vessels, but we are trying to stay out of that at this point.

CHAIRMAN WALLIS: Well, if a pump is what’s saving you, I think you may need to be more cautious about your assumption that the pump is well mixed.

DR. DIMARZO: Yes. But I got a substantial amount of data from Maryland that when I passed a lot of slugs through there that tells me that it does do something. That I can use and validate against. The scaling that comes out of it is based on the volume of the pump and the volume of the slug. So it's portable. You don't have to make too much argument of scaling with that type of an assumption.

CHAIRMAN WALLIS: So the down-comer floor is not just one dimensional either.

DR. DIMARZO: No. Since you asked, the down-comer floor is of this kind. We have an experiment. You mentioned about an experiment. I just brought it so that you could get an idea. But that's how misleading a computation could be versus an experiment. For example, this is a CFD of the down-comer done by the owners group in their report. As you can see the slug comes in the cold leg and basically goes straight through down.

CHAIRMAN WALLIS: It makes a -- and goes straight down.

DR. DIMARZO: Yes. That's what they say. These are experiments. They're from Maryland. Here is the first slide where the cold leg is the blue spot.

CHAIRMAN WALLIS: The blue stuff is --

DR. DIMARZO: Where the cold water is going to start to come in. Now the first upper portion of the down-comer has been flooded. As you can see, there is a significant amount of uncirculated region below that cold leg which indicates that the slug is going anywhere but down. Even later in the transient, everything happens except for that thing to go down in that direction. It goes around and down and then even pops up from the bottom up again.

These are obviously just information because you can't scale it to the real thing. But what I'm trying to say here is that to touch the CFD in the vessel is a very complex enterprise.

CHAIRMAN WALLIS: But it may well be that it'll never go pump critical at all.

DR. DIMARZO: Absolutely.

DR. BANERJEE: All you are doing is you're getting a residence time distribution. You could do this without any --

DR. DIMARZO: Yes. But the problem is I could take the experiments in Maryland and say scale them, in other words, get an idea of how much mixed region there is in that. You could translate this into saying for example that if you take the volume of the down-comer and lower head and you imagine that 20 percent of that is fully mixed and run some simple model, get the curve out of there and plot it.

We did all these exercises at Maryland.

That's been put in our reports. The problem is that I don't know how to answer the question what does it mean to prototype. That's basically where I think if you go down that road you have to have a validation of a CFD model against Maryland perhaps and then at that point scale it up with that code once you're convinced that what you see is what you get. That is not a simple enterprise.

DR. BANERJEE: But a level is always below the pump in the pipe --

DR. DIMARZO: The level?

DR. BANERJEE: Of the deborated water.

DR. DIMARZO: When you form the deborated --

DR. BANERJEE: It never reaches into the --

DR. DIMARZO: No. It might come into that pipe. But the problem is that this is a slow process, this formation of the deborated. The deborated is somewhat lighter. Being the same temperature of the borated water that it displaces, it's lighter. So as it enters the vertical portion of the pipe towards the pump it would tend to mix with it. What's in that particular leg is not really deborated. At best, it's some kind of a smooth mixed type thing.

CHAIRMAN WALLIS: Isn't there a slow flow because of the condensation that comes through and keeps washing out that borated?

DR. DIMARZO: Right. Exactly.

CHAIRMAN WALLIS: So you get some dilution before the slug actually gets in.

DR. DIMARZO: Exactly. In the front of the slug, there's not even that chance because of that effect. To that you add the fact that there is some limited amount of HPI injection too which borated the slug as it goes back.

CHAIRMAN WALLIS: I'm saying that the deborated water actually flushes out some of the boron from the pump.

DR. DIMARZO: No. The deborated at best can come to the level of the pump really. It can also trickle through the pump. But you have an HPI injection that you haven't considered here. So it's a wash. In one way, your pump could be more deborated then what I anticipated, yes. But on the other hand, all the water between the water and the steam generator will be far more borated than what I guessed.

CHAIRMAN WALLIS: How do you know about that?

DR. DIMARZO: That's because it mixes.

CHAIRMAN WALLIS: Does it? If you have enough low trickle flow you called it or enough flow of deborated water because of condensation and flow around so that it keeps on flushing out some boron, then you could have less boron.

MR. SCOTT: I think it depends on how long that BCM went on.

DR. DIMARZO: Right.

MR. SCOTT: If it just goes on forever and ever, then it's really clean water.

DR. DIMARZO: Yes. Then you have clean water coming through to your core from that.

CHAIRMAN WALLIS: Right.

DR. DIMARZO: But you would be very slow.

CHAIRMAN WALLIS: Well, very slow. I guess you would have to have an analysis that shows it.

DR. DIMARZO: I mean, by deboration alone you have basically -- If you imagine the condensation process to go on indefinitely and to have the deborate come through the core with that kind of a rate, I don't think we'll get anything.

CHAIRMAN WALLIS: That's not a problem. The problem is if the condensation builds enough deborated water that it actually flushes out the pump then you can't take credit for the boron.

DR. DIMARZO: I can't take credit for the boron in the pump. But the pump remember is a minuscule volume compared to down-comer and lower head.

CHAIRMAN WALLIS: You're assumption is that you get mixing in the pump. If there's no boron left --

DR. DIMARZO: I see your point. The realization here is that's what you take credit for. The practice of the fact is that you are a down-comer and a lower head of which you don't take credit at all which is a tremendously conservative assumption. So I understand your point and it's well taken. The problem is that I'm not taking credit of a potentially mixing volume which is enormous compared to the pump itself.

CHAIRMAN WALLIS: But I'm saying you could be more conservative and not take credit for that boron in the pump because it's being trickled out by condensation.

DR. DIMARZO: Right. I'm not an expert. I'll let Diamond discuss that. I think that the leading edge of that slug is very important in what you’re saying too. So if you’re saying to a square wave which is virtually impossible considering all the mixing volume, he has a problem with it. The results would change dramatically.

DR. BANERJEE: For a thermal shock situation, what happened in the down-comer? There were a lot of studies done. Weren’t there?

DR. DIMARZO: You mean in the PTS.

DR. BANERJEE: Yes.

DR. DIMARZO: We didn’t look at that.

DR. BANERJEE: Well mix ups or --

(Inaudible.)

MR. ROSENTHOL: (Away from the microphone.) We had the OSU experiments.

DR. DIMARZO: The down-comer what appears to happen is that there isn’t even a plume. In other words, by the time you are five or six diameters of the cold leg down you can’t find anything anymore.

DR. BANERJEE: Why wouldn’t you expect something like that here?

DR. DIMARZO: Absolutely. But what I’m saying is that I cannot come here and quantify how much mixing occurs in the down-comer. I can simply say there will be a tremendous amount of mixing in the down-comer. But I cannot say exactly how much. In other words, I do not have a scalable transfer function from the flow coming into the down-comer to the entrance of the core. All experiments that have been done do show that there is a significant amount of mixing there.

DR. BANERJEE: There's not been any quantitative experiments.

DR. DIMARZO: Not scalable.

CHAIRMAN WALLIS: Why not scalable?

DR. DIMARZO: In the sense that you have to come here and tell me that I've seen in experiments one through ten how does it relate to prototype. Nobody has ever done that kind of a study in detail. There is the problem of a small item in the geometry which alters tremendously what you see. For example, the enlargement of the down-comer. For example, the equipment that's in the lower head and all of that.

MR. SCOTT: The Germans are trying to do that at Wasendorf (PH) in Dresden. They have a big glass see through type device.

DR. DIMARZO: Right.

DR. BANERJEE: You would think that scale effects can be very important.

DR. DIMARZO: Oh, yes. This is decided on a smaller scale.

DR. BANERJEE: So you cannot quantitate.

DR. DIMARZO: It's not that you cannot. You can definitely do it. The problem is that relative to these issues if you can close it with this very lasting assumption, that's it.

MR. SCOTT: The point that Jack Rosenthol made earlier was the 3-D kinetics thing is sort of washing all these things out. It's so benign.

CHAIRMAN WALLIS: Okay. So you're going to close it with worst assumptions about the flow mechanics.

DR. DIMARZO: Exactly.

CHAIRMAN WALLIS: Otherwise you would think that all of those experiments at University of Maryland must be good for something. They should give you a handle on mixing.

DR. DIMARZO: I mean, we could definitely go down that route. The route would be very simple. You have to take a CFD code and try to duplicate it as an experiment and go from there.

CHAIRMAN WALLIS: That would allow you to continue. You're going to show that even if you make very bad assumptions, the kinetics saves you.

DR. DIMARZO: Right. That's my point.

CHAIRMAN WALLIS: Okay.

MR. ROSENTHOL: And we did use the Maryland because without the Maryland we would have to have square wave. But now we can have what I call diMarzo rounded edges.

CHAIRMAN WALLIS: Well, I was thinking about the diMarzo rounded. It seems to be related to this mixing in the pump.

DR. DIMARZO: No. In the -- what you are saying is that in a real scenario that situation may not occur which is okay. But there is a lot of mixing going on anyway in the real scenario.

CHAIRMAN WALLIS: No. I understand that if you don't have the diMarzo mixing in the pump you get a square wave and you're still in trouble.

DR. DIMARZO: Yes.

CHAIRMAN WALLIS: So you better be pretty clear that the diMarzo mixing in the pump is real and that you don't get flushing out of that.

DR. DIMARZO: Yes. But remember that you're not taking credit for what happens in the down-comer.

CHAIRMAN WALLIS: You're taking the credit.

DR. DIMARZO: No. We are not taking credit for that mixing which is pretty substantial.

DR. BANERJEE: Yes. But you're trying to close the issue. And you either say okay we are not going to consider this mixing but we are sure that there's going to be mixing in the pump.

DR. DIMARZO: Right.

DR. BANERJEE: So I'm not going to do a CDF calculation. I'm just going to do this backmixing in the pump.

DR. DIMARZO: Right.

DR. BANERJEE: And then if the pump itself would be full of deborated water by any stretch of the imagination.

DR. DIMARZO: Full it's not going to be because the rate at which it goes the best you're going to have is a trickle.

CHAIRMAN WALLIS: So you're going to quantify that trickle and do an analysis.

DR. DIMARZO: Yes. We could do that.

CHAIRMAN WALLIS: Yes. I think you have to.

DR. DIMARZO: Yes. It makes sense. Basically that amount is to a reduction in the volume of the pump.

CHAIRMAN WALLIS: I have no idea what you mean by "trickle."

DR. DIMARZO: We know what the power is. We know basically the condensation rate. So we can quantify exactly what the trickle is.

CHAIRMAN WALLIS: The other thing is how long does that trickle take to wash the boron out of the pump. Is it days or months?

DR. DIMARZO: You are bringing up an interface. First of all you have to deborate all the legs. Then you're bringing up an interface of deborated water which can flow out of the pump.

CHAIRMAN WALLIS: If you're going to deborate all that volume down here, why can't I deborate the pump as well?

DR. DIMARZO: In order to pass through the pump, you have to deborate, you have to pass only through the level that sees the exit of the pump. You don't have to go through the whole volume of the pump.

CHAIRMAN WALLIS: See what I mean. If you've created all that deborated water by condensation, you fill all this 1000 cubic feet. Why can't you make a little bit more and deborate the pump as well?

DR. DIMARZO: The whole pump you can't because at some point you start to get out of the pump.

CHAIRMAN WALLIS: Well, I guess --

DR. DIMARZO: But I see your point and we'll make an argument of this type. We compared the volume of the pump with the volume of down-comer and lower head.

DR. BANERJEE: What about the pipe? You are saying that deborated water should rise through borated water. Right?

DR. DIMARZO: It should push borated water ahead of itself. There is a G.I. Taylor paper --

DR. BANERJEE: Taylor and stability.

DR. DIMARZO: Yes.

DR. BANERJEE: So why would the pipe be full of deborated water.

DR. DIMARZO: No. The point initially you're absolutely right. Initially the deborated won't stay together. It would start bubbling through the back. That's fine. We went through that.

DR. BANERJEE: (Inaudible.)

DR. DIMARZO: You well it up in the pump. That's okay. And it will flush out on the other side and drain out. So through all that process what Graham is saying that we fill the whole pipe with deborated completely, flush the pump completely with deborated.

CHAIRMAN WALLIS: It keeps bubbling through the pump as you described it.

DR. DIMARZO: As you completely flush it out. Right. The argument that I would like to make is that the volume of the pump is a certain amount and then compare that to down-comer and lower head volume. Then we can make a claim to that effect.

CHAIRMAN WALLIS: (Away from microphone.)

DR. DIMARZO: Yes. But assuming that you have a mixing volume which is equivalent to the volume of that pump.

CHAIRMAN WALLIS: Yes. But it depends on what’s in that pump when you start to move the slug.

DR. DIMARZO: Absolutely.

CHAIRMAN WALLIS: We are not convinced that there is boron left in the water in the pump.

DR. DIMARZO: Right.

CHAIRMAN WALLIS: I think that has to be shown.

DR. DIMARZO: Well, that cannot be shown. What can be shown then we’ll still have to go to the vessel at some point.

CHAIRMAN WALLIS: But your whole analysis I thought depended on there being borated water left in the pump.

DR. DIMARZO: Yes. I understand that.

CHAIRMAN WALLIS: We are not convinced that there is borated water left in the pump.

DR. DIMARZO: That's a good point. The point is that in order to show that you have to basically say that the deboration takes place over a very long period of time and so forth.

CHAIRMAN WALLIS: I don't know. How long?

DR. DIMARZO: We can calculate that. It's clear.

DR. BANERJEE: But if it's bubbling through so you're talking about having deborated water bubbling up through up borated water, of course as it bubbles up, it mixes.

DR. DIMARZO: It mixes. There is no way of keeping it --

DR. BANERJEE: This seems to me something which is ameanable to calculation by hand.

DR. DIMARZO: Yes. I'm sure of it. That's fine. There's no question about that.

DR. BANERJEE: I mean, you know the wavelength of the --

DR. DIMARZO: Yes. But I can do another calculation too. I can basically say once finally we start moving the slug by natural circulation the assumption that Graham is putting forward is that the whole system is basically deborated ahead of the slug because of this very extensive --

DR. BANERJEE: It won't flush out. It will mix because it's too --

DR. DIMARZO: Right. That's the point. There is a paper by G.I. Taylor that I didn't touch which basically says that as soon as you move this thing it's going to start mixing within the pipe just because at the wall the water drags.

DR. BANERJEE: Forget that complexity. If you had a straight vertical pipe full of salt water and you put fresh water in it --

DR. DIMARZO: It's going to mix before it gets up there. There's no question about it.

DR. BANERJEE: You can calculate the concentration.

DR. DIMARZO: Yes. There's no question about that. If you keep putting fresh water which is what he suggests, at some point you'll have it all fresh water. That is what he's saying.

DR. BANERJEE: If you put in enough.

DR. DIMARZO: Yes. That's what he's saying. That's the question. How ultimately is ultimately. That's the whole point. So I can push this and resolve it that way. What he's saying is if we sit in that predicament for --

DR. BANERJEE: Days.

DR. DIMARZO: Right. Then eventually you have the square wave.

MR. ROSENTHOL: But the scenario doesn't go like that.

DR. DIMARZO: Right.

MR. ROSENTHOL: Here's a small break LOCA which is going to be over in a couple of hours one way or the other.

DR. DIMARZO: I don't think there is the time to do what is predicating. But I can calculate that. That's the way I'm going to get out of this.

CHAIRMAN WALLIS: I'm not sure you can calculate this flushing out of --

DR. DIMARZO: Yes. You need a certain amount of time and volume of water to do it.

CHAIRMAN WALLIS: The vertical part of this pipe by the bubbling water.

DR. BANERJEE: It's not bubbles.

DR. DIMARZO: It will mix. So that volume becomes like another mixed volume.

CHAIRMAN WALLIS: It depends a lot on how big the entities are that come around the bend and are released. If there’s some kind of a oscillation and big hunks of water come through, they probably --

DR. DIMARZO: There aren’t any big hunks of water coming through. The condensation process is a very slow process.

CHAIRMAN WALLIS: So it comes oozing across the top of the bend and up the walls.

DR. DIMARZO: Exactly. And it’s fully mixed by the time it goes up there.

CHAIRMAN WALLIS: I don’t know.

DR. DIMARZO: You’re dealing with a very long pipe. But I’ll show that.

DR. DIAMOND: It mixes with water that has become more highly borated than before because the --

DR. DIMARZO: Now remember one thing though. The scenario without the pump calls now that the system is refueled. So you are now taking borated water and you fill the pump with borated water. You push the borated back down. You lift the deborated all the way to the top of the steam generator. At that point, natural circulation starts. At that point, you basically have the slug totally in the steam generator.

CHAIRMAN WALLIS: So you say when you fill with borated water you know the level in the pump by which it comes to.

DR. DIMARZO: What I'm saying is this is a two-phased scenario. Scenario part one you generate the slug.

CHAIRMAN WALLIS: Because there is a free surface.

DR. DIMARZO: Yes. Let's imagine the pump by then is totally deborated. Now you have to resume natural circulation. In order to do that somehow HPI flow starts to be larger than break flow so that the system refills. So the bottom of the system now is being filled by HPI water. Right? This is at full system right now. You start putting HPI system in and it trickles over also from the pump side because it fills the system on both sides. At which point everything in that leg is full of HPI water which is borated.

DR. BANERJEE: Where is the HPI coming in exactly on that diagram?

DR. DIMARZO: In the incline portion of the cold leg.

CHAIRMAN WALLIS: Well, I guess it's hard to follow this description which is all verbal.

DR. DIMARZO: I don't have a mic. That makes my life complicated. But initially you are filling this with deborated. (Indicating.) Then imagine the trickle over here and makes this deborated completely. There's no question. There's no problem. Now the water is deborated up to this level. I have to refill the system. HPI comes through here. (Indicating.) So HPI starts to flow on this side.

CHAIRMAN WALLIS: It goes up to the candy cane. Doesn't it? It fills up that pipe there.

DR. DIMARZO: In order to fill up this pipe, it has to fill up also this pipe.

CHAIRMAN WALLIS: But it can't get there.

DR. DIMARZO: The deborated --

CHAIRMAN WALLIS: It has to push the slug back into the steam generator.

DR. DIMARZO: Right. So the slug is all the way up there. (Indicating.) By the time the slug is all the way up there, all these regions are full of HPI water which is deborated.

CHAIRMAN WALLIS: Oh. You have to tell us all that.

(Inaudible.)

DR. DIMARZO: Which is totally borated. When the slug starts to move down, it will go to --

CHAIRMAN WALLIS: You told us about Act I and Act V and missed out Acts II, III, and IV.

MR. ROSENTHOL: Okay. We'll write it up that way for the final.

DR. DIMARZO: The point is this. There is a confusion here between the paper and what you are talking about here as a scenario. So that's probably what the problem is. The issue that you move being a situation of natural circulation is not really there. In the situation where we pump, it could be potentially there.

DR. BANERJEE: You are saying that the HPI will tend to keep the pump full of borated water. Is that it?

DR. DIMARZO: Not really.

DR. BANERJEE: I mean that's --

DR. DIMARZO: (Away from microphone.)

CHAIRMAN WALLIS: Yes.

DR. BANERJEE: The HPI. Where does the HPI come in?

DR. DIMARZO: Right there. (Indicating.)

DR. BANERJEE: Okay. Does it tend to go into the pump?

DR. DIMARZO: It will fill both sides.

DR. BANERJEE: Both sides.

DR. DIMARZO: Correct. So you have now a flush of HPI water in here.

CHAIRMAN WALLIS: It also pushes the green stuff back up.

DR. DIMARZO: Yes.

DR. BANERJEE: Okay. That makes more sense.

CHAIRMAN WALLIS: So when the green stuff comes to the pump, it has borated water in it.

DR. BANERJEE: Okay.

CHAIRMAN WALLIS: That's much more believable. Why didn't you tell us that an hour ago?

DR. DIMARZO: I tried.

CHAIRMAN WALLIS: I think this is the Italian sense of drama. You get the audience totally confused and then tell them the answer.

(Laughter.)

MR. ROSENTHOL: If you make him put his hands in his pockets, he can't talk so much.

DR. DIMARZO: I couldn't stand and just talk.

CHAIRMAN WALLIS: So when this whole thing comes to the full committee, this story is going to be clear.

DR. DIMARZO: Yes.

MR. BOEHNERT: Well, we also have the option of inviting him back in late August at the subcommittee meeting if we think we need to hear this.

DR. DIMARZO: The pump part.

MR. BOEHNERT: This pump part which we may need to do.

CHAIRMAN WALLIS: I think it would be very good that before anything goes to the full committee we make sure that the story is clear.

MR. BOEHNERT: I think so too.

MR. ROSENTHOL: At one time we thought that we would go to the subcommittee and then the full committee a week later. Then we recognized that we needed to satisfy the --

CHAIRMAN WALLIS: So I will tell the full committee in July. I guess I probably have to make some report that we had a presentation which needs to be worked on and we will hear it again before it comes to the full committee.

MR. ROSENTHOL: If you desire.

CHAIRMAN WALLIS: I think it has to be. This was not clear. If you get into this kind of confusion with the full committee, they won't accept it.

MR. BOEHNERT: It will be fatal.

MR. ROSENTHOL: Agreed.

CHAIRMAN WALLIS: I think this

presentation has to have a proper description of the scenario. You have green water and blue water or something. You show where it goes and how it comes back and there's interface here and the worst possible assumption. But it must mix in the pump anyway. Give us a proper story.

DR. DIMARZO: So we need to provide you with a much better description of the scenario which we didn't include this time at all.

CHAIRMAN WALLIS: Right.

DR. DIMARZO: We just simply said this is the slug that gets through.

CHAIRMAN WALLIS: Are we going to hear with this mixing in the pump the neutronics save us, but without the mixing in the pump, they don't? Are we going to hear after the break?

DR. DIAMOND: With or without the mixing in the pump the neutronics are probably going to supply feedback so that it's not a --

CHAIRMAN WALLIS: The calories per gram or whatever the figure of merit is are low enough.

DR. DIMARZO: Even with a square wave.

DR. DIAMOND: But we don't have a square wave.

DR. DIMARZO: With or without mixing in

the pump, you said the neutronics can save you.

DR. DIAMOND: No. You can't have a square wave.

CHAIRMAN WALLIS: A square wave is bad?

DR. DIAMOND: A square wave is bad.

DR. DIMARZO: So you need mixing in this.

CHAIRMAN WALLIS: So you need to have a good argument that there is mixing.

DR. DIMARZO: In the natural circulation scenario.

CHAIRMAN WALLIS: The fact that Marino feels there's mixing in the pump is not good enough.

DR. DIMARZO: No. That's not the correct view. I said this. I have data. I made a model.

CHAIRMAN WALLIS: Show us the data.

DR. DIMARZO: The data is in the paper.

CHAIRMAN WALLIS: Show us the evidence of mixing in the pump. Show us the evidence.

DR. DIMARZO: No. I have data of what the front looks like. Then I said if mixing occurs in this volume I get that.

CHAIRMAN WALLIS: Show that your model for mixing in the pump correlates with the data from the experiment.

DR. DIMARZO: Right. That's what is here.

You have it in front of you.

CHAIRMAN WALLIS: In this?

DR. DIMARZO: Yes.

DR. BANERJEE: Is that a measurement of the pump outlet?

DR. DIMARZO: Yes.

DR. BANERJEE: It’s not --

DR. DIMARZO: No. That’s the slug.

DR. BANERJEE: I think I would buy the fact that you get backmixing in the pump if the pump was full of borated water.

DR. DIMARZO: Right. That’s the argument.

DR. BANERJEE: Deborated water too.

DR. DIMARZO: Absolutely. There’s no question. But in this particular scenario it must be full with borated water in the natural circulation part. The question that keeps lingering in my mind is how do I show you that it’s full of borated water under the hypothesis that you start the pump. That becomes a more complicated thing to do.

CHAIRMAN WALLIS: It also gets mixed in the region downstream of the veins.

DR. DIMARZO: Yes.

CHAIRMAN WALLIS: The veins that create --

DR. DIMARZO: Yes. What I’m basically

saying is if you consider the volume of the pump as a representation of both you get the data to correlate.

CHAIRMAN WALLIS: That has to be clear too somehow.

DR. DIMARZO: Right. One issue remains open. If I pump, I cannot in any way state that the pump will be full of borated water. You understand that.

DR. BANERJEE: If you start --

CHAIRMAN WALLIS: Because it's already been deborated and you --

DR. DIMARZO: If you presume that, exactly.

CHAIRMAN WALLIS: All right.

DR. DIMARZO: That is the part that I cannot show but it's not really part of this scenario.

CHAIRMAN WALLIS: All right.

MR. SCOTT: Some of these pumps you see did have higher borated water.

CHAIRMAN WALLIS: All the way through the pump.

MR. SCOTT: (Away from microphone.) It's not always very deborated --

CHAIRMAN WALLIS: The green is slightly borated.

MR. SCOTT: (Away from microphone.) This one seems to have higher than -- This one's an intermediate. This one has low. This was just before we started the circulation which now you would be injecting this unborated water. This is a PKL experiment.

CHAIRMAN WALLIS: So each LOOP is different too.

MR. SCOTT: It's a PKL.

DR. BANERJEE: But this is a once through scenario.

MR. SCOTT: No.

(Inaudible.)

CHAIRMAN WALLIS: This is a Westinghouse.

MR. ROSENTHOL: That's a Westinghouse full LOOP. PKL is the experiment facility. That's an interpretation of what PKL would be to the Westinghouse four looper.

CHAIRMAN WALLIS: Okay. So we're now going to take a break. At 4:00 p.m., we will hear the end of this story. Thank you. At 4:00 p.m., we will resume. Off the record.

(Whereupon, the foregoing matter went off the record at 3:48 p.m. and went back on the record at 4:03 p.m.)

CHAIRMAN WALLIS: On the record. We will hear the final part of this story of GSI-185.

DR. DIAMOND: All right. I'm going to talk about the consequences in the core of having this diluted slug.

MR. BOEHNERT: Could you introduce yourself, sir?

DR. DIAMOND: Yes, sure. David Diamond from Brookhaven National Laboratory. The background for this is that there was a study done by Framatome. It's been mentioned before. That was supposedly a conservative study.

They estimated the boron concentration as a function of time at the inlet to the core and also at the lower plenum. Then they used a lump thermal-hydraulic/point kinetics model. This was a RELAP5 calculation to assess the consequences. We had looked at that and noted that because of this rather simplistic model which didn't take care of the significant spatial effects that go on during this event that it would be worthwhile to consider the event with a much more rigorous model.

So we said to ourselves what can a three-dimensional coupled neutronic and thermal-hydraulic analysis tell us. One thing is that it can contract the radial and axial distribution of the boron changes. Those are significant as I will demonstrate.

It can also take into account the fact that when this reactor goes critical again the other situation where all of the control rods are inserted and so we have a checkerboard pattern of control rods in the reactor which means that the neutron flux in the reactor is non-uniform. So we know that the radial and axial power distribution are complicated. Therefore, it makes sense to treat this problem using a three-dimensional calculation or at least address the neutronics with a three-dimensional model.

CHAIRMAN WALLIS: Now, you're assuming uniform fuel or do you know something about the burn up patterns?

DR. DIAMOND: Yes. This is a real reactor. So that's one part of the problem that I'm going to address today. I'm going to show you some results which demonstrate the physical phenomenon that takes place in the core and show that the spatial effects are important and what the differences are between the detailed neutronics calculation and the simplistic calculation that Framatome did.

Then of course at the end of the day we're interested in the consequences. So I'm also going to show a result to explain what sort of fuel enthalpy increase one gets during this event. There are essentially two different calculations that I want to leave you with today.

We've discussed this. I don't think that we have to go any further here except to say that of course the reactor is going to go critical because there is a considerable amount of deborated water.

CHAIRMAN WALLIS: How much does the level of deborated water have to rise before it does go clear? How far does it have to go into the core?

DR. DIAMOND: I will show that to you specifically, quantitatively what that looks like. Let me tell you a little bit about the core model that we used. We modeled a BNW reactor, specifically TMI-1. It was a beginning of cycle model because in that case the reactor starts off with a need for boron in the core. Therefore, the deboration has a much larger effect than say an end of cycle.

This is a core with 177 fuel assemblies. It's very much like the core but not exactly equal to the core that the Framatome people used when they did their analysis. There's a starting point for these calculations. I won't get into the details of this. The only reason that I mention this here is because our boron dilution accident will begin at 200 seconds into the transient that I'm going to show.

So we have some sort of starting point. After 200 seconds, we get to reactor condition which emulate the identical conditions that Framatome said would occur after several hours of this small break LOCA scenario when natural circulation has just started again and the boron dilution even can take place. So at that time as I said all banks are inserted. The fuel and the moderator have cooled down considerably or at least a little bit. They're down to 500 Kelvin. In this first case that I will show you the boron ppm is at 1165. The reactivity is at zero.

CHAIRMAN WALLIS: Why has it gone down to that?

DR. DIAMOND: Well, this first case that I'm going to show you is an attempt to make a comparison with the BNW calculation. So we tried to duplicate the reactivity insertion that BNW applied in their calculation. This is a detailed calculation preserving the same reactivity insertion and rate of reactivity insertion as in the BNW calculation. After I explain the physical phenomenon that take place during this event, I'm going to show you a calculation in which we apply our best estimate of the inlet conditions and show you what the consequences are of that particular event.

CHAIRMAN WALLIS: This starting point is the reactor is full of boron at 1165 ppm.

DR. DIAMOND: Yes.

CHAIRMAN WALLIS: It's gone down from 1700 in some way.

DR. DIAMOND: Yes. In this case, artificially.

CHAIRMAN WALLIS: Why hasn't it gone up?

DR. DIAMOND: It has. That's correct. In the actual scenario, it has gone up to 2500 ppm.

MR. BOEHNERT: Are you accounting for the Xenon growth?

DR. DIAMOND: No. We're neglecting that.

MR. BOEHNERT: So that's a conservative assumption.

DR. DIAMOND: Yes.

MR. BOEHNERT: Okay.

MEMBER RANSOM: How does it get down to 1165 ppm?

DR. DIAMOND: The realistic reactor conditions would be at 2500 ppm.

MEMBER RANSOM: So why did you take this

lower number?

DR. DIAMOND: Because we were trying to emulate the Framatome calculation. They did a point kinetics calculation. Well, it was a RELAP5 calculation which uses point kinetics. In that point kinetics calculation, they start from zero reactivity and add three and a half dollars worth of boron positive reactivity. So we wanted to go through the same point in order to emulate that. In reality, you would be starting at 2500 ppm of boron and you would be considerably subcooled. You would have to come up to zero reactivity and then go some.

CHAIRMAN WALLIS: So they're assuming boron ppm in order to make the reactivity zero essentially.

DR. DIAMOND: Yes. No, we are. In their calculations, they don't do a boron transport calculation. They just insert a certain amount of reactivity based on what they would expect in the core.

This first calculation is a little bit contrived. As I say it's to get you to understand that the physical phenomenon that are taking place. Then I'll show you something that's a little bit more realistic.

The layout of the core is shown here. This is 177 fuel assemblies. Because we assume uniform inlet conditions across the core, we can focus on one-eighth of the core. This is that one-eighth of the core. The numbers at the top of the boxes are the top of each fuel assembly just as the number of thermal-hydraulic channel. There are 29 fuel assemblies in this one-eighth core and 29 thermal-hydraulic channels in our model.

The burn up for each fuel assembly is the lower number. We see that there are yellow and white fuel assemblies. The yellow assemblies are assemblies that have a control rod in there because one of the first things that happens is all of the rods are SCRAMed into the core. So you can see this checkerboard pattern. Rod in. Rod out. Rod in. Rod out. If you look at the burn up numbers, you see that these fuel assemblies along here without control rod have the lowest burn up. (Indicating.)

CHAIRMAN WALLIS: They're all new essentially.

DR. DIAMOND: Those are new. Right. These that are shaded here are going to be the assemblies where the fuel enthalpy is going to be the highest in this particular scenario.

To do the calculation, we used PARCS. You heard Joe Kelly mention PARCS a little bit earlier today. It was a code that was originally developed at Purdue and is now incorporated as part of TRAC-M. The code models the neutronics and three-dimensions. It is able to break up the core fuel assembly-by-fuel assembly and axial node-by-axial node. There are 24 axial nodes in a neutronics calculation and actually four neutronic nodes in each assembly in these calculations.

The code takes into account the neutron kinetics. So it takes into account the effect of delayed neutrons. It uses two neutron energy groups. It uses diffusion theory. The diffusion equation is solved based on a nodal method. I think that you're going to learn more about this code when you learn more about the models within TRAC-M because this is a part of TRAC-M.

The code has feedback from the appropriate feedback mechanisms; fuel temperature, moderator density, the boron concentration, the change in position of control rods. Of course, the thermal-hydraulic conditions here need to be calculated from a thermal-hydraulic model. In this particular case, PARCS is coupled with RELAP5. So this is really

PARCS-RELAP5 or RELAP5-PARCS.

The cross sections are generated with a different code. These are the cross sections which enable you to solve the two neutron energy group diffusion theory equations. Those cross sections are obtained for each of the fuel assemblies, again for the TMI-1 reactor's beginning of cycle.

There was one problem with the cross sections. They're not good to below 500 K. That's why our calculations started at 500 K. The actual reactor conditions would ger you down to about 425 or 450 K. Since we were not able to go down that far, we made sure that we preserved the same subcooling as would be expected in the actual plant.

The RELAP5 calculation took advantage of this octant symmetry. As I explained there were 29 channels to represent the 29 fuel assemblies. There's one channel to represent the reflector regions. These of course are parallel channels. There's no mixing.

CHAIRMAN WALLIS: Now, voids are formed in the core.

DR. DIAMOND: Yes.

CHAIRMAN WALLIS: So you need to have some regions for the thermal-hydraulic analysis.

DR. DIAMOND: Yes. But the thermal-hydraulic analysis proceeds as multiple parallel channels rather than with any mixing.

CHAIRMAN WALLIS: It doesn't analyze each channel separately. Does it?

DR. DIAMOND: Yes it does.

CHAIRMAN WALLIS: It does.

DR. BANERJEE: 29 channels.

CHAIRMAN WALLIS: 29 channels.

DR. BANERJEE: And one reflector.

CHAIRMAN WALLIS: All RELAP5. That's quite a lot.

DR. DIAMOND: 29, yes. The reason we're able to do this is again as I explained because of this octant symmetry.

DR. SCHROCK: Is the symmetry really that good?

MR. BOEHNERT: Virgil, use the mic please.

DR. SCHROCK: I asked is the symmetry really that good. You have previously burned bundles mixed with new bundles and so forth. Are the burn ups really that close to preserve this symmetry?

DR. DIAMOND: Yes. They certainly are. I will mention something later where there is a problem in symmetry of course. That is that there is always this question of the flow into the core inlet and how uniform that flow is. But of course we would have to have additional knowledge to really understand.

CHAIRMAN WALLIS: From what we saw, the pictures of the down-come are where it's probably not very uniformed in terms of boron concentration.

DR. DIAMOND: Yes. Okay. So this first calculation that I'm going to show as I said it has about three and a half dollars worth of boron reactivity as the maximum value. That's why I say we can't have a square wave coming in here. That's an awful lot of reactivity to come in instantaneously.

When we talk about the rod ejection accident, generally we're talking about one and two dollars worth of reactivity. So if we have a maximum of three and a half dollars and put it in the square wave, I don't think that anybody would accept that.

DR. SCHROCK: In your previous statements, you said you were trying to replicate the BNW owners group calculation. Their reactivity assertion only goes to one dollar.

DR. DIAMOND: That's the total reactivity. So the total reactivity is of course the boron reactivity less the feedback.

DR. SCHROCK: Oh, yes. I see.

DR. DIAMOND: All right. The mass flow rate at the lower inlet plenum was about three percent. As I said, we had about 200 seconds of simulation to bring the core to the same conditions before the boron dilution accident.

CHAIRMAN WALLIS: Are you going to show us far the boron front goes up before the core goes critical?

DR. DIAMOND: Yes. We can infer that. I won't show that exactly. This just gives you the boron concentration versus time. As I said we're starting really from 200 seconds and going through this particular transient which is a transient from almost 200 to almost 600 ppm of boron concentration in this slug of water.

CHAIRMAN WALLIS: Why doesn't it go to zero?

DR. DIAMOND: This is based on Framatome.

CHAIRMAN WALLIS: Oh, this is Framatome.

DR. DIAMOND: Yes. This is based on the Framatome analysis.

CHAIRMAN WALLIS: I put diMarzo on that fuel.

DR. DIAMOND: Well, we have another curve which is a little bit more severe than this but it still doesn't go to zero.

MR. ROSENTHOL: This is the concentration in the core.

DR. DIAMOND: This is the concentration at the inlet plenum.

CHAIRMAN WALLIS: I thought he had a dilution of 100 percent.

DR. DIMARZO: Graham, what you are looking at is a paper from Maryland. It's contained in the paper from Maryland that the concentration does go to zero. What we're talking about here is a scenario which is defined differently. You don't have zero.

CHAIRMAN WALLIS: What's conservative?

DR. DIMARZO: It's not a question of conservative. It's a question of where the slug is initially. Remember the slug is confined completely in the steam generator before this process starts. Therefore, that slug has to go to the steam generator out of plenum and mix. Then it has to go to the pump and mix. That's the front of the slug.

DR. BANERJEE: This is Framatome.

DR. DIMARZO: Right.

DR. BANERJEE: That's why it's so mild.

DR. DIAMOND: Okay. So that curve shows you that we start to get some dilution around 230 seconds. It's about another 20 seconds of dilution and one sees a large increase in the power. The power goes up to between 70 and 80 percent of nominal power.

Then as it typical in power excursions like this, the power turns over because of Doppler feedback. That's the nice thing about low uranium cores. They have a very strong Doppler feedback. The power turns over but the core is still being diluted. Therefore, there's this pull. There's this positive reactivity being put in. There is this pull from the Doppler trying to hold it back. Then with time, the moderator heats up and you have moderator density feedback.

Another nice thing about a PWR is that it has a negative feedback coefficient from the moderator temperature or the moderator density. So this competition between the boron and the feedback results in the power coming down and then up and then down and then up a little bit and then it settles down as the boron slug moves off. What this means in terms of fuel enthalpy and this is fuel enthalpy at the node in the core that has the highest fuel enthalpy is that the fuel enthalphy starts from about 14 and goes up initially only to about 34. So initially there's only about a 20 calorie increment in fuel enthalpy.

If you look at the peak change in fuel enthalpy, you see that it goes up maybe a total of 37 calories per gram from here to here. (Indicating.) But if there was going to be any fuel failure, it would probably be the result of this initial increase in enthalpy. So that's fairly low.

DR. SCHROCK: So what is the cause of the second peak?

DR. DIAMOND: Again, it's the competition between the positive boron reactivity which is still coming into the core and the feedback effects from Doppler and from the moderator temperature.

CHAIRMAN WALLIS: But it's the second power peak that puts more --

DR. SCHROCK: There's no boiling in this case.

DR. DIAMOND: There is a little bit. I'll show that momentarily. There is some boiling.

MR. BOEHNERT: Localized?

DR. DIAMOND: Yes. Localized. The behavior here will become clearer as we go through a few more of these curves. This curve shows the power versus time but on a logarithmic scale. I just wanted to point out that when we looked over here, we saw that it looks as though the power doesn't increase until close to 250 seconds. (Indicating.) But in reality, the power begins to rise soon after 200 seconds and then goes up to as I say about 80 percent nominal power.

This curve is the curve that Harold Scott showed earlier. This curve shows the boron concentration during the period from 230 to 330 seconds. You can see that the boron concentration is decreasing during this first roughly 50 seconds. So the reactivity change is a result of this positive reactivity insertion due to the boron concentration going down, this is the scale for the boron concentration, and also the negative effects from fuel temperature and moderator temperature feedback. This erratic behavior as a result of the competition between those feedback effects accounts for the corresponding curve of power versus time.

Okay. This gives you an idea of how the front moves through the reactor. This is the relative power along a channel. This is the bottom of the core. This is the top of the core. (Indicating.) If we look at say 240 seconds, we see that initially the power is quite flat. Then if we look at later times, this is 249 seconds, we see that of course the power is no longer flat.

The power is quite peaked at the bottom of the core because that's where the slug has entered. So at the bottom of the core, it's becoming critical and where the power is increasing rather than uniformly through the core. This is another reason why you need a spatial representation in your neutronics model.

If we look now at the radial power distribution, this happens to be at 260 seconds. These numbers are the relative power in each assembly.

DR. BANERJEE: So this is the second peak, not the first.

DR. DIAMOND: Yes. Right. This is at the second peak. It doesn't matter. You would see the same effect at other times. The effect that I wanted to show is that these bundles, these fuel assemblies that have the low burn up are the ones that have the relatively high power. Again, you see the importance of having to have that spatially dependent calculation. You can see how the power is down, up, down, up depending on which --

DR. BANERJEE: What are the units for the power here?

DR. DIAMOND: This is just relative units.

DR. BANERJEE: In terms of, are they twice normal operating power or what does that mean?

DR. DIAMOND: No. The average here is 1.0.

DR. BANERJEE: Okay.

DR. DIAMOND: Here is a graph of void fraction versus axial position at different times. Again, if we look at one particular time here, 289 seconds, we see in this particular channel that we have a little bit of void formation at the bottom of the core. That's the hot spot. If we look at later times, for example 291 seconds, we see that the void has shifted further down and has increased in this particular case. But these void fractions in this case are quite low.

CHAIRMAN WALLIS: But still doesn't that have quite an effect on the neutron balance?

DR. DIAMOND: Yes. It certainly does. It's also the result of the fact that we have a very low flow in the reactor.

DR. BANERJEE: But this is much later than the power peaks.

DR. DIAMOND: Yes.

DR. BANERJEE: So they are just giving you negative reactivity later on, shortly after that.

DR. DIAMOND: Right. But again this is in a very small fraction of the core.

DR. BANERJEE: Did you use the negative void co-efficient here, or is it negative?

DR. DIAMOND: Yes. The void co-efficient is negative. So any void formation is --

DR. BANERJEE: It will shut it down.

DR. DIAMOND: Yes. This shows you the average boron concentration in each of the assemblies.

DR. SCHROCK: It doesn't mean axial average.

DR. DIAMOND: It is averaged axially. So it is for a particular radial position.

CHAIRMAN WALLIS: This is in the liquid phase or it takes care of the voids.

DR. DIAMOND: It's in the liquid phase.

DR. BANERJEE: There is no void at this time.

DR. DIAMOND: Right. There is very little void in this particular case. But what it shows is of course that there is a radial distribution of boron concentration. The reason for that is that if you look for example at these three fuel assemblies here that have the highest power level, we see that it has the lowest boron concentration. What's happening is that where you have more power you're sucking up the diluted water faster. So what you have is an autocatalytic type of reaction here. That tends to feed the power.

CHAIRMAN WALLIS: So if you avoid formation rapid enough, you'd be expelling the boron at the bottom.

DR. DIAMOND: Yes. Well, in this particular case, you don't get void in these assemblies because the flow rate is a little bit higher. You get the void in the assemblies where the flow rate is lowest.

DR. BANERJEE: This is natural circulation.

DR. DIAMOND: Yes. But we imposed a flow rate at the --

DR. BANERJEE: At the boundary conditions.

DR. DIAMOND: At the inlet plenum.

DR. BANERJEE: So this is a distribution effect.

DR. DIAMOND: This is a distribution effect. Okay. So that gives you an idea of the complex physical phenomenon that are taking place there. As I said, this first calculation that I wanted to show you was really to compared the detailed three-dimensional calculation with the lumped point kinetics calculation.

In that calculation, the Framatome calculation and remember this is not apples and apples because they're model was actually Crystal River which was very similar but it's a different core than whatever cycle of TMI we were using. So it's not exactly apples and apples. Anyway, the peak reactivity in their calculation was about $1.2. In our case it was about $1.02. This is a typo here. It should be $1.02.

Peak power in their case was about 83 percent occurring about six seconds after dilution. In our case it was a little bit lower. Similarly, their peak enthalpy was 69 calories per gram. Of course it's difficult to estimate that when you're doing a lumped parameter calculation. When you're treating the entire core as a single unit, it's hard to say what the peak is within the core. Anyway, their estimate was 69. Our calculation was 37. That was the peak enthalpy.

DR. BANERJEE: That's the hottest channel.

DR. DIAMOND: The hottest axial position in the hottest channel.

DR. BANERJEE: Hottest axial.

DR. DIAMOND: Yes. So it's the hottest

node over the entire reactor. Whereas as I said, there are 24 axial nodes and one radial node per fuel assembly.

DR. BANERJEE: What time does that occur actually? 13 seconds is after the dilution starts. Is that right?

DR. DIAMOND: No. That's the peak power. The peak enthalpy occurs much later than that. If you recall that the peak enthalpy occurred at that second enthalpy peak.

DR. BANERJEE: Why does that happen?

CHAIRMAN WALLIS: To integrate.

DR. DIAMOND: Yes. Because enthalphy is an integral. So even though the power came down after the first power pulse, enthalpy is an integral. There is some heat transfer out of the fuel. So it's a question of the energy deposition less the heat transfer out of the pellet. The net result is that it occurs not after the first peak but later in the event.

DR. BANERJEE: So the power pulse is so sharp in the first case that when it is integrated it doesn't --

DR. DIAMOND: Right. Remember that when we examined that curve it was an increase of only about 20 calories per gram after the first peak and then 37 calories per gram after the second peak. Sporadic voids. And here the core return subcritical 45 seconds after prompt. In our case it was 24 seconds after prompt. So the calculation generally with the point kinetics model seem to be more conservative then our calculation.

DR. SCHROCK: You have a small difference in beta shown between those two calculations.

DR. DIAMOND: Yes.

DR. SCHROCK: I presume that's because you've weighted the beta in your -- calculation somehow to reflect some plutonium.

DR. SCHROCK: No. The beta that we calculate is the beta for that beginning of cycle condition at TMI. So it's based on the fuel in that particular reactor.

DR. SCHROCK: Which has some pleutonium.

DR. DIAMOND: Yes. It has a considerable amount of burn up.

DR. SCHROCK: Right.

DR. DIAMOND: The average burn up in that core at beginning of cycle is probably around average.

DR. SCHROCK: If anything it's surprisingly high, that value of beta.

DR. DIAMOND: No. I wouldn't say it's surprisingly high. I'm not surprised.

DR. SCHROCK: Well, if you had much pleutonium.

DR. DIAMOND: Well, if you go back to here, you have a burn up of 30 gigawatt days per ton. So you do have pleutonium here. But here you have essentially fresh fuel. So you have a mix.

DR. SCHROCK: Yes. You have a mix. Somehow you're weighting beta. You get a beta core wide.

DR. DIAMOND: Yes. And it's weighted,

DR. SCHROCK: What's the weighting at joint flux?

DR. DIAMOND: The weighting in this case is a volumetric weighting.

DR. SCHROCK: Volumetric weighting. So I guess we'll get another look at that when we review PARCS. That's a feature of PARCS that's used.

DR. DIAMOND: The PARCS calculation can put in a different beta for each fuel assembly. In this case, we used an average beta. But it can have a different beta for each assembly. That's not a problem.

DR. BANERJEE: So remind me beta is

related to the kinetics.

DR. DIAMOND: Yes. Beta is the delayed neutron fraction. A smaller beta as you get with pleutonium means that you have less delayed neutrons. Therefore, the control is a little bit more sketchy.

DR. BANERJEE: Right.

CHAIRMAN WALLIS: Within a rapid transient I thought it was the -- You have to look at the distribution of the beta among the different precursors. It's a really good answer.

DR. DIAMOND: Well, as I said, PARCS enables you to put in the appropriate beta for each fuel assembly which takes into account the burn up in that fuel assembly and therefore the distribution of material.

CHAIRMAN WALLIS: What I'm saying is today neutron fraction is an average over a lot of different precursors each with a different time.

DR. DIAMOND: Yes.

CHAIRMAN WALLIS: There's a rapid transient. It's the ones with the long time that matter most to something. You don't just take the average. Do you? I'm trying to remember how this works.

DR. DIAMOND: There are actually six

groups of delayed neutrons. The beta that we show there is actually the sum of those six groups of delayed neutrons.

CHAIRMAN WALLIS: Rapid transient, it's the slowest group or something. It eventually ends up dominating. Doesn't it?

DR. DIAMOND: Well, in addition to the delayed neutron fraction you have to specify the delay time for the delayed neutron to come out.

CHAIRMAN WALLIS: That's right.

DR. DIAMOND: Of course those with the shortest delay times are most important for fast transients, and those with the longest delay time are more important when you're looking at a LOCA for example.

CHAIRMAN WALLIS: Yes.

DR. DIAMOND: Okay. So this is now the second type of transient that I want to present to you. This is a calculation based on our best estimate of what the inlet plenum boron concentration would be based on Professor diMarzo's model of mixing.

CHAIRMAN WALLIS: Why doesn't it go to zero?

DR. BANERJEE: The front meets that back.

DR. DIMARZO: No. Because the regional

slug here proposed by Framatome which generates those two curves, it doesn't go to zero.

CHAIRMAN WALLIS: I'm just saying that in your spiel you talked about percent dilution 100 percent, you had this pure water --

DR. DIMARZO: Okay. In the Maryland experiment, we go to zero.

CHAIRMAN WALLIS: Why?

DR. DIMARZO: The Framatome experiment does not go to zero. I have an overhead here.

CHAIRMAN WALLIS: Okay. I guess I'm confusing the two. There has been mixing in the real phase.

DR. DIMARZO: Yes. Framatome gives you an initial slug in the steam generator.

CHAIRMAN WALLIS: Okay.

DR. DIMARZO: Then they proceed to mix it that way. I proceeded to mix it my way along that model.

DR. BANERJEE: But you have only two mixing mechanisms. One is at the front and one is at the back. Right?

DR. DIMARZO: The mixing depends on --

DR. BANERJEE: How does it not go to zero? Otherwise you get some smoothing.

CHAIRMAN WALLIS: I think it never was there or anywhere except in Maryland.

DR. DIMARZO: (Away from microphone.) This is what the initial slug looks like.

CHAIRMAN WALLIS: It never went to zero.

DR. DIMARZO: (Away from microphone.) It never went to zero, no. Framatome mixed it somehow and got this dashed line. If you take this slug considering what it is in the scenario and you move it appropriately to the steam generator upper plenum and through the pump according to where it is you get this. (Indicating.)

DR. BANERJEE: So that time is actually space. The distribution of the slug in space. Moving at some velocity. Right?

DR. DIMARZO: Exactly. This is the original slug in space.

DR. BANERJEE: So why is that sloped to begin with?

DR. DIMARZO: That’s because the scenario prepares the slug in a certain way that results in that.

DR. BANERJEE: How does the scenario prepare it?

DR. DIMARZO: It’s very complicated.

That's the part that we didn't present here.

DR. BANERJEE: I see. So you are already assuming part of it is mixed and so on.

DR. DIMARZO: Yes.

DR. BANERJEE: What part of it is mixed already in front? Is that in the pipe rising up?

DR. DIMARZO: That is the pipe rising up to the pump. The steam generator upper plenum is -- around here and then all this is in the steam generator. That's the slug that you can see there.

DR. BANERJEE: It would be interesting to see how you arrive at that.

CHAIRMAN WALLIS: It would be interesting to see how certain you are about that.

DR. DIMARZO: (Away from microphone.) And the pump is totally borated in this particular -- So now you remove this from the pump and move all this through the steam generator upper plenum and the pump.

CHAIRMAN WALLIS: It's really suspicious to me that it has all these sharp corners.

DR. DIMARZO: The sharp corners are --

DR. BANERJEE: Component changes.

DR. DIMARZO: (Away from microphone.) That's the way -- drops initially. That scenario we did not -- It's the result of hours of operation but we didn't do that scenario. We could simply find it and say we have a slug in the steam generator.

CHAIRMAN WALLIS: Right. You wouldn't like that.

DR. BANERJEE: Right. It seems that there's already a lot of credit taken for various things in generating that.

DR. DIMARZO: (Away from microphone.) One case that we can easily do and that we don't have any problem is to start with causing a LOOP like this. That's not a very major difference --

DR. BANERJEE: The reactor would probably go back.

DR. DIAMOND: No. He means start that in the steam generator. You can't have that in the core. You're correct.

DR. DIMARZO: Exactly. Absolutely. But we should take a square wave in the steam generator and move it along. You could probably take the one that you have there without the Marino diMarzo thing and because the front end is sloped that would probably help you. Wouldn't it?

DR. BANERJEE: But you --

DR. DIAMOND: Yes.

CHAIRMAN WALLIS: Well, I guess when you

do the whole story you’re going to have to say where this curve came from and why.

DR. BANERJEE: The critical part is that front slope I guess.

DR. DIAMOND: Yes.

DR. DIMARZO: And since you have established by now that in the natural circulation the pump isn’t deborated if we passed a step through the pump --

DR. BANERJEE: Well, even if we don’t take credit for the pump, what’s happening is you’ve already got a slope there. That would be interesting to know.

DR. DIMARZO: Yes. Because this slug is sitting. There’s a little bit of -- that vertical leg like we discussed before.

DR. BANERJEE: So you’ve already taken credit for that.

DR. DIMARZO: That’s what he said. I didn’t take credit for it. That’s what we were given.

DR. BANERJEE: Who gave you that?

DR. DIMARZO: This is the owners group.

CHAIRMAN WALLIS: Well, you ought to do it yourself.

DR. BANERJEE: Are you going believe the

owners group?

DR. DIMARZO: That's the point. We could do a curved slug which I was just thinking about --

CHAIRMAN WALLIS: I think you should.

DR. DIMARZO: To the steam generator and pass it through. The question is this. If I go for a scenario of a square slug in a natural circulation scenario, it would have to be pushed up into the steam generator before I start.

CHAIRMAN WALLIS: Yes.

DR. DIMARZO: That slug will go through the steam generator upper plenum and through the pump before reaching. That's no problem.

DR. BANERJEE: Whatever it takes.

DR. DIMARZO: It will look more like probably going much slower in here and then going up again like that. (Indicating.) It would be --

CHAIRMAN WALLIS: But for regulatory purposes, you might want to make some conservative assumptions about that slug. That might lead you to conclusions that you didn't particularly like.

DR. DIMARZO: I was just trying to make that case.

CHAIRMAN WALLIS: Well, I think that when you make a presentation eventually to the full committee you're going to see what was the origin of that curve you just showed us and how secure it is, that one with the shape of the slug, the distribution of boron in the slug.

DR. DIMARZO: I think for simplicity it would be much more practical to start with the square slug.

CHAIRMAN WALLIS: Well, then that might not be tolerable though in terms of the transient.

DR. BANERJEE: No. He's saying it would be if you allowed him mixing in the plenum and in the pump.

CHAIRMAN WALLIS: In the steam generator pump.

DR. DIMARZO: Yes.

CHAIRMAN WALLIS: Okay. Well, maybe you need to do that too.

DR. DIMARZO: That would be more like another case bounding this.

CHAIRMAN WALLIS: Okay. So you have some more work to do.

DR. SCHROCK: Could I bring up one point here? The power distribution in the core is quite interesting. Could you compare it to the power distribution in the steady state operating condition?

DR. DIAMOND: You're talking about this particular curve.

DR. SCHROCK: Yes.

DR. DIAMOND: Let's see.

DR. BANERJEE: Is it a factor due to less boron in those channels that you get such high powers?

DR. DIAMOND: First of all, let me explain that this is the axial average.

DR. SCHROCK: Yes. You said that before.

DR. DIAMOND: So this number may be higher at some particular axial position. This is higher than one would expect during normal operation. But it's not a crazy number.

DR. SCHROCK: No, no.

DR. DIAMOND: It's only three times the average.

DR. SCHROCK: I'm not saying it's crazy at all. I'm just interested in seeing how much distortion spatially occurs in the power distribution as a result of this kind of transient. It's pretty large.

DR. DIAMOND: But this core already has a power distribution distortion because of the presence of control rods. Look at this. 0.246. I mean that's only because there's a control rod there. Even in the center of the core, 0.51 is a distortion.

DR. SCHROCK: Well, it's on the edge of the core too.

DR. DIAMOND: No. Even at the edge it's much too low.

DR. SCHROCK: It's too low. I agree.

DR. DIAMOND: So this entire core is already distorted by virtue of the control rods.

DR. SCHROCK: Down here you have one that's near the edge that's 2.1.

DR. DIAMOND: That's right.

DR. SCHROCK: What would that be in the operating steady state?

DR. DIAMOND: In the steady state, it might be 1.5. But in the steady state you wouldn't have 1.5 here and 0.2 here. You wouldn't have such a severe gradients.

DR. SCHROCK: But when you're looking for potential core damage, are you looking at that element or are you looking at some average?

DR. DIAMOND: You're looking at all of the axial positions within this fuel assembly.

DR. SCHROCK: That particular fuel assembly.

DR. DIAMOND: As it turns out, yes, this

assembly and this assembly. I don't know which of these two assemblies and which axial level has the highest pellet temperature and therefore enthalpy. But it's somewhere at the bottom of the core, maybe about a foot above the bottom of the core and it's in one of these two assemblies.

But that's what this calculation does for you. It looks throughout the core at where you have the hottest fuel rod. I should also say --

DR. SCHROCK: And it's important that it gives you something quite different than the picture you would have if you made the assumption that the power distribution in the transient is the same as the power distribution in the operating study state.

DR. DIAMOND: Correct.

DR. SCHROCK: It might be much worse.

DR. DIAMOND: Yes. Primarily by virtue of the axial distortion but also because of the radial distortion.

DR. SCHROCK: Yes. Thank you.

DR. DIAMOND: Okay. So this next calculation that I wanted to show was again with our diMarzo curve which we're saying is our best estimate at the moment of what the boron concentration would look like based on a restart of natural circulation.

In this particular calculation that I'm going to show, the dilution starts at 100 seconds rather than 200 seconds. As you can see the change in boron concentration is from about 2500 to below 500 ppm. It's a dramatic change in boron concentration, an enormous change.

But in this particular case, we're starting from whatever the shut down condition of the reactor is. We're not starting from zero reactivity as I described for the previous calculation. In this particular case, the power peak is between 300 and 350 percent. In the previous case if you remember the power peak was down here at about 70 or 80 percent. So that initial power spike now is quite a bit larger. It's also narrower. But it's quite a bit higher.

DR. BANERJEE: That's also because you started from a much lower, I mean, the thing is completely shut down and you have to bring it back up. Right?

DR. DIAMOND: Yes.

DR. BANERJEE: If you start from zero reactivity this would just go.

DR. DIAMOND: Well, starting from zero that would be different.

DR. BANERJEE: That would be a big bang.

DR. DIAMOND: That would be different. The point is that this is now our best estimate calculation.

DR. BANERJEE: So the conditions are different between the two rods.

DR. DIAMOND: Yes. But this is meant to be the more realistic condition now, starting from the shut down condition and using the diMarzo curve.

DR. BANERJEE: What was the logic for the other one, zero reactivity?

DR. DIAMOND: Because the other one we wanted to see the differences between the Framatome point kinetics calculation and a spatially dependent calculation.

DR. BANERJEE: What was their logic to start from zero?

DR. DIAMOND: Because when you're using point kinetics that's how you're going to, the code easily starts from zero reactivity.

DR. BANERJEE: I see. It was a matter of convenience.

DR. DIAMOND: Yes. A matter of convenience, right.

MR. SCOTT: This is what I mentioned. When BNW got 90 calories per gram, that was in a range where there was some concern particularly for a high burn up fuel. So we wanted to see for a very similar case what we would get with 3-D PARCS.

DR. DIAMOND: So this is the power trace. Again, here we're starting from very low power. This is what it looks like on a logarithmic scale. Now, if we focus on a shortened time scale from 130 to 190 seconds, this is the power pulse here. (Indicating.) It actually goes up to about 330 percent and oscillates. This is what the peak fuel enthalpy looks like.

Again, we have a situation where the enthalpy rises due to that initial power pulse. It goes from about 14 to 37. It's about a 23 or 25 calorie per gram increment during this initial time. Then eventually it goes to its peak value of about 70 calories per gram.

DR. BANERJEE: And that's because your power pulse is so sharp. That's really the reason because you're not getting much enthalpy in the power pulse.

DR. DIAMOND: That's right. The pulse is very sharp.

DR. SCHROCK: The second peak is not has high but it's a broad peak.

DR. DIAMOND: Yes. It’s a broad peak. This initial increment here is the integral of that power trace essentially. Of course there is heat transfer because this is taking place over about a second. If you want to look at fractions of a second you can look at --

DR. BANERJEE: How much credit does that heat transfer out at that point? Suppose your gap conductors were wrong or something. What would happen? Is there 150 percent of the heat being lost or ten percent or one percent? What’s the number?

DR. DIAMOND: That’s a good question.

MR. ROSENTHOL: Your fuel rod time constant is eight, nine, ten seconds.

DR. DIAMOND: There is. In other words, if we assumed an -- reaction would this be 40 or would it be 50?

DR. BANERJEE: Right.

DR. DIAMOND: And I think that it would be closer to 40 here. There is some heat transfer but since the time constant for heat transfer is on the order of a couple of seconds it isn’t that much.

Okay. So if we’re looking at this first peak as I say it’s an increment of about 25 calories per gram. This really shows the consequences that we're interested in.

DR. BANERJEE: So because there are so full power seconds in the pulse that you're getting away with this very low amount of energy deposition.

DR. DIAMOND: Yes. This is the basic physics of a light water reactor. That is when you give it a jolt, you get the Doppler that pulls it back. In this case, you're not only giving it a jolt, you're still pulling on it because the boron concentration is continuing to go down during this period here but you have not only the fuel temperature contributing to the negative feedback but also the moderator temperature and density.

DR. BANERJEE: And the void.

DR. DIAMOND: Yes.

MR. ROSENTHOL: Let me just add that when you did ejected rod calculations over the decades you again saw that it wasn't the initial pulse turned around by Doppler that gave you the enthalpy rise. It contributed to it. But it was the tail of the distribution that when added up gave you the enthalpy. So I'm not surprised at all by that.

DR. DIAMOND: Yes. That's right. This is still sensible power over here. (Indicating.) So after that initial rise the fuel temperature is still increasing, so the enthalpy is still increasing.

This is the maximum local void fraction in this particular event. So in this particular event we get some higher void fractions. But again it's only in very isolated parts of the reactor where the flow is particularly low and it is not sustainable. But it contributes to the overall --

DR. BANERJEE: Now, in that power pulse there must be very high local temperatures within the fuel. Right? I mean, if you get 1000 percent power pulse, it's going to vaporize some piece of the fuel somewhere.

DR. DIAMOND: There is a distribution of temperature within the fuel. We know that the distribution is skewed toward the outside of the fuel. As you burn up the fuel, it becomes skewed even more towards the outside of the fuel because there are more and more plutonium builds up at the rim of the fuel.

DR. BANERJEE: So you're taking the fact that there is a flux depression within the fuel in itself.

DR. DIAMOND: The fuel enthalpy numbers that I show you are average. In regulatory space, we always talk about the pellet average fuel enthalpy.

DR. BANERJEE: What's the highest

temperatures the fuel gets to?

DR. DIAMOND: Well, the average temperature here is still not that high.

DR. BANERJEE: The average temperature. But local.

MR. SCOTT: In the report, there are some numbers. This scenario is not in the report that we gave you because these are just new results. The number I was going to say was at high burn up the calories per gram that would cause some melting at the edge of the fuel pellet is 170 calories per gram, maybe 160 calories per gram. It's way up there. I don't know that the temperature here is --

DR. DIAMOND: Yes. But we're not talking about peaking factors that would get you up to those high fuel enthalpies. Certainly not in this case.

DR. SCHROCK: So, how expensive an effort is this? What is the cost of doing this for the calculation?

DR. DIAMOND: For doing this calculation? Well, the incremental costs. Harold has just given me a curve of inlet boron concentration versus time which includes assumptions about the one pump starting. I have a post-doc working with me who's name is on the cover page. I would say if he's around tomorrow he'll probably give me the results on Friday.

DR. SCHROCK: So, it's not a terribly expensive proposition to do this these days.

DR. DIAMOND: That's the incremental cost is not expensive. To get set up and have a beginning of cycle model --

DR. SCHROCK: That doesn't include cross section evaluation preparation and all that.

DR. DIAMOND: Right. That's another matter.

CHAIRMAN WALLIS: You've run a whole lot of scenarios. If Marino came up with different slugs and so on, you could run a whole lot more.

DR. DIAMOND: Not a problem, no.

CHAIRMAN WALLIS: You might think about what you need to do to complete the story.

DR. DIAMOND: As I say these are coupled RELAP calculations. I mean, even with the RELAP --

CHAIRMAN WALLIS: Are you going to complete the story or is everyone going to say that risk analysis makes it not a problem?

DR. DIAMOND: Well --

MR. ROSENTHOL: I would say we're trying to run the scenario that corresponded to the BNW postulate transient. In the preparation for doing that we said events that involve let's say cold shutdown really have nothing to do with GSI-185. We thought that inadvertently starting with pumps was so close to the transient at interest it's just one more operator error that we ought to include.

So then Marino and I are whispering at each other well should we run the pump start or we should do some square wave that is even a little bit worse than that or maybe we'll run both. Yes. The promise is that we'll do the one, two, three more mechanistic calculations to bring back to you.

DR. BANERJEE: What's the physical reason that you get such a change between the point kinetics and the distributed calculation?

DR. DIAMOND: Well, there are so many spatial effects here that are not taken into account in the point kinetics calculation. Point kinetics calculation assumes a certain average boron concentration versus time in the core. Whereas in the spatial calculation we're assuming that the boron slug moves in and the bottom of the core feels that effect of the diluted water first. Then the whole thing evolves.

DR. BANERJEE: So that's the reason.

DR. DIAMOND: Yes.

DR. BANERJEE: It smooths it.

DR. DIAMOND: Right.

DR. BANERJEE: The transient time of the boron. It's going to start going and then it just --

DR. DIAMOND: Right. The point kinetics calculation is meant to be somehow bounding. At least in the best of all worlds you would justify the point kinetics calculation by saying that it's bounding or conservative in some fashion. I think Framatome's rationale was that they claimed that their inlet boron concentration versus time was already bounding. Therefore, they could just apply that in the core and assume that the results for power and enthalpy would be bounding. But it's not only the axial effect as I explained. The core is so radially non-uniform that it's important to take into account that variation as well.

DR. SCHROCK: Does the RELAP5 calculation beta of 0.0065 come because that's the default number in RELAP5?

DR. DIAMOND: I have no idea where that came from.

DR. SCHROCK: I'll bet that's where it is.

DR. DIAMOND: It could be.

DR. SCHROCK: You get your 35 numbers.

DR. DIAMOND: Yes it is. You're right. I know what you're saying. All right. Let me just analyze this presentation. I said that 3-D analysis gives a lower energy deposition relative to the point kinetics. It's very important to observe that here the evolution of the energy deposition is much slower than in a rod ejection accident. In a rod ejection accident, the reactivity is inserted in 100 milliseconds, essentially the square wave which we're avoiding in this scenario.

Thermal-hydraulic feedback limits the fuel enthalpy during the boron dilution accident. The calculation that I showed shows an initial enthalpy increase of less than 25 calories per gram. There is some void formation sporadic. We haven't looked at the possibility of DNB. It may be possible in more severe cases however. That's not really the problem here. This core has already boiled. What we're really concerned about here is energy deposition.

I should also mention that we have some preliminary comparisons with a completely different code system. It's called BARS/RELAP5 which is Russian. Well, the BARS part anyway is a Russian code, totally different methodology. It models the entire reactor on a pin-by-pin basis. I didn't show any comparisons. It's one example of what we have done to try and understand the validity of our model. We've done a lot more than that. I think as you learn more about TRAC-M, you'll be learning more about the validity of the three-dimensional neutron kinetics within it.

A couple of items where when I generated this slide I thought there could be additional refinement and extension. One is mixing in the core. I think someone already mentioned that we don't have that of course. I think that would tend to smooth things out and make things less severe.

The non-uniform boron concentration at the inlet would be nice to have but of course that's a difficult problem. When I put this on the slide here "the effect of turning on pump" I didn't realize that I would be making a commitment to have a result by Friday.

CHAIRMAN WALLIS: I think this non-uniform boron concentration would be worth while to try something on it. Try half of it here. Instead of putting uniform, try some sort of a distribution because you've already shown that there's a lot of variation between challenges. If you have much less boron in some place, you know that's a much more reactive place.

DR. DIAMOND: Right.

CHAIRMAN WALLIS: The critical reactor is here and the rest of it is like a reflector or something. There's a certain region which is -- So if you have a certain region that has much less boron than other regions, you know that's a critical thing. I would suspect that non-uniform boron concentration would give you higher powers. And there will be deposition in that particular area which might make things look worse. The question is what the regulators do with that assuming uniform boron concentration may be non-conservative.

DR. DIAMOND: Well, certainly with the pump on.

CHAIRMAN WALLIS: That may reunify things.

DR. DIAMOND: Then one could argue that it's conservative.

CHAIRMAN WALLIS: We showed from Maryland that there's a lot of variation in the down-comer in the boron concentration. So I would think that you could just run a calculation and instead of taking uniform concentration take an extreme case where half of it is zero and the other half is the rest or something.

DR. DIAMOND: Okay. Then I have to change my answer to the question asked of me before of how easy is it to do these calculations. In order to do that calculation, then I would have to represent half of the core rather than an octant so that I could have half of it at zero.

CHAIRMAN WALLIS: Okay. So there's a problem.

DR. DIAMOND: If that's the change.

CHAIRMAN WALLIS: Maybe what you can do is a symmetrical non-uniform distribution.

MR. ROSENTHOL: Let's think about it.

(Inaudible.)

MR. ROSENTHOL: We also know that there's very effective mixing in the lower plenum. Right?

DR. DIAMOND: Yes.

MR. ROSENTHOL: Surely very effective when the -- this would be a natural circulation case. There's supposed to be very good mixing in the lower plenum by design. It's one thing to do a variant and another one a --

CHAIRMAN WALLIS: It would be how sensitive your results are to the mixing in the lower plenum. So think about how you might do it and don't just not do this because it might give you an answer you might not want to see.

MR. ROSENTHOL: No. But we should do something that's reasonable.

MR. SCOTT: But, Jack, is Froude going to give me money to spend on ten to the minus six accidents?

MR. ROSENTHOL: Yes.

DR. BANERJEE: Is it a ten to the minus six accident?

MR. ROSENTHOL: It was estimated that the scenario that we're talking about is of the order of ten to the minus five.

MR. SCOTT: But that was for all small breaks. If we get it down to the small breaks that can produce these kind of boron slugs, it's going to be lower. If you turn on the pump, it's going to be lower.

CHAIRMAN WALLIS: You're going to make the whole thing go away by means of risk analysis.

DR. BANERJEE: This break is not too big so it's much more likely than a large break.

MR. ROSENTHOL: Yes.

MR. SCOTT: But I think Vander Molen already assumed he knew what the percentage of S-2 size breaks were. That's part of the risk numbers to see the core damage function frequency.

DR. BANERJEE: Ten to the minus five is the number that comes out of this.

MR. SCOTT: Yes. Assuming that size break, yes.

MR. ROSENTHOL: Well, the way I see it is we've been guided to do a modest amount of additional sensitivity studies that could put this to bed deterministically and somehow would be more satisfying than appealing to risk numbers. I think that we're close enough to it that we need to do some more work.

MR. MYER: This is Ralph Myer from NRC Research. I just wanted to comment on what you would need to do to the fuel to start getting into trouble. You're going to have to roughly triple that fuel enthalpy number and get that fuel enthalpy in within 20 milliseconds before you're going to have a situation where you crack the cladding and disburse any fuel. So if you can't get 60 or 100 calories per gram in there in under about 20 milliseconds, you'll have benign fuel damage.

CHAIRMAN WALLIS: So, you're not concerned about the eventual peak. You're only concerned about the initial rise right in the beginning there.

MR. MYER: That's correct. The eventual

peak may in fact cause the cladding to crack. But unless you can insert energy quickly in high burn up fuel, it won't even happen at all in low burn up fuel, you need the energy in there quickly so that the fission gas bubbles on the grain boundaries will blow the fuel out the crack.

DR. DIAMOND: Steady state fuel enthalpy on average for the reactor is about 45.

DR. BANERJEE: This is based on ppm.

MR. MYER: No. It's based on test data from CABRI in France and NSRR in Japan, both of them.

MR. BOEHNERT: Did you say 200 milliseconds? What was the time?

MR. MYER: 20.

MR. BOEHNERT: 20?

MR. MYER: 20.

MR. BOEHNERT: Thank you.

CHAIRMAN WALLIS: So you have a huge margin it looks like.

MR. MYER: Right.

MR. ROSENTHOL: Well, that was the reason that I wanted to make the comment because with small changes you're not going to get that.

CHAIRMAN WALLIS: I think if you assume something about very poor mixing in the lower plenum and you actually allowed it to be a very diluted piece of slug as a constant part of the course. You might be able to get a much more extreme initial rise.

MR. ROSENTHOL: Right.

DR. DIMARZO: If I may make a comment. The reason why we didn't go to the extent of making square slugs and so forth was because our understanding was exactly of that nature, in other words, whether we tweak that scenario a little bit isn't going to make that kind of a change.

CHAIRMAN WALLIS: I don't think we were just talking about tweaking it.

DR. DIMARZO: If you start making a front that's very sharp, yes.

CHAIRMAN WALLIS: Then I think you may well get into trouble. You should. You should go there and then figure out why that's not a good assumption or something. You should go there. You shouldn't just not go there because you might get an answer you don't want.

DR. DIAMOND: Actually a square wave at the bottom is really not a square wave to the core. It's a square wave to the first node.

CHAIRMAN WALLIS: Right.

DR. BANERJEE: There's a smearing effect.

DR. DIAMOND: Yes.

DR. BANERJEE: There's always going to be some smearing effect because it's not like a rod ejection which was a bang.

DR. DIAMOND: That's right.

CHAIRMAN WALLIS: Well, Jack, do you have enough to know where you're going from here and what you should come back with in a month or two?

MR. ROSENTHOL: Yes. Thank you for hearing the side on the subcommittee level because it will help us when we come back to you again. Then we will go to the committee.

CHAIRMAN WALLIS: I expect you'll get the usual comments from the consultants too which should be helpful.

MR. ROSENTHOL: Right. But what I'm also hearing and actually it was Marino's idea again and that is that rather than suffering through years of thermal-hydraulic analysis the idea was let's do something fancier on the physics side and see where we stand. It looks like we have a fair amount of margin. I mean, whatever the answer is I think we've done good work. What you're saying is (1) we ought to do some more pessimistic cases to make sure that we've bounded this situation and (2) when we come in to tell the story we should tell the story of the scenario, the evolution and tell the story better.

CHAIRMAN WALLIS: Yes.

MR. ROSENTHOL: But conceptually if it all bares out, it seems like a satisfactory way to go to you. Yes?

CHAIRMAN WALLIS: Yes.

MR. BOEHNERT: Yes. I think so.

CHAIRMAN WALLIS: So are we ready to adjourn? Does anyone have a burning desire to --

MR. BOEHNERT: I was just going to say you're going to report to the committee about this issue.

CHAIRMAN WALLIS: It will be pretty short.

MR. BOEHNERT: Pretty short, yes.

CHAIRMAN WALLIS: Okay. Thank you. Off the record.

(Whereupon, the above-entitled matter concluded at 5:23 p.m.)