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UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION
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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS (ACRS)
SUBCOMMITTEE ON THERMAL-HYDRAULIC PHENOMENA
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WEDNESDAY
JULY 17, 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
THOMAS S. KRESS, Member
VICTOR H. RANSOM, Member

ACRS Staff:

SANJOY BANERJEE, Consultant
FREDERICK MOODY, Consultant
VIRGIL E. SCHROCK, Consultant
PAUL A. BOEHNERT

Also Present:

STEVE BAHIREJM RES
NORM LAUBEN, RES
JACK ROSENTHAL, RES
JOSEPH STAUDENMEIER, RES
AKIRA TOKUHIRO, RES
RALPH CARUSO, NRR
V. J. DHIR, UCLA

A-G-E-N-D-A

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

(8:30 a.m.)

CHAIRMAN WALLIS: Please come to order. This is a meeting of the ACRS Subcommittee on Thermal-Hydraulic Phenomena. I'm Graham Wallis, chairman of the subcommittee. The other ACRS members in attendance are Tom Kress and Victor Ransom. ACRS consultants in attendance are Sanjoy Banerjee and Virgil Schrock. We expect Dr. Fred Moody any moment.

For today's meeting the subcommittee will continue its review of the NRC Office of Nuclear Regulatory Research, draft Regulatory Guide DG-1120, Transient and Nuclear Accident Analysis Methods. And associated NRC standard review plan Section 15.0.2. That's the first of our tasks.

The second one is to review the RES thermal-hydraulic research program dealing with subcooled flow boiling phenomenon. 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 Boehnert is the cognizant ACRS staff engineer for this meeting. I am very happy to notice that Dr. Fred Moody has managed to make it through the badging procedure and is up here.

Welcome, Fred.

DR. MOODY: Good morning.

CHAIRMAN WALLIS: The rules for participation in today's meeting have been announced as part of the notices of this meeting previously published in the Federal Register on July 2 and July 15, 2002.

A transcript of this meeting is being kept 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 nor request for time to make oral statement from members of the public.

Now, this regulatory guide was being worked on when I joined the ACRS four years ago. We've had a couple of meetings on it. Again, they were a long time in the past.

It's become evident that the gestation time for regulatory guide is somewhat longer by not an insignificant factor than the gestation time of an elephant. We are hoping that this regulatory guide will be born in the near future. That's what I hope we will find out today. I'm very happy to welcome Joe

Staudenmeier to be the midwife.

Joe, you have a formidable array of consultants and committee members this morning.

MR. KRESS: We're ganging up on you.

DR. STAUDENMEIER: I think you outnumber the rest of us here.

Today I'm just going to go over the Regulatory Guide. The SRP is property of NRR and they didn't want that presented today although the changes made to it were less than what was made to the Regulatory Guide.

CHAIRMAN WALLIS: They are not going to present it ever?

DR. STAUDENMEIER: They've made the decision that it will be issued without anymore public comments so it will come to you before final issue. I believe the process is it has to go back through the ACRS before it's issued as final. Even though they didn't want it issued for public comment, they wanted to wait until the Regulatory Guide was ready for final issue before they issued the SRC.

CHAIRMAN WALLIS: So it's going to be twins then that will be born.

DR. STAUDENMEIER: Yes.

CHAIRMAN WALLIS: Thank you.

DR. STAUDENMEIER: On the title slide, the draft guide used to be 1096 but when it goes out for public comment again it gets a new number. That's why it's now 1120.

CHAIRMAN WALLIS: This will be its final number?

DR. STAUDENMEIER: Hopefully it's the final draft number. Hopefully this is the final draft that will go out for public comment and after this round of public comments it will be issued.

CHAIRMAN WALLIS: It will be 1.120?

DR. STAUDENMEIER: I don't know what the official -- I don't think it's been assigned an official Reg. Guide number yet.

In this presentation I just want to present the background in common of the Reg. Guide. I know there are some new faces here that haven't been through the presentations for this Reg. Guide before. Feel free to ask any questions for clarification.

The point of this talk is to go over more of the changes that have been made to the Reg. Guide as a result of public comments. Feel free to ask any information about the background.

CHAIRMAN WALLIS: I think you might give some of the background information.

DR. STAUDENMEIER: Okay. I have a little overview of it here.

CHAIRMAN WALLIS: Three or four people haven't seen it before.

DR. STAUDENMEIER: Okay. Mainly this Reg. Guide is to be used for analyzing events that are put in standard safety analyses for operating plants which most of the events are in Chapter 15, the so-called Chapter 15 events, but there are some other events that are in different parts of the standard review plan that also would be covered by this Reg. Guide.

CHAIRMAN WALLIS: Can you give us a few examples of what is in Chapter 15?

DR. STAUDENMEIER: Sure. Chapter 15 is just about any type of transient that would be analyzed for a plant like loss of feed water, pump trips, turbine trips, things of that nature. Events that wouldn't be in Chapter 15. There's a low-temperature over-pressure transients like station blackout which is something that may be analyzed with codes. That isn't really a classical Chapter 15 accident.

ATWS. LOCA is something that would be covered by this, although there is a specific Reg. Guide for best estimate LOCA. The development process of a code for LOCA would also come under this Reg. Guide.

MR. KRESS: Just about all of them.

DR. STAUDENMEIER: Yeah. Reactivity transients.

CHAIRMAN WALLIS: These are the anticipated operational occurrences? They are in there, too?

DR. STAUDENMEIER: Right. Any anticipated operational occurrences in there.

MR. BANERJEE: So everything which is the design basis like severe accidents are off site and everything else is on site?

DR. STAUDENMEIER: Yeah. Severe accidents aren't analyzed for safety analysis.

CHAIRMAN WALLIS: It seems sort of illogical. The only thing you really worry about is the severe accidents. They don't analyze it.

MR. KRESS: That's because the DBAs take care of everything else.

DR. STAUDENMEIER: By definition there's no risk in design basis accidents.

CHAIRMAN WALLIS: You weren't here last week but there was some discussion about whether we really needed a focus on design basis or whether we should focus on things that really matter like accidents which could actually harm somebody. Anyway, that's beyond today's conversation.

DR. STAUDENMEIER: I guess one thing that comes out of design basis accidents is the only reason the plants are so robust in severe accident situations is because they were designed for LOCA.

CHAIRMAN WALLIS: That's the whole -- yes, if you chose your design basis accidents wisely.

DR. STAUDENMEIER: Okay. I'm going to go over as part of this talk the background and need for the Reg. Guide, the contents of the original Reg. Guide, the response and public comments that we've gotten in response to public comments, new content and status and summary of where we go from here.

CHAIRMAN WALLIS: When you say public comment, do you mean anything other than industrial comment?

DR. STAUDENMEIER: There was one person that wasn't part of an industrial organization but he had recently retired from an industrial organization. There were no comments from the public at large, I guess.

CHAIRMAN WALLIS: That's too bad because we put out the draft Reg. Guide and it's supposed to protect the public. Yet, the only public who will respond to it are the people who want us to cut back on what they are being asked to do, not the other side. Maybe you could stimulate attention on the other side and get more of a balance.

DR. STAUDENMEIER: Yeah. I guess most of the people that would be interested in thermal-hydraulic codes don't follow Federal Register notices or come to NRC meetings. Else they thought it was good and they didn't need to comment on it.

CHAIRMAN WALLIS: Very exciting, yes.

DR. STAUDENMEIER: Okay. The background in need. This all arose out of findings or allegations at Maine Yankee about their safety analysis and findings made by some investigation teams that went up to look at this. As a result of that, one of the conclusions was that the NRC needed to provide guidance on code development for accidents and transient analysis.

And also guidance on reviewing methods for accidents and transient analysis because the review methods weren't documented in one place. It was more tribal knowledge that went along. People picked it up from the reviewers before them but it wasn't clearly documented.

CHAIRMAN WALLIS: When was that?

DR. STAUDENMEIER: Oh, '96. It was a long time ago. I can remember right after this happened sitting in a meeting with Dr. Ransom. I think he was a consultant for the State of Maine on this talking about safety analysis at Maine Yankee.

Okay. What we hope to come out of the Reg. Guide is to ensure sufficiency and consistency in the level of documentation and validation. That was to try to correct the fact that if you looked at different codes that had been developed and reviewed for essentially the same type of accident, there was a wide range of level and quality of documentation and a wide range of safety analysis reports written by the staff.

There just wasn't consistent quality or consistent standards set in the code development process or the code review process. As part of this have a documented process in place that could be followed by the industry that would give a standard set of content that they would be submitting for a certain type of analysis.

MR. BANERJEE: Were there some specific areas in this safety assessment team report that was cause for concern?

DR. STAUDENMEIER: I guess it depends on what you mean by concern.

MR. BANERJEE: Their statement is very broad so it's hard to get any meat out of it. What were the types of problems?

DR. STAUDENMEIER: For instance, the safety analysis at Maine Yankee. I know the transient analysis there's a main steamline break in particular. I think there was no real code assessment done to show that code was good for calculating what it was doing during the safety analysis.

The LOCA methods that they had were based on an early version of RELAP-5 and they had some numerical stability problems and behaved erratically when applied and weren't even able to run through all the whole break spectrum like they are supposed to and, as a result of that, Maine Yankee went through some contortions to try to justify what they did but it didn't follow what they said they were going to do when the LOCA methodology was submitted or what was written in the safety analysis report.

They didn't follow the procedure that was required of them for LOCA analysis. Part of it was because they are having lots of code problems. It just couldn't perform the way that they had represented that it would.

CHAIRMAN WALLIS: Actually, a lot happened since then because in '98 or something we started reviewing these codes. ACRS got involved and got dissatisfied with some elements of the codes. We tried to have an -- we did have an influence on the way in which this Reg. Guide was put together.

It was our recent experience with the codes that we had to review, particularly for best estimate codes which really are more demanding than the old type of codes. You have to be clear on what you mean by best estimate. Is it a good estimate or is it not, how good is it, and so on.

I think it's not just the Maine Yankee lessons learned. It's the codes of the future and are they going to have to meet the same standards as the codes of the past or are we going to specify something that is more stringent or more appropriate.

DR. STAUDENMEIER: Another driving, I guess, event outside of the Maine Yankee experience is the AP600 review for all of this. There was a wide criticism by the ACRS at the time about the documentation, Westinghouse's documentation of their safety analysis methods for AP600 and their assessment against experiments at the time.

CHAIRMAN WALLIS: We had a very long letter to the Commission on this issue a couple of years ago now.

MR. SCHROCK: Well, one of the things that has impressed me in these reviews is how we get to a certain point where some problems have been identified. At the end the approval of a code goes through but there are things left that presumably by agreement between staff and the industry are going to get resolved some place down the road. The documentation never seems to follow through on that. That kind of thing gets lost.

I can think of some examples in connection with AP600, for example. This is kind of a general point but I think it is an area that you've got to look at a little harder to see how you get the documentation into the right shape after a review is finished. What are the principles that are followed in declaring it really a final product.

CHAIRMAN WALLIS: Some of these are trivial matters. I remember AP600, just to take an example, but with other codes we got the same thing. We would ask for details of the code and we get some handouts and we look at them and it turned out that some of the equations were garbled. There was no assurance that they were ever fixed up. We were assured, "Oh, we'll fix that up," but we never saw a proper equation.

We have no knowledge aye or nay about whether the equation was fixed up or was not fixed up. Then there is always the question about if the equation was garbled, what was in the code. That we never even got a chance to look at. We don't really want to look at codes but somebody has to look at them. Those are minor points in a way but they may be symptomatic of something.

DR. STAUDENMEIER: Yeah, I agree. I know the AP600 thing. I don't know if it was a specific case you were talking about for their large break LOCA cobra track. I remember going back through their final documentation and auditing some of the implementations that they said they were going to do.

Obviously we didn't go through the whole list of REIs at the time because it was enormous. We did some auditing of their final implementation and the documentation.

That may not have been the code you were talking about but I think we try to some extent to make sure the final -- at least when I was in NRR we tried to some extent to make sure the final documentation was what it was represented that it was going to be.

MR. LAUBEN: Joe, Norm Lauben. I think all these things that are said are exactly correct. It started with Sanjoy's question, what prompted what we did. I think Jack and I were on the non-LOCA part of the team that investigated at Main Yankee and Joe was on the LOCA part of the team that investigated what went on at Maine Yankee.

As far as we were concerned when we were looking at non-LOCA things, the principle issue was assessment. We were concerned that the assessment base was rather small so the thing we looked at and saw that something like CSAU had developed a process by which you could do assessments. That was a strong part of assessment.

Then I think, Graham, your comments on some of the other codes indicated that there really wasn't a strong basis for the development part. Do you have the right things in the code to start with. That prompted us to put in the hierarchical methodology from SASM that Novak had developed. Novak's hand is in this a lot with both the assess and process and the CSAU.

I think that the fact that we could,

therefore, relate the development and the assessment through a logical process was something that we finally came up with in large measure because of the comments we received from a lot of people.

CHAIRMAN WALLIS: You mentioned assessment. I think that is the key part. I remember one of the reviews we made when the applicant had compared the code with two, I think, LOFT tests. We said, "Why is two enough? Why choose those particular ones? Shouldn't one probe more to see if the code is doing its job and be satisfied with comparison with two LOFT tests that look sort of okay.

MR. LAUBEN: This is exactly what -- when Jack and I were up at Maine Yankee we saw that -- we were looking at non-LOCA transients. It appeared to us that the assessment base for steamline break, as Joe pointed out, was seriously lacking. It was for a BNW plant and Maine Yankee is not a BNW plant.

You can hardly draw any relationship between the behavior of a BNW steamline break and CE. That was a focus that we had. We thought there was no process here. There's no standards for saying this is an adequate assessment base. That's a good bit of what our comments were.

CHAIRMAN WALLIS: It would perhaps help --

it's a bit late in the day -- if we had made a separate document in which we said, "These are the things we need to fix up. This is the problem we are addressing." Then you could see whether your document actually addresses the problems. Solves, resolves, or whatever the problems that you identified.

MR. LAUBEN: Right. But the thing you really need to do when you do a Reg. Guide, though, is make sure that it's not just addressing some narrow problems that you may have focused on in some investigation, but rather does it cover the entire subject you are trying to address in the Reg. Guide. That's really why this turns out to be a bit broader than what some of the problems were that we discovered at Maine Yankee that we felt we were addressing.

MR. BANERJEE: What were they using for the steamline break? Was it RELAP?

MR. LAUBEN: No, RETRAN.

MR. BANERJEE: A large steamline break?

MR. LAUBEN: Well, yeah. The spectrum of steamline breaks, right. You know, because a code has a name that doesn't necessarily mean it's bad for doing it. It's just that their assessment base we felt was pretty inadequate. Also depending on what the problems were, Len Ward was the other member of our team and he looked very carefully at the equations appropriate for what they were really trying to address.

MR. SCHROCK: This mention of RETRAN is something that I've been wondering about. You've got RETRAN mentioned throughout the draft guide. RETRAN is in kind of limbo, isn't it? Is it ever going to get out of it?

DR. STAUDENMEIER: What do you mean by kind of limbo?

CHAIRMAN WALLIS: Well, there's an original RETRAN which was approved. There was a RETRAN and then there was a new RETRAN that we had some problems with.

MR. SCHROCK: Right.

CHAIRMAN WALLIS: I think there is an old RETRAN which is approved.

MR. SCHROCK: The old one is better than the new one?

DR. STAUDENMEIER: No, I don't think it's better.

MR. SCHROCK: The old one just never got the same level of review as the new one I think is the truth.

MR. RANSOM: Well, taking a look at this

I think, Graham, you were talking about assessment. I think that to me in reviewing this Reg. Guide it tries to address a lot of the documentation and the history of the code which is very good and must be done and has in the past always led to a lot of problems.

One of the biggest ones, I think, is that the NRC through their development often times neglected funding of the documentation right from day one which should happen. That led to poor quality documentation in many of these code developments. Catching up later is always more difficult than, I think, doing it right at the time.

A lot of those things are helped by this Red. Guide, but the word that I have the biggest problem with is assessment. I think assessment, and everybody talks as if we know what that means, has addressed qualitative aspects of agreement with, say, physical phenomena but still falls far short of, say, quantitative assessment in the sense that you can actually say how well this does fit a certain situation, plus what are the uncertainties associated with that.

As near as I can tell by reading the CSAU documents, I can't see that it sheds a whole lot of light on that subject. I think that in the end they come off as biased to account for, say, phenomena that are not modeled or modeled incorrectly in codes.

Plus, you know, some uncertainty associated with the things that we do are in the code and should fit and know to combine those together to use in a licensing sense to me is still an open issue. I don't know but I hope maybe these meetings can help clarify some of that, for me at least. I see that as still a major problem.

MR. LAUBEN: Vic, I think you're right. I think CSAU -- there's two focuses on CSAU as I see it. One was to outlie a process, and the second was to show that you could apply the process in terms of uncertainty. It did not go into a great deal of detail on how you do assessment because it was assumed that the assessment base was already complete when the CSAU was done. We ran into the same problems that you are talking about in AP600.

People, I think, struggle with standards for assessment all the time. I don't know if we can put into words always what we all can competently agree is the right things to say about assessment. We tried to do a little bit of that in the Reg. Guide and do a lot more by reference.

I think you end up having to do a good bit by reference. What we tried to reference was some of our struggles we had with AP600. I'm not sure that's the best and it probably takes people a lot smarter than me to figure out how to do it in the best way possible.

CHAIRMAN WALLIS: Well, I think --

MR. RANSOM: I'm not trying to be critical. I'm just saying that I think this is still an open issue and I know people have struggled with it since 1973 with what do we mean by assessment and what are the measures for assessment.

CSAU did go a long ways towards a process -- putting in place a process for assuring that you have addressed the significant phenomena in an accident and that the code generally is capable, say, of modeling those.

The last loop or step in that, though, in which you come up with how do you use the code in the best estimate plus uncertainty in a licensing framework, I think, is still gray and somehow needs to be further closed, I believe.

One example is in the CSAU examples. They will have biases that are plus and minus and in the end you allow the lack of one to compensate for another. I just can't understand how that's a good philosophy.

CHAIRMAN WALLIS: I should point out to the new members of this group that Norm Lauben was the original drafter of this guide, I believe, and he was the one who presented it to us some time ago. One of the things we said is, "We don't like the way you're doing assessment," because it was done in an even more qualitative way at that time.

What I think the committee would like to see eventually is some more rigorous ways of saying now you've got these data points. What information do they tell you about the code in a qualitative way, which not sort of just looking at some graphs which present something that looks reasonably okay, but is more in a sort of Baysian form. What does this new information tell you about what you thought you knew before?

This guide which still looks much like the last one that Norm put together, I don't know quite what's happened. Maybe he'll tell us what happened since then. Still doesn't face that question of whether the more formal rigorous ways of doing assessment and evaluating uncertainties.

I would welcome sometime down the road

both the academic community developing these methods or the research community whoever can do it. Maybe we need a supplement to that guide of something down the road which says now we know how to evaluate and assess more rigorously and these are the methods which we will accept.

DR. STAUDENMEIER: Yeah, I agree.

CHAIRMAN WALLIS: But this is developing because even the applicant's are doing it with order statistic or something. They are actually applying methods which have some logical basis.

MR. LAUBEN: Graham, it ends up requiring that somebody do something like the CSAU application part again using different methods. That's not a cheap process.

CHAIRMAN WALLIS: No, it's not.

MR. SCHROCK: That's the comment I was going to make is that the major difficulty with the use of CSAU in the regulatory process is that the demonstration of it was very expensive. As I remember, it was on the order of a $6 million effort.

Extensive documentation and then, I think, even the code that was being assessed on that basis was found not to be in existence in a workable form in a very short period of time after that.

All of the follow-up uses of CSAU that have been reviewed in my experience with ACRS have been very shortened-up versions of CSAU and there are always arguments as to why the shortened versions are sufficient and give satisfactory results.

The Reg. Guide, I think, if you're going to protect yourself against this kind of use of CSAU methodology, you're going to have to be very specific about the requirements that you're going to impose here.

DR. STAUDENMEIER: We do have some guidance about what constitutes uncertainty analysis, I guess, but we don't have a specific answer that says if you do this, this, and this you'll get the right answer. I guess as you know there's been quite a few different methods for determining uncertainty throughout the nuclear safety world. I don't think there's any consensus that one stands up and above all the rest that everybody is going to be flocking to.

We did write the Reg. Guide so that things can be plugged in later as appendices when we have better answers for things and more specific guidance. We had planned on that in the future like specific guidance for different accident classes to go on in uncertainty. If we can get more done on uncertainty, that would be another good place to do that.

As part of the code assessment for the consolidated code and research we are going to be looking at more quantitative methods for assessment instead of the qualitative thing, the code looks close to the experiment and it's pretty good. We're going to try and get quantitative measures of the assessment and maybe that will lead us to some answers on giving better guidance in the future.

As you said, it's only -- the CSAU method has only been applied -- best estimate with uncertainty has been applied in two cases so far that have been licensed, Westinghouse with COBRA TRAC and GE for transient analysis with TRAC-G. I guess you could call them abbreviated in some sense.

I think there is justification for their abbreviations where they said, "We looked at this and we don't think this needs to be applied." They had reasons for why specific parts didn't need to be applied backed up by calculations or experimental assessment.

Even with their abbreviated method I think both of those were multi-million dollar efforts. The Framatone method, which is formerly the Seamans method, I think that is probably also a multi-million dollar method. I don't know what the status of that is. That will be the --

CHAIRMAN WALLIS: They're a lot cheaper because they just have to run RELAP a lot of times and you can do that much cheaper now than you used to be able to do it.

DR. STAUDENMEIER: That's true. You can do a lot more. Once you standardize your method and set up automated runs, you can certainly do uncertainty analysis a lot quicker now. Things that would take a day or so with very expensive craton back in the '80s you can get done relatively quickly on PCs now.

That's one thing that will maybe bring the use of best estimate plus uncertainty up to the forefront because you are no longer computation power limited in applying these things.

CHAIRMAN WALLIS: While we're on this background and need, it think it's appropriate we talk about some of the other ACRS concerns which constitute a need for you. We talked about assessment here and it's a very broad topic. We'll probably come back to it.

We were concerned about the cavalier way that sometimes basic equations were derived with the use of handwaving with Stokes or something. But the equation is actually used so that the most trivial equation that comes from first course and fluids for a very idealized situation so just decreed this will now be used for everything.

There's no discussion about why a method derived for simple geometry applies to the actual geometry of a reactor, for instance. It's just decreed we're now going to use it.

The other area where we had some problems was in the sort of scaling and range of applicability of correlation to equations or constitutive laws or whatever you call them. Someone derives an equation for nitrogen water experiment in an university for a Ph.D. thesis and then it gets used for steam water, high pressure, and bigger dimensions of a reactor.

How should the staff view this? Should it be thrown out? It's probably unreasonable to expect everyone to do all the experiments in full scale so how do you make the choice of whether or not to accept something when it seems to be used -- a recipe that seems to be being used beyond its range for which it was developed. I think those are two other key issues that we have with these codes.

DR. STAUDENMEIER: I thought the Reg.

Guide did address something like that where if you're going to use a correlation it has to be for the range of conditions you're using it in. Or if it wasn't originally developed in that way, you have to do some sort of assessment to show that it will be okay to use it in those conditions.

CHAIRMAN WALLIS: I think on the correlation the guy is quite good. I think we still might have a way to go if the ACRS applied the guide in terms of thoroughness with which the approach is laid out of the code rather than just saying this is the only way we can think of to model this so we'll do it and then going ahead and doing all this tremendously complicated correlation stuff.

Maybe if we wrote the guide we would say, "Well, you should really go back and say what was the influence of the assumption we made right up front on the answers that we got."

DR. STAUDENMEIER: I agree with you. I do think that type of stuff should be dealt with when you come up with your mathematical model that you're going to solve that you really have to have justification for why your mathematical model is adequate to model the situation.

That involves looking at all simplifying assumptions that you've made going from a detailed model to your simplified mathematical model and make sure that these assumptions still hold or to some good approximation and estimate of what the error is in dropping out these other terms that you left out when you've come up with your simplified model.

CHAIRMAN WALLIS: The approach of the applicant usually is to push all this over to the assessment process. I mean, they either do not present or they present very quickly as a basic approach and then they go ahead and say, "Look, here's a curve and here's some data points for LOFT or something or other. Therefore, it works. Everything gets presented in terms of the assessment process, whereas ACRS starting at the beginning of the document may have questions about the first page.

DR. STAUDENMEIER: I agree with that. A lot of people do push it out for assessment like someone may say, "Oh, we're going to assume incompressible fluid." Instead of going out and showing why it's okay to assume an incompressible fluid in this stage, they say, "We'll just show that it's okay with our assessment against test data."

That test data may not cover the whole range of conditions that it may end up getting used in. It can get carried on and used in situation where it shouldn't. I know one example where that happened with Westinghouse using MAP calculations for AP600 for PRA studies doing LOCA calculations.

Once you got over a break size of about four inches, the map results just were totally bad. It really had to do with incompressible fluids. You get depressurization rates too fast and MAP couldn't really handle it, along with some of its other two-phase models couldn't handle the situation. You're right. That type of situation really does need to be addressed up front.

CHAIRMAN WALLIS: That's why we used to be always encouraged the party assessment process should be the staff running the applicant's code and their own code and experimenting with it. If they have some query about whether or not something which is not modeled very well matters, then try it.

Test with your own code and the applicant's code and see if it matters. If it doesn't matter, there's no sense in trying to make it perfect because it's not appropriate. There are some things that matter.

MR. ROSENTHAL: This is Jack Rosenthal. Let me say to your last comment, we totally agree and that's what NRR is trying to do. Joe Staudenmeier and I were looking at his slides yesterday late and we thought that like slides 11, 12, 13 would spur a fair amount of discussion.

It may just pay to let Joe get on with some of his presentation so that we're all sort of thinking about the same thing at the same time. Much of this discussion will come up again. Then we can focus on the key issues of how much do you have to do in the assessment.

CHAIRMAN WALLIS: Fred had a point but, first of all, I think it's appropriate that we have this discussion now because Sanjoy asked the question of what's the need. What needs does this address. That's what we're talking about. I think if we have that in mind it will help us to assess whether or not it's okay.

My feeling is it's been through so much it would be very difficult to turn around now and say a major revision is necessary. I think it has been good. You're very patient, Joe, to stand up there. You can sit down. I think it would be good to talk about some of the reasons why we need a Reg. Guide.

DR. MOODY: This is just mostly to put things in perspective for me, but it sounds like all this discussion has roots in what has gone before. Is it not the purpose of this Reg. Guide to fix a lot of these problems? This is the main reason this is done.

CHAIRMAN WALLIS: That's right. That's why we wanted to identify some of the problems so we can look and see does it fix them or does it half fix them and do we need to go further.

DR. STAUDENMEIER: Okay. And to end with Maine Yankee lessons learned, what was decided is we needed a standard review plan in our Regulatory Guide.

CHAIRMAN WALLIS: It's very helpful to have something happen, and then learn lessons from it, isn't it?

DR. STAUDENMEIER: Yes. Okay. We first came to the ACRS in 1998, I think, with the first presentation of the Reg. Guide even though it had been put in motion before that. What we proposed is we wanted to address analytical methods for all events on a generic basis as much as possible, and address verification, validation, documentation, and quality assurance. There had been problems identified in all of those areas in past code reviews.

As part of this what was done was to generalize the evaluation model concept that was originally in place for LOCAs and extend it to encompass any type of transient or safety analysis. What we really wanted to do is lay down in this Reg. Guide a description of what constitutes an acceptable evaluation method or a development process that would lead you to an acceptable evaluation model for the transient or accident that you were working on.

MR. RANSOM: Joe, could you define evaluation model? In the old days we used to use that to mean Appendix K as far as LOCA-type analysis is concerned. Does it have a broader meaning?

DR. STAUDENMEIER: Evaluation model, yeah. It's really any model. I mean, it goes beyond just LOCA. People use their evaluation model definition for their transient analysis methods or things like that. What it encompasses is not only the code itself but the input assumptions of how you generate your input for going into the code.

MR. RANSOM: In particular we used to use best estimate versus evaluation model. I think the way it's being used here it includes the best estimate method.

DR. STAUDENMEIER: Yes, it would include a best estimate method. As I said, it goes just beyond the code. It goes beyond the way -- it goes into the way you apply the code so the methodology that you use in applying the code.

MR. LAUBEN: That's right. Vic, I think there was a mis -- well, when the ECCS rule was modified in 1988 the only evaluation model for LOCA was Appendix K. But in the revision in '88 you could do best estimate but the definition of evaluation model included both a best estimate evaluation model and the Appendix K evaluation model.

Even LOCA does not distinguish between best estimate and Appendix K, at least the rule. It's just that the idea sort of carried over in a lot of people's minds that evaluation model only applied to Appendix K. For this purpose we decided we are going to generalize the evaluation model concept which is simply, as Joe said, it includes all the things that go into your analysis.

In the old days in particular, there may be a lot of codes that go into one analysis because it was cheaper in the old days with computer time and so forth. You may have had to run a string of six or eight codes or six or eight little subcodes separately to get the answer that you wanted, whereas today we lump them all into one big code more often than not.

MR. BANERJEE: I have a question. Does the evaluation model include how you nodalize the system, for example, because the impression I get is that this is not sort of -- there's no conventional convergence testing done so you model the steam generator in this way, the pipes in that way, the core in that way. All this is some sort of folklore that has grown up around use of these codes. Am I right on that?

DR. STAUDENMEIER: There is supposed to be convergent studies that the vendors do when they put together their nodalization. The evaluation model does include the nodalization. We consider a change to the nodalization to be a change to the evaluation model.

If you look at Appendix K it specifically says that. 50.46 in Appendix K specifically includes the nodalization as part of the evaluation model because essentially you are really solving a different set of mathematical equations. When you change the nodalization you've added for each equation -- for each node you have equations you're solving so you've changed the number of equations you're solving.

There are supposed to be nodalization. They may not be as rigorous as formal convergent studies but they are supposed to be sensitivity studies applied to make sure that you are converged in space and in your time integration when you do those.

MR. RANSOM: So your results, are they supposed to be independent of nodalization?

DR. STAUDENMEIER: I think with two-phase codes I don't think you can get independent of nodalization. I think if you took TRAC or RELAP and went towards large number of nodes, I think you will run into Calvin Helmholtz two-phase instabilities that aren't damped out because you don't have discus trends and other things in there.

Specifically you cannot mean convergence in the classifical sense. Numerical convergence is you take the limit as you go to zero and see. CSAU, for example, addresses this problem. Admittedly in the old days you were probably driven somewhat by the limitations of the computer and the expense of the time that you could spend actually in solving a transient by how many nodes you might actually use.

The allocation of nodes between, say, steam generator core, and things like this, some sensitivities were done to find a nodalization which gave satisfactory results in comparison with LOFT or one of these experiments.

What CSAU says is that whatever you use in the way of nodalization for that assessment, you must use those same nodalization guidelines when you apply this to a nuclear power plant so there is a constant more or less. Admittedly there may not be a one-to-one mapping in all cases between these two. Generally the nodalization guidelines should be the same for the application as they are for the assessment process.

MR. BANERJEE: But why? The scale is different.

MR. RANSOM: The scale is different but the geometric scale presumably there should be some similarity.

MR. BANERJEE: But is that proven in some formal way that the scaling is correct?

MR. RANSOM: I don't think so.

MR. BANERJEE: Otherwise why have a set of differential equations you're solving? If you want to make a model and say this is a physical model for the steam generator or this and that throughout the differential equations, it makes no difference. I don't see the logic of having results which are at all dependent on nodalization.

MR. RANSOM: For example, it may be important to track levels in some cases like a small break in the steam generator where the boil-down level may be important. Nodalization is about the only way you can refine that level in the computer code calculations so you must find at least a satisfactory one.

Another good example I could give, and this is one you could take some issue with, but Marviekin critical flow experiments. If you do a course nodalization of the vessel much like you might use in the core of a reactor, you're going to get a terrible result because of numerical diffusion in both the energy and the mass. Rather fine nodalizations are used in those cases.

It's still a subjective matter of judgement. This is why assessment is still a qualitative sort of science as opposed to real quantitative in my mind. As much as I would like to see it tightened up, it's a difficult problem.

Another thing would be, for example, does it make any sense to have nodalizations that are finer than, say, one pipe diameter when you have ignored all the transverse gradience in the pipe to start out with. It's the averaging process you've used to drive the equations you use. You're going beyond the limits of those. Does that make any sense?

So there are these kinds of limits to which you can actually apply these models, I believe.

Some of that I would like to see addressed in time in something like a Reg. Guide but it's somewhat of a debatable issue.

If you talk to the French, they have another opinion in terms of, say, what well posiness of the equations and presumably you could then carry out, say, a refinement of the nodalization ad infinitum if you wanted to. I don't think that's necessary. It's not my opinion but these are issues which are not really settled.

MR. BANERJEE: So the evaluation model includes in some way a definition of what is an acceptable nodalization, however it's arrived at?

MR. RANSOM: Yes. And typically in the past the way this has been done is like the last time I reviewed Appendix K LOCA model upgrade the main problem they were having was in the core of their DMB correlation and their disperse flow heat transfer from boiling heat transfer correlation.

What they would do is specifically for the core nodalization they looked at 12 nodes, 18 nodes, 24 nodes and compared it to experimental data that was applicable for that case which in that case, I think, was THTF dispersed low-film boiling heat transfer, and came up with a nodalization that was adequate to predict the temperature profiles and dry-out profiles for that experiment.

For different parts of your model like the steam generator it may be based on having an adequate number of levels to track when you break natural circulation in the loop, or there may be various other constraints in other parts and nodalization studies will be done to make sure that you are adequately resolving the phenomena in those locations in the plant for the transient that you're looking at, transient or accident that you're looking at.

The same with time step studies. All the codes have some time step. They are not really smart enough to control time step automatically and make sure everything is resolved.

They don't look at truncation and do a truncation error analysis of the equations that they are solving. They look at rates of change of various things and some error measures for global iteration conversations but they don't really do classical convergence in terms of their time step control.

Part of this study would be making sure you have a small enough time step to resolve your time history and looking at time step sensitivity studies to determine what an adequate time step to calculate the accident that you are looking at.

MR. BANERJEE: So if I understand what you're saying correctly, the way I get it is that you are trying to capture certain types of physical phenomena which you think are important which is fine. And your evaluation model then would have appropriate nodalization so that it would allow you to capture those phenomena.

But you are not trying necessarily to solve the mathematical set of equations that you posed to begin with with their boundary conditions, initial conditions, and so on. You are simply trying to solve some integral representation which conserves mask momentum or whatever for a lump parameter almost description of the system with adequate nodalization to capture certain phenomena.

DR. STAUDENMEIER: Yes, I would agree with that statement.

MR. BANERJEE: Is that the philosophical basis? I'm having trouble with the philosophy actually.

DR. STAUDENMEIER: I think that's the philosophical basis because, as you know, the equations in these codes aren't really a well-posed set of differential equations. I mean, you look at the correlations and they are kind of like subgrid models and have discontinuities in them and things like that. It would really be hard to do formal convergence analysis I think.

MR. BANERJEE: Katarah uses bellpost equations with proper transition because they have to calculate a Jacobian.

DR. STAUDENMEIER: It depends on what you mean by proper transition. Proper transition may be the functional fit that made the code run faster in that case.

MR. BANERJEE: It could well be.

DR. MOODY: I'm reading on page 19 of this masterpiece here. This is the second paragraph. I thought it kind of addressed that.

MR. BANERJEE: Maybe it does.

DR. MOODY: In the middle of that paragraph under the heading, "Prepare Input and Perform Calculations to Assess Model Fidelity or Accuracy." In the middle of the second paragraph it says, "In particular nodalization and option selection should be consistent between the experimental facility and similar components in the nuclear power plant." I guess that's true. I guess it's just how you get from here to there.

MR. BANERJEE: That's really the problem, yeah. I don't see how you scale it. It's embraced by this structure.

MR. ROSENTHAL: Let me try this. Bear in mind that the Reg. Guide pertains to physics analysis and fuels analysis as well as thermal-hydraulic analysis to all analysis in chapter 15, anticipate operational occurrences and transients that Joe Staudenmeier talked about.

Let's take a physics example. Clearly in all cases you are solving Baltzmann equations. If you use a final mesh treatment, then you'll convert the Baltzmann equations into finite difference equations that are reasonably simple.

If you go to a coarse mesh treatment where there is one or maybe four nodes per assembly radially, then you tend to have -- you use different numerical approximations to the differential equations. You tend to do more arithmetic at each node and fewer nodes.

Now, in my own mind when we talk about the evaluation model, you have written Baltzmann equation of course. You've written down whether it's coarse mesh or fine mesh and what the numerical approximations are in those cases. Then the evaluation model consist of both numerical methods that you're using and the nodalization because that clearly coupled.

MR. BANERJEE: There's a formal process that we use in turbulence called coarse screening. It uses certain types of renormalizations to go coarser and coarser. That's a formal process and I don't see any such formal process here.

I mean, you may stop with a very fine-grained description of the system. You say this is not very useful to me because one extreme there are intermolecular forces, but now I want something which I can microscopically observe.

At that moment you don't make a leap of faith. You go through a formal process of coarse graining and there's no such coarse graining process that I see going on here. We do that all the time in turbulence. That's how you get eddie viscosity at the end. You renormalize the molecular viscosity but there is nothing like that being done here.

CHAIRMAN WALLIS: But what you're addressing is I remember a few years ago I tried to write out my own sort of summary of what should be in these codes. What I would really like to see is an opening chapter which says this is what we're asking the code to do. These are the kind of geometries and flow regime or whatever. It's got a handle.

This is the approach we're using and this is the philosophical way. This is missing completely and it's not even there hidden because I find a great difficulty in going from a statement, "We are going to use this momentum equation," to how it's applied to the actual system. If I look at the nodalization, I see a box, a square box which is supposed to describe a piece of the reactor.

Well, if I look at the lower plenum, what is this box? It's either in a gross way something like looking down on half a grapefruit that you are going to eat. How do you represent that as a box? No one tells me. If you're going to break it up, they are going to take each little segment and scoop out with a spoon and they will model that in some way.

It never tells me how the picture which was drawn for a little square box to derive a momentum equation has any relationship to what goes on in that little segment of a grapefruit which is not a square box. That never gets addressed.

I would like to see an approach that says we have to model these kinds of geometries. We have to model these kinds of situations. We are going to formally develop some reasonable approximation or something to represent them showing that we understand what we're doing in a way which I think no code ever does.

No code documentation ever does. They just launch into writing down some vector equations or something and then say, "We can't handle that so we're going to use something very much simpler," and then going ahead.

DR. STAUDENMEIER: Yeah, I agree with that. I thought the Reg. Guide had some sections to address that you really need to start out with your specific scenario and the equipment that you're modeling and start from there to see what is adequate to model in your situation.

CHAIRMAN WALLIS: I think the words may be there.

MR. RANSOM: Use some reason in the application of these methods to things like reactor systems. It doesn't matter whether you're talking about chemical systems or whatever. They are, in a sense, more integral type models that worry about mass and energy hopefully and a conservation within major components. We have gone steps further and subdivide these.

Invariably we replace the heat transfer with a heat transfer coefficient using Neuton's cooling law or a Darcy-type friction factor and ignore all the transverse gradients. It doesn't matter if you're talking about modal dimensional formulations of these codes. They do the same thing.

You simply don't have enough detail, or can't afford it at the current time, to actually model the physical transport process as they go on is the result of these transverse gradients.

CFD, I think, is getting closer to that kind of thing where they can do some modeling of turbulent phenomena from a fundamental point of view. Some day we may do that in reactors but, as far as I can see, probably beyond my lifetime.

CHAIRMAN WALLIS: Take the lower plenum. It's not a grapefruit now. It's a ball you put in the sink. You force it down, it squirts around, and comes up again. It's like a turbine bucket. Treating that as if there were no transverse gradients seems to me inappropriate. Show me that the model that you're using applies to that kind of geometry.

MR. RANSOM: Well, that's why I think the mass and the limitations of these models, some care needs to go to that because I'm not sure there's anything better you can do at the current time.

CHAIRMAN WALLIS: Because you don't know how to do anything better means that you ought to admit it rather than just writing it down as if it has some authority. Everyone is going to believe that this equation is the only way to do it.

MR. SCHROCK: To add to the confusion if you look at this TRAC-M documentation you find that, in fact, you can do three dimensional calculations in your plenum and also in your downcomer. I mean, TRAC-M does that. It does that because TRAC did it before and there it is in the documentation. It says it's able to do that. I've never been able to fathom what the basis for that claim is.

DR. STAUDENMEIER: Well, limited 3-D capability.

MR. SCHROCK: Let's talk about the basis of an argument that you're doing a 3-D computation in either the plenum or the downcomer.

DR. STAUDENMEIER: I think the assumptions and what you mean by that should be spelled out in documentation. I agree with that. Like modeling downcomers, I mean, you have an open space. You have structures on both sides, water coming in during reflood.

How do you tell if the water is touching the inside wall or the outside wall? We throw in these constitutive relations that hopefully do something reasonable. You know you're not modeling it entirely correctly but hopefully it's doing something reasonable for the situation.

Largely we are held together by our code assessment both in terms of the correlations themselves at the separate effects test and in terms of how it holds together in a situation like a downcomer big integral test.

In that case we have UPTF data which is full-scale reactor downcomer. We really are largely held together in this analysis field by our experimental database.

CHAIRMAN WALLIS: Dr. Kress.

MR. KRESS: I just wanted to throw in a contrary thought. That is, in terms of the description of what the geometry is supposed to be, I think everybody assumes that we know what it is. There's not that many geometries out there. Do describe each geometry every time in a general sense is probably not worthwhile.

From the standpoint of the crudeness or the lack of sophistication of the model, I think you have to ask yourself how good do we want the answer to be? How good of an answer do we need? Sometimes that allows one to have some pretty crude models.

For example, a lot of the BWR results is just a pot boiling away. You can be pretty crude with the pot boiling out. Some places you need better nodalization than others. I agree that if you're trying TRAC level or TRAC two-face flow entrainment and things of that nature, you need to look at how to refine your models.

It all boils down to how well your nodalizations and your model describes the data in relatively large-scale experiments and then the question of how can you be sure that is the case for the full scale, I think, is real valuable.

I've never really -- you know, there's these scaling equations to show you that at least the phenomena ranges are about the same, but that doesn't really tell me much about whether if you use the same nodalization in a sense of similarity nodalization, that is the appropriate way to do it. It would be nice to see that somewhere.

CHAIRMAN WALLIS: I think what concerns us is, it's true, certain accidents are just a pot boiling. In probably a two-node system or something we work fine. Then if you say, "Ah ha, therefore, anything we assume about our momentum equation is going to work because it's just a pot boiling. Therefore, this validates the momentum equation," that's not true.

It may be that in something like AP600 where it's more delegate to the balance between whether the flow goes from this reservoir to that one or to some other one because there's a balance so the pressure drops and so on. Maybe you need to know these pressure drops much more precisely.

MR. KRESS: I think that's exactly the case where you need to have better and more sophisticated modeling because the small pressure drops make a big difference.

CHAIRMAN WALLIS: Another peeve of the ACRS is that something gets approved in 1965 or something and never examined ever again.

MR. KRESS: It can apply to AP1000.

CHAIRMAN WALLIS: Even if Vic Ransom wrote it.

MR. KRESS: That's an automatic approval.

MR. BANERJEE: Well, I guess coming back to the evaluation model, the philosophy is probably defensible in the sense that you've got to take your best guess at some expert's perk or something and then resolve the phenomena that you can.

I think the last step on how you go from relatively small-scale facilities to the full scale and sort of defend your position in terms of whatever nodalization you have chosen and whatever model is not clear.

I was involved with the scaling study for the AP600 and it was relatively easier for that case because most of the things you could describe with a locked parameters set of ordinary differential equations tracking levels here and there and you could get some scaling groups that were useful.

Actually, you could get analytical solution to the equations which almost gave you what you were looking for. You didn't even need to use a code really. But that was a unique situation. It was sort of a situation dominated by this ADS-4 and levels up and down.

In general I don't see how you can derive this way to bridge from the nodalization that you are choosing for your experiment to the full scale. That's why people have gone to saying, "Well, if you refine the nodalization and your answers are not dependent on the small scale, then they are not likely to be dependent on the full scale.

As soon as you get into the trap of saying, "This is the nodalization that works for this small-scale system. Now I have to find how it works for the full-scale system because my results are very nodalization dependent, then I think it's a difficult position to defend.

If it's nodalization independent, then you can defend it because you can say, "Well, it doesn't really matter how I nodalize it."

DR. STAUDENMEIER: Well, in most cases you can say there are some cases where you get fooled by even that because you are not modeling something. If you were really modeling it they wouldn't depend on nodalization.

Most of the cases we look at are fairly well behaved in terms of nodalization and do converge fairly well as you add nodes. In most transients I think you can boil down into some simple phenomena that are driving the whole thing just like AP600. Like a pump trip in a plant is driven basically by the frictional losses in the system in the pump.

In PWR it will be in single-phase flow during the pump trip. You have plant data that measures the coast down to the pump and coast down to the flow in the plant that you can compare your code results against it to show that you are doing something reasonable in that case.

I think a large majority of cases we're looking at are like that. I mean, there are some cases you get into like severe accidents, but before severe accidents like ATWS and BWR, you get into these super large full oscillations coupled with power oscillations.

We obviously don't have plant data to compare when we get into that range. We have some plant data at small oscillation amplitudes when some BWRs went into instabilities. For the full analysis you do for ATWS you don't have plant data that covers that whole range of conditions.

You have some heated channel data that shows where the onset of flow oscillations is. Your data probably doesn't cover the full amount in your amplitude that you get into in some of these beyond-design basis situations like ATWS.

When you are in a situation like that, you have to recognize that beyond a certain point there are just large amounts of uncertainty in my prediction and take compensations for the fact that you really are going out into some place you don't know a lot about and don't take confidence in what those calculation results are giving you out in that range.

You don't make decisions pretending that you know with fine details of accuracy that this is how the plant is going to behave in that place. I think the regulation in here has taken that attitude where you get into regions where you are in the unknown you apply conservatism or apply some sort of resolution to the problem that we don't really know what's going on out there so you work around it.

You don't have to know what's going on out there. Your solution to the problem would be taking measures that keep you from going out there or if it did go out there, if you are beyond design basis like in PRAs, you count that up in the failure bin for when you are doing core damage assessments or things like that.

CHAIRMAN WALLIS: What you're showing is that it's not just the paper, it's the people. In order to do a proper review using this guide, the staff has to have the kind of awareness and knowledge of all these things that you've been revealing and the things you've been saying.

A new trainee coming in here would have difficulty getting your experience and reviewing code using this guide. There have to be other things that have to happen in order for it to be a good review.

DR. STAUDENMEIER: Yeah, I agree with that. There has to be knowledgeable people here and you just can't -- you couldn't just get rid of all the knowledgeable people in the NRC and bring in a whole new crop of people out of college.

CHAIRMAN WALLIS: It requires a commitment in terms of funding and personnel over a long period of time.

DR. STAUDENMEIER: I think so. I think that is actually one weakness of the NRC in that area. They should really come up with some sort of formalized training program or process where people learn all this stuff in a more formal manner.

Right now the way it works you have a junior level reviewer working with a senior level reviewer and he might go and ask a few other people who had written SERs on this process, but there's no real formal training program.

CHAIRMAN WALLIS: Then you put them in front of the ACRS and that's a learning experience.

DR. STAUDENMEIER: You can call it that, I guess. It depends what you think you'll learn.

MR. RANSOM: Discussing this Reg. Guide it would be interesting for maybe the NRC to put together on all of these issues that have been discussed around this table what is known about these issues and what is the accepted approach and get some agreement on that and get some specificity into a document like this.

Whereas right now it's very qualitative, you know. It specifies what you must do in sort of general terms but it doesn't say you need a 1,000 page document to do this and I'm not suggesting that kind of thing. Somewhere in the assessment of how good is good enough there does need to be some more detail actually.

We have argued and talked about these issues and it would be very helpful if some of this would simply attack some of these problems and get them down and get agreement on them that this is the approach and accepted approach. Then maybe we could not have to redo this so often.

CHAIRMAN WALLIS: Maybe this committee could contribute to that. Maybe not today but in the next few years.

DR. MOODY: If you hire all your NRC employees from either Berkeley or Purdue or Dartmouth or U.C. Santa Barbara you ought to be okay.

CHAIRMAN WALLIS: No, the way to do it is to hire them from General Electric and Westinghouse. Then we know what's really going on there.

MR. CARUSO: Dr. Ransom, this is Ralph Caruso from the staff of NRR. I've just been sitting here listening to this discussion and I would make the observation that right now all three vendors have been through this process. G has been through it, Seamans has been through it. They've been through this process using this methodology. There is ongoing dialogue with them continuously about how much is enough.

I agree strongly with Joe's comment and Dr. Wallis' comment about the people. It's nice to write it down. It's nice to write down what we've done that is acceptable. We have written it down when we write our SERs but a lot of this is going to be tribal knowledge.

There was an ACRS member last week who talked about the dark side of certain things. If you write things down in too much detail, there's a dark side to that and that is you get people that do reviews who think they are doing the review right but if they don't understand what they're doing, they give you an answer which is incorrect.

There's a balancing that has to go on here. I'm under a lot of pressure from my management right now to write down more stuff so it can be done by less experienced people. I'm not sure I entirely agree with that.

I'm going on the transcript. I've said this to my management and I'll say it to you. I don't think that's necessarily the right way to go. I would much rather depend on smart people like Dr. Staudenmeier here to do the review done on somebody who has never done it before.

CHAIRMAN WALLIS: We need to develop a way in which someone like Norm can never retire.

MR. CARUSO: He'll become an ACRS consultant and sit up here with you guys.

MR. LAUBEN: The economy has done that for me.

MR. RANSOM: And I agree that a lot of this reactor safety analysis work isn't that hard. It's dependent on good judgement on the part of engineering people or engineering judgement if you want to call it that, and a lot of the design in safety analysis. It is very difficult to tighten some of those up.

In that sense, a popular phrase today, "It ain't rocket science," but a lot of this is rocket science.

MR. CARUSO: Yes.

CHAIRMAN WALLIS: Vic, I think you have to speak into your microphone. I'm sorry. You have to speak in the microphone.

MR. RANSOM: Okay. I was just saying that it is rocket science. That's where a lot of this came from, as a matter of fact. You could say the same methods are used in those cases, the model transient and rocket engines. Again, it depends a lot on engineering judgement and I think we will a long time.

There does have to be some balance and I understand probably the vendors would like loop holes because that means it's easier to get the thing through than if there is more specific algorithm for that. I understand that's the balance you fight.

MR. CARUSO: Everybody wants, you know, a certain amount of flexibility but everyone also wants predictability and there's a tension between flexibility and predictability. You can't have it all one way.

The example I give people about difficulties in coming up with analytical methods is numerical methods. We had Westinghouse come in with

W-COBRA TRAC and they had one numerical method to assess code uncertainty. Then Seamans and Framatone came in with another method. Westinghouse seeing the method that Seamans came in with are thinking about revising their method to something else. NGE has yet a forth method and I'm sure there must be a fifth and a sixth method out there to do statistical uncertainties.

MR. ROSENTHAL: GRS.

MR. CARUSO: GRS. GRS method. Okay. There are other methods out there and I'm constantly being asked why don't you figure out a way to tell people how to produce predictable results when they review these codes.

I don't know how I can do that if I can't predict in advance how creative they are going to be to develop new methods. I mean, it's like asking me to come up with a review standard for a starship drive. I don't know how to do that.

MR. RANSOM: Well, one way, of course, is that you have to focus on the end result that you want. Not necessarily the method for getting there.

MR. CARUSO: Well, then you end up with the common complaint that you treat these things like black boxes and you don't understand what's in them.

MR. RANSOM: All the documentation and the standards you put forth for that allow it to be reviewed so that is part of the check and balance system I think, to make sure that it doesn't just float through.

CHAIRMAN WALLIS: One of the checks which would be very useful and a little bit difficult with proprietary codes and methods is if the audience were not just a few NRC reviewers in ACRS but the public and if these things were actually presented at professional meetings.

This is the new way of evaluating uncertainty that has been developed by X company. It's actually out in the open. That gives you a lot more assurance that people would find errors if there were some.

MR. CARUSO: Well, it would be a good thing except that companies treat this stuff as proprietary.

CHAIRMAN WALLIS: They are using a statistical approach which has been used in many other fields and it has credibility.

MR. CARUSO: I agree. I would like it to be publicly available. Unfortunately a lot of times it isn't.

CHAIRMAN WALLIS: Sometimes it's the things which are not public that give us the most trouble.

MR. CARUSO: I understand. I understand.

CHAIRMAN WALLIS: Well, Joe, we're taking time because you've got all morning. I think we may have addressed a lot of things that we would otherwise address later. Perhaps we should let you get on to your track.

MR. BANERJEE: In more ways than one.

MR. STAUDENMEIER: Okay. The basis for all this lies in the regulation 10 C.F.R. 50.34 which says that you have to base technical specifications and plans on safety analyses. That's the whole driving regulation behind all of this is that you have set points and various other tech. specs. at plants that have to be based on analysis.

As guiding principles for developing this, Norm started with CSAU. I think Novak informed them he had gone beyond that with SASM, Severe Accident Scaling Methodology. The principles from that were also incorporated into the Reg. Guide.

CHAIRMAN WALLIS: That's what was mentioned. I thought someone said assassin.

MR. STAUDENMEIER: SASM, S-A-S-M.

CHAIRMAN WALLIS: I was wondering what's the difference between assessing and assassin. SASM is another word. Okay.

MR. RANSOM: Joe, could you give me an example of what this hierarchial system decomposition principles does for you?

MR. STAUDENMEIER: Really it's a top-down scaling method is how I would describe it. You look first at the system you are describing and try to divide it up into different parts where different phenomena are going on and then down at more detailed levels.

You say for this component model these physical processes are going on underneath there and do I have adequate models to model these physical processes going on within these components. Then the top level would be integrating these components into a couple systems and doing the calculations.

MR. RANSOM: It sounds like a PIRT.

MR. STAUDENMEIER: It's similar to the PIRT.

MR. BANERJEE: It eventually ends up as a PIRT.

MR. RANSOM: But I guess you try to give a quantitative figures of merit or something to the different things.

MR. BANERJEE: It's qualitative.

MR. RANSOM: Qualitative?

MR. BANERJEE: I think it's qualitative.

MR. STAUDENMEIER: It's a structure -- I mean, somewhat quantitative and somewhat qualitative but it's just a structured method of decomposing it into parts that you can understand and generate models for is how I view it.

MR. BANERJEE: It gets down to phenomena. It should be getting down to phenomena.

MR. RANSOM: That's what they do in the PIRT, too, isn't it? It's phenomena identification.

MR. BANERJEE: It's a more formalized way of arriving at that. I don't know. There may be more to it. I'm not sure.

MR. RANSOM: I was wondering what it does for you in the end.

MR. LAUBEN: Well, there are about seven levels. I'm not an expert in it by any means but the idea is that it's more simplified at the top. If you are missing something at a high level, you're not going to be compensated for it at a lower level.

You have to make sure at each level as you go down through it that you have what you need to solve your problem. It includes things like do you have enough phases, do you have appropriate conservation equations, do you have all these things. I can't even remember what each level is.

MR. BANERJEE: It's somewhere in the Reg. Guide.

MR. STAUDENMEIER: Yeah, there's a fairly large new reg. describing the whole thing if you're interested.

MR. RANSOM: I'll try to get it.

MR. STAUDENMEIER: I think it's maybe about this thick or something.

MR. LAUBEN: The principle purpose of it when it was being developed for SASM was actually to look at assessment but it applies very well to code development principles. Are you putting the right things into code.

MR. STAUDENMEIER: Yeah, I would call it kind of almost like a PRA type structure only applied to analysis where you are dividing things up and you have different contributions and it gives you a formalized way to take what is the most important thing that you have to invest your time and money into getting better models.

CHAIRMAN WALLIS: You have your equations and you have to have your principles. You have to have the equations based on them and you have to have the coefficients and the equations and so on.

MR. STAUDENMEIER: Okay. This is what Norm came up with, that it's an organization in DG1096 introduction discussion. It's very similar to 1120. I'll put up the table of contents for that later. Then Norm wasn't satisfied with CSAU or SASM so he had to come up with his own acronym and came up with EMDAP, Evaluation Model Development and Assessment Process.

That's the main piece of the Reg. Guide is describing this evaluation model development and assessment process covers quality assurance, documentation, and the section on general purpose computer programs was added really as a result of the RETRAN review experience. Also there's an appendix on additional considerations for using this for ECCS analysis.

The principles of this EMDAP process is that up front you have to determine the requirements for what your evaluation model has to do. You decide on a power plant type and accident scenario.

Once you've picked that out, you look at the components you have to model, the processes that are going on that you have to model, and assure that you've come up with a well structured system in analyzing these requirements up front before you go on deciding what sort of code or equations you need to solve to do this.

After you develop all these requirements you develop an evaluation model that meets the requirements. Developing a model doesn't mean that you may not be writing some code from scratch.

You may be picking up some code like RELAP. After you've done this analysis up front you may determine that RELAP has all the required models in there and physical processes in there to be part of your evaluation model.

After you've made that choice, developed it, you do a specific assessment base that is appropriate to the requirements of your evaluation model which depends on the accident scenario in the plant you are looking at for this model.

You come up with all the physical phenomena that are important in that case which is the PIRT like process. Come up with experiments to assess your important models against those physical phenomena and hopefully some integral experiments which in the case of transients could be actual plant data.

So you go and assess your model. During the process you have to be following quality assurance protocols to make sure that you have a paper trail documenting that your model is actually good to do what you're going to do so that it's traceable back to all your assessment and showing that it's adequate to use it for what you're doing.

MR. RANSOM: Joe, right here you're talking about here is a road map for developing an evaluation model and things that I had difficulty with. This is in response to Ralph's comments, too. It says "specified figures of merit." Then down below it defines those as, "Figures of merit are those quantitative standards of acceptance that are used to define acceptable answers for safety analysis."

MR. BANERJEE: BCT.

MR. RANSOM: Well, these should be defined. Is there a table or something that says what they are for different accidents?

MR. CARUSO: It depends on how the licensee or the vendor or the person who is using the code what they are using it for.

MR. STAUDENMEIER: But that's where this appendices would come in later. This was going to be a top level framework and then there would be an appendix for a specific type of accident or transient where you would have that actual success criteria or safety criteria that you are examining. But there are very -- I mean, DMBR ratios, CPR ratios.

MR. RANSOM: You read on down further and it says, "Determine evaluation model biases and uncertainties." Again, that's about as much as it has to say about it.

CHAIRMAN WALLIS: This is much more difficult. in the case of 2200 you have a criterion but what is the criterion for uncertainty?

MR. RANSOM: And finally in the last block it says "inadequacy standard." What is it? I mean, these are the problems that give me real grief.

CHAIRMAN WALLIS: Even with uncertainty we get things thrown around like 95 percent confidence in the 95th percentile of the predictions or something. That may help you when you protocol efficients in the code. It doesn't help you when you're addressing the scaling. How can you be 95 percent sure that you are within this 95th percentile of some scaling situation. I don't know how to do that.

CHAIRMAN WALLIS: If your tests have only been validated at half scale, what's your assurance that they apply to full scale?

MR. LAUBEN: Usually these things are found some place else. Not in this Reg. Guide but some place else. They may be found in a regulation or they may be found in a different part of the standard review. It may say that --

MR. RANSOM: 95 percent certainty that no more than 1 percent of the rods were experienced below transition.

MR. LAUBEN: I think it's in the SRP.

MR. RANSOM: It's in the SRP with GEC.

CHAIRMAN WALLIS: Vic, you have to talk in the mic.

MR. RANSOM: Why wouldn't you incorporate these by reference at least. They are all over the place.

MR. LAUBEN: Because in the SRP which section is a companion to this, it will relate that to a section in the SRP that is right next to it.

MR. RANSOM: Well, could you put that in this Reg. Guide and say just those words? If I were a guy coming in here and I wanted to do this and I wanted to pick this up and go through this process, I mean, every one of these you have to go find out what it means, I guess.

MR. BANERJEE: Are you saying that for

each accident in Chapter 15, or whatever, you would like to see a reference to --

MR. RANSOM: Either that or just describe how --

MR. BANERJEE: -- merit or --

MR. RANSOM: -- you establish those. How are those established.

MR. LAUBEN: I think when I first presented this to the ACRS in '98 I had a chart because you had to have a chart showing that this regulation pointed to this Reg. Guide pointed to this SRP section and so forth.

It's true, you almost -- it's a road map. Everyone is always talking about road maps. It's true that you sort of do need a road map. Maybe the Reg. Guide isn't the right -- maybe there's a higher level that has to have a road map.

MR. BANERJEE: You probably have it in your head but there can't be too many people who do.

MR. LAUBEN: Well, that's a bad place for it.

MR. RANSOM: The people doing these calculations know where everything is.

MR. STAUDENMEIER: Yeah, I guess we just assume that the people who would be using this knew what those appropriate criteria were. We could add a reference to the SRP right there saying that many of the success criteria are defined in the SRP or in various parts of the Reg.

MR. ROSENTHAL: It might make a very nice summary paper to product this road map to show some place. Let me remind you again that the Reg. Guide was meant for broad application, not only in the thermal-hydraulic area but in one case it's going to be a fuel rod internal pressure and some other applications going to be how well do you know the moderate temperature coefficient. I think by the time you draw the full map, it will look like a street map of the United States.

CHAIRMAN WALLIS: We wrote a letter last week on PRAs and many of the paragraphs there would apply to thermal-hydraulics. I was just saying that if you make assumptions they should be justified and if you make simplications, there are reasonable representations of a more complex approach and so on. All these things which would apply to almost any analysis you do of anything.

MR. STAUDENMEIER: Okay. The last principle here would be the accurate up-to-date documentation would be part of this process because that's been I guess a problem point that's been happening in many reviews of codes at the NRC by both the staff and the ACRS is that there wasn't a good up-to-date documentation.

MR. SCHROCK: On the assessment we have often complained that assessment that is presented is so frequently extremely limited and I don't see how you are going to improve that situation through this Reg. Guide.

MR. STAUDENMEIER: No. I mean, I think the approach that most vendors take is throw in what they think will be the minimally acceptable amount of work that they have to do and if the staff tells them they have to do more work, they will add some more work.

MR. SCHROCK: You don't have standards really.

MR. STAUDENMEIER: There are some cases where they are proactive and come in with lots of assessment up front. Those cases don't happen too often.

CHAIRMAN WALLIS: You see, if you had a formal method, something like order statistics or something, you could actually show from some mathematical way that you had enough data points to have your 95 percent confidence of some criteria.

You actually show it mathematically and you could say we need at least four LOFT tests or something in order to have enough data points to satisfy this mathematical criterion. You don't have anything like that now so it's a question of negotiation and judgement.

MR. STAUDENMEIER: Yeah. The only place that really formal criteria comes in is DMB or CPR correlations where they do have a fixed uncertainty that they are trying to get at and they really do need a certain number of data points and different ranges or conditions to get their uncertainty down to that level. Other than that you're right, there is no good solid way of putting those numerical criteria on there.

CHAIRMAN WALLIS: But if it only comes from one side, we just see curves from an applicant which show a curve and the data point is close to the curve for this LOFT test. One wonders what happened to the other comparisons with LOFT tests that they didn't show us? Were there any other ones? That's where, again, the staff if they do independent assessment can do this. They can say, "Ah ha, we're going to take a different LOFT test and see how it works."

MR. STAUDENMEIER: The reason they only had a few tests is probably that they picked these tests back when computer time was very expensive and they could only run on a limited number of tests and they had just been carried through for historical purposes since then.

CHAIRMAN WALLIS: But they have a choice on what they show you.

MR. STAUDENMEIER: Yeah, they do.

CHAIRMAN WALLIS: It's usually that the criterion has to be that it makes the code look good I would think.

MR. STAUDENMEIER: Not always. Like lots of LOFT tests like in the large-break LOCA case there are certain LOFT tests that really -- I mean, you may say that maybe they should run them all but you look at some of them and you come to the conclusion that it's not going to do you any good for approving that your code is good in some case.

Like there's actually one that the NRC runs that is good for low-down type phenomena but you look at reflood stage and the accumulator dumps all the nitrogen in and it pushes the water up and refloods the core.

Because that LOFT has a short core it really isn't prototypical for nuclear plants for the reflood stage so it really isn't adding any support that your code is good for reflooding a 12-foot core. In many cases I think it's examining the data and coming up with conclusions like that that they don't run the whole break LOFT series of tests.

Okay. We sent out draft Reg. Guide out for public comment in December of 2000. We received 13 sets of public comments. Twelve were from industry organizations. One was from an individual who was a former employee of an industry organization.

There were comments on both the SRP and the draft Reg. Guide but most were directed at the draft Reg. Guide because that was what most impacted the industry and also had a lot more information in it.

Most of the comments, I would say the majority of the comments, were that applying this methodology would be expensive. It wasn't justified based on the risk involved in these accidents and various things like that. It would stifle innovation that you put all these requirements on.

I think one thing was the utilities thought this was going to be a large onerous process that they've broken themselves away from the field vendors and doing some types of analyses but this would be so burdensome that they didn't have the manpower to keep on doing that and they would have to revert back to using field vendors.

CHAIRMAN WALLIS: We heard this litany of excuses when we complained about quality documentation I remember. "It's too expensive and we don't have the people." So on and so on.

MR. STAUDENMEIER: There were actually, I guess, some disturbing comments, minor comments like there's not adequate data for assessment under all ranges or conditions that are important.

CHAIRMAN WALLIS: So what are you supposed to do?

MR. STAUDENMEIER: Well, how are you justifying your code right now if there isn't adequate data for assessment. There were all different sorts of comments but the biggest concern was a burden that would be placed on this. Especially they had concerns that this would try to be back-fit on to their existing evaluation models they had been running for long periods of time that were approved in the past.

MR. BANERJEE: They're grandfathered, aren't they?

MR. STAUDENMEIER: Yes, they are grandfathered but what they are concerned about is making small changes or error corrections to these approved models and that would suddenly throw them into the bin where before they had this small amount of documentation and testing and it would throw them in that to improve one model and a small amount they had to suddenly bring in this whole new volume of work. That was the main concern.

CHAIRMAN WALLIS: The real extra work is if you are going to use a realistic model rather than a conservative one. If you're not going to use Appendix K it will ask for some relief then you've got a pretty good case for it. That's where I think the work comes in when you use a realistic model and somehow show that it's a good estimate. We don't like the term best estimate because it doesn't mean much.

MR. STAUDENMEIER: Yeah, realistic is a better word than best estimate I think.

Since the comments that we received indicated that this Reg. Guide was fairly controversial and there were a lot of concerns, Norm organized a public workshop to bring in the concerned parties to discuss resolution of their concerns and resolving their public comments in a way that would be acceptable to all parties.

We had a one-day workshop where everybody gave their presentations on what they thought was wrong with the Reg. Guide or what could be done to improve it. We had some roundtable discussions, took notes, and went back to revising the Reg. Guide.

MR. KRESS: You don't feel constrained to act upon these public comments, do you? I mean --

MR. STAUDENMEIER: Constrained?

MR. KRESS: Do you feel like it's in your best interest as a regulator to keep it like it is, you'll keep it like it is.

MR. STAUDENMEIER: Oh, yes. There are public comments that we just said to make a decision. You can't accommodate this public comment.

MR. CARUSO: What if the vendors fully supported this and they wrote in and said, "This is good. We'd buy into this."

MR. STAUDENMEIER: Yeah, that was --

CHAIRMAN WALLIS: If we're looking for positive feedback.

MR. STAUDENMEIER: They had one negative comment, that we are too biased towards CSAU as an uncertainty.

CHAIRMAN WALLIS: That's a good comment.

MR. STAUDENMEIER: That was a good comment. There was one vendor that said, "We think this is great and everybody should do it this way." I think partly because they were doing it that way and they wanted everybody else to have to go through the work data.

CHAIRMAN WALLIS: That's fair.

MR. STAUDENMEIER: Completed revisions to draft Guide 1096 in February were provided to NRR for comment and we received NRR comments back in June of this year.

CHAIRMAN WALLIS: NRR didn't seem to have all that much to say.

MR. STAUDENMEIER: Their comments require some revisions of the SRP more than the Reg. Guide. There were no real revisions of the Reg. Guide that were required by the NRR comments but there will be a needed revision of the SRP.

I have a page of what I consider to be some significant revisions. The main revision was adding a section on a graded approach to the EMDAP so that people with their legacy models out there that if they made small changes to it that they didn't have to go through the whole EMDAP process. They would be able to go through a more limited process for review and approval. That was to resolve the main concern of everybody.

CHAIRMAN WALLIS: Section 5.

MR. STAUDENMEIER: Yeah, we added a new Section 5 to accommodate that. The other changes you can see there are a lot of rewordings and making some clarifications like some people don't use FORTRAN codes. They do calculations. They have models they developed in MathCAD or Mathematica and we believe something like that is covered the same way as a FORTRAN code would be. It's just a different program.

CHAIRMAN WALLIS: They don't change the structure of the Guide at all but they are just more inclusive, let's say.

MR. STAUDENMEIER: Right.

CHAIRMAN WALLIS: The big change is the first one which might provide an out for some applicants to do less work than they might otherwise feel they had to do. But it might actually work the other way. It might work that someone said, "Now, look, because of the complexity of this event you've got to do more work."

MR. STAUDENMEIER: Yeah, it may turn out that way.

CHAIRMAN WALLIS: It's not clear that it's all just relief that is being offered in Section 5.

MR. STAUDENMEIER: The things in parentheses after the various changes are the organization it came from. NRC is us; GNF is Global Nuclear Fuel; CEOG is Combustion Engineering Owners Group.

MR. BANERJEE: What is GNF?

MR. STAUDENMEIER: Global Nuclear fuel. It used to be GE and Ge split off their fuels division to have a combination with some Japanese companies so their fuels is now called Global Nuclear Fuels and that is a separate entity than General Electric which is still based out in San Jose. The fuels group is down in Wilmington.

MR. BANERJEE: What is CEOG?

MR. STAUDENMEIER: Combustion Engineering Owners Group. The next page there are other definitions. BWROG is BWR owners group. WOG is Westinghouse Owners Group.

The number of comments was far greater than the number of changes that were made. I tried to accommodate all the changes made.

CHAIRMAN WALLIS: This reminds me a bit of student evaluations of a course. The professor goes back and changes a bit about how it's done next year.

MR. STAUDENMEIER: That was distilled from about this much set of comments so it was quite a distilling process going on. So the revised Reg. Guide essentially had the same structure. There was some more material added in various sections to either clarify, make corrections, or more detailed explanations.

This new section was added which is this graded approach to applying the EMDAP process which was to alleviate the industry concerns that as soon as they made a change to an existing model, that they would suddenly jump into this super amount of documentation and testing.

CHAIRMAN WALLIS: I wonder if this is a good time to take a break since you're on all morning and you've got to the point where you've told us the history and how this thing got created and what the changes were. Now we've got to what it is. We can break and come back and discuss how we like what it is.

MR. STAUDENMEIER: Sure.

CHAIRMAN WALLIS: Then you won't have to stand up.

MR. STAUDENMEIER: I could use a drink, too. My throat is getting a little dry.

CHAIRMAN WALLIS: Why don't you sit down at any time.

MR. BOEHNERT: Kind of early to be drinking.

(Whereupon, at 10:23 a.m. off the record until 10:37 a.m.)

CHAIRMAN WALLIS: We'll now come back into session. Joe, are you ready?

MR. STAUDENMEIER: Yeah.

CHAIRMAN WALLIS: I think we'll probably finish early. Don't count on that now.

MR. STAUDENMEIER: These are the slides that I thought would take longer.

CHAIRMAN WALLIS: Come back into session and Joe will finish up the presentation that he started earlier this morning.

MR. STAUDENMEIER: Okay. So the new section in the 1120, the biggest revision is this graded approach to applying the EMDAP process. It's to alleviate the concern by vendors. "We have this model we've been using for years and we want to make a small change to it. Why do we have to suddenly apply this 30-page document and go through all these additional steps?"

CHAIRMAN WALLIS: One reason is that some of the things you've been doing for 30 years don't apply anymore. They applied when we were being conservative and now we're being realistic and it's got to be examined.

MR. STAUDENMEIER: In some cases that is true and they do need to go and apply the whole process or a substantial amount of the process. In other cases we agree with them that they may not have to go do a full-scope uncertainty study and large-scale assessment for small changes for a simple model.

Although I guess our argument at the public workshop to them was if they have simple models to start out with with a small number of parameters, then it doesn't really take a lot of effort to apply an uncertainty analysis to that type of model.

CHAIRMAN WALLIS: So in the case of a new design of reactor with different features, that would be covered by the last one.

MR. STAUDENMEIER: Yes.

MR. KRESS: I'm not sure I know what you mean by novelty.

MR. STAUDENMEIER: Okay.

MR. KRESS: Newness or neatness? There are a lot of meanings to the word novelty. I'm not sure what you mean here.

MR. STAUDENMEIER: By novelty we meant something totally different than what they were doing like if they had an HEM model or something and they changed over to a full two fluid model, then there would be a lot bigger requirements on showing that this new model was adequate and good for what they were doing and would require a lot of assessment that wasn't required for their old model compared to, say, just changing a heat transfer coefficient in the old model.

What we meant by novelty is something much different than what they had been doing. The change introduces some sort of new physics or new mathematical model or something that is qualitatively different from what they had been doing.

MR. BOEHNERT: Doesn't this really come down to the staff's engineering judgment on what the extent of the change is?

MR. STAUDENMEIER: Yeah, I think a lot of it does come down to that.

CHAIRMAN WALLIS: It gives you a basis for argument, though. They will come back and say, "There's nothing new or very little new. Therefore, we don't have to do so much." And you'll say, "Ah, but this is new and that's new and something else is new and this influences something else." It gives you a basis for negotiating.

MR. STAUDENMEIER: Yeah, I think that is the case because a lot of times the way things work is the staff may feel one way. They get overruled by their management. It isn't written down anywhere and there's nothing that says that it shouldn't be the way the manager is. He says, "I've had more experience in this than you and overrule you."

MR. KRESS: In the forth bullet what do you mean by the event? Is that when you're talking about the model will be used to evaluate various transient events?

MR. STAUDENMEIER: Yes.

MR. KRESS: It's usually more than one of them if you change something in the model but the model is used to evaluate a number of events.

MR. STAUDENMEIER: Right. I guess our concept of it is that the model is really your analysis for evaluating a specific event. If it covers more than one, I mean, you have to consider all the events that you are analyzing with that computer code when you make that change. If it was used for multiple --

MR. KRESS: Do the words "safety

importance" imply something different than risk significance?

MR. STAUDENMEIER: I think it could be considered the same as risk significance, although you may be able to extend it beyond something that would be pure risk. I mean, there are some things that we may consider that have safety importance but in terms of overall risk to the public, it may --

MR. KRESS: I would almost interpret that to mean if it has to do with ATWS, then it's important. If it's not ATWS, then it's not. That's the only one I can think of with real risk significance.

MR. STAUDENMEIER: They are sort of interchangeable and there are some small cases I can think of where we think like protecting the fuel in terms of DMB margin. We think that's of safety importance but it doesn't really contribute to risk to the public.

CHAIRMAN WALLIS: The text only talks about risk significance. It doesn't use the word safety importance. If it's just risk significance, then it goes back to a conversation we had a couple of hours ago. All these design basis accidents don't really affect risk.

MR. KRESS: Except ATWS.

CHAIRMAN WALLIS: So it's a different world. Yet it's the risk world we're probably most interested in in terms of affects on public safety and so on. How is it supposed to fit into this?

MR. STAUDENMEIER: Well, I guess the way we saw it as fitting in is if for each accident you're looking at you have a safety parameter that you're looking at that you're trying not to exceed like overpressure, peak pressure during a pressurization event or something like that.

I guess the risk significance would come in by what are the consequences. Obviously you've analyzed this. It's the design basis. By that definition you don't have to worry about it because your plant is going to stay under this safety criteria in the worse case.

I guess we are thinking of it and there maybe is some uncertainty or finite probability that the plant in this situation may really exceed the safety parameter and what are the consequences of exceeding a safety parameter.

Maybe in some cases it's not real high consequences in exceeding it. Something fails but it's not going to cause some propagating consequences that go down stream and lead you into a severe accident.

CHAIRMAN WALLIS: I think you may need to rewrite this section because if you're looking at, say, 2,200 degrees, if you're using realistic code with uncertainties, there's going to be a probability of getting more than 2,200 degrees. Bound to be.

And you're going to use some criterion like 95 percent assurance or in 95 percent of the cases you won't exceed 2,200. What happens if you do? If you have a long tail, you might get up to 3,000. We don't know until you estimate it.

I think this is a very vague requirement that they look at consequences exceeding a safety limit because I don't think the staff ever does in the case of something like 2,200. You don't say, "What happens if it's 2,201 or 2,210." You don't have a way of evaluating the importance of that.

MR. STAUDENMEIER: Not in the way you're talking about but events that if you are reviewing something for an event that had more safety, say, if you did exceed whatever safety criteria it had. You might review that analysis more closely than something that doesn't really have any severe consequences if you didn't really meet the safety criteria.

CHAIRMAN WALLIS: What is going to be --

MR. STAUDENMEIER: So there is in some cases a disconnect between the safety criteria or tech. specs and what is actual risk to the plant.

CHAIRMAN WALLIS: We're talking about performance-based regulation where instead of saying 2,200 degrees you simply say you've got to assure the integrity of the cladding or something. Then it's going to be even vaguer how you are going to assess whether or not they meet the regulations.

MR. STAUDENMEIER: That issue is currently under consideration.

CHAIRMAN WALLIS: You might need to rewrite the section 54, risk significance, in a way that helps anticipate some of these things. It's a very short section with two sentences. I don't think it helps the reader. It doesn't help me. I'm not sure that the present regulations for use of these codes for design basic accident risk is relevant at all.

MR. BANERJEE: It also allows to do very little if they wanted to because risk is usually small, right?

MR. STAUDENMEIER: Yes.

MR. BANERJEE: Not very significant in any case.

CHAIRMAN WALLIS: PRA shows the large-break LOCAs unimportant so we want to analyze it.

MR. STAUDENMEIER: And that is the industry position. They really want to get away without analyzing just about any of these events or taking them out of the regulatory arena.

MR. SCHROCK: I have some difficulty with the degree of conservatisms aspect of this. I guess it's somewhat related to Vic's question earlier about meaning of evaluation model. It used to be those words applied to the Appendix K and along came the concept of a realistic analysis or best estimate analysis.

I think for many people, certainly in my mind at the time the realistic analysis was being introduced it was an either/or proposition. I think you gradually move towards a situation where you're going to be confronted with analyses that span the whole spectrum between.

This wording in this seems to me to be opening the door for that where you want to do something less costly than a full CSAU so you introduce a little more conservatism here and there and then you can have a less costly process for the industry.

I may not be making myself clear on where I see the pitfall but I think there is one here. It also came out in our last discussion about risk-informed considerations and the conservative models that are involved in that context.

It isn't very clear what one would do in evaluating the degree of conservatism in the evaluation model in deciding how detailed this model development has to be.

MR. STAUDENMEIER: Actually this was more to accommodate existing models than for developing any new models, I guess. I understand where you see the pit falls.

MR. SCHROCK: It says the extent to which the full model development process may be reduced for a specific application.

MR. STAUDENMEIER: Right. Yeah. I mean, we hadn't thought of -- I guess you could apply that to developing a new model from scratch. If it was extremely conservative and if it was defensible, then I think you would have to accept something like that.

When you are developing a model, I mean, you just wouldn't say that it's conservative without supporting information to back it up. I think you have to have technical defensibility in your model.

I think you have to also consider that if there's no -- if you're not gaining any additional benefit from going through this whole process that you would have a technically defensible model with less elaborate development or less costly development process, then I think that would have to be acceptable.

I think we have to be sure that we're not putting additional burden on the industry for no reason when they could have an acceptable or technically defensible model with less effort or cost.

MR. SCHROCK: You put yourself into a corner sometimes like on the determination of an overall conservatism. The idea that the old Appendix K had compensations which you understand somehow, but on the basis of what do you understand these compensations?

You kind of end up with a scheme which seems to me to be basically unsound where you are trying to weigh one thing against another thing and then judge overall the result of the computer calculation as a suitable basis for making a determination that you meet this criteria.

I can't see there is any way you can

relate what one inaccuracy does to what another inaccuracy. It has no relationship to that in the calculation as it does to the bottom line determination of, say, peak-life temperature.

MR. STAUDENMEIER: I guess another type of example I would use is if you had -- I guess part of this you'll see later on that part of this degree of conservatism in the evaluation model is that you actually in order to apply this justification for going through the reduced effort you couldn't just say, "I have a conservative model."

You have to put forth some effort in quantifying that you really do have a conservative model and have a fairly good estimate on the degree of conservatism in your model before you use this as a justification. You couldn't just say, "I have an Appendix K model. It's extremely conservative and, therefore, I'm not doing assessment on my new reflood E-transfer correlation," or something like that. That's a case that would not be acceptable, I don't think. I can see how you see that these worse cases may slip by because they probably have in the past.

CHAIRMAN WALLIS: You've got to be careful by what you mean by conservative. What is conservative? Heat transfer coefficient for reflood is not conservative if it's used for something like pressurized thermal shock.

Even so if you just took a brief flood it may be that the heat transfer at sometime produces more steam which changes the scenario so having it lower later still isn't conservative because of the interaction between what happens earlier and what happens later. It's not obvious that particular assumption is conservative unless you look at the consequences of making some other assumption.

MR. STAUDENMEIER: Yeah, I agree with that. I guess the ultimate goal is that we would really like people to move towards realistic calculations with true uncertainty evaluation. We're just trying to make some attempt to culminate some older methods that are still going to remain with us on this transition process.

MR. SCHROCK: I guess my concern is that I am completely convinced that it takes a lot of nudging to move them in that direction and that you have a lot of pressure on you to accommodate them in other ways. You are doing things which are not consistent with the idea of nudging them in that direction.

MR. ROSENTHAL: If I could, instead of

thinking about LOCA and steamline break which we all acknowledge are thermal dynamically challenging to analyze and the staff puts a lot of effort into it, let's go to the full other end of the spectrum.

There's a pressurized water reactor rod drop accident analyzed in Chapter 15 of the FSAR in which having dropped the rod in the core of the reactor saves power and the pressure and temperatures hardly change at all.

The consequence of the drop rod is that you may on the other side of the core from where you dropped the rod experience DMB in some limited number of fuel pins. I think we would all agree there is life after DMB.

Here we have a reasonably benign scenario. We would like it not to happen and, of course, we'd like not to challenge the fuel, and it has happened so the probability is reasonably high. You can say just how much analysis do we want them to do.

Now, you compound that by saying that for a large-break LOCA analysis it may be years before you have to redo it. Conditions have changed in your reactor. You do fuel shuffles every cycle and you may well find yourself reanalyzing this reasonably benign drop rod scenario which puts you into this mode. It just seems that the graded approach ad not requiring all the qualification was appropriate for that sort of event.

MR. KRESS: The problem I have with that is once you have a change in your model and you give it some sort of stamp of approval, then it may have originally been intended to be used for this rod drop problem, but then it's proved in the code for any use that they want to put it to and they want to come back later for a power upgrade. This model may have a significant affect on the peak clad temperature or something whereas that wasn't the original application change.

I worry about not having -- for example, the risk significance or safety importance, I worry about that one particularly because what I think is needy is, two things, how good is my code that I've gotten now, the one that has been approved, how good is it with respect to reality. I don't know how in the hell you know that.

My guess is you have your best guess at reality by using some realistic code like TRAC-M that you developed. You could say this is my best guess at reality, but that's going to be specific for a specific plant.

I would love to see some sort of a system where here is my best guess at reality for this plant and then here is how close this particular approve models or set of models for this plant come to my best guess at reality and why is that acceptable to me. There is some criteria need there.

Then they will come in and say, "I want to make a change." What you've got is now a new set of models or new set of code. Then you ask yourself how did that affect my assessment of how close this comes to reality and how does it relate to my acceptance criteria.

I don't really see any of that. That's the process that makes rational sense to me. This is to me what you have here is a reasonable process except I think it's too judgmental and it's going to lead to a lot of negotiation and discussion. I'm sure you guys know how to do that and you'll come down on the right end of it. What worries me is from the standpoint of how an outside might view it.

MR. STAUDENMEIER: Yeah, I think that's a valid comment. This is the comment seeking period so I invite you to express that. I mean, maybe it isn't the proper thing to do what we have proposed here. We may have not thought through all the nuances of these various things and we would like to hear possible pitfalls that we haven't thought of.

MR. RANSOM: I'd like to comment just a little bit more following on what Professor Schrock has indicated, the need for conservatism. Appendix K was an attempt to provide an overarching conservatism to a calculation by doing a worse case type scenario in all cases. A lot of conservatism is maybe not so good because you weren't assured of safety.

I still think there's a need for some overarching layer of conservatism in a calculation. The one example that I'm going to give, and this is something that maybe would be something the NRC really should almost legislate, would be the effect of the user of one of these codes.

In the calculations that I've seen where there were blind calculations using the same code but different users to model the phenomena. The largest variation was due to user. My question would be how do you account for that?

This is not an unmodeled piece of physics necessarily but simply a looseness in the user guidelines or whatever that allows different users to get different results. Some of them might even be due to human error. Human error also needs to be considered in these kinds of calculations.

MR. KRESS: I think the only way to deal with that is to have their own assessment model that they believe and know the user affect and to compile the results.

MR. RANSOM: How do you know their user effect isn't wrong?

MR. KRESS: They have to have that one studied and down.

MR. RANSOM: This I believe is their role.

MR. CARUSO: Dr. Ransom, I'll give you an observation on this. I've seen this myself. I think I've made some comments about this in the past. In the case of the vendors with things like the LOCA codes there is a lot less user options than you might imagine.

The vendors have very strict rigorous processes which we've seen which we look at all the time to make sure that they get the same answer every time. They don't allow their individual engineers to go off and model and nodalize plants differently.

They do production runs and they have to be able to do them in a very predictable way. They have books and books and books and piles of books and rooms full of books that describe in exquisite detail how they do these things.

The calculations that we do are much less predictable because we don't -- this agency doesn't have as rigorous a set of guidelines as the vendors do. I've seen the same sort of variation that you have seen.

I've seen it interestingly enough in comparisons among people who had nothing to gain other than their pride. I mean, I have one ISP that I drag up as an example all the time in which some regulators and some university professors did some very creative things with one of your computer codes to get the answer that they thought was correct. They had nothing at stake.

The vendors who have an enormous amount of money at stake have proceduralized how they do these calculations very, very carefully. Licensees, on the other hand, are more creative and that's difficult to police but it has to be policed, I think.

Remember we talked about this. They want predictability but they also want flexibility. They want to be able to be creative and you don't want to shut down that creativeness but you don't want them to go off and use RELAP-5 to do containment subcompartment analyses which some of them have done.

Then they file Part 21 reports with us saying the code is deficient because it doesn't do it very well.

These sort of examples are always going to show up and it's our job, it's the job of the inspectors, it's the job of the people that understand how the codes work and how they should be applied to monitor this and correct them when they do the wrong thing. I honestly believe that is the only way to fix it. I don't think the guidelines are going to fix this.

MR. RANSOM: They won't entirely but, you know, good guidelines and guidelines for nodalization, like I think Dr. Banerjee was talking about, those things help. All of these things help. The only thing I'd like to know is maybe it's only a fraction of a percent or something in there in the uncertainty that is associated with the human aspect of this thing.

CHAIRMAN WALLIS: I think you have to space things out.

MR. CARUSO: When we do the reviews of these methodologies we try our damnest -- the word is going to get in the transcript -- we try our damnest to make sure that we nail down how they are going to do the analyses. We try to write that in the SER. We try to make them define it in the topical reports.

That's part of this dialogue that occurs back and forth between the staff and an applicant about how to use these methods. We try to define it well but we try not to make it so strict that they can't use it in a realistic way. Once again, I think this is a judgement. That's why we have smart people like Dr. Staudenmeier to help us do this and Dr. Lauben.

CHAIRMAN WALLIS: If you try your damnest, I think we should talk about damnest estimate codes.

MR. CARUSO: Or realistic, not best estimate.

CHAIRMAN WALLIS: Your next slide, I think, shows what my colleague Dr. Kress was saying. There's a huge range for maneuver in terms of what is required here because you can be anywhere on this scale in any code evaluation.

MR. STAUDENMEIER: Yeah, I agree with that. You can be anywhere. Like I said, maybe it's not appropriate that we allow this wide range of variation. This was trying to come up with a proposal for accommodating this graded process.

If there are deficiencies in it, which I'm sure there are, then I would appreciate they wrote them down and said what they are. We're putting out this proposal and I wanted to put out something -- get something down on paper to comment on to get a starting point in trying to, I guess, address the main public concerns.

CHAIRMAN WALLIS: This is exactly the opposite from the way that ACRS is going on PRAs and the ACRS got fed up with seeing minimum PRAs and wants every PRA to be good. It seems to be the line we're taking now.

MR. STAUDENMEIER: I can see the holes in this process. I guess one thing we thought is people would be either close to one end or the other end and that there wouldn't be this continuous spectrum in between.

CHAIRMAN WALLIS: Where there are two bosses, not a spectrum.

MR. STAUDENMEIER: Right. This would be maybe their old conservative evaluation models here and this would be the new generation of evaluation models coming in like COBRA TRAC and TRAC-G and things like that and that you wouldn't really be moving across a continuous spectrum like that.

MR. KRESS: I think on the first one you could be on either end but for the rest of them, you are more likely to be on the minimum end.

MR. STAUDENMEIER: Yeah. Well, a lot of the cases while it may not be like conservatism, it may not be that the code you're using is real conservative. It may be that the input assumptions for how you evaluate that event are conservative.

Like you ignore the first safety grade trip or your ACCS flow that you assume is 20 percent below what the high-pressure injection flow really is in the plan, or something like that, where it's more conservative input assumptions than the code actually being conservative.

There are other cases on this chart that I could think of that it's not really exactly. I guess there are exceptions to one box or the other box.

CHAIRMAN WALLIS: The first line any code that is derived from RELAP is simply going to say RELAP has been around for a long time. Ours has this great heritage and everything so nothing much has changed so it's a small change.

MR. STAUDENMEIER: Well, we haven't really accepted that in the past. Actually, there have been vendors that have tried that and referenced old assessment that was done out at INEL on some older code version that had maybe nothing to do with their code version.

We require them to come in with their own assessment that they have performed their own version of the code. That is something that we have considered and hopefully that type of thing doesn't slip by.

The cases where that maybe slipped by is actually where there's lot of analyses that are done without using a formally approved code that aren't really safety analyses like this.

It may be some event analysis or some special type of analysis that a licensee will come in with the support of tech. spec. amendment or some change in the plant and they pull out the NRC version of RELAP-5 which hopefully they have done some QA to make sure it works properly on their system or RETRAN.

They are using it for an event that isn't covered under any approved transient or accident methodology because it's just a special case they are looking at.

In those cases I think that is the most dangerous cases people invoke the goodness of the code because it's been around so long and it's been used for a wide variety of things even though maybe those things have nothing to do with what you are using now. Hopefully in those cases the staff does know that specific assessment may be required for that specific application and what they are using it for in that case.

I have seen a few cases like that that have come across my desk and hopefully we're not letting them slip through the cracks. There is a possibility that may happen. Actually in one big case these risk-informed amendments where it goes through and part of the amendment may have something to do with reactor systems branch.

They used the risk-informed amendment process as a way to bypass reactor systems branch and only the PRA people look at it and may see it done with RELAP or some other code that they know the name of and think this must be good and approve it on the basis of PRA and maybe some of the supporting calculations aren't very good.

CHAIRMAN WALLIS: What you're saying, though, is that full application is going to be required not just for a completely new evaluation model but for an evaluation model which has significant newness to it but not completely new. That would never happen probably.

Also, a new plant design, uniquely new plant design that is an extraordinarily different plants. There are light water plants which significantly differ from old light water plants which require full application.

They don't have to be uniquely new but the difference needs to be significant so I wouldn't pick some of these superlatives or adverbs over here. They make it look as if you only have to do the whole job when things are really tremendously new and different.

MR. STAUDENMEIER: Yeah, I agree with that.

CHAIRMAN WALLIS: Maybe this is just for us to see rather than being part of the Reg. Guide. Is this part of the Reg. Guide, this picture here?

MR. STAUDENMEIER: This picture is not part of the Reg. Guide. I was trying to point out the spectrum of cases that you may have run into.

MR. BANERJEE: So extent of plant change, would that include, say, a request for an uprate?

MR. STAUDENMEIER: Yes.

MR. BANERJEE: That would be significant. Let's say they are using an evaluation model to look at ATWS. Because we operate the plant, you might be driven into different regions of instability and stuff on ATWS. Does that mean we have to requalify the tool that you're using to look at it?

MR. STAUDENMEIER: If the new range of plant operation conditions went beyond what the original methods were approved for, they would have to qualify the tools in that new range of operation. But that is the main thing is to look at your plan operation and accident conditions and see if they are still in the range of what the codes were approved to be used for when the SER was written on the codes.

MR. BANERJEE: So how would you sort of address that specific issue, let's say? The stability maps are going to change and things so how would you quantitatively know that the code has been approved for this or not? Is there sort of limits for what a code is for?

MR. STAUDENMEIER: Yeah, usually in SERs there is usually some set of limitations that the code can be used under this range of parameters and with this specific input or things like that, or specific options so there usually are restrictions put on the use of the code, that it is only applicable for this range of conditions.

More likely there may be some linear heat generation rate on the reflood data or something that you have assessed against and pushing -- if you tried to push a fuel bundle up higher than that. If there was an approval basis on that linear heat generation rate, you would have to go out and perform assessment of new data that was outside the range, or else make some argument that your model is still valid outside that range.

I think one case that has come up recently is that GE Safer Jester LOCA Evaluation methodology where there was an artificial temperature limit of 1,600 degrees put on that so it wasn't valid if you were calculating temperature higher than that.

I don't know but GE at one time was talking about coming in -- that was the result of the data they had assessed against was limited to that temperature range. I think GE is going to come in with more assessment against some other data besides that to raise that limit.

MR. CARUSO: They did the testing. They have assessed it against the data and the limit has been lifted.

MR. BANERJEE: And what about ATWS for the E uprates? How are they analyzing that right now? Fuel is more subdevised so it's faster and all sorts of interactions and instability.

MR. STAUDENMEIER: Well, I mean, they're not -- if you look at ATWS and what are required for ATWS analysis it's not as complex as going through a full ATWS with all these power spikes and calculating them correcting.

If you look at ATWS what's required for BWRs is that you have a slick system that can eject a certain amount of borated water into the system. Also what they have to evaluate is their operating strategy during the ATWS. You are going to do certain things like one thing you do is based on suppression pool temperature.

If your suppression pool temperature gets up to a certain temperature, that's when you initiate your slick injection. You have a level control strategy during the ATWS to minimize power produced and power dumped to the suppression pool.

You're not really evaluating to the point of large-scale oscillations well beyond that design basis where you see in lots of ACTW calculations that's not really part of the regulatory process of ACTS.

There's stability constraints that are put in, stability maps that are derived, and they have to operate within the stability limits. Those stability maps are updated at the new range of operating conditions for the power uprate.

MR. BANERJEE: So you would just take whatever is the existing methodology and, say, in this case it would be okay?

MR. STAUDENMEIER: It may be okay or it may not be okay. You have to look at the methodology to see if the new application lies within the range of applicability, and if it doesn't, you have to do something to update your methodology so it does.

MR. BANERJEE: How would you judge in this case? I mean, are there experiments under this?

MR. STAUDENMEIER: There are some experiments related to ATWS in the FIST facility at GE. Obviously it doesn't have the full kinetics feedback. There's various parts of your -- you would have to compare various parts of your evaluation model to applicable experimental data if you have it.

Like with the onset of instability there are some tests. I can't think of the name but the experiment now is over in Sweden, the full-power bundle experiments. The void fraction is there, too.

CHAIRMAN WALLIS: FRIGG.

MR. STAUDENMEIER: FRIGG. Yeah. They have onset of stability experiments in there. GE has their own experiments on heat transfer when you get into the oscillatory flow regime at, I think, the ATWS facility. They have these FIST test that were done in conjunction with the NRC for looking at transient behavior in BWRs.

There is data out there and piecing it together to get, I guess, a unified picture of how well your evaluation model is may take some work because you don't have it all in one place. There are ways to determine whether your model is operating within its range of applicability.

As far as uniquely new plant design, I mean, the pebble bed would probably have been the first plant where we could have maybe applied this full process in all its glory and see how well it works because it's way different than anything we looked at before, new evaluation methods that haven't been used before or licensed before in the NRC.

Actually I think we told the pebble bed people that to look at this draft Reg. Guide as an example of what their evaluation models were going to have to meet when they came in with them for a specific review and approval.

MR. RANSOM: Along that line, is there any data for the pebble bed modular reactor from the

German work that were transients or experienced in those reactors and could be used for code validation?

MR. STAUDENMEIER: I think there is data. I'm not familiar with it but I know people were -- I thought they went over and visited Germany and some Germans came over here so I think there was a project there. There is also a small, I think, pebble bed reactor in China that people were looking at data from that reactor also.

MR. RANSOM: In order to apply this process you would need something like that I would think.

MR. STAUDENMEIER: Right. Yeah. I mean, for the pebble bed process people coming in with that would have to do field qualification testing. Since they were touting risk as a big thing under severe accident conditions, I think they were going to have to do this field qualification testing and characterization.

MR. RANSOM: Along these lines, has Reg. considered that if somebody comes in with a radically new reactor with a rather limited data base, particularly in comparison with light water reactors, they pretty well know how they behave and everything, how would you factor in conservatism, or should there be some overarching conservatism applied to, say, concept like that that presumably could be reduced with time, you know, as more experience is gained with the thing.

It would seem like that would be a rational approach to licensing a brand new type of reactor. Insist initially on a fair degree of conservatism that could be reduced in time as this process matures you might say. Has any thought been given to that sort of thing?

MR. STAUDENMEIER: I don't know. I don't think about it. People that would be thinking about it are higher levels than I am. That is one thing to consider. I guess that would be follow the light water reactor process when they were first licensed they had large amounts of conservatism in everything. As more data became available, that was reduced over time.

MR. RANSOM: I would think the risk-informed regulations would have to have some kind of mechanism like that where things that are less certain would be -- you know, there would be a higher degree of conservatism than there would be, say, in --

MR. KRESS: I think you are on to something there. That was my concept of how you deal with defense in depth. It depends on the uncertainty. Then as you gain in experience and using that uncertainty, it gets smaller because of more knowledge that you ought to be able to reduce some of the defense in depth which is reduce some of the things.

I think it's a very reasonable approach. It ought to be built into the risk-informed reg. someway. I don't see it there in what I've seen so far. That belongs maybe not here but in the risk-informed reg. stuff it belongs there.

MR. BANERJEE: But then you would have to take into account also unexpected phenomena that occur as plants get older.

MR. KRESS: You have to keep that in mind.

MR. BANERJEE: It's sort of a risk.

MR. KRESS: It takes you the other way.

MR. RANSOM: They've been talking about aging, you know, and how do you factor that into the risk informed regulation. Some of that is possible, I think, because something is known about it.

MR. BANERJEE: Every few months we get a surprise, you know. A new sort of break occurs so I'm sort of skeptical.

CHAIRMAN WALLIS: Every how many months?

MR. BANERJEE: Every few months. I mean,

it's some sort of -- at least recently it seems to be. There are hydrogen explosions and David Bessie.

CHAIRMAN WALLIS: So we are due for another one now?

MR. BANERJEE: Possibly. A new surprise.

MR. BOEHNERT: Well, actually --

MR. KRESS: We have a new frequency of surprises.

MR. BOEHNERT: They do have a problem at Quad Cities with the uprate. Actually they've had problems with a steam drier. I haven't had the details yet.

MR. KRESS: I would be real interested to hear more about that.

MR. BOEHNERT: Yeah. I'm going to find out what it is.

CHAIRMAN WALLIS: As they try to operate the power they found things happening.

MR. BOEHNERT: They had something in the drier break or something.

MR. ROSENTHAL: But we're not sure yet that is due to the power uprate. We know there is higher steam flow. We know the thing was vibrating. We know they analyzed it. Or is it simply something that is 20 years old that broke? So let's be cautious on the leap.

MR. STAUDENMEIER: Okay. Conservatism in evaluation models. Many of the public comments stated that the current evaluation models had a large degree of conservatism and they didn't want to apply this full EMDAP process to something that was so obviously conservative. If you do a close examination of their claims of model conservatism, you see that nobody has ever quantified how conservative this is and what they are really referring to really isn't how conservative the underlying thermal-hydraulic code is. It's how conservative the scenario input is to the scenario like you may not take credit for all the safety grade trips or you may have less flow as you input your pump output for safety injection or things of that matter.

CHAIRMAN WALLIS: Something like a Bubble Rise Model. No way of telling whether this is conservative or not. The very idea of trying to show it's conservative is probably preposterous.

MR. STAUDENMEIER: Or like they do DMB analysis on the worst pain in the core under the worst starting conditions and the worst transient scenario that you can think of. That is what they think of as just being conservative and that the plant doesn't really operate in a way that they have assumed in these safety analyses.

CHAIRMAN WALLIS: It's like sort of assuming that the large-break LOCA is the worse that can happen. It turns out actually the small-break LOCA is more challenging. Just because something seems to be in worse condition doesn't mean it is the worse condition.

MR. STAUDENMEIER: So I guess the question I struggled with a little bit is how can the degree of conservatism in the evaluation model be demonstrated without a full CSAU analysis. I came up with a simplified method that may or may not work very well. It hasn't been tried out in practice yet.

MR. SCHROCK: I don't understand. Is that your question or their question?

MR. STAUDENMEIER: This is my question. I'm questioning how -- they want to take credit for all this conservatism.

MR. SCHROCK: I would ask you further how can you demonstrate the conservatism of what was formerly called an evaluation model or specifically an Appendix K model by doing a full-scale CSAU. Isn't that implied here? If you do a full CSAU analysis on an Appendix K evaluation, that you would come to a better understanding of the degree of conservatism.

I don't understand that logic.

MR. STAUDENMEIER: Yeah, I think that is the logic. I think you would come to an understanding of how conservative it is unless you are saying the CSAU method falls apart for conservative models.

MR. SCHROCK: I don't see what its relevance is to the Appendix K model.

MR. STAUDENMEIER: Well, I guess, the industry's point is it isn't relevant because we think we have this largely conservative model and we're just going to --

MR. SCHROCK: Maybe relevance isn't the right word. It isn't apparent that it's possible to apply a full CSAU analysis to an Appendix K calculation.

CHAIRMAN WALLIS: And lacks all of the Appendix K assumptions and see what the consequences were.

MR. SCHROCK: They are two different things. I mean, one is an assessment of realistic calculation and the other one is something off in the never never land of fantasy.

MR. STAUDENMEIER: Not in all cases. I mean, like a small-break LOCA Appendix K calculations, the only real difference between best estimate or realistic small-break LOCA calculation and Appendix K small-break LOCA calculation is generally decay heat is the ANS-73 times 1.2 and the break flow. Essentially everything else in the small-break LOCS calculation is the same as what you would do in a realistic calculation.

CHAIRMAN WALLIS: And you capture the break flow.

MR. SCHROCK: Are you saying that the physics of small-break phenomena are addressed in the thermal-hydraulic models in Appendix K calculations?

MR. STAUDENMEIER: Yes. I mean, most of the vendors use something like -- I mean, a lot of the vendors use something based on RELAP-5 and it's essentially NRC based version of RELAP-5 with some modifications to make it comply with Appendix K and maybe some more enhancements in places they thought the code was deficient when they picked it up. But for all essential purposes, at one time except for the Moody Break Flow, which I guess you are responsible for, and decay heat, which I guess you are somewhat responsible for.

MR. KRESS: You guys are to blame.

MR. STAUDENMEIER: Then essentially they are the same calculation as a realistic calculation except they are not -- obviously they have done assessment against various stuff to show that they think these models are conservative but there's no real difference between the models and the codes and the models and a realistic code in terms of small break.

DR. MOODY: There's a term I haven't heard mentioned here and that's called the worst case. When you've got a model, usually when you input or your boundary conditions are picked for that model to give you a so-called worst case, it doesn't necessarily mean the model itself is conservative or not.

I guess a model is conservative based on what the result of that model is. If you are concerned about how fast flow comes out of a pipe, if the model neglects friction, that might be conservative, at least as far as pressurizing a room.

If you are trying to get flow to the reactor, lack of friction is nonconservative because it will allow more flow to get into the reactor than you would expect. I guess it really does -- all these things that have been discussed there has got to be a human being involved here somewhere that has a conscience and is accountable to somebody else.

As I read these guidelines, in my opinion, our guidelines but you can't take the human element out of it. Somebody that does this has got to be a little concerned about his reputation and somehow standing up in front of a group that is going to be critical and making a good story that can be defended, in other words, the accountability, track record, all these things, I think it's great to have good guidelines.

We are kind of nit-picking a lot of these items but I guess that has to happen, doesn't it, in order to put in perspective what the eventual human being is supposed to do that has to come and say, "Look, we've done a reanalysis. We've changed these parameters and we want to convince you that this shouldn't give you gas pains. We got a conservative result."

MR. STAUDENMEIER: Yeah, and for the most part I think that is built into our regulatory assumptions that the people out there are making a good faith effort and are concerned with safety themselves and want to get answers to send into the NRC and that they are not out there looking for every loophole and trying to do things that aren't on the up and up.

DR. MOODY: That's usually happens because of some engineering manager doesn't want to spend the money to go any further than he just has to and so forth.

MR. STAUDENMEIER: The Maine Yankee case is one example of that and that is exactly what happened was management pressure being put down. The analysts knew that everything wasn't quite right but they were pressured into doing something that wasn't officially correct.

I don't think any one of them thought they were going to melt their reactor because of this shortcut or anything or that they were going to cause harm to the public. In terms of following through what they had committed to, they didn't do that.

Yeah, it was a place where our process broke down because we assumed they were going to be following through what was written in the SER and they would be applying the code like that and that just didn't happen.

CHAIRMAN WALLIS: Maybe you need a Reg. Guide for managers.

MR. BANERJEE: It's generally accepted as accounting practices.

MR. STAUDENMEIER: As part of the Maine Yankee thing there was a criminal investigation on that to see how it did break down. That is beyond what we do with code reviews.

MR. ROSENTHAL: I think there was what we termed a tacit understanding and no manager had to tell an analyst at Yankee. This could pertain to any plant. If you are short of MPSH on a plant with a LPSI pump at a certain elevation and a containment sump at another elevation, barring getting out the jackhammers and relocating pumps, you are going to change your analysis.

I'm only use this as an example. I think there was a tacit understanding that on this plant that had been built and run for years that there weren't going to be expensive hardware changes to the plant.

No manager ever had to say anything to an analyst. He understand that so he was going to get out his ever bigger fancier code and do more and more analysis to demonstrate that it was okay.

MR. STAUDENMEIER: The NRC was partly at fault at Maine Yankee for the RELAP-5 part anyway, The code shouldn't have been approved to do LOCA analysis.

If you look at what was written in the SER you see things like the code does too much condensation so as part of your input you are going to artificially raise the temperature of the safety injection water by 80 degrees or something like that to compensate for the fad condensation model and various things like that interspersed.

If you read the SER, it really reads like a code that should have never been approved. When it was approved, they went through all kind of contortions to make it work in some manner but I don't think it ever worked reliably.

MR. KRESS: We call it intentional compensating errors and we don't like them.

MR. STAUDENMEIER: Yeah, it was.

MR. KRESS: The question that I think is a great one to ask, if it were an ACRS letter, we would ask you to put the question mark after the word "demonstrated" and mark out the rest of the question.

MR. STAUDENMEIER: I think that would be a good choice of words.

CHAIRMAN WALLIS: I think today we are just looking at this piece of paper but, in fact, our conclusions are going to be influenced by when we evaluate the four human beings who have spoken to us. I'm just wondering if we had four different human beings, the same piece of paper and we read the same conclusion.

DR. MOODY: Interpretation

MR. STAUDENMEIER: So, anyway, here we go on to my proposed simplified method to demonstrate model conservatism which is the slide I thought was going to take the bulk of the time.

CHAIRMAN WALLIS: So we have another two hours of discussion.

MR. STAUDENMEIER: This was to try and come up with a way to demonstrate your model conservatism for these conservative evaluation models with their conservative input assumptions.

Part of it depended on using this code as best you can in a realistic or best estimate mode to show that your model did have some fidelity to predicting reality and that would be to test your code against some plant transient or scale test and show that your code was actually good at predicting the reality of the situation.

MR. KRESS: I would have chosen number one to be a performed set of analyses or a set of benchmark transients with a best estimate code that is different than the one you have. I wouldn't say use the same code in a best estimate mode. I would say use a code that you consider to be a best estimate code already with uncertainty.

That code would be my choice in the first place to help approve the code that is being the evaluation model in the first place. That along with its comparison with the experimental data. I would have had a different view of what item one ought to be.

MR. STAUDENMEIER: That's another possible way to look at it.

MR. BANERJEE: But if there is already a best estimate code that you can use, why not use that instead of developing an evaluation model.

CHAIRMAN WALLIS: I don't understand. The evaluation model I thought covered everything. Evaluation model is simply a code plus all the things you have to do to make it work.

MR. STAUDENMEIER: It is.

CHAIRMAN WALLIS: You mean using Appendix K assumption?

MR. STAUDENMEIER: You are actually using the evaluation model the way it was meant to with the evaluation model assumptions.

CHAIRMAN WALLIS: What do you mean by that? You mean Appendix K assumption?

MR. STAUDENMEIER: Well, it may not be

Appendix K. In the case of LOCA it may be Appendix K but in the case of transients it may be some other assumptions.

CHAIRMAN WALLIS: I thought we determined that when you say evaluation model you mean simply the whole process of running a code.

MR. STAUDENMEIER: Right.

CHAIRMAN WALLIS: So a best estimate code is an evaluation model.

MR. STAUDENMEIER: The code plus the way you use it is I guess the evaluation model. The underlying code itself, the transient or thermal-hydraulic engine.

CHAIRMAN WALLIS: The best estimate mode and then, too, you use Appendix K?

MR. STAUDENMEIER: Appendix K or the way they would evaluate the transient in the plant.

CHAIRMAN WALLIS: You might find out Appendix K isn't conservative.

MR. STAUDENMEIER: You may and that certainly would be a problem for you.

MR. RANSOM: Well, actually the degree of conservatism should be in comparison with the data. I mean, I think that's what you're suggesting in step one, right?

MR. STAUDENMEIER: Yes.

MR. RANSOM: You would do it for a case where there is actual data.

CHAIRMAN WALLIS: How good does that comparison have to be?

MR. RANSOM: Well, I think you are trying to establish a degree of conservatism.

CHAIRMAN WALLIS: Is this in the guide, this piece of paper?

MR. STAUDENMEIER: It is in the revised guide actually.

CHAIRMAN WALLIS: I don't like this at all. I think this is dangerous. We've got to require that they estimate uncertainties. It's comparing something against something else which is artificial. It's not a good comparison with reality.

MR. LAUBEN: I think what you're saying is really sort of with or without uncertainty.

MR. STAUDENMEIER: Right.

MR. LAUBEN: Or with and without --

MR. STAUDENMEIER: Which the uncertainties may be that --

MR. LAUBEN: Artificial conservatism.

CHAIRMAN WALLIS: I don't know why you need to do this because all these applicants, the

Westinghouse, Seamans, GE are developing formal ways of making estimates of uncertainty and bias. Why should somebody else get a shortcut like this that has all kinds of pitfalls?

MR. LAUBEN: I think maybe, Joe, you were probably thinking of other transients.

MR. STAUDENMEIER: I was thinking of more simplified transients like a pump trip or something like that where you have good planned operational testing data and that the model is fairly simple and only depends on wall friction and the pump essentially.

MR. LAUBEN: And almost all conservatisms are tied up in the input.

MR. STAUDENMEIER: Right.

MR. LAUBEN: If you use nominal flow versus flow that you use for your transient analysis, that is the way it appears. I agree that I think you are going to find this stuff tied up in the process for LOCA but for the transients he's trying a method here that could be done possibly fairly simply.

CHAIRMAN WALLIS: You see, there's no such thing as a best estimate mode. These are simply just estimates and you put different assumptions in and you get a different estimate. There's no way of telling which is best until you compare it with things. The best is presumably something which when you compare it with things has lower level uncertainty than all the other things. Until you've done this comparison and evaluated all the uncertainties, you have no figure of merit in which you could judge best, good, worse, or anything.

MR. STAUDENMEIER: Best is a bad choice of words there. Realistic I guess would be better.

CHAIRMAN WALLIS: But then it means could you assume anything in an estimate mode.

MR. RANSOM: Maybe I don't understand what best estimate means either but I have always thought it meant that if you had a set of data you would hope to get a best estimate calculation that basically is the mean of the data or fits the mean in some way. No conservatism in that. The conservatism comes in when you take best estimate plus uncertainty and that's why I have always said it's very critical.

CHAIRMAN WALLIS: Instead of making a conservative assumption, let's say that you don't allow counter current flow at all. Someone comes up with, "Oh, engineering. Well, this counter current flow and this horizontal pipe is limited by some interfacial stability and we know that is absolutely a Froude number and we know the Froude number probably is somewhere around 1 so we'll say that when we get a Froude number 1 we get instability which they've got in the code. This is an engineer's guesstimate. Then it becomes a best estimate because it's not conservative. There's nothing best about it. It may be a lousy estimate. Because he's not conservatory it gets called best estimate.

MR. RANSOM: It's your best shot at it.

CHAIRMAN WALLIS: It's your best shot but it may be a lousy shot.

MR. RANSOM: Sure. That's where the uncertainty comes in to play.

CHAIRMAN WALLIS: Our view -- my view is that you shouldn't use the word best ever unless you've got some measure of how good it is.

MR. SCHROCK: Or define what it means. I remember discussions about what best estimate meant when the rule change was under consideration. The image that I recall for it is that it's a fluid thing.

Best estimate means it's the best engineering calculation you can do at any point in time with the expectation that it's going to get better as time goes by. The best estimate is something that will be constantly changing, constantly getting better.

DR. MOODY: We would have no trouble deciding what a worst estimate would be to help us understand what a best estimate is, would we?

MR. RANSOM: Well, I think I agree. That's my understanding of what was meant by best estimate but it would probably be helpful if this thing was clarified. I mean, at this stage if we don't understand what best estimate means --

CHAIRMAN WALLIS: We don't use the term. We call it realistic code and a realistic code, which is a code where you try to do a good job, is not complete without estimates of uncertainty, without quantitative assessments of the uncertainty in those estimates.

MR. RANSOM: You're saying realistic.

CHAIRMAN WALLIS: It is simply saying tell me what you predict and tell me the uncertainties in what you predict. Presumably if it's a better code this uncertainty band will be smaller presumably. We don't use the term best. The uncertainty rate is a measure of how good the code is.

MR. KRESS: Which is a real nice academic concept but nobody knows how to do that uncertainty.

MR. RANSOM: That's the problem.

MR. KRESS: Graham you gave an example of something where the counter current flow situation was not included, but yet it was maybe your best shot at the time so is that realistic? If it is realistic, then I guess realistic would include some kind of uncertainty to allow for that.

CHAIRMAN WALLIS: That's not good enough because I look at this documentation and I read it as the knowledge I have and I think that's a lousy estimate. What am I supposed to do?

MR. SCHROCK: But it can be the best among lousy.

DR. MOODY: I think best has become a dirty four-letter word here in the last five minutes. Shall we using things like most realistic? Is that going to --

CHAIRMAN WALLIS: It has become an excuse. Giving it your best shot often means you're doing a bad job because you don't know what you're doing.

DR. MOODY: It helps to know where the target is.

CHAIRMAN WALLIS: But, anyway, let's get back to what you are proposing. I think you make a lot of discussion of this as you anticipated. I think you should be very careful what you put in for simplified way to demonstrate conservatism.

MR. STAUDENMEIER: Yeah, I agree with that. I would appreciate your comments.

CHAIRMAN WALLIS: I think something like a CSAU or better or equivalent.

MR. STAUDENMEIER: There may be no way to do this simplified method that I had in mind or may not be defensible in all cases. If that is the case, then we really can't have this and they do have to go through an analysis.

CHAIRMAN WALLIS: I suppose you could have sort of a screening thing which says look at your estimate model and look at the Appendix K calculation and see if yours is conservative. If it's not conservative, you go to another block in your diagram as sort of a screening thing. I don't think it's a substitute for a better uncertainty analysis.

MR. STAUDENMEIER: I guess kind of what I had in mind in this thing was a case like a pump trip or like a turbine trip. In BWR you have turbine trip data from Peach Bottom. Run your code in its realistic mode and see what that is. Run your code with its evaluation model assumptions that you have to put in. That shows you an idea of how much conservatism there is in terms of real data.

Then do the same thing at your own plant for the calculation that you're interested in to give you an estimate of how much you are over-predicting whatever parameter you're looking at for the transient you're interested in. For like a turbine trip you would be looking at CPR margin.

That sort of process applied to real data and using comparison between running in the realistic mode and in your evaluation model mode would give you an estimate and you would really only be able to use this if there was really a very large amount of conservatism in your ER analysis method.

It's not something that if it was barely conservative if it turned out that your method wasn't as conservative as you thought it was, then maybe you wouldn't be able to use this method at all to evaluate your uncertainty.

You would have to come up with a better method to do it. This would only apply in cases where it was truly highly conservative and this would be a simple method at getting at that amount of conservatism.

CHAIRMAN WALLIS: It only covers then the assumptions of the evaluation model which are rather few.

MR. STAUDENMEIER: Right. It would only really apply to simply evaluation models.

CHAIRMAN WALLIS: Appendix K. If you use something -- there are very few assumptions specified and you can relax those but that's no measure of the quality of the code itself.

MR. STAUDENMEIER: Now, in Appendix K -- like Appendix K I wouldn't consider -- I mean, your realistic estimate would be transfer correlations you had in there. Actually you would be surprised that some of the Appendix K reflood heat transfer correlations is good or better than realistic heat transfer correlations.

I mean, you compare the data and they do very well and all the conservatism in reflood is in your decay heat assumptions or your reflood or your calculations that you're getting what the reflood rate is which it may be helpful to downcomers or something like that. If you look at the reflood heat transfer correlations themselves, they are as food as any you can get.

MR. BANERJEE: During the discussion maybe you should, if you would, go through the logic of these five steps.

MR. STAUDENMEIER: Okay.

MR. BANERJEE: Let's assume that instead of best estimate we call it realistic estimate or something. But still I'm not understanding what it is you're proposing. I haven't got the sense of it completely.

MR. STAUDENMEIER: Okay.

MR. BANERJEE: Just reading it.

MR. STAUDENMEIER: Yeah, this would -- well, let me go through an example like pump trip in PWR. There is good data for all the plants out there because they have to do those type of testings when they start up the plant so you know how your pumps are going to coast down and how your flow is going to coast down.

Performing analysis of one of these transients you have data for using the realistic mode of your calculational tool to show that you are fairly good at predicting the realistic behavior in the plant. Then --

MR. BANERJEE: In this case what might that be? What would you change in your model?

MR. STAUDENMEIER: You might take out like conservative assumptions based on wall friction or lost coefficients. They may stick in conservative methods for computing lost coefficients, for instance. They stick in extra lost coefficients to make it coast down faster than the plant would in reality because in something like that you would be looking at DMV margin. The worse case is the flow coasting down as fast as possible.

In that case you would put in what your best assessment of the lost coefficients was or any other type of input assumptions like pump head curve. They may have in pump head curves or something or the inertia in the pump they put in conservative values for lots of those things.

Then in the realistic mode you would try to model the plant as best as you could to show how good the code was predicting if you had realistic values for all these things.

MR. KRESS: What is your figures, DMB or is it flow rate?

MR. STAUDENMEIER: For that it would be -- the ultimate measure of merit is DMB so you would have to relate it somehow to the safety parameter. Perform the analysis of that test again with these conservative assumptions that may be in your evaluation model.

That would show using them that maybe the flow coast down much faster using the evaluation model assumptions so that would make the DMB much worse. But this test that you performed isn't the worse case conditions in the plant that they do their safety analysis for.

The power shape may be different or various other things may be different that they perform their actual safety analysis for. You would perform the same sort of exercise in your safety analysis calculation where you have all your trip set points and other things set the way you would.

MR. BANERJEE: So you are still at step two. You are still running the transient for which you have data.

MR. STAUDENMEIER: Right. At step two you are still running the -- okay. Then you jump to step three and take the event --

MR. BANERJEE: What do you mean by compare the key figures of merit? In this case would it be the rate at which the coast down is occurring or would you actually make the next step and go to the DMB prediction because you may not have any DMB prediction there.

MR. STAUDENMEIER: Right. I think you would do both in that case. You would show DMB prediction, although in that case --

MR. BANERJEE: And you may not have data for that, right?

MR. STAUDENMEIER: Data for the DMB? Oh, you don't have plant data for the DMB but you would be comparing your DMB prediction using your approved correlation for your DMB model.

MR. KRESS: Implicit in that is that the reason that's an approved correlation or approved original code is somebody originally made a judgment that it is conservative enough and there is margin enough there. You just go ahead and say we'll accept that because we wouldn't have approved it in the first place otherwise.

MR. STAUDENMEIER: You would compare maybe the pump coast-down rate or flow coast-down rate and the DMB and it's ultimate affect on DMB. The DMB is something there and you have taken your data for that specific bundle and those specific grid spacers and things like that and have this range of DMB data over the whole range of conditions that it needs to be applied for. That is probably the best part of the model actually.

Then that pump trip event may not be the exact event you're going to analyze in your safety analysis but you pick the pump trip because it's similar to the pump trip and it has all the same phenomena going on to your actual safety analysis pump trip calculation.

Use initial conditions at the associated tech. spec. limits that are the worse case for this pump trip and run it again in realistic mode with the realistic lost coefficients. Look at the plant response and run it again with your evaluation model assumptions that you have to use for the approved evaluation model and show that change.

If it was DMB, show the ultimate result on the DMB prediction to give you an idea of the amount of conservatism in your calculation. Like I said, if you is a large amount of conservatism in the calculation which this would demonstrate.

And you have also demonstrated your code was okay at predicting these phenomena, then maybe that would allow you to put less effort in and not have to evaluate uncertainty in making a small change to this model. If you really didn't have much conservatism at all, then you would say, no, this isn't good enough.

CHAIRMAN WALLIS: Let's look at what we see with, say, LOCA analysis. We get these LOCA analyses and they use Appendix K and they get up to 2,199 degrees and everything is fine. Maybe this involves also a link to containment performance or something. Then they say, "Well, when we do a realistic analysis we only predict 1,400." There is obviously a great deal of conservatism. What do you do with that?

I mean, now they say we've got to relax it so we've got this margin of 800 degrees to play with. Therefore, we are going to make other assumptions and change the model and so on. Because it's conservative you have no way of evaluating what you can let them do. They might as well use Appendix K.

MR. STAUDENMEIER: Well, Appendix K has specific rules that they have to follow.

CHAIRMAN WALLIS: It doesn't help them in saying we are now going to replace Appendix K with this other model and now we are going to use it to predict temperatures which Appendix K would predict to be above 2,200. Because our models is a good estimate with uncertainty, it still meets the intent of the regulations.

MR. STAUDENMEIER: Well, that would put you into the realistic LOCA calculation mode where you would have to be --

CHAIRMAN WALLIS: It seems to me that you can compare with Appendix K and show your estimates but it doesn't let you do anything because you are still regulating according to Appendix K. As soon as you try to regulate in accord with the realistic model, you throw away Appendix K. Comparison with that is absolutely irrelevant. What is the uncertainty --

MR. STAUDENMEIER: Yeah, I wouldn't apply this to LOCA calculations at all actually. This would be applied more to transient calculations. If that's what you believe, you should comment that way and we'll consider it. Like I said, this is a proposal.

MR. BANERJEE: I guess the concern that I would have, and maybe Graham has similar, is that if you do a very limited amount of work under one there, you may get the wrong idea about the realistic model. Let's say for the sake of argument it was LOCA. It doesn't have to be. So you decide that the realistic model will not use the decay heat so you reduce it by 20 percent or something.

You decide that you will be able to rewet even if the reflood rate is below one inch per second or whatever. You maybe still will not allow rewetting, you know. Whatever it is. In any case, your realistic model will come up with some estimate like 1,400 or 1,100. It will come much lower.

If you just have a few tests even, let's say, LOFT but only in the blow-down phase, it may give you completely wrong information because you might not take into account boiling and the downcomer and all these other things which would be part of a realistic model.

You may falsely think you have a big margin because you've got a very limited small scale set of experiments against which you have done the comparison. That would be a concern.

MR. STAUDENMEIER: I agree that is a concern. You have to be very careful about the amount of phenomena going on and competing with each other and making sure that you had a real good handle on this.

That's why I guess my original intent was to have this applied to more simplified scenarios or events and not allow you to apply to complicated events where you can't sort out everything. This would be more or less where you knew there was one dominant phenomena and knew we had a good handle on that.

MR. BANERJEE: That would be fairly

realistic but, in that case, how do you sequester this so that people don't use it where there are multiple phenomena, some of them not well understood like boiling in the downcomer or whatever the hell. How do you ensure that they don't do that?

MR. STAUDENMEIER: Well, by the review process at the NRC. I guess it lies on the reviewer to pick up on these things and recognize that this isn't really a place where it can be applied well and make sure that they've done enough assessment against adequate data to show that they know what all these dominant models are, that it is simple enough that you could really apply those.

MR. ROSENTHAL: Let me back up a little bit.

MR. STAUDENMEIER: And I think in these simple events you could actually end up doing something similar like in a pump trip. I mean, you're only -- your main uncertainty is wall friction and lost coefficients and momentum.

You are transporting momentum along loops here. Decaying momentum around the loop and your full-blown CSAU type analysis may not be very complicated for that case either because there's just a small number of phenomena dominating the whole thing and the code assessment is fairly simple.

It may be single phase pressure drop in various geometries and know what uncertainty your wall friction or lost coefficients may have around the base value. It may be almost as simple to apply the full-blown uncertainty analysis in that case also.

MR. ROSENTHAL: Let me try to just remind everybody that no Reg. Guide ever replaced a regulation and Appendix K would still be enforced. Similarly, this Reg. Guide is not intended to replace Reg. Guide 1.157 which is our best estimate ECCS analysis.

Having said that, the next thing, and maybe we could do a better job at it, but when you review the code you are supposed to say what accidents or transients do you think it's applicable for and which are not.

With that, about a year -- actually, I think 15 months ago we had a public meeting in the building and the regulated community said for things like LOCA we understand the large investment and analysis that we have to do and I don't think we had very much resistance.

The way you are writing this Reg. Guide for all transients and anticipated operational occurrences you are asking for a fair amount of analysis and that seemed disproportionate to the challenge, either the reactor dynamics, the time scales of what's going on in the reactor, or just how many pins would go through DMB.

I think we were focused now on what one might do for transients or AOOs. The first bullet says, "Well, you ought to benchmark this against some operating experience or experiment." That's a leg up. That's a leg up right there against stuff that is maybe even more obscure. We were trying to come up with some middle ground for these less severe transients, not for LOCA.

CHAIRMAN WALLIS: I don't know that you can.

MR. STAUDENMEIER: It wouldn't be for major code changes. These would be what we would consider relatively small code changes.

CHAIRMAN WALLIS: Suppose you say that there's a Moody Break Model in this evaluation model or whatever it is, No. 2. We know that is a huge simplication of reality. We know that thermal nonequilibrian plays an important role, probably more important than some of these film mechanic things and thermal nonequilibrian isn't in the Moody Model at all.

It's obviously more realistic to model these things. We've got a two-fluid, two-temperature model for break flow and we have evaluated all the terms and it does a tremendously good job of modeling break flows. It's obviously a much better representation of the physics than this Moody Model.

When we use it in the code for a lot of different transients, sometimes it's more conservative and sometimes it's less. It's all over the place but it's a darn sight better model than the Moody one. I don't think you can make any evaluation of its conservatism on the basis you have suggested here but it's a much better estimate of what happens and we ought to be better.

Therefore, you can justify if you compare only with data. If you start comparing with some figure of merit, I think it depends on which scenario you pick and all kinds of things. I'm not sure that it's a good way of saying is this a better estimate. Do you see what I'm getting at? Maybe I'm not being clear.

MR. BANERJEE: It's a more realistic estimate

CHAIRMAN WALLIS: Yeah, more realistic.

MR. BANERJEE: It may be sometimes conservative and sometimes not.

CHAIRMAN WALLIS: But compared with a Moody Model which really should be consigned --

DR. MOODY: Careful now.

CHAIRMAN WALLIS: -- to mythology.

DR. MOODY: Well, there was 15 minutes of glory.

MR. RANSOM: The Moody Model is consistent with the maximization of entropy, right?

CHAIRMAN WALLIS: It doesn't say anything about thermal time nongrouping.

MR. SCHROCK: I think I heard you say that this is in the Reg. Guide. I don't --

CHAIRMAN WALLIS: It's not.

MR. SCHROCK: I don't know where it's in the Reg. Guide.

MR. STAUDENMEIER: What is in the Reg. Guide?

MR. SCHROCK: This that you have on the board now.

CHAIRMAN WALLIS: It's not, is it?

MR. STAUDENMEIER: I'm sure it is because I copied it out of there to make this Reg. Guide. Actually, I corrected a couple errors that were in the part in the Reg. Guide I think.

CHAIRMAN WALLIS: Where is that?

MR. BANERJEE: 27.

MR. RANSOM: Where is it?

MR. BANERJEE: Page 27.

CHAIRMAN WALLIS: The simplified method is on page 27?

MR. RANSOM: Graded approach.

MR. BANERJEE: It's under 5.3, Degree of Conservatism.

CHAIRMAN WALLIS: Oh, that's where it is. Okay.

MR. STAUDENMEIER: I hid it at the end. I was hoping you would stop reading by that point.

MR. SCHROCK: There's no qualification in it as to what you had in mind it applying to. You have indicated now you wouldn't think of this in terms of LOCA but just plant transients.

MR. STAUDENMEIER: Yeah, that would -- if it was deemed that it is a feasible approach, then we would have to put some qualifications on that. Right now it looks like there's a lot of controversy over whether it's even feasible.

MR. RANSOM: There's even a sort of nit pick. I don't know what you mean by model change in number five. I notice it's on your number five both in the write up because nothing prior to that on the slide, at least, talked about any change.

MR. STAUDENMEIER: Okay. This is a process you would apply to making a model change. That's what this whole section was about.

MR. RANSOM: You mean you reduce the model to --

MR. STAUDENMEIER: Yeah, a graded approach. This would only apply to making small model changes is what we had in mind. If you were changing a heat transfer coefficient or a loss coefficient or some kind of correlation somewhere in the code it would be evaluating the impact of changing that model compared to what you thought was your degree of conservatism in the model. If it was a small pertibation to this estimated degree of conservatism, it's okay. But if you are cutting out all your perceived or estimated conservatism, then you wouldn't. This was meant to be applied to small changes with large amounts of conservatism in the model.

MR. RANSOM: With the idea here, I guess, and using best estimate I think in the sense that it's used here you would have best estimate plus uncertainty. Now the attempt would be to get best estimate plus uncertainty to equal reality I guess. Right?

MR. STAUDENMEIER: Right.

MR. RANSOM: That would be your limit. So you could iterate through this thing until you actually changed your model enough that you finally are just predicting best estimate plus uncertainty equals reality. In other words, no margin left.

MR. BANERJEE: But there is no uncertainty estimate here.

MR. STAUDENMEIER: We wouldn't allow this method if it would be applied while down in that range.

MR. RANSOM: It keeps saying that you would -- the methodology --

MR. BANERJEE: The methodology, as far as I can see, does not require an estimate of uncertainty.

MR. STAUDENMEIER: The estimate of uncertainty comes from this simple process here. That was --

MR. BANERJEE: That's a different issue.

MR. RANSOM: So the degree of conservatism is synonymous with the uncertainty, I guess. The degree of conservatism as it's used here is synonymous with the uncertainty apparently.

MR. STAUDENMEIER: It was, I guess, meant to be applied where we have it.

MR. BANERJEE: If you put it back into the way we looked at the CSAU stuff if you want to translate it into those terms.

MR. RANSOM: I'm really confused, I guess, because this Reg. Guide talks about best estimate plus applying the CSAU process which implies evaluating the uncertainty. I'm not quite sure what best estimate means now because of our discussion today. Then you move over to the method here which seems to eliminate the uncertainty and makes that synonymous with conservatism.

But implied, I guess, in all of this is the degree of conservatism is going to be limited by whatever the uncertainty. The degree of conservatism is going to be limited by the uncertainty associated with the model and the process.

MR. STAUDENMEIER: Right.

MR. BANERJEE: That's the problem I guess.

MR. STAUDENMEIER: We would try to limit this to things that you had well understood models that had a fairly quantifiable level of uncertainty like something with simple phenomena governing the whole thing.

MR. RANSOM: I guess my problem with this whole thing is I think we know what we mean and want to do but it somehow is not very tight. Certainly to the uninitiated who would come in and read this process would say that it's like wondering all over the map.

I've always thought that anybody who worked for the Union of Concerned Scientists if you really knew what went on here he would have a lot of room to attack this process.

CHAIRMAN WALLIS: This is also for industry. If the only model which is allowed in terms of a change is one which is more conservative than you had before, which is what this seems to suggest, that's not much advance. The only thing that is being evaluated as a criterion is how conservative is it.

MR. STAUDENMEIER: Well, this would be -- if it changed the conservatism by a small amount, we would allow it. If it changed it by a substantial amount of your estimated conservatism, then you would have to go through a much more detailed process. This would only be for model changes that were a small partibation of bation on the amount of conservatism.

MR. KRESS: Sort of like Reg. Guide 1.174 concept.

MR. RANSOM: Well, actually it permits a small change increase at risk. Right?

MR. KRESS: This will, too.

MR. STAUDENMEIER: This would allow a small change in the nonconservative direction.

DR. MOODY: Let me ask, and maybe Vic has a good answer. How is this different than rocket science? You mentioned rocket science a while ago. People never put a person on the moon but did they go for best estimate? Did they go for realistic? Did they test everything full scale? Did they test everything in zero or one-seventh gravity?

MR. RANSOM: Part of the answer to that is yes, they tested in full scale. Every engine is tested before it is ever put on one of those vehicles and statistically tested to the point that the probability of failure could be estimated to be 99999. I mean, not fail for liability. That's been proven to be. Well, there are other things, too, like defense and depth is really to them is, I think, bail on fail safe. Some philosophies like that and design and redundancy and design. They will have four valves where really in the real process you could have one.

Parallel series so it allows for the possibility of, say, at least one valve failing. That kind of thing is done there and to a large extent -- well, the main difficulty between that and nuclear science, I think, is you cannot test this under full scale conditions and worse accident type situations. You do blow up engines on the test stand. That's happened. When that happens, there hell to pay on down through the design review process, the refits, and the amount of retesting which must be done to verify that, indeed, that problem has been fixed.

Actually, when that's been violated, you saw the challenger accident where the indications of problems with the o-rings on those solid rockets was swept under the rug and, indeed, later led to a catastrophe.

MR. BANERJEE: Even though there were memos.

MR. RANSOM: There were what?

MR. BANERJEE: There was a memorandum from the engineers.

MR. RANSOM: Oh, yes.

CHAIRMAN WALLIS: Some management decisions were made.

MR. RANSOM: It's a safety culture

problem.

MR. STAUDENMEIER: In Arian 5 they never tested the control software on it. Range of conditions had a numerical overflow and the control system was no good and the rocket crashed.

CHAIRMAN WALLIS: I still don't understand this. Suppose I come up with a better momentum equation to use in my code, I do all sorts of evaluations against data and is far better than the one that is used now. I've run the same transient using this momentum equation and making Appendix K assumptions. That's what you asked me to do for step 2 and I come up with 2,210 degrees. Therefore, I'm not allowed to do anything? I want to use my realistic code and not make these assumptions. That's what I'm driving at.

MR. STAUDENMEIER: I mean, in that case where we uncover severe deficiencies in the model, that's considered a model error and you have to go and fix the thing.

CHAIRMAN WALLIS: For these assumptions you are required to do an Appendix K and say nothing about fidelity of your momentum equation.

MR. STAUDENMEIER: Appendix K says that you have to access your code against applicable experimental data to the best extent possible. If you have data they will assess how good your momentum equation is. You are supposed to assess against that data.

CHAIRMAN WALLIS: I'm trying to see how your method here, your subsequent defined, enabled me to put a different momentum equation in my coat. I just can't see how they let me do that because I don't really see what you're asking me to do in number two provides any assessment at all of how food my momentum equation is.

MR. LAUBEN: Could you put figure 13 back in? Maybe if I were looking at Figure 13 or assessing it because I think there are five properties in Figure 13. One of them is -- the first one is the novelty of model or, if you will, the change that you are talking about that you might put in your code. If it is something like a new momentum equation, that's a pretty significant change to a code, especially a LOCA code. I think right there you are not on the minimum application side. You are definitely on the full application side.

The next one is complexity of event. It's a large break LOCA that we're looking at. The event is very complex. Now you are around the full application side of at least two of the properties. I think what this whole thing that Joe was trying to show was in cases where you're on the other side, at least most if not all the time.

So I think in the example you cited, Graham, where you are going to put in a new momentum equation, by golly, that's really purchasing a completely new evaluation model.

MR. STAUDENMEIER: Yeah, the case I ought to apply this more to is changing something in the momentum equation like your wall drag or something. Or maybe you want to put in Reynolds number, dependent WAS coefficients so you are putting a small partibation to your momentum equation under the conditions you're using.

CHAIRMAN WALLIS: Okay. How does that help me with something like Appendix K? I want to change the world drag somewhere.

MR. BANERJEE: Appendix K you are not allowed because of the complexity of the event.

MR. STAUDENMEIER: Yeah. Appendix K they would have to meet all the Appendix K requirements. Appendix K they would have to go and meet all the Appendix K requirements. Any change they made to that they would have to assess against the proper data.

Appendix K relies on these large imposed assumptions to ensure that you are conservative. In some cases we find maybe that is not the case like the downcomer boiling issue. But then I think that gets treated as more or less a code error or code deficiency that you have to do something about to correct.

MR. BANERJEE: That gets back to the decay heat.

CHAIRMAN WALLIS: But I think this gets back to the whole thing that codes have been criticized for through the ages. By focusing attention only on these figure of merit, you allow all kinds of nonsense in the code simply because it turns out that it seems to meet some figure of merit in some conservative way until the nonsense is in the code forever simply because at some time it was shown not to have much effect on preclad temperature, let's say.

MR. SCHROCK: Yeah, it looks like you have a disincentive to doing the calculation correctly. Having a code which is based on I'll say correct equations. I mean, more correct equations.

MR. STAUDENMEIER: Why do you say it's a disincentive?

MR. SCHROCK: Well, because you are going to require a full-blown new justification of the so-called evaluation model.

MR. STAUDENMEIER: Right.

MR. CARUSO: You mean for new codes, Mr. Schrock?

MR. SCHROCK: I mean if you said you look at the momentum equation in TRAC-M and say, "Uh oh, this isn't right and here is another equation which is closer to the truth," if I put this better equation in there, then I am going to have to start in step 1 and do the complete CSAU thing all over again.

MR. RANSOM: That's my understanding of CSAU.

DR. MOODY: Just recently, Graham, you put out a note using Bernauli's equation for two separate streams, liquid and vapor, going into a branch for something. It was apparently to make some correction because there was a quibble with the way it was being handled in one of the codes.

CHAIRMAN WALLIS: This was no quibble. No, no, no. This was -- actually, this might have got into a code. This was low on the branch which was a research program to development them all which would go into TRAC-M code.

DR. MOODY: Does that apply now? Is that

a kind of an example where like someone comes along with a better representation, more physics in the problem or the right physics. Then they want to incorporate that in the process here. Is that what we're talking about? Where do you get on the loop here and how far do you go?

MR. STAUDENMEIER: This was to come up with a way that was simpler than going through the whole process, this graded approach that small changes that would be improvements or something if they were a small change wouldn't need to go through the whole detailed process because we don[t want to put constraints on people correcting known deficiencies in the code.

DR. MOODY: I guess in that case that would not have been a small change though. Is that right? That was a significant issue but if it were just a matter of whether you leave out the velocity in the large pipe and simplify, that would be perhaps a small change.

CHAIRMAN WALLIS: I think our criticism there was that the model was not believable in its form as presented to us. Here was another one which was not very good but at least was somewhat more believable. That was the gist of that.

DR. MOODY: This process would handle something like that.

CHAIRMAN WALLIS: I don't think it would because you would have to show that one is more conservative than the other.

MR. STAUDENMEIER: No, you don't have to show that it's more conservative. What you have to show is that you have conservatism in your overall evaluation model, not that the new model is more conservative than the old model. In fact, people wouldn't be making model changes probably if the new model -- they are not going to move to more conservative models unless it's an actual error correction.

CHAIRMAN WALLIS: They are trying to give us conservative models.

MR. STAUDENMEIER: Right, unless it's an error correction. That make is more conservative. They are not going to be doing that.

CHAIRMAN WALLIS: The whole purpose I understood of Berlistic models was that by understanding -- by taking out the conservatives and making good estimate with this model and then evaluate the uncertainty, you could tell from that basis how conservative your model is.

You're predicting 1,400 degrees in LOCA. That's plus or minus 200. In order to get 1,700 it's going to be a one in a million chance or something. Now we can jack up the power or do something because this isn't a threat to any safety system.

That has nothing to do with the fact that Appendix K might predict 2,500. Appendix K is irrelevant when it comes to considering whether this is a good code for evaluating this thing which is predicting the 1,400 and the uncertainty.

MR. STAUDENMEIER: Right. And this wouldn't be really applied to Appendix K model.

CHAIRMAN WALLIS: I don't see how it's applied to anything because this could apply to a pump transient, whatever your criterion is. You've got an estimate X which is half of the conservative evaluation model Y and you want to now change your design or operation in order to use up that margin.

MR. BANERJEE: I guess the concern is there's no estimate of uncertainty in even what you call your best estimate or realistic estimate.

MR. STAUDENMEIER: Yeah, I guess in this case there is an inherent assumption that the uncertainty is small compared to the amount of conservatism.

MR. KRESS: That was my comment. Was it an implicit assumption you're making and are you approved in the first place.

MR. BANERJEE: But it needs -- that could need a fairly more extensive comparison with experiments certainly before you --

MR. STAUDENMEIER: Yeah, that's true. That would be -- to make this really work there would be some requirement to show that your uncertainty is small compared to the margin.

MR. KRESS: Or comparison with another code like TRAC-M which supposedly has a better base in experiments already.

MR. STAUDENMEIER: You know when you are comparing two codes at least one is wrong.

MR. RANSOM: Well, in response to that TRAC-M is kind of a good example because I think -- and Professor Wallis' complaints about the momentum equation and whatnot, the only way those things get corrected, or could be corrected, is through peer review, general acceptance of whatever models are in there. You take TRAC-M as an example. I think it's been five years or more since there has probably been any peer review of what's gone into that code.

What happens is after that long a time,

even if you do put it through peer review, it's very hard to make the changes or make any substantial changes in the code. I think it behooves whoever is developing codes to make sure that this peer review process is in place and it works.

In the old days when Tong and Fabic were running the code development, they had what they called blue ribbon committee. I don't know. A few of these people here I think were on that committee. They used to hold the developer's feet to the fire. I don't know that they were told.

MR. BANERJEE: Not with much effect, though. They did whatever the hell they wanted.

MR. RANSOM: Well, there was some effect.

MR. KRESS: But I think you'll find with TRAC-M that they managed to formulate it in such a way that changes will be easier to make.

MR. RANSOM: Being a code developer I have to see that.

MR. KRESS: You have to see the proof of that in the pudding. I think that you have.

MR. RANSOM: There's a good story and here's how it's really done.

MR. STAUDENMEIER: I was going to say one thing maybe we can -- there's a lot of controversy in this obviously and maybe get you to -- I guess we have to decide whether it's worth even going out for public comment at this stage or some additional work should be done before it went and come back to you like maybe showing a concrete example of how this method would be applied to a small code change and have that become part of the Reg. Guide, or whether we should just abandon this whole thing and come up with something new that has to go into the Reg. Guide. We will have to decide on that and that is what I'll need to find out.

MR. SCHROCK: This was not in what has already gone out for public review?

MR. STAUDENMEIER: No, it wasn't. This is going before you to get your comments before we go back out for the second public comment period.

MR. BOEHNERT: This is in response to the public comments they got.

MR. STAUDENMEIER: Yeah, these changes were made in response to the public comments. We have to go back out for public comment again because the changes made are fairly significant.

MR. SCHROCK: Well, I think in the most global view it seems to me you started with a highly conservative set of regulations that served as the basis for licensing the plants. It came time to recognize that so much of that was very antiquated and you initiated a rule change. The rule change provides for realistic calculations with assessment of uncertainty.

The two things are pulled apart and what you should have done was to set a time limitation, time maybe associated with license expiration or whatever. There should have been a phasing out of the old Appendix K method and a phasing in of the new method.

What you've been doing now in the last five to 10 years if creating some kind of morass between which is causing a great deal of confusion I think. This would make it so much worse because what you are trying to do is to provide the industry with a way to maneuver more simply through these two different things and get what they want at minimum cost.

I think that their responsibility to find that and I think they've shown a lot of ability to do that. I think your problem is to figure out how you are going to state what the ground rules are for what you are going to do in reaching judgment about middle ground fixes to this situation.

I think you would have been so much better off if you could keep the realistic assessment in Appendix K assessment totally separate things and make it a one or the other. If they do it one way, that's fine. Here are the ground rules for that. If you do it the other way, this means a CSAU or equivalent kind of evaluation of the uncertainty in the calculations.

MR. LAUBEN: I wish we could take LOCA off the table.

MR. SCHROCK: It not just LOCA.

MR. LAUBEN: But LOCA has nothing to do with rule change, nothing to do with Appendix K -- excuse me, non-LOCA. Appendix K of 50.46 are LOCA and LOCA only. These other transients and accidents have nothing to do with Appendix K, nothing to do with --

CHAIRMAN WALLIS: We're just using it as an example.

MR. LAUBEN: Absolutely nothing.

CHAIRMAN WALLIS: I'm going to break for lunch. I didn't think we were going to go all this time but we have managed to do it. You have a half-hour after lunch to come back when we have mellowed and decide what to do next.

MR. STAUDENMEIER: I was going to say there are two options. Either we can decide that it's not even fruitful to keep talking and come back later.

MR. LAUBEN: There is one thought and that is to take LOCA completely out of this Reg. Guide.

CHAIRMAN WALLIS: No, no, no.

MR. LAUBEN: Because the discussion always seems to merge LOCA and non-LOCA. If our examples are always about LOCA, then to try to talk about examples are non-LOCA, then I don't know. I don't know.

CHAIRMAN WALLIS: I'm going to break for lunch until 1:30 and then we have half an hour to pull this all together and figure out what should be done next.

MR. STAUDENMEIER: Okay.

CHAIRMAN WALLIS: So we will break now until 1:30.

(Whereupon, off the record for lunch at 12:35 p.m. to reconvene at 1:30 p.m.)

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

(1:32 p.m.)

CHAIRMAN WALLIS: I'd like to give you a chance to finish your presentation, Joe, and then we should back and see where we are, where we need to go, what we need to do.

DR. STAUDENMEIER: Okay. Well, we are I guess at the much discussed simplified method of determining margin or conservatism. I don't really have much more to say about that. I guess one thing that maybe could clarify it is if we carried out an example and brought it back to you to illustrate what we had meant by that and add in some more information to narrow the scope of that of when it would be applied for things with large margin or small uncertainties that were at least inherent in the assumption as I was making about when this would be applied, or it could be that this whole thing just isn't worth implying at all or even putting out for public comment. And that's something that I guess we'll all have to think about.

CHAIRMAN WALLIS: Certainly not the whole thing. The whole thing's been out. It's just the changes which --

DR. STAUDENMEIER: That's right. Yes, the change -- what I was referring to is, yes, the changes. Putting out the changes. I mean, maybe those changes are worth making or maybe we don't allow a little more simplified method of determining how much conservatism is in the model in that they would have to go through a more rigorous uncertainty analysis. But I guess that's something you'll have to decide when you make your recommendations.

MR. BANERJEE: How much more work would be required for problems like this where there are two or three important phenomena to actually do --

DR. STAUDENMEIER: Actually, I don't think there would be that much more work, my personal opinions.

MR. BANERJEE: Perhaps the thing is to try to understand if you didn't do this and they had to follow usual procedure --

DR. STAUDENMEIER: Yes.

MR. BANERJEE: -- would this be really a great burden or is it something which the industry has simply commented because they are looking for something?

DR. STAUDENMEIER: Well, I mean, something I don't think is a great burden to someone whose out there trying to get out production work all the time.

It does add additional burden to them and, I guess, it's your point of view where you're at in the process of whether it's a great process or not.

MR. BANERJEE: Right. But is there a way to quantify that?

DR. STAUDENMEIER: The amount of extra work --

MR. BANERJEE: Like I imagine --

DR. STAUDENMEIER: For a specific case you could show how much extra work would be done.

MR. BANERJEE: I imagine that you're suggesting this be used for relatively simple models where there are a couple of phenomena which dominate, or maybe a few, so the burden may not be all that high. And if that can be shown quantitatively, then that would be useful. But on the other hand, if the burden is very high, then there might be more reason to do this because you might say, okay, we don't have to go through the whole rigorous CSA methodology, that could really impose a large burden for very simple changes. You know, so I don't know.

I personally sort of think if the burden isn't large, then it simplifies everything to just follow the usual route.

DR. STAUDENMEIER: Yes.

MR. BANERJEE: If the burden is large, then it's probably worth looking at.

DR. STAUDENMEIER: You know, the cases I've thought of where this would be applied, I don't think the burden is large but I may not be thinking of the same cases the industry is thinking of. It might just be maybe a matter of my tunnel vision on this of how I think it's applied that I think it wouldn't be large. But --

MR. BANERJEE: Okay.

DR. STAUDENMEIER: Well, this is the last slide, the status and summary. We still hope that the revised Reg. Guide will address the findings of the Maine Yankee review panels and other review groups.

We'd like to get ACRS comments from this current round of discussion, and revise with respect to your comments as soon as we can and send this back out for public comment, which we hope would be the last round of public comment. After we get your comments, the next step in the process would be putting it through OGC and CRGR for review. Then stick it out for public comment. The public comment period I think is probably about 45 days or something like. I think that's the minimum we can have. And we would get the comments back in, process them.

Hopefully, there won't be any controversial comments this time. Hopefully, we've sorted through most of them from the last round of public comments. And if everything went smoothly, we would incorporate the public comments, come back to the -- have to go through the ACRS and CRGR and OGC again.

CHAIRMAN WALLIS: Again?

DR. STAUDENMEIER: Yes, before it's issued for final. You have to review the final issued product and it would get issued as a document, an official document sometime, and I think would be on the order of April next year or something if there were no more delays.

CHAIRMAN WALLIS: Do you want the full committee comment?

DR. STAUDENMEIER: I think the full committee are the only ones that can apply.

CHAIRMAN WALLIS: Are we scheduling a performance by you in front of the full committee in September or something?

MR. BOEHNERT: Yes, we are.

CHAIRMAN WALLIS: We are?

MR. BOEHNERT: We are.

CHAIRMAN WALLIS: So this is just a preliminary --

MR. BOEHNERT: Subcommittee and then, you know, the full Committee is going to pass on it.

CHAIRMAN WALLIS: Depending on what my colleagues say on the subcommittee, I would think since the full Committee has seen this before that we ought to focus attention of your presentation on what's new, and particularly the items that the subcommittee had trouble with.

DR. STAUDENMEIER: And I'd welcome your unofficial comments, I guess, before the full Committee meeting.

CHAIRMAN WALLIS: Well, it's a long time, too. It's 2 months or something before that.

DR. STAUDENMEIER: Yes.

CHAIRMAN WALLIS: Well, the first thing I think we all need to do is we need to agree that comments will be written and sent in within a week or two, whatever. Given a good chance; say by the end of the month or something. The sooner the better, really. Because you remember what you're doing.

And I think we've got an opportunity now, we've already aired some interest and concerns. And we want to reenforce those now or raise some other points, and we should do it now. That would be most helpful to Joe.

MR. SCHROCK: Well, Norm Lauben made a comment about the distinction between transients and LOCA just before the lunch break, and I --

CHAIRMAN WALLIS: Are you taking that back?

MR. SCHROCK: Well --

CHAIRMAN WALLIS: So lunch had a solitary effect.

MR. SCHROCK: I guess my comment would be as you read this Reg. Guide you don't find a clear delineation of those two cases. And if there that kind of significant difference, it should be spelled out in the Reg. Guide somehow. And I guess there is.

DR. STAUDENMEIER: That's a good point. It was obvious to me what I meant when I was writing it, but I had it in my head and that's why it's good to have independent review like this to point out things that we haven't thought of.

CHAIRMAN WALLIS: It was never my impression that there was any difference between LOCA and all these other things. This is how you go about evaluating, trying analysis methods in general.

DR. STAUDENMEIER: Well, in terms of the general prescriptions for doing things, I don't think we made a difference. But in terms of applying these simplified or allowing these simplified methods to be applied, I think that's where we would make a distinction.

CHAIRMAN WALLIS: Well, I think you might make that clear, that simplified methods only apply to rather a small class of small changes or something and in rather insignificant events.

DR. STAUDENMEIER: Yes.

CHAIRMAN WALLIS: Are there other points that my colleagues want to raise at this time?

Do you have any comments on the bulk of the document as opposed to the section 5 that's been in most of the discussion?

MR. BANERJEE: Well, I already brought up my thoughts about in some way diagraming what this business of inputs which included nodalization and all these other things, even though at some point it's mentioned specifically, I mean I'm very uneasy with making specific models for specific transients for specific plants in fixing the nodalization. And the arguments going forward from a small scale experiment which uses a certain of nodalization to get the right results and a plant then using similar nodalization seems a big job to me, and I don't know how that's treated.

I mean, in a way this issue has been with us forever. I mean, this is not the first time it's been raised. So whether it's a problem with this Reg. Guide or just a general problem that's around, I don't know. But I feel very uneasy about not having some sort of requirement to show that with nodalization is sufficient to capture the phenomena which are of importance, at least. Some words to that effect.

CHAIRMAN WALLIS: Nodalization is not independent of the point I was making about geometry. I mean, how you nodalize something like a lower plenum. It's going to influence how well you capture normal in there. It's not just a question of dividing a pipe into pieces so you can say it's just straight forward, but a number of pieces. But in something like a lower plenum you have a choice of the shape of these nodes and things. And it's not just numbers

MR. SCHROCK: Well, most of them are connections of pipes. And so there's no way that the geometric description you're thinking of can be brought into it when it's represented as a set of pipes and junctions.

CHAIRMAN WALLIS: There is a way, because they use it. I mean there must be a way because there's --

MR. SCHROCK: Yes.

DR. STAUDENMEIER: Yes, I mean like in the TRAC 3-D vessel component, there are things that are dependent on cylindrical geometry or cartesian geometry. The vector, like the gradient or divergence operator depends on what geometry you're in in the general sense and general conductivity of like RELAP or a lot of the other codes that are more lump perimeter based, it's more --

CHAIRMAN WALLIS: Yes, that sort of assumes you've got a cylindrical thing or a plane thing. But a thing like upper and lower plenum, they're not cylindrical or plane, they're sort of a mixture of things in there.

DR. STAUDENMEIER: Right. And instead of thinking in those like differential equations in geometry, it's more finite volumes with connections between them and fluxes of different quantities going through the connections, and that's what tends to dominate these problems more than the specific geometry does, I think.

MR. RANSOM: The problem is actually a little more obscure than you'd think from, you know, first examination of it. And since these codes don't include any of the sheer terms and any mixing, you know, associated with sheer or turbulence just simply is not included in TRAC or reapplied type calculation. I believe and it's my opinion that you're better off treating a lower plenum as simply a homogeneous -- which is any volume is and nodalization of the code. I mean, it's well mixed. And if that assumption is not really acceptable, then a CFD code or something should be consulted to find out what really goes on there.

CHAIRMAN WALLIS: Even if it's homogeneous volume, you can't write the momentum equation for it when stuff's coming down and --

MR. RANSOM: Well, I believe you can, and that's a subject of debate, I guess.

CHAIRMAN WALLIS: Okay. Let's see it.

MR. RANSOM: Well, we've done it.

CHAIRMAN WALLIS: You simply can't do it from the usual --

MR. RANSOM: And I guess again, fortunately, in the case of the lower plenum, the momentum flux terms are quite unimportant because velocities are quite low so that really that's not as important --

CHAIRMAN WALLIS: What's what saves you. I think that's what saves you.

DR. MOODY: Maybe my view is awful simple.

It certainly depends, doesn't it, a lot on what really you're trying to calculate. If you want a mass in a lower plenum of an empty vessel that's being filled, you can do it with one node if you know the inflow and outflow. If you're pressurizing it, you can do it with one node.

If you want the flow patterns then, of course, you got to break it up more. If you want to get the temperature distribution, then you have to have many more nodes.

I guess there's not too much arguing with that, is there.

CHAIRMAN WALLIS: If you have boran sled-- not a boran sled coming in from one side, and you want to know how well it --

DR. MOODY: Mixes.

CHAIRMAN WALLIS: -- mixes. And then you need to -- right.

MR. RANSOM: Well, it's the kind of thing that I think, to give you an example, the thing that people sometimes do is in a 1-D code they will divide the lower plenum into several levels. But that's sort of nonsense because they're connected by one junction. And if you talk about incompressible flow, there will be no flow through that lower --

CHAIRMAN WALLIS: Well, you can't --

MR. RANSOM: You can only go through up the upper layer and back up. And so it's kind of a phony type -- probably doesn't cause any harm, but on the other hand you'll see situations where hot water or cold water is now sitting on top of hot water and simply won't mix. And so you have to use a little engineering judgment, I think, with these kinds of thing not only in proximate models. Either experimental data, CFD calculations, this kind of thing needs to be used -- and I don't know. I guess that wasn't really addressed in here either, although the evaluation model could consist, I presume, of dependence on a CFD type code or something like that to validate certain parts of the calculation, and that would be a reasonable thing to do.

DR. MOODY: I used to have a boss that said "Bring it to me when you're willing to bet your paycheck on it on how well it matches reality."

CHAIRMAN WALLIS: Well, the even better one is when you're willing to bet your company on it.

DR. MOODY: Yes.

CHAIRMAN WALLIS: Well, we're going to have comments for you. I think probably the usual way will be each person write something and it get passed on. And since we've seen most of this before and had an influence on it, I think there will probably two kinds of comments. There will be comments sort of bear in mind and using the guide but not recommending changes. And then perhaps in this section 5 we'll actually be recommending that you do something different.

I think my comments might reflect that intent, but I haven't yet written them down.

DR. STAUDENMEIER: Okay.

CHAIRMAN WALLIS: Because, as you know, I have some concern with section 5.3 and section 5.4.

Well then, I think we would really like to see this get out there and have an influence. Because it's useless to have it in there. Unless it actually influences what's done out there by the applicants and the staff, nothing has been achieved.

DR. MOODY: This may be the wrong time to even ask it, but why does all this go out to the public?

CHAIRMAN WALLIS: Oh.

DR. MOODY: Do you want staff that comment and go on something else? I mean, NASA didn't do it when they put men on the moon, they didn't ask the public.

CHAIRMAN WALLIS: It's kind of --

MR. KRESS: It's one of NRC's strategic goals.

CHAIRMAN WALLIS: Yes, but it's sort of self-defeating. And I think the original intent was to involve the public. The public is not involved. It's industry comments that you get. So it's a very select --

MR. KRESS: But the public gets an opportunity.

CHAIRMAN WALLIS: That's right.

MR. SCHROCK: NASA's not that big of risk to the public.

DR. STAUDENMEIER: And actually, in some things actual members of the public do get involved in it.

MR. BOEHNERT: Yes, sure. Depending on the issue.

CHAIRMAN WALLIS: The industry comments make a lot of sense, because they're the ones who are going to have to use this. You know, they're sort of the customer for this thing.

MR. BOEHNERT: Or be subjected to it.

CHAIRMAN WALLIS: So it's very important that they have some input.

The thing that's frustrating is how long everything takes. We do something and then, if we're lucky, a year and a half later then something comes back. And that gets frustrating if we go around this loop several times and then ACRS membership has changed completely by the time it's reviewed.

DR. MOODY: I recall that one time, I don't remember who it was back in the Phil Brady days, the Director of NRC made the comment that someone from the public could make a claim and have no basis, and you didn't have to prove -- you had to spend all this time proving they were wrong. And that was eating up tons of time and money.

CHAIRMAN WALLIS: I don't think that happens here. I mean, not with this sort of thing.

DR. MOODY: Okay.

CHAIRMAN WALLIS: So I'm about to say that we move on to the next item on our agenda, unless Jack or anyone has anything to -- I felt that we everything went along very well and we were sort of in agreement with everything until we got to section 5.

MR. ROSENTHAL: Yes, I think, you know, we anticipated that that would be the source of discussion.

My only frustration is that Norm and I and

Ms. Lynn Ward went on a maniac -- in '96, and it's 2002. So --

CHAIRMAN WALLIS: Well, the PRA guys are talking about to the Rasmussen report, you know, 30 years ago and we still haven't become risk informed.

MR. ROSENTHAL: In any case, we go to the public meeting and the reason that we were going to go at a second time, and it's not a requirement, was that we felt that there were sufficient changes that it would pay to go out. And just depending on what your comments are, maybe we can work something out where we make some incremental progress between now and the time that we go to the full Committee, just to get some momentum going.

But we look forward to your comments.

CHAIRMAN WALLIS: Yes, I think you have to be very careful with this conservative thing. Code isn't conservative. Show the conservatism in a context.

MR. RANSOM: Can I ask a question. Why is RES doing this as opposed to NRR? It seemed something more that NRR would be concerned with.

MR. LAUBEN: I think in the reorganization of years ago, or whatever, it was decided that regulatory guides would be the principal responsibility of RES, but they always had to be coordinated with the user on NRR. And the standard review plan is the province of NRR. The two are related. So it actually works pretty closely with --

CHAIRMAN WALLIS: That was one of our questions I remember before. Is you got this sort of review plan on this Reg. Guide and what's the sort of correlation between the sections.

MR. LAUBEN: And in fact, sometimes they have, you know, organizationally we may have be assigned the responsibility, but we can actually by agreement it can switch to --

CHAIRMAN WALLIS: Well, okay.

I'd ask this Committee then to give input. And it occurs to me that it might be helpful if you guys gave additional input. And having heard the comments today about section 5.3 and 5.4, if you came back and said we understand what you're saying, how about doing it this way just for these small sections. Then we could comment on that, wouldn't have to have a full meeting here. Then you'd have an assignment to come back with some feedback as well as us. Can we do that? Without having to go through -- so you might say that the full committee meeting in September, realizing we had all these comments from the subcommittee, we rethought section 5.3 and 5.4 and these are some of the things that we suggest would be an improvement.

MR. BOEHNERT: Well, in fact, Jack had suggested that.

CHAIRMAN WALLIS: We'll do that, and then let us know. Let us know so that we can through the loop before September.

DR. STAUDENMEIER: Okay.

CHAIRMAN WALLIS: I thank you very much. You've been very helpful and patient and informative.

DR. STAUDENMEIER: And I thank you for all your comments.

CHAIRMAN WALLIS: I don't need to use by gavel. I think we can just invite the next speaker. Are you going to do this all yourself?

MR. RANSOM: No, Steve is next.

CHAIRMAN WALLIS: Oh, Steve has given out -- but then V.J., you're the one presenter.

MR. DHIR: Piece de resistance.

CHAIRMAN WALLIS: So Steve is going to put into perspective what we're going to hear from Professor Dhir?

MR. BAJOREK: Well, I'd like to put it in perspective and also from our last meeting when we talked about ACWS, one of the items we brought up was you have in TRAC-M right now. So what I'd like to do is try to put the research program in perspective, and while we're working on that, but also just a couple of overheads to explain what's in the code right now and why it essentially needs to be replaced.

What we'd like to present to you this afternoon is the work being sponsored by the Office of Research to develop the models for subcooled boiling applicable for safety analysis and rod bundles.

Subcooled boiling is one of the two or three regimes that are typically encountered in a two-fluid code where you have a particular --

(Whereupon, microphone adjusted)

MR. BAJOREK: As I mentioned, what we'd like to talk about is the work being sponsored by the Office of Research to look into subcooled boiling. It's one of those heat transfer regimes that's particularly difficult to deal with in two-fluid code, because you have to deal with this idea of heat-flux splitting.

When you have these regimes with a lot of thermal non-equilibrium you have to make a decision on how you partition the energy either to the liquid field or to the vapor field. And because of that non-equilibrium, you also have to start worrying about things like the interfacial area, bubble size, droplet size and the interfacial heat transfer. The whole process as shown over here on the right hand side, means you have to make some decision in the code on how you partition things that generate vapor in the case of subcooled boiling, how much of that energy goes to the liquid heating regardless of how you split or partition it at the wall. You also need good models to transfer to account for the condensation and the transfer of energy that occurs at the interface as those bubbles are departing and moving into the bulk fluid.

The difficulty is compounded in a two-fluid code because you're not really able to deal with the physics as you would like to. Meaning, you can't really model, per se, a subcooled region and immediately next to the wall. You're left with a relatively large node with some amount of subcooling, and a void fraction that the code, for all practical purposes, wants to assume is mixed everywhere throughout that cell and not necessarily at the wall.

Now, applications that rely or depend quite heavily on getting the subcooled boiling correct have been shown to be very troublesome. A couple of fairly recent examples, the AP600. They found some cases where there are tremendous large oscillations in the core wide void fraction seen from the collapsed liquid level. They were particularly acute because they were at low pressure.

Well, in evaluating those models what this was traced to was a subcooled boiling model that had a bubble pumping term that was a function of the density. It had a row L over row V. Although that model had made use of data and seemed to work quite well at relatively high pressures, when it got down to the low pressures typical for the AP600, that ramp would turn on and off causing very large swings in the core void fraction.

Another recent example was an evaluation of a Peach Bottom turbine trip. Because the subcooled boiling model was not able to get the axial variation to void fraction correct, it was very difficult in those simulations to try to predict the right kinetics.

MR. SCHROCK: Well, isn't this 1-D channel model that you're dealing with in TRAC?

MR. BAJOREK: Yes, in this case.

MR. SCHROCK: And so that vapor flow perpendicular to the wall really is not a variable in

TRAC notation?

MR. BAJOREK: No. I mean, in TRAC you just have one large cell to work with. I've sort of drawn this just as a way of -- if you could try to nodalize that a little bit better, what you would like to try to do is to break this down into a region with high subcooling and a relatively large void fraction as opposed to what the code is really doing out there, which is distributing all of these bubbles over one large cell that has some globally known subcooling.

MR. SCHROCK: So I guess an explanation which is couched in terms of the variables that are employed in the TRAC calculation would serve better to tie this together with V.J.'s experiments?

MR. BAJOREK: Yes.

CHAIRMAN WALLIS: Well, that's a question I had was how his experiments are related to what you need to know in the TRAC code. You can study the collapsible bubble, but I don't know quite how this fits into what TRAC needs to know.

MR. BAJOREK: Well, you need to know everything associated with these three Os over here on the right hand side.

CHAIRMAN WALLIS: You just need to know. So across the whole cross section what's the average void fraction, isn't that the question it's asking?

MR. BAJOREK: That's what the code is basically -- well, the code's going to do that. Okay. Unless you do make provisions in the code to tell it it's in a regime where you are going to have concentrations within that cell.

MR. BANERJEE: You can do that by thermal non-equilibrium. So you can have a void --

MR. BAJOREK: You can look at conditions that will tell you that you are in subcooled boiling and that you should have a concentration on one side or the other. But in order to get the amount of void generation, this thing here, the net vapor generation -- correct. Okay. You need to be able to know what that partition is across other things like the interfacial heat transfer, the condensation rate, the bubble size and the bubble behavior near the wall before you can --

CHAIRMAN WALLIS: But I think it actually depends on where you draw your boundary. I mean, at the wall presumably all the heat transfer is to the liquid and there's some superheated liquid making vapor. And then there's some vapor migrating, finding itself in a subcooled liquid and condensing again. And all this, presumably, fits into answering the question what's the average void fraction. The partition I always thought was a kind of artificial thing. Because depending on where you look there are different partition.

At the wall all heat transfers are liquid, and then somewhere out here it's mostly vapor migration carrying the heat out. And out here it's condensation causing the heat transfer. And then it's all liquid again.

It depends where you draw your boundary, how you partition things.

MR. BAJOREK: Unless you get all of those correct, you're still going to be off when the net --

CHAIRMAN WALLIS: The pool is partitioning in somewhat artificial way, isn't it?

MR. BAJOREK: Yes. Yes.

MR. SCHROCK: But that's I really think you've got to have a logical starting point in terms of what the TRAC variables are and how you imagine what's happening in the subcooled boiling process. The bubble that's growing attached to the wall has evaporation on part of its surface, condensation on part of its surface. It may or may not detach before it reaches it maximum size. If it does, it moves to a new location where there's less subcooling eventually

-- or more subcooling, rather, eventually may be totally quenched. Further downstream it may be only partially quenched.

But how do you translate all of those recognized phenomena into TRAC variables? I think that's the key thing.

MR. BAJOREK: I think in looking at the models that Professor Dhir is coming up with, we haven't seen anything in there that prohibits us from being able to put it into the code in terms of the variables that are either used or could be used by TRAC-M. The only thing to prohibit us from adding new capabilities, adding new variables.

MR. KRESS: And in fact that's your job, the job of V.J. is to understand what's going on --

MR. BAJOREK: We need to know what's the basic physics that goes on here.

MR. KRESS: Yes. And that's what he's doing.

MR. BAJOREK: Yes. And I think one of the first things that Professor Dhir is going to show that, hey, the existing database to try to get at these various heat flows is lacking. And the point I want to make, you know, in what we've got in TRAC-M or RELAP or Cobra TRAC, these are all largely based on the same types of data. They use, for practical purposes, the same types of models and have the same questions.

We would expect TRAC-M to get those similar types of problems. For the AP600 this is the type of thing that was being predicted. This shows the core collapsed level when much of the core was predicted to have been in a subcooled heat transfer regime.

You look at some of these oscillations that were going on, this is a dimensionless height of the core, from zero to 1. We're looking at values on the order of .3, .35. This is 3 to 4 feet in oscillations --

CHAIRMAN WALLIS: This is all subcooled --

MR. BAJOREK: Most of the core is --

CHAIRMAN WALLIS: This is all bubbles and then subcooled boiling --

MR. BAJOREK: Yes. And in looking back at what was causing this, you'd go into subcooled boiling, you'd generate a very large void. Then this would collapse. Okay. Once it got to saturation. Okay. And the process would start and repeat itself. This was being aggravated by basically ad hoc ramps in that last figure that was trying to partition things between the liquid and the vapor. That's the type of thing that indicated we needed to understand the physics of this a lot better.

CHAIRMAN WALLIS: Well, it seems to me --

MR. RANSOM: Just a few comments of caution. You know, when you report things on zero to 1000 second time scale and it's a very large scale. And you call that numerical noise. So I guess it's not numerical noise, it's actually if you plot those oscillations, there are many time points in each oscillation. So it's a mixture of modeling and, you know, so maybe the numerics. I don't know.

MR. BANERJEE: It's modeling noise.

CHAIRMAN WALLIS: It's modeling noise.

MR. RANSOM: Well, yes, it can be. I don't know that it's not physical. I mean, that's an assumption that it's not physical.

Then also I think that Saha-Zuber correlation that you showed -- or you haven't shown it yet, that partitioning has only to do with the -- how it's changing the overall heat-flux from the wall.

MR. BAJOREK: Right.

MR. RANSOM: Not the partitioning between vapor and liquid. There's a secondary model in the code that does that.

MR. BAJOREK: We have to partition it. It's basically using model by -- and you come up with a partition and then you also have to come up with a model for the condensation in order to --

CHAIRMAN WALLIS: To get back to Professor Schrock's question, I would think that Professor Dhir would have the assignment of develop better understanding of the physics and then tell us how to put this into the code. It looks as if he's got an assignment to understand the physics. And the way in which this is related to what actually has to go into TRAC seems to be a very important part of the problem. Is he doing that?

MR. BAJOREK: No, that's going to be our job.

CHAIRMAN WALLIS: I'm not sure you can. I think he has to do it. I would assign him of making the burdens, because then he knows what he's got to measure and what he's got to model. If he just goes off into an academic world and models everything he's interested in, that's not the same thing as getting an engineering model that goes into TRAC-M.

MR. BAJOREK: No, I don't --

MR. KRESS: You're presupposing that V.J. doesn't know what the code meets?

CHAIRMAN WALLIS: I don't know. I'm just saying it should be part of his environment.

MR. BANERJEE: He may know, but he may not want to do it.

MR. SCHROCK: I'd just like to say amen if I do it.

DR. MOODY: Well, as I read this report, I got knowing a little bit about the way V.J. operates. I got the feeling that you were asking him to do kind of a -- on a bottoms up study that you could incorporate into a top down model. In other words, a microscopic lab study that will give some clue and I thought probably on his data that he presented in correlations, that's what you were going to use to incorporate into the TRAC code.

MR. BAJOREK: Yes.

DR. MOODY: Somehow.

MR. BAJOREK: We need to know the individual mechanisms that dominate that split. It's going to be up to us to make that we can take those things, put in the variables, code those in such a format that it can replicate the model that you might come up with in a lab or, you know, in a more academic setting. And we realize that there are always going to be shortcomings in a two-fluid code that's not going to get us down to a CFD type of modeling of any process. But at some point if we know the physics and if we know models which are mechanistic and can be faithfully used to represent what goes into a rod bundle, whether it's, you know, a GE type or a Westinghouse type over a range of subcoolings, then we're going to have the confidence to put the models in the code and get realistic results.

And I think, you know, in going back to this there may be something physically real that's going on, but we went additional steps in this to go back and note when you had a large blip it was when a ramp was being turned on and off in the subcooled boiling model. So it wasn't just, you know, pointing finger --

MR. RANSOM: Well, I would take issue with that, too. What actually turns out interface drag is very important to this kind of prediction. And if you don't pay attention to that, you know, it doesn't matter what you do in the heat transfer partition, you're not going to get the right answer either. So, this has to be looked at in that global way.

And, as a matter of fact, you know, in the subcooled boiling experiment that we're going to talk about I didn't see any real discussion or consideration of that. And that goes back to how are you going to put this in the code. And I think there are other issues, too, that we have to talk about.

CHAIRMAN WALLIS: Would you go back to your first slide and do it properly, and you could say that in order to predict the average boiling and I have to know how many nucleation cites there are, at what temperature they're activated. I need to know how rapidly those bubble grow attached to the wall. I need to know when do they move away from the wall. Do they grow some more when they leave the wall.

MR. BAJOREK: Right.

CHAIRMAN WALLIS: How do they move away from the wall in a transverse direction. Are they carried along in the axal direction. And then when do they begin to condense and how rapidly do they condense.

And what I see from his book is there's some work on when they start to form and how many nucleations sites are there. And there's some work on isolated bubbles condensing. But where's all the rest of what's going on that you need?

MR. BAJOREK: If we're concentrating on the head transient, we haven't looked so much at the interfacial drag at this point, no.

CHAIRMAN WALLIS: And they've tracked this interfacial drag, which has nothing to do with what I've just talked about, how does it, you know, influencing the result anyway because of the TRAC model?

MR. BANERJEE: Well, interfacial drag will surely influence the condensation rate. Because in a way that's Reynold's analogy, right. So you have to have an effect of the drag on the --

CHAIRMAN WALLIS: Well, it would probably accelerate the bubbles to the same speed as the fluid and there won't be any Reynolds --

MR. RANSOM: Well, in fact the nestled number is the function of the relative Reynolds number. The Reynolds number is based on relative loss--

MR. BANERJEE: I guess the main point here, Steve, that there should be framework laid out to receive these results. And what is not clear is what that framework is.

MR. BAJOREK: Okay.

MR. BANERJEE: So we don't have a set of equation saying this is what's lacking in these equations or this is where we need more information. These the TRAC equations. These are the numbers that we're going to get out of -- whether that interfacial area or whatever, heat transfer --

MR. BAJOREK: Yes.

MR. BANERJEE: It's not clear how that data is getting fixed into TRAC. And that might not be trivial to do. That's really the issue. It's sort of difficult because you've got -- if you put distribution coefficients in for the temperature or something so you have subcooling at the wall, something like that, you might get somewhere into that regime. But it's not trivial to phrase this in, at least I don't see it as trivial. You did this job.

CHAIRMAN WALLIS: You have to go to a microphone. You have to say who you are.

MR. DHIR: I'm V. J. Dhir from UCLA.

The key difficulty when we started this work was with the cores that they could not -- they did not know what the repagination date was. What was the source -- fraction. When you give a source term you've got to give number density of bubbles, size of bubbles and rate at which they're being injected into the boil. So that information we're trying to provide.

Then they also need to know what is the local liquid temperature. That is effected not only by the heat transfer from the wall the liquid, but also by condensation. So you need to know what rate the bubbles will be condensing. So that's the problem that we have looked at.

MR. RANSOM: In fact, that brings up an interesting issue. It's not the local temperature that you need to know in these codes, it's the bulk temperature that you reference to. It's the bulk liquid temperature which in the heat transfer coefficient between the bubble and the bulk of the liquid you must use.

And I know in a paper that I was reading it went to great pains to measure the temperature at the bubble, which of course is not the code variable.

MR. DHIR: Right. But if you look at it it really depends on how much resolution you want to have in the code. If there's only cell over the whole -- you could have some average temperature. But what you look at it, or we looked at it, there is a thermal boundary layer which is, you know, temperature changes very rapidly in that region. Beyond that it's like bulk temperature.

MR. RANSOM: And that's quite a different model than the model that you use in these one dimensional codes. Maybe there needs to be some coming together.

MR. BAJOREK: Okay. I mean we aren't finished with this. I mean, we still have to get these models, understand them better, find a way to put them in the code. But, you know, our looking at what is in TRAC-M right now and similar codes leads us to believe that the models and the way they're treated now aren't acceptable. You do your best with the available data to look --

MR. RANSOM: Steve, let me ask you a question along that lines. There's quite a database out there for subcooled boiling and internal geometries. And did you ever -- and I'm sure that was utilized in the development of these models. So what is the explanation between, you know, those separate effects assessment in all the models, then not giving you good results in this case?

MR. BAJOREK: What is it? I'm sorry. I couldn't hear you.

MR. RANSOM: I can't speak for the TRAC-M, I guess, but I can speak for the RELAP-5 part. They did use very -- they used Christian, they used St. Pierre you, his experiments to validate the subcooled boiling models. And you got reasonable results in most cases. So I'm wondering why -- there should be some explanation, I guess, for why this --

MR. BAJOREK: Why? If that model's so good, why couldn't it get things like bubble diameters or interfacial drag in a newer test like McMasters?

MR. RANSOM: Well, the guess that I would have is that's simply a critical number that's used to decide the bubble size and --

MR. BANERJEE: Maybe I should interrupt here. Because I think, you know, Dick Lahey did an interpolation between Unal's experiments and some other stuff. And he never actually broke it into interfacial and heat transfer coefficient. He just call it product of them.

MR. RANSOM: Yes, the multiplier.

MR. BANERJEE: Yes. And in fact what you guys put into RELAP was Lahey's thing.

Now, what happens here this is -- Dave, I don't know how they've separated it into bubble diameter, but it looks like the -- in my opinion, the interfacial area in that was roughly right. They got the heat transfer coefficient completely wrong. So it's the opposite problem to what you're seeing here. So because of the getting the heat coefficient completely wrong, they got the void fraction completely wrong because --

MR. RANSOM: In this case you mean?

MR. BANERJEE: In this case.

MR. RANSOM: Well, this is the Chen correlation for the overall --

MR. BANERJEE: No, no. This is the McMasters experiment where there were bubbles condensing in a subcooled liquid. So the bubble diameter was followed. This was not attached to the wall.

MR. RANSOM: I see.

MR. BANERJEE: These were just steam bubbles. So it's a condensation experiment basically.

MR. RANSOM: Who did that?

MR. BANERJEE: This was an old friend of mine, a guy name Shukri or something.

And what happened was in these experiments that they could measure of the diameter of the bubbles as well as the rate of condensation. If remember right, though, the reason these experiments are so wrong compared to RELAP-5 is not the interfacial area. It's the heat transfer coefficient.

MR. RANSOM: Between the bubble and the bulk of the --

MR. BANERJEE: Yes, between the bulk. And there's a very nice graph in the report by Joe Kelley which actually shows the incorrectness in the heat transfer coefficient. So the problem has not been-- Lahey's coefficient was basically an interpolation.

MR. RANSOM: Well, Lahey's correlation does not tell you what the heat transfer between the bubble and the bulk is. It only divides between the sensible heat and the heat of vaporization. So it tells you how much vapor is being produced.

MR. DHIR: That's Lahey's model, but it doesn't work.

MR. BANERJEE: It doesn't work.

MR. RANSOM: Well, it works rather well in the codes of --

MR. BANERJEE: It works in certain regime, but doesn't work.

MR. BAJOREK: That's the rollover term that's in there.

MR. SCHROCK: These data are steam injected into flowing subcooled liquid.

MR. BANERJEE: Yes, single bubbles. Well, this area of bubbles.

MR. RANSOM: Oh, this is the McMasters -- I see.

MR. SCHROCK: Doesn't that depend on what the diameter of the bubbles injection.

MR. BANERJEE: Well, it was larger. It went up, it condensed and they were followed with a camera.

MR. SCHROCK: But single bubbles going up the core --

MR. BANERJEE: Yes, single or multiple, but they were in tubes. In fact, you get a very good correlation of the heat transfer coefficient with the interfacial drag which you can estimate very easily in this problem. So when you correct the heat transfer coefficient with a Reynolds analogy here, you get almost a perfect correlation. That was the heat transfer coefficient, you get that.

MR. SCHROCK: It seems to me that the situation in the subcooled flow boiling channel is different from that in the sense that there's a radial distribution of liquid temperature which you don't know.

MR. BANERJEE: No.

MR. SCHROCK: But it exists. And the amount of vapor that's formed at the wall is in part dependent upon that. So the amount that detaches from the wall and is then part of the flow process is different than in this kind of experiment.

MR. BANERJEE: It's a pure condensation

experiment.

MR. SCHROCK: Both the growth and the condensation occurred in the subcooled boiling flow situation, but only condensation occurs here.

MR. BANERJEE: Correct.

MR. SCHROCK: And so you can't get the right void fraction in subcooled boiling, shouldn't expect to bring those two things into reconciliation--

MR. BANERJEE: Yes, we shouldn't give too much credence to this other than knowing that the condensation rate is wrong.

MR. BAJOREK: What's in TRAC-M right now could have been used to compare to FRIGG, some other experiments. Some comparisons look good, others don't. It's based on the Saha-Zuber model. It's shown graphically over here in Saha-Zuber to get that total heat transfer where things are thermally dominated. Assume it's a Nussett number of 455 when it's hydrodynamically dominated, it comes to a constant Stanton number of .0065. That's what was used, that's what was in the --

CHAIRMAN WALLIS: How do you know if it's .0065 and 01065. There are two different statements there.

MR. BAJOREK: That's part of my point.

This is what was used to correlate various water and freon data in the original. That's not within TRAC-M, which in some cases get good agreement, some cases doesn't. Based on some work that was done at Savannah River in a single tube and down flow, at some point those were adjusted so that in a thermally dominated region, which they defined as being a Peclet number less than 7,000, not a Peclet number of 70,000 in the original model. They say let it be a Nussett number of 74.55. When you get to bubble liftoff, make it a Stanton number of .0165.

I've taken this and plotted this versus Saha-Zuber, which in the TRAC-M documentation is being claimed as the basis, the foundation, for the subcooled boiling model.

MR. SCHROCK: Where did the TRAC-M modification come from?

MR. BAJOREK: It claims to have been based on some Savannah River work that had been done for looking at single tube flows and downflow. This is what --

MR. SCHROCK: Another unnecessary complication thrown into this. We're not interested in downflow here.

MR. BAJOREK: Well, that's the point. I

mean, if you have a good model, you should have been able to match that subcooled data as well as this.

MR. SCHROCK: Why?

MR. BAJOREK: If your model is truly mechanistic and you're getting the bubble size, the condensation rates.

MR. RANSOM: Steve, a little further comment. The results you show are using TRAC or RELAP-5 and yet the correlation you're talking about is TRAC-M. And so I'm wondering what is the connection, you know, between the two

MR. BAJOREK: The connection is both of those are attempting to base their models on something that looks like the Saha-Zuber model.

MR. RANSOM: Well, do you have some TRAC-M calculations that show that they don't behave correctly then:?

MR. BAJOREK: Not today, but next time we get together I'll try to get those.

MR. RANSOM: Well, you're blaming on this correlation. And what I'm wondering was the basis for criticizing the correlation if you have not actually utilized it.

MR. BAJOREK: What is in TRAC-M is many ways very similar to what is in RELAP.

MR. RANSOM: I agree with -- well, RELAP uses the 70,000 transition point.

MR. BAJOREK: Right. But it's using the same -- overall the same type of scheme to come up and do the splitting. Our bottom line is when we look at those models and how either one of them as compared to data, we're not comfortable with either one of those models as an eventual subcooled boiling model in TRAC-M. So the fact that we had oscillations in RELAP which was the code that we needed to use for the AP600, we weren't satisfied with what had been pointed to as the subcooled boiling model there. When we look at what had been put into TRAC-M, however it got there, that's what's in there right now.

MR. RANSOM: And they used the Lahey model for partitioning the energy between --

MR. BAJOREK: It's close. It's not the same thing. I don't see the roe over roe term.

MR. SCHROCK: Who is "they"? Could I ask it in this case?

MR. BAJOREK: They being -- I think this was -- this was Los Alamos that had redone the heat transfer several years ago.

MR. RANSOM: This TRAC-M modification was done several years ago?

MR. BAJOREK: Yes.

MR. RANSOM: As a part of the TRAC-M thing or --

MR. BAJOREK: I don't know for sure.

MR. RANSOM: I mean, TRAC-M has been going on for more than 4 years, but --

MR. BAJOREK: I don't know if this was specifically put into TRAC-M to be TRAC-M or was something that had been in one of the TRAC-P or TRAC-B that had been grandfathered over into TRAC-M. I could find that out, but I don't know.

MR. RANSOM: Okay.

CHAIRMAN WALLIS: You have a new slide now?

MR. BAJOREK: Yes. This is a new slide to show you what is in TRAC-M, not by way of saying this is what we think is the right way of doing it, but to show you that what TRAC-M does in a way of getting at this partition is to put on a subcooled weighting factor and another evaporation factor that it claims goes back to Lahey to adjust the heat transfer coefficient that you get out of this modified Saha-Zubar.

Now, if you go to other codes they're doing something similar, okay. Or they'll change this and they'll use a different model for this ramping, and they'll change something over here with the vaporization in order to try to get this split. In some cases it works against the data. It other it falls flat. And our argument is that the model no longer has a basis, okay. If the one in TRAC-M has been changed, some data, that we may not even care about for reactor safety applications.

CHAIRMAN WALLIS: Professor Dhir is going to come with a better alternative, you know, to this.

MR. BAJOREK: Yes.

CHAIRMAN WALLIS: He's going to have --

MR. BOEHNERT: He said that's right.

CHAIRMAN WALLIS: He's going to have a different -- or whatever.

MR. BAJOREK: No. We're going to get away from this taking a overall heat transfer coefficient and slapping on a couple of ramps and at the very least split this up into the individual mechanisms. And if we understand those individual mechanisms, now we an come up with better models that we could somehow eventually put in the code and get the overall net vapor generation that's going into the cell.

CHAIRMAN WALLIS: You mean the amount which is formed by bubble growth minus bubble collapse?

MR. BAJOREK: Yes.

MR. BANERJEE: But it must depend on the size of the cell, right?

MR. BAJOREK: Size of the cell --

MR. SCHROCK: I've got a question about your equation 3 on the last slide.

CHAIRMAN WALLIS: Well, since it's going to be replaced anyway.

MR. BAJOREK: Well, go ahead.

MR. SCHROCK: So what is H subscript WL?

MR. BAJOREK: That's the total heat transfer -- that's the heat transfer or the total heat transfer from the wall. And you get that out of the Nussett number from the Saha-Zuber.

CHAIRMAN WALLIS: Which itself has more correlating perimeters in it.

MR. BAJOREK: Yes.

MR. SCHROCK: But see, WSB is a pure number and FE is a pure number.

MR. BAJOREK: That's a pure number. That's dimensionless. Yes, those two are dimensionless.

MR. SCHROCK: And so the dimensions of H gamma and WL are the same, those are the ordinary dimensions of a heat transfer coefficient?

MR. BAJOREK: Yes. And when you put it all in here with B cell, which is the cell volume and -- it does work out to be -- it's actually a volumetric vapor generation.

MR. SCHROCK: So it's in the definitions of these W and F that there's some physical sense to this H gamma, is it?

CHAIRMAN WALLIS: We should move on. I think we'll move on.

MR. RANSOM: Quickly, is this written up in the TRAC-M manual?

MR. BAJOREK: Yes, it is. Appendix G.

MR. RANSOM: The new one that we got?

MR. BAJOREK: Yes. July 2000 I think is the most recent one.

Okay. So just to quickly conclude. We don't think what we see in there has an adequate database. We don't have an adequate basis for it and the ramps are essentially ad hoc. We wouldn't expect that model to work over --

CHAIRMAN WALLIS: But you're going to release this code before Professor Dhir is finished.

MR. BAJOREK: The beta version.

CHAIRMAN WALLIS: You going to leave it the way it is?

MR. BAJOREK: For the first release it's probably going to have to be that way. But later releases we hope we can change that.

And that's all I have.

CHAIRMAN WALLIS: Thank you.

MR. BAJOREK: Thank you.

CHAIRMAN WALLIS: Very reassuring computation.

MR. ROSENTHAL: While V.J. is going up, we had -- you know, at least from perspective we had some separate effects experimental programs going on and we had some code development programs going on. And I didn't have some key staff, which I now have.

And we're working very hard now to play catchup to glue the experimental programs and the code development far better together.

Steve's been with us for about a year. Joe Kelley has returned to the staff. And now we have the staff to do it. And these guys are starting what ideally would have taken place over the years.

MR. DHIR: Good afternoon.

You know, about 4½ years ago we started on this project of subcooled flow boiling at low pressures, so that was the specific topic. And there we wanted to investigate subcooled boiling through experiments and model development. With me there is one student, one post-doctorate fellow working and they did most of the work.

In the last 4 years we have made several presentations to NRC with respect to the progress we have made. We have published some of the work in various journals and present to conferences. And today I think I appreciate the opportunity to discuss with you what we have done and look forward to your critique.

And it's also kind of interesting to stand here rather than sit there.

The key objectives of this work were to develop a mechanistic basis for subcooled boiling, heat transfer for incorporation in advanced reactor codes. And we had to support this development through laboratory scale experiments on a 9-rod bundle, although before going to 9-rod bundle, we did experiments on a flat plate heater. That provides good geometry for visualization and to do some detailed studies.

A range of parameters of interests were pressures from 1 to 5 bar, mass velocities from 100 to 1000 kilogram per meter square per second, and liquid subcooling, that inlet from zero to 50 degrees celsius. That was the understanding we had when we started the work.

The whole effort was divided into seven tasks.

MR. SCHROCK: Does the at low pressures imply that the interest in the model is just at low pressure or is --

MR. DHIR: Low pressure. Okay. Our main objective was to develop these models at low pressure and validate them, but evidently I think that's doing half the job. We got to extend these models to high pressure and see if we can describe the whole pressure regime. So what we are doing now while validating the models, we are looking at high pressure data as well. Hopefully, to describe the boiling process, if we understand correctly for all pressure.

MR. SCHROCK: Okay.

MR. DHIR: So this total activity was divided into seven tasks. The first task was to conduct the literature search and see what was out there, whether that was sufficient to develop models or we needed more data as the development proceeded.

And from this literature review we developed the database, and then the forming on task was that we now have already what's out there, what we wanted to develop the test plan for the experiments. And that first pre-task almost took the first year of activity.

Thereafter we designed and fabricated a test loop, and that test loop we used first a flat plate geometry for the heater and subsequently we used a 9-rod bundle.

And then the task 6 was to develop a preliminary model.

And last task is to validate the model with subcooled flow boiling heat transfer data at low pressures and then eventually all pressures.

And currently we are in the last stages of task 7. We are told that there's a sunset rule, so in the next 3 or 4 months this activity will be stopped and something new will start.

MR. BANERJEE: What's the sunset rule?

MR. DHIR: Namely the 5 year limit on these activities. So 5 years will be over, I guess, in a few months.

MR. BANERJEE: So at that point you can go on to incorporating these into the codes, right?

MR. DHIR: I don't know.

MR. ROSENTHAL: We just have contracts, commercial contracts that go five years and we'll be renewing contracts. There's no -- do the work as long as you have to do it. But, of course, you like to start work and conclude work which frees up monies so you can do other work. But there's no rule.

MR. DHIR: Okay. I give you a little more details of the tasks as we go along.

So we did a thorough search of the open literature and found that there was a number of models had lots of empiricism built into them and these models were very often were inconsistent at subprocess level.

MR. RANSOM: This may be a nitpick, but in your literature review I didn't find any reference to the current models that are used in the code or any discussion of the deficiencies in those.

MR. DHIR: We looked at only mostly published literature. We did not look at the code themselves.

MR. RANSOM: Well, I think a lot of this is in the published literature. You know, the experiments which have been used for validation of the models. I believe Lahey's work is in the literature. The Saha-Zuber work is in the literature.

MR. DHIR: Yes.

MR. RANSOM: Why weren't they discussed?

MR. DHIR: What do you mean discussed?

MR. RANSOM: Well, there was no reference or discussion of the existing models.

MR. DHIR: Saha-Zuber --

MR. RANSOM: Is that to say that existing models are inadequate, I guess.

MR. DHIR: Right. But in the report Saha-Zuber was discussed.

MR. RANSOM: I don't believe it was. I never found any mention of it.

MR. DHIR: Okay. But I think you will see that I would mention to Saha-Zuber.

MR. RANSOM: In these two papers that we received.

MR. DHIR: Okay. But we submitted the reports, and it should be in the reports.

MR. RANSOM: You have delivered a report?

MR. DHIR: But I don't have it here.

MR. RANSOM: But it was given to the NRC?

MR. DHIR: Yes.

MR. RANSOM: Yes.

MR. SCHROCK: So there's a NUREG report that has additional detail?

MR. DHIR: I don't think it's a NUREG report. It was UCLA report which we submitted periodically to NRC.

MR. BAJOREK: We have a couple of progress reports, but in preparation for this meeting we thought a more concise way of looking at the models were the technical papers.

CHAIRMAN WALLIS: It looks as if you're going to cover a lot more than was sent to us.

MR. DHIR: Right. I have submitted viewgraph. So that shows what was mostly summary of what --

MR. BANERJEE: Wear us out, huh?

MR. DHIR: Right.

CHAIRMAN WALLIS: He's going to surprise us.

MR. DHIR: So we found that application of these models to low pressures are suspect. We did a number of studies which have shown that. And there was very limited low pressure experimental data available. Most of the data were at high pressures.

So we compile all of the database, also the experiment to conditions, test setup and so forth. And that report which are titled Experimental and Analytical Studies in Subcooled Flow Boiling and just containing database was submitted to NRC.

Now, let's look quickly at what we are

interested in. As you all know that initially in the heated channel as subcooled liquid enters in and this is first phase, first heat is removed by single phase -- forced convection and if there's resulting flow -- then you have a resulting boundary layer so the heat transfer coefficient will be -- in the actual direction.

At some point on the heated surface you see the nucleation start to occur, and that location we call ONB, onset of nucleate boiling. If one is to predict the complete physics of the processes downstream, one should be first able to determine where nucleation occurs.

This is followed a region where the bubbles are detached to the surface, they're just sitting there, vapor is produced and is condensed on the surface but bubbles do not lift off from the wall and migrate into the bulk liquid. And as we move further downstream, downstream at some location the bubbles start to leave the heater surface. Where this process begins we call OSV, onset of significant voids.

And beyond this point we are producing at the wall some condensation is occurring as the bubbles are attached to the wall or slide along the wall, thereafter the bubbles depart the heated surface and move on to the bulk. Okay.

So the key items we need to discuss as we're looking at the wall region, the physics of the process, is that how the wall heat flux partitions.

CHAIRMAN WALLIS: What does that mean?

MR. DHIR: Partitioning means first just as the wall, how the heat is transfer to, let's say, the liquid. Separate the liquid. Then beyond that how much of that energy goes into production of vapor that goes into the bulk and how much is going on condensation as the bubbles surface, and how much goes directly into the liquid.

Q Is this very different from a model for partitioning that may be in TRAC? Because TRAC doesn't look at all these phenomena. Does the meaning of all heat flux partitioning is something else in the code.

MR. DHIR: No, code need -- what is your-- let's say I have a heat flux of -- what fraction of that energy is going into the bulk as vapor, that's what the core needs.

MR. RANSOM: Into the bulk or into --

MR. BANERJEE: Into just generating --

MR. DHIR: Into the liquid. What fraction is going into the liquid as vapor.

CHAIRMAN WALLIS: I don't understand.

MR. BANERJEE: The code is interested in predicting the void fraction.

MR. DHIR: Correct. Right.

MR. BANERJEE: So there is partitioning-- used by Solbrig if I remember it correctly, of partitioning this -- it was a fix at that time. Was say how much went into generating vapor right at the wall, which means to an attached model. That's where it really --

MR. DHIR: That's a different concept. I'm not going to talk about it. I'm just saying this.

MR. RANSOM: Well, let's say the Lahey model is the same, it's how much energy goes into producing vapor. So you can assume that energy is divided by -- and produces vapor.

MR. DHIR: Right.

MR. RANSOM: The other part goes into the bulk heating for the liquid. And I think that's the same thing.

MR. DHIR: No. You will see that. Mine are different. Okay. What I'm trying to say is this, say you -- again, I will repeat myself.

You have a certain imposed heat flux on

the wall.

MR. RANSOM: Right.

MR. DHIR: I draw an artificial boundary here, okay. And I say how much of this energy from the wall is going into this liquid as vapor.

MR. RANSOM: Right.

MR. DHIR: Okay. So that is that fraction. How much went into the liquid either because these bubbles moved or the liquid removed some heat from the wall, plus how much came in through condensation which occurred when the bubbles-- beyond their boundary and that heat also went as a sensible heat to the liquid. So the liquid got energy either through condensation or directly from the wall, and the vapor was added from the wall. So now you have number density of these bubbles, the size of these bubbles, so that gives you source term and it also gives you what the LOCA and -- of the liquid is.

CHAIRMAN WALLIS: The void fraction is also made it of the ones attached to the wall, which you don't have in that description you just gave.

MR. DHIR: The void fraction of the wall?

CHAIRMAN WALLIS: Yes, that's a separate model as the void fraction --

MR. DHIR: We could put it, but we have

not addressed that. Yes.

MR. SCHROCK: I think there are two points that need clarification on this description.

MR. DHIR: Okay.

MR. SCHROCK: And one is does the code description include vapor generation in this first region of attached bubbles? Bubbles grow and collapse in that region, but don't detach from the wall. There is no two phase flow problem in the sense that the vapor is moving in the axal direction.

In the second region --

MR. DHIR: Right, in this region, that's what we talk about.

MR. SCHROCK: No, the last comment referred to the first region.

MR. DHIR: Right.

MR. SCHROCK: The lowest region.

MR. DHIR: Right.

MR. SCHROCK: Now in the upper region there still is need to sharpen that description in terms of whether this gamma vapor includes the volume of bubbles growing at the wall before they detach or does it account only for vapor which is detached and moving with the stream.

MR. DHIR: I don't -- again, I'm not doing the code. But I think if I were to advise them, they would not include this. They would be just looking at what is leaving the wall.

MR. SCHROCK: Well, there's probably a significant difference in the meaning.

MR. DHIR: Right. Yes.

MR. SCHROCK: I think there has to be a convergence of what you're describing and what they're trying to describe in the code.

But this implies that the void in the attached bubble zone is insignificant.

MR. DHIR: No.

MR. SCHROCK: Yes, how do you find it then?

MR. DHIR: What do you mean how do I find it? Why do I need to know it?

MR. SCHROCK: There is no gamma --

MR. DHIR: Again, see, you're going --

MR. SCHROCK: There is no gamma vapor in the attached region.

MR. DHIR: That's right. In this region.

MR. SCHROCK: Right.

MR. DHIR: Yes. There's no gamma vapor.

CHAIRMAN WALLIS: Well, how do the bubbles get there?

MR. DHIR: There's such a thing on the wall.

CHAIRMAN WALLIS: Is there nucleate on the wall?

MR. DHIR: They're -- on the wall.

CHAIRMAN WALLIS: They're just attached to the wall in residence, they grow and collapse.

MR. DHIR: Beg pardon?

MR. BANERJEE: Those bubbles even before detachment --

MR. DHIR: Right.

MR. BANERJEE: -- have a significant void fraction.

MR. DHIR: Right. They have -- I don't know how significant you call it, but it's maybe --

MR. BANERJEE: It depends on the size of the channel.

MR. DHIR: Yes, right. But it's a -- it's very low density bubble population on the surface.

MR. SCHROCK: Well, they're small compared to the ground stream region.

MR. DHIR: Very small, yes.

MR. SCHROCK: But I'm just looking for some more rigor in definition of terms linking the experimental observation with the code. That's what

I'm looking for.

MR. DHIR: Right. Again, we have not done that part. Okay. We can speculate on it.

MR. SCHROCK: Well, that's a mistaken, V.J. It's not your fault, but it's a mistake.

MR. BANERJEE: Bad boy.

MR. DHIR: What?

MR. BANERJEE: Bad boy.

MR. SCHROCK: Well, in the codes, for example --

MR. DHIR: And that's why I came here to listen to that.

MR. RANSOM: It terms of clarifying it a little, there are no bubbles attached to the wall in the code models, you know. So they sort of only begin at OSV or somewhere around there.

MR. DHIR: Right. Exactly. They don't look at this part. They only begin calculating from here. And they don't know where OSV.

MR. RANSOM: Right. Is that the need? Is the need only beyond OSV?

MR. DHIR: That's right. That's what I would do.

MR. RANSOM: They always tell you the other region.

MR. BANERJEE: The code doesn't distinguish to recommends this partitioned the heat flux --

MR. RANSOM: Well, the beginning or onset of significant voids is the first region, that's considered the beginning of boiling, even subcooled boiling.

DR. MOODY: You've got one part of your study is -- bubbles, diameter --

MR. RANSOM: So it is not really -- that are being attached to the wall.

DR. MOODY: -- so you could track the life of a bubble, is that right?

MR. DHIR: Yes. You can go to -- detail as you want to, but I think we are first discussing what terms mean, what I'm trying to talk about.

This region the void fraction is very low. We can tell you how much void fraction would be, approximately. But it doesn't mean anything to the code. Code are basically starts calculating void fraction from --

MR. SCHROCK: So that's a separate justification. There has to be a prediction of that and a demonstration that --

MR. DHIR: Exactly.

MR. SCHROCK: -- that amount of void is insignificant in terms of whatever the code is interested in.

But in the region of detached bubbles --

MR. DHIR: Right.

MR. SCHROCK: -- there remains the issue of the volume of steam in attached bubbles.

MR. DHIR: Why are you interested in it?

MR. SCHROCK: Well, it's a part of the total void fraction.

MR. DHIR: Right. The question is you're looking at these bubbles are sitting in here and how is that going to effect your -- if you say it will be a secondary effect.

But key question you are wrestling with it, how is this Y profile developing in actual direction.

CHAIRMAN WALLIS: V.J, everything would be helped tremendously if you had your picture here, and you got a picture beside it which says this is what the code says is happened. Code says you have two fluids at different temperatures and so on, but you have interactions between them.

MR. DHIR: Right.

CHAIRMAN WALLIS: Then you have to somehow go from this picture of reality to the idealized picture in the code. It doesn't have anything on the wall, as I understand it.

MR. DHIR: Yes, right.

CHAIRMAN WALLIS: But it -- injects vapor--

MR. DHIR: Injects vapor is right. Exactly

CHAIRMAN WALLIS: Okay.

MR. DHIR: Then that, I'm going to provide that information to them.

CHAIRMAN WALLIS: Okay.

MR. DHIR: You inject the vapor into that and you inject the -- into the liquid, and you know how much is coming out which way.

CHAIRMAN WALLIS: Then the liquid and vapor then interact because they have different temperatures.

MR. DHIR: Temperatures and that's how it is.

CHAIRMAN WALLIS: There's more heat flux, but this would mean a kind of code phenomena --

MR. BANERJEE: Well, it's not exactly what the code does, that's the problem. What we're wrestling with is the wall's partitioning is really --

MR. DHIR: See, again --

MR. BANERJEE: --to the wall.

MR. DHIR: That's not done correctly. Codes are not doing it correct. So why are we going after that? I think they should rewrite that part and, in fact, what I can give you my conclusion, what I find, and Graham mentioned it correctly earlier. I was surprised. That basically the heat going from the wall, all of the energy goes to liquid. Then part of that is converted into vapor and we track how much vapor is leaving the heater surface and how much is going just -- the rest of it is going just to the --

MR. SCHROCK: I'm glad you said that, because I've written that for so many reports to the ACRS I'm a little tired of saying it. The heat is all transferred to the liquid first and then vaporization occurs within that super heated liquid.

CHAIRMAN WALLIS: Were you surprised because I said something correct?

MR. DHIR: Right.

CHAIRMAN WALLIS: Or were your surprised because I gave you new knowledge which you didn't have before?

MR. DHIR: No, I had the knowledge before. But I was surprised because if you look at the codes, that's what we are talking about. And they keep splitting right over from the wall.

MR. BANERJEE: It's an idealization.

MR. DHIR: Not -- incorrect way of counting.

MR. BANERJEE: Whichever way you think of it. But what's happened is the region of the attached bubbles you have to argue has a low void fraction and therefore doesn't give you any significant void for the reactor dynamics calculation.

MR. DHIR: If you want to interpret -- yes, that's the second issue.

MR. BANERJEE: That's what you have clearly show that the void is --

MR. DHIR: No, no, no. No, no, no. This is a different question you're asking.

MR. BANERJEE: But that is a significant--

MR. DHIR: Yes, that is a significant, we will provide you. Because we know the number density of bubbles. We know -- of the bubbles, so I can give you what the void fraction is if that is needed. But I think the recent question we were asked provided --

CHAIRMAN WALLIS: Okay. Presumably it also increases the interfacial fiction fraction.

MR. DHIR: Right.

CHAIRMAN WALLIS: The bubbles on the wall are like a --

MR. DHIR: Right. It's improved heat transfer, too. Basically we look at heat transfer here, all the bubbles are sitting on the surface, the heat transfer basically single phase, and it's higher than would be without bubbles.

CHAIRMAN WALLIS: Okay. Can we move on to the next linked slide.

MR. RANSOM: I would just like to make one suggestion, and that is that you add interface drag or relative velocity between the phases of significant effect, at least in the work that I've done that seems to be a factor.

MR. DHIR: Right.

CHAIRMAN WALLIS: That is part of how we get the heat transfer coefficient between the phases.

MR. RANSOM: Well, so on that slide it should be --

CHAIRMAN WALLIS: On the slide.

MR. RANSOM: -- a variable.

CHAIRMAN WALLIS: Okay. Ready for the next one.

MR. DHIR: Good.

MR. BANERJEE: Is velocity of variable a

net velocity in your --

MR. DHIR: Yes. In that velocity subcooling.

Okay. So basically single phase, your heat is removed by forced convection. The bubbles that are attached to the heater surface, again although there's some condensation going on at the surface, but some heat is gone as a single phase heat transfer from the wall, but there's no vapor production in terms of vapor leaving and going into the bulk. So all of the heat is basically -- as a forced convection heat transfer into the liquid.

In the region beyond OSV we think from the wall heat goes into the liquid either forced convection or transient conduction. That's the key contribution we're making as the bubbles detached or slide along the surface, they break the thermal boundary layer. It has to redevelop and that's the period during which transient conduction occurs. That's the key contribution, I think.

CHAIRMAN WALLIS: Well, we're not going to talk about the middle region at all, so --

MR. DHIR: No. We are going to talk to you a little bit, no, nothing much. Mostly we'll talk about this region and maybe a little bit --

CHAIRMAN WALLIS: So this QSP and QC, you're some sort of drawing a control volume which cuts out the region where you transfer heat to the liquid and then it evaporates in vapor?

MR. DHIR: Right.

CHAIRMAN WALLIS: That's not allowed. So you've joined their control volume beyond the place where there's anymore evaporation occurring.

MR. DHIR: No.

CHAIRMAN WALLIS: Yes. For some of that single phase from the -- we agree that all of the heat transfer is single phase at the wall.

MR. DHIR: Right.

CHAIRMAN WALLIS: And then you've taken out of that the bit which goes into evaporation and then condensation?

MR. DHIR: Right.

CHAIRMAN WALLIS: And that's called something else.

MR. BANERJEE: That's this force convection.

MR. DHIR: Right. This is transient conduction and force convection contributions. That's the total heat from the wall. Okay.

CHAIRMAN WALLIS: I don't understand that.

MR. DHIR: Because the region where there are no bubbles, in this region, heat is still being removed but that's basically by forced convection.

CHAIRMAN WALLIS: I don't understand the subdivision between transient conduction and forced convection.

MR. DHIR: Okay. Let me describe what transient conduction. Transient conduction, let's say bubble departed from this surface. Okay. It's going to slide along the surface and then lift off from the surface. The process is that a nucleation site above it starts to grow, it grows to some diameter which we call departure diameter, thereafter the bubbles start to slide. And then it grows to a certain size and lifts off from the surface.

As it is sliding along the surface it disrupts the boundary layer and over that period or over that region it is basically removed by transient conduction. And I'll show you how graphically what we mean by it, but for the time being we are saying that the heat is removed over that portion over which the bubbles slide is by transient conduction or if the bubble detaches from the surface in a given location, then the boundary layer has to redevelop around that location and then heat will be removed by transient conduction. And the remainder of the area where there's not much activity, heat will be removed by forced convection.

MR. BANERJEE: But is the bubble that's sliding you're always wiping the area.

CHAIRMAN WALLIS: You always force convect, yes.

MR. BANERJEE: So you're always in transient. It's like a surface renewal --

MR. DHIR: It's a question of the timing. How much time is there? How much time it takes to slide before it lifts off. And that time I'll show you.

MR. BANERJEE: But it'll still be a transient.

MR. DHIR: Right. It's a transient, but maybe I'm really jumping ahead.

MR. BANERJEE: Your tunnel layer will be developing and then destroyed and developing --

MR. DHIR: Right. Exactly. It will be keep repeating that time period. Yes, sure.

MR. BANERJEE: It's always transient?

MR. DHIR: Yes, it's a transient.

MR. SCHROCK: Well, what's the force convection for? What's the initial condition for that transient conduction problem, although that's the problem, isn't it, V.J.?

MR. DHIR: Right. You see -- okay. Let's look at this slide.

MR. SCHROCK: There's a surface renewal.

MR. DHIR: Surface renewal basically, yes.

MR. SCHROCK: Yes.

MR. DHIR: And so you see just once you wipe it out, you start a time sequence to zero, your transient conduction is -- and the heat will be -- heat flux will be dropping as inverse of square root of time, and then forced convection has its own value. We are saying that this transient time will be only up to the point where the heat transfer by diffusion equals the forced convection areas to --

MR. SCHROCK: But you didn't address the point that I made that the initial condition for your transient conduction problem is basically unknown. So you have to put in some idealized initial condition for that.

MR. DHIR: Right. We're saying zero. You just wipe -- you have wiped out the thermal layer completely and you're starting all over again.

MR. SCHROCK: But you cannot do that because you've got a radial temperature distribution in that liquid that sort of overlays the whole thing. The bubbles are disrupting that, but as a bubble goes by it doesn't leave the uniform temperature field behind it.

MR. DHIR: Right. Bubble is sliding on the surface, heater surface.

MR. SCHROCK: Yes.

MR. BANERJEE: I guess that there is some temperature gradient, it's not completely --

MR. SCHROCK: You don't know what it is is what I'm arguing.

MR. DHIR: Right.

MR. SCHROCK: You have no basis for claiming you know a -- a uniform temperature initial condition --

MR. DHIR: But again, that's -- that's assumption you make. You have to make some assumptions. Right. And the key thing is that we say that wipes out whatever the thermal layer existed, therein --

MR. SCHROCK: And then subcooled liquid comes in contact with the wall? The slightest subcooled liquid comes in contact with the wall?

MR. DHIR: Right. That's what we're saying.

MR. BANERJEE: It's a typical surface renewal theory.

MR. DHIR: Right.

MR. SCHROCK: If you bring in the mean subcooling at that point, you're going to way overestimate the heat transfer by this transient process.

MR. DHIR: Right. And so that's true. It depends on how subcooled the liquid is. But basically heat flux it would be calculated to the total whatever you're subcooling is.

MR. BANERJEE: What's delta Tw?

MR. DHIR: Delta Tw is T wall minus T set, and this is the liquid subcooling you have. So this is the -- so the liquid slug of the slab which is coming in has a bulk temperature -- the liquid.

MR. BANERJEE: So it's the full bulk temperature of the wall is --

MR. DHIR: Into a slab which is initially a temperature T liquid.

CHAIRMAN WALLIS: I don't understand this at all. I thought these bubbles were attached to the wall and then they sort of came off --

MR. DHIR: Look, what we see is this. This bubble starts to grow on the surface. It grows to a certain size and then it will slide on the surface.

CHAIRMAN WALLIS: Yes.

MR. DHIR: And as it is sliding, we are saying it's wiping out the thermal layer which is --

CHAIRMAN WALLIS: Which is dragging the same thermal layer along behind it.

MR. DHIR: And then it leans back, goes away. And the new bubble starts, it will start the process all over again.

CHAIRMAN WALLIS: It's like a kind of Reynolds analogy. When the bubbles goes, it brings in some stuff from the cool. It's like Bankoff's sort of, whatever he called it.

MR. SCHROCK: I think Graham just said it, it drags along some temperature structure from the upstream region behind the bubble as it goes. And that upstream region is your region one where the bubbles grew and collapsed in -- and they give you a considerable superheat at the wall. Not a subcooling at the wall.

CHAIRMAN WALLIS: Well, I guess we have to move along. I can see we're probably going to have to discuss --

MR. DHIR: Again, I don't know why that region has an effect. That region brought in thermal--

developed. But we are calling this process as you move downstream and the bubbles are at every location, the bubbles are going, forming and then sliding along the surface and then lifting off. Then merging between with some of the bubbles along the way. And the only function you can argue with me is that when the bubbles slide they disrupt the total thermal layer or not and when the liquid comes in it's at what temperature it is. And the assumptions we are making it wipes out the thermal layer and the new thermal layer starts by the -- or if you consider the -- slab and the initial temperature is this.

MR. SCHROCK: Well, it's not like it has to move radially in and out. It goes around the bubble. The bubble slides through the liquid and liquid is moving around the bubble, not just over the top of it.

MR. DHIR: I'm not saying it -- you're talking about the wall.

MR. SCHROCK: I'm talking about how the liquid is displaced by the sliding bubble.

MR. DHIR: Right.

MR. SCHROCK: Okay. It's not as though the bubble is a ring around the tube and all -- and it has to slide through the liquid in that way, in which case liquid from a region far away from the wall would be induced into that zone.

MR. DHIR: Right. Right.

MR. SCHROCK: And go around the bubble. What you're doing is more akin to what Graham Wallis said. It's like dragging along behind it whatever is upstream of the bubble, it's begun to slide.

MR. BANERJEE: The liquid can't slip onto the wall. The bubbles can slip but the liquid can't in some sense.

MR. SCHROCK: We got on this discussion by my point that the initial condition in a transient conduction modeling of the heat transferred directly from the wall to the liquid is a major problem because the initial condition for that transient conduction model is essentially unknown.

You've chosen to model it as though the mean subcooling comes to the wall instantly as the bubble goes by.

CHAIRMAN WALLIS: It seems upper bound.

MR. SCHROCK: And I think that is a great extreme. I mean, it's --

MR. DHIR: Okay. You have the --

MR. SCHROCK: The temperature of the liquid at the wall is going to be much higher than that and your transient conduction to the liquid is going to be way overestimated with that assumption.

MR. DHIR: Okay. Now I will give you -- we have the numerical simulations also of this process.

MR. SCHROCK: Okay.

MR. DHIR: And if you look at it the liquid that is coming onto the surface after the bubble has gone out is very close to the bulk temperature. I'm not on subcooled case, but the saturated case, numerical simulation. It's close to the saturation temperature, the temperature of the wall almost drops to the saturation value before it picks up, actually.

MR. BANERJEE: In the numerical simulation did you have the liquid laminar or --

MR. DHIR: Laminar.

MR. BANERJEE: -- is giving you next mixing effect to --

MR. DHIR: Right, exactly. And the question now -- you know, we can belabor this point, but it's extremely difficult to see what exact the temperature will be and how much the region is. And when you develop the model you're going to have certain assumptions and now it's questionable assumptions, one can validate them, but those assumptions are based on what we can --

CHAIRMAN WALLIS: I think if you move a bubble through a liquid and allowing the flow of the streamline, it's coming back to about where they started. So you haven't done any mixing at all.

MR. BANERJEE: Well, you have a wake.

MR. DHIR: The wake of --

CHAIRMAN WALLIS: But not in the very low--

MR. BANERJEE: It is not?

CHAIRMAN WALLIS: No. If there's a wake, then they -- okay.

MR. BANERJEE: There will be a wake.

CHAIRMAN WALLIS: It's a wake relative to what? Because the bubble's presumably moving because the liquid's pushing it to the wakes on the other side of it.

MR. BANERJEE: You have to look at it as something --

CHAIRMAN WALLIS: Okay. Okay.

MR. DHIR: Anyway, we've gone farther --

MR. BANERJEE: So did you submit that CFD for review yet?

MR. DHIR: We have done full boiling and

flow boiling as yet.

So basically, you know, from the wall we said energy is going by full convect not transient conduction. Then it split, some goes into the liquid to sensibly heat the liquid and some goes into evaporation, that is how much vapor bubbles -- how many vapor bubbles and what size are leaving the surface. And that's your -- leaving the surface as they were.

Whatever the condensation that occur at the surfaces will be counted in --

CHAIRMAN WALLIS: Those were two different layers. There's a Qfc and Qand a Qtc. It's happening at the wall.

MR. DHIR: Qtc is basically showing what is happening as the bubble has slided and --

CHAIRMAN WALLIS: At the wall. And Ql and Qev are happening somewhere further out.

MR. DHIR: Right. Qev can be happening as the bubble is -- from energy what you dumped in as the transient conduction goes back into evaporation. But we don't know how much that is. We are just looking at how many bubbles are leaving and at what sequence.

CHAIRMAN WALLIS: I think Ql and Qev are what you need to put in the two fluid model for the code. Code's never been to model things like Qtc.

MR. DHIR: The code won't. This is for our purpose.

CHAIRMAN WALLIS: That's it. So they were at two levels?

MR. DHIR: Right.

CHAIRMAN WALLIS: One is for the physics and one is what you need for the code.

MR. DHIR: Right. Exactly. Code doesn't need that first part. And I'll show you both calculations if we get to it.

MR. BANERJEE: As long as the void at the wall is negligible, what you're saying is the case.

MR. DHIR: Okay. What happens if void is not negligible?

MR. BANERJEE: Than you have to make an estimate of that.

MR. DHIR: Estimate of what?

MR. BANERJEE: The void fraction at the wall, not -- void fraction at the wall. What you're doing is you've set up the way to handle the situation for all the detached bubbles. So, see, the way you've got it there is the bubbles are detaching from the wall and you've got a split between vapor generation because it's detached bubbles --

MR. DHIR: Right.

MR. BANERJEE: And, of course --

MR. DHIR: The remainder wasn't liquid.

MR. BANERJEE: And how much goes into the liquid.

MR. DHIR: Right.

MR. BANERJEE: There were a layer of bubbles sitting at the wall --

MR. DHIR: Right.

MR. BANERJEE: Just sliding along or doing whatever the hell they're doing --

MR. DHIR: Right.

MR. BANERJEE: Depending on the size of the channel --

MR. DHIR: Right.

MR. BANERJEE: -- you know, they may or may not be significant part of void fraction. How big are these bubbles?

MR. DHIR: It depends on the pressure and velocity, whatever.

MR. BANERJEE: Well, it was a size --

MR. DHIR: It's about a millimeter or less.

MR. BANERJEE: Okay. Millimeter or less. So if you have, say, tubes or rods in some areas that they -- what is the typical flow of area?

MR. DHIR: Three millimeters.

MR. BANERJEE: Three millimeters. So the gap itself would be almost -- completely by the -- it would have a significant --

MR. DHIR: Right. Right.

MR. BANERJEE: I mean I haven't done the sums.

MR. DHIR: Right. But, again, that void fraction we can give that value.

MR. BANERJEE: Right.

MR. DHIR: Because we know the number and sizes of the bubble and so forth, and how much packing is there. So one can get an estimate.

So basically an isolated bubble, you're saying that as a bubble slides along your thermal layer is developing and that's the transient conduction is occurring. The region where there's no activity we're saying the heat is removed by forced convection.

MR. SCHROCK: Do your data show that the bubble always begins to slide before it reaches OSV?

MR. DHIR: OSV they're all stationary, OSV. They don't -- they're sizes below what is needed to slide. They never get to that size.

MR. SCHROCK: Your picture shows it's sliding before it gets there.

MR. DHIR: OSV starts here.

MR. SCHROCK: And, see, you got a bubble below that that appears to be sliding.

MR. DHIR: I think that should not be shown, actually. But anyways, there should be no arrow there. This bubble is just sitting there. Beyond that point the bubble start to slide.

MR. SCHROCK: So does that mean that the sliding phenomenon is akin to bubble departure from the standpoint of OSV?

MR. DHIR: Yes. Bubbles have to grow to a certain size before they can slide.

MR. SCHROCK: That I understand.

MR. DHIR: Okay.

MR. SCHROCK: But the question is whether or not OSV is defined in such a way that it means any axal movement of the vapor, any axal movement of the bubble whether it's attached to the wall or detached?

MR. DHIR: That's correct. But the bubble has to --

MR. SCHROCK: I mean, it can be one way or the other.

MR. DHIR: No. Once it's start to slide --

with other bubbles it will lift off when getting the lift off side. So our definition is the moment the bubble start to slide, we say OSV begins.

CHAIRMAN WALLIS: Do the forces from the fluid make it both slide and lift off and is there a lift force on it lifting it off?

MR. DHIR: Yes, that's correct. There is definitely a lift force.

CHAIRMAN WALLIS: What's important is that you break the contact with the nucleation center, and after that figure out what its trajectory is.

MR. DHIR: Right. Yes, and that's a very important thing. But the lift has been ignored in the past and bubbles -- to the surface. And if you make the fourth balance you cannot describe it.

MR. BANERJEE: In fact, if you inject bubbles -- with tiny holes, you see exactly this; that they grow to a certain size and then they slide --

MR. DHIR: Right. Right.

MR. RANSOM: Well, this model is similar I think to -- the other one is the critical enthalpy model which predicts enthalpy the liquid has to reach before a bubble can survive. And that's considered the point of onset of significant void --

MR. DHIR: That's what people have done

mostly.

MR. RANSOM: Right. But this one is quite similar. And I'm not criticizing it necessarily. I mean, because I think you're also trying to predict when will a bubble depart from the wall and can survive in the bowl.

MR. DHIR: Right.

MR. RANSOM: Without immediately condensing.

MR. DHIR: Right. And we are basically saying what is this point. I'm not -- I will show you the data what we got with respect to this point and what old correlations are and so forth, and show you later comparisons. But I am not focusing my attention to just theoretically or mathematically predicting this mark, this location. There's a correlation. But the key issue what we are trying to resolve -- which means beyond this location.

And this is a balance here. How long the bubble sitting here is the balance on how much evaporation is occurring and how much condensation is occurring can you have -- to create bubbles that are large enough to slide. Bubbles have to grow to certain size before they can slide. Smaller bubbles will just sit there.

MR. SCHROCK: Part of the detachment process, I think, has to do with the radial thickness of the liquid superheat. And when the bubble extends a certain distance from the wall, if it's still surrounded over all of its surface by liquid superheat to some degree, certainly a variation over its surface, then where it's superheated, it can -- there's no question it can still grow.

MR. DHIR: Right.

MR. SCHROCK: Whether or not it can still grow goes beyond that --

MR. DHIR: Right.

MR. SCHROCK: And that's the balance between the rate of --

MR. DHIR: Evaporation and condensation.

MR. SCHROCK: -- and the rate of condensation.

MR. DHIR: Exactly. That's what determines its location.

MR. BANERJEE: But the lift off --

MR. DHIR: Lift off is a separate issue. Lift off is a separate issue.

MR. BANERJEE: Because even with air bubbles they will slide for a while and then they left off.

MR. DHIR: Lift off, right.

MR. BANERJEE: Which they're not growing anymore. They stop growing at that time.

MR. DHIR: Exactly. That's lift force. Definitely.

MR. SCHROCK: I guess the point I was trying to make is that there's a thermal condition that's a part of the attachment process. It's not just the flow conditions.

MR. DHIR: Right. But thermal condition gives you -- right. Thermal condition is going to give to you how big the bubble is going to grow at that site. If the bubble does not grow to that size, it's going to sit there. So it's -- thermal conditions basically what size it gets to. And then the forces are the -- how much forces are acting to slide. It can push it out --

CHAIRMAN WALLIS: It can be still growing while it's sliding --

MR. DHIR: Yes, sure it does grow. Yes.

CHAIRMAN WALLIS: So that it may -- it's sense of gravity may detach, but it's --

MR. DHIR: No. Yes, it's still growing. The substantial growth occurs during that.

CHAIRMAN WALLIS: I don't want to take a

break too early, because I want to make more progress.

I think we may have to accept that you got a model here and then go ahead and then sort of see --

MR. DHIR: I just -- right.

CHAIRMAN WALLIS: -- see your experiments.

MR. DHIR: Right. But only thing -- one more thing I would show you that when the bubbles start to merge on the heater surface, are your superheat goes up -- so at least have your criticism on this part. As the bubbles grow nucleation sites become very large. The spacing S can be smaller than the lift off diameter you need or smaller than even the bubble diameter departure. So in this situation the bubbles while they're growing, they're -- and then once they get to that size, they will lift off.

CHAIRMAN WALLIS: They merge together in two dimensions, why don't they produce --

MR. DHIR: What do you mean?

CHAIRMAN WALLIS: This is a one dimensional picture, but presumably they're merging --

MR. DHIR: Right. In that area. But there's always liquid so it's not -- they form like a bridge, like a mushroom, so the bubble mushroom is there and then there are several stems, but the liquid is still there. It's not only that area.

CHAIRMAN WALLIS: -- then you get a raft of bubbles attached to each other, not just a chain.

MR. DHIR: No, no. This is one dimensional picture here. But if you look the other direction --

CHAIRMAN WALLIS: Yes, but in the other direction you've also gotten an S.

MR. DHIR: Right, it's a square grid.

CHAIRMAN WALLIS: So they will touch in the other direction.

MR. DHIR: Right. They will touch like--

CHAIRMAN WALLIS: So you get a complete layer of bubbles coming up.

MR. DHIR: In this model, right. That's correct. The bubbles are coming out just in unison.

CHAIRMAN WALLIS: That's probably what you need for TRAC?

MR. DHIR: This is how we calculate our transient conduction for this case; that all the bubbles lift and then form the thermal layer again and then calculate the -- so this will be at high heat fluxes.

So, you know, I'm skipping these slides. It's just on the conduction calculation, at different times and so forth.

MR. RANSOM: Well, in your TRAC model do

you envision this being a transient effect -- a periodic effect, I guess, right in which bubbles form on the wall and then depart?

MR. DHIR: No. TRAC will just get the source term, how much --

MR. RANSOM: You somehow merge it all together so it's a uniform thing with time more or less.

MR. KRESS: If the -- conditions -- if the -- conditions change then --

CHAIRMAN WALLIS: You still have to split these into the Ql and the Qevs.

MR. DHIR: Right.

CHAIRMAN WALLIS: How do you do that?

MR. DHIR: Ql and Qev -- Qev we calculate from our -- you know, I will show you how we calculate Qev.

CHAIRMAN WALLIS: So you take the total and then --

MR. DHIR: And then subtract Qev.

CHAIRMAN WALLIS: And then you take away Qev?

MR. DHIR: Right. Exactly.

CHAIRMAN WALLIS: Well, how do you know d?

MR. DHIR: You measured.

CHAIRMAN WALLIS: Well, how do you predict it?

MR. DHIR: Correlation. We measured, we got the data and from that we recorded --

CHAIRMAN WALLIS: -- how do you predict Qev -- you need to know frequency in a d cubed and Na.

MR. DHIR: Right, right.

CHAIRMAN WALLIS: And you know all those things?

MR. DHIR: That's exactly what you need to know to predict Qev. That's a key point. That's what I'm leading to. I discuss the model -- right, exactly.

CHAIRMAN WALLIS: Okay.

MR. DHIR: That's the whole point of this present -- but the key point I was trying to make showing you early on this modeling effort that what detail we have to make measurements to get all the ingredients which we need to put into the model. This requires you need to know frequency, number of density, bubble size and so forth.

MR. BANERJEE: How do you measure the rod bundles?

MR. DHIR: We measured most of them on the flat plate. And then on the rod bundle we -- there's some there but just to verify it, but not too much data. The rod bundle becomes very difficult because very quickly the liquid becomes saturated and after that you can't see much. And so rod bundle we got some data, but not whole lot.

Okay. So when we do the experiment basically from the preliminary model, what we see that there are a number of variables which need to be measured in the experiment and here in this table I give you which quantity we are measuring and what measurement that we are using to measure that quantity.

Wall heat flux for the flat plate heater, we measure from the thermocouples that are imbedded in the copper block. And for the rod we measure just from the power that is input to the rod. We have thermocouple embedded in the copper block and so we get the temperature profile and then from that we get the surface temperature. For the rods we have a thermocouple attached to the -- wall and those thermocouples are calibrated and from those thermocouples you measure the wall.

MR. RANSOM: Can I ask a question about that. That I didn't quite understand from the writeup, but you have cartilage heaters and then you're also measuring the temperature gradient using the temperature gradients to extrapolate to the surface and determine the heat flux

MR. DHIR: In the copper block, yes.

MR. RANSOM: I wasn't clear how you separate the effect of the heaters on the surface. Are the heaters on the backside of this copper block or -- so you're only looking at conduction through the copper?

MR. DHIR: Just wait.

MR. RANSOM: I was wondering how you separate the effect of the cooling, you know, from the boiling process from the heating of the cartilage heaters and --

MR. DHIR: Okay.

MR. RANSOM: So where is the heat transfer surface actually?

MR. DHIR: This is the surface.

MR. RANSOM: Okay.

MR. DHIR: And this is a larger area so the heaters are going this way.

MR. RANSOM: Yes.

MR. DHIR: And they're going up to this portion here.

MR. RANSOM: Up to that shoulder?

MR. DHIR: That shoulder or even below

actually.

MR. RANSOM: So do they generate heat over their length?

MR. DHIR: Yes, right.

MR. RANSOM: So there's uniform heat generation --

MR. DHIR: Right. And then we have this section where the heat even if there were some nonuniformities in this portion the heat flux will be mostly --

MR. RANSOM: And that's where you're measuring the gradient --

MR. DHIR: Gradients, right.

MR. RANSOM: -- of the temperature?

MR. DHIR: Right. This is the portion that you see -- we're jumping ahead. This is about 3 centimeters wide for test purpose and about 30 centimeters tall. Okay. And we have 7 axial locations where we put thermocouples. And we -- this is a cross section and this is a treated surface. We have three for thermocouples in the middle, three on the side, three on this side to see if this heat flux is uniform

MR. BANERJEE: It's copper, right?

MR. DHIR: Copper.

MR. RANSOM: Okay. I understand what

you're doing now.

MR. DHIR: Okay.

MR. RANSOM: And then flow is along this block, right?

MR. DHIR: Right.

MR. RANSOM: And it's vertical, is that correct?

MR. DHIR: Vertical.

MR. RANSOM: Okay.

MR. DHIR: Liquid temperature we use a microthermocouple so that we could get temperature profiles in the liquid, at least in the single phase case already close to partial nucleate boiling region. And then ONB we measure for -- of the boiling surface as released from thermocouple outward. Number density of nucleation sites, we took pictures of the heating surface and then counted the number that were active per unit. And, again, the temperature at which we measured the nucleation inside was obtained from extrapolation of the temperature -- temperature profiled in the solid.

MR. BANERJEE: How close to the surface do you have the thermocouples?

MR. DHIR: About 3 millimeters, 2 to 3 millimeters.

MR. BANERJEE: And you can actually see-- do you see any temperature fluctuations or it's too far.

MR. DHIR: No, it's too far.

MR. RANSOM: There's one other perimeter that appears -- yes, the Fourier number there, which has a timed perimeter in it. And I don't see that on here. But I presume that's time from the bubble initiation or --

MR. DHIR: No. That Fourier number is for the -- heat transfer of the condensation when the bubbles leave the heater surface and is moving into the bulk, for the moment it leaves the heater surface, that is the time you start counting.

MR. RANSOM: Okay. So it's the history of the bubble?

MR. DHIR: Bubbles, right.

MR. RANSOM: How do you measure that or how do you determine that?

MR. DHIR: You'll be jumping ahead again.

MR. RANSOM: I am?

MR. DHIR: Yes.

CHAIRMAN WALLIS: He measures time and then calculates --

MR. RANSOM: Time for what? It's time

from when the bubble has departed the wall and --

MR. DHIR: It starts to go into the -- so your condensation starts. Now there's only -- there's no heating from the wall. All the energy what the bubble has is being lost to the condensation at the surface. Bubble is shrinking, so we are tracking bubble trajectory and what the location is from then knowing every point release happens, you know where the position of the bubble is.

MR. BANERJEE: This is like the McMaster experiment?

MR. DHIR: Right, but it's on a heater surface. No, we're not getting -- it's on a boiling surface. The bubbles are creating on the boiling surface of different sizes and so forth. I show you some -- but we are jumping ahead so I have to change the whole thing.

The bubble departure and lift off that -- and location of OSV from visual observation.

MR. RANSOM: Is that how you get the time, you see it lift off and then you follow the bubbles?

MR. DHIR: Actually we do. Let me show you.

MR. RANSOM: Yes.

MR. DHIR: Just wait one second.

CHAIRMAN WALLIS: But the collapse of the bubbles in superheat liquid seems to be the most straight forward part of this --

MR. DHIR: Right, exactly. That was the easy thing to do.

MR. BANERJEE: To measure.

MR. DHIR: Beg pardon?

MR. BANERJEE: To measure.

MR. DHIR: Right. Bubble release frequency in high-speed films and count the number of bubbles at least for a unit of time.

Condensation heat transfer coefficient for attached bubbles, in this case one needs to have a liquid temperature profile and bubble growth rate in the vicinity of the solid surface. And by noting the difference in bubble growth rate for saturated and subcooled liquid, one can determine that how much energy is going to support condensation on attached bubbles.

This exercise requires auxiliary experiments and we have done some for another study, but for this case that portion is lacking. For the time being we are using Ranz and Marshall correlation basically to calculate the condensation heat coefficient.

Condensation heat transfer coefficient for detached bubbles, that we measured and I'll show you some results. Again, from films.

Bubble number density, high-speed films counting the number of bubbles per unit of time. And then the void fraction, we use gamma densitometer.

MR. RANSOM: How do you establish the bubble relative velocity?

MR. DHIR: You look at the bubble velocity.

MR. RANSOM: You know the bubble velocity, what's the liquid velocity?

MR. DHIR: Liquid we are using the bulk as axial to flow.

MR. BANERJEE: No, no, he's measuring the void fraction.

MR. DHIR: Resolution in terms of what?

MR. SCHROCK: Spacial?

MR. DHIR: Spacial resolution. I think it's close to 2 to 3 millimeters closest to the wall we can go -- is about 3 millimeter in size. So we can't go close --

MR. SCHROCK: And the channel thickness is what?

MR. DHIR: Channel is 3 centimeters.

MR. SCHROCK: Three centimeters. So you look at a tenth of the cross section --

MR. DHIR: Average at a given time.

MR. SCHROCK: And do the profile.

MR. DHIR: Profile.

MR. KRESS: Do the bubbles condense so fast that they don't have time to interact with each other? You're looking at individual bubbles.

MR. DHIR: Right. Yes, they don't interact.

MR. KRESS: They don't interact.

MR. DHIR: Different bubbles we are talking.

CHAIRMAN WALLIS: All this is happening in a time scale of milliseconds?

MR. DHIR: Milliseconds, right.

CHAIRMAN WALLIS: So you don't have any time to average the turbulence in the flow. So I think you get a lot of fluctuations because the environment around the bubble depends on the instantaneous contact

MR. DHIR: Right, you see --

CHAIRMAN WALLIS: Average that through the whole --

MR. DHIR: But we have measured the

temperature profile --

CHAIRMAN WALLIS: But it's only an average.

MR. DHIR: Exactly.

CHAIRMAN WALLIS: You get a lot of variation depending upon what the turbulence --

MR. DHIR: That's true, but -- and the question is what level of detail do you want.

CHAIRMAN WALLIS: Well, I'm just saying when you make these measurements --

MR. DHIR: Right.

CHAIRMAN WALLIS: -- you're going to get a fluctuation, get a lot of difference in the results because your instantaneous fluid conditions depend on --

MR. DHIR: Mixing, yes.

CHAIRMAN WALLIS: So there's no time to average them out.

MR. BANERJEE: But you're ensemble averaging --

MR. DHIR: Exactly. We take many of the cases --

CHAIRMAN WALLIS: Your average will be okay, but there'll be a big spread around that average.

MR. DHIR: Even thermocouple when we put, we are looking at liquid temperate -- quite a bit.

MR. SCHROCK: Now in your rod bundles you can't get these gamma densitometer measurements?

MR. DHIR: Right. We look at only mid-plane. We cannot go close to the wall because uncertainty becomes very large.

MR. SCHROCK: So what do they mean?

MR. DHIR: What do you mean what they mean? We are giving -- rod bundles are --

MR. SCHROCK: So I guess I should repeat the question. How do you -- what kind of resolution do you get on your void in the rod bundle measurements?

MR. DHIR: Well, in the rod bundle we have only measured at the mid-plane. We have not done radial profile. We have gotten it because uncertainty is too much.

MR. SCHROCK: What does mid-plane mean? It's a rectangular bundle and you've --

MR. DHIR: Right. In the middle channel, we look at the center channel.

MR. SCHROCK: You make a measurement midway between the rods?

MR. DHIR: Right.

MR. SCHROCK: But the rods are closer than the walls in the flat plate experiment, so how does the --

MR. DHIR: We are focusing the beam through this passage and we are aiming at the mid-plane here.

MR. BANERJEE: How can you focus a gamma beam?

MR. DHIR: What do you mean focus?

MR. BANERJEE: You just said you're focusing --

MR. DHIR: Focusing means you remove the gamma beam, you could be hitting the wall and then you can --

MR. SCHROCK: So you've collumnated to a smaller beam --

MR. BANERJEE: So you go at -- beam?

MR. SCHROCK: You've collumnated the gamma densitometer to a small beam.

MR. DHIR: Right.

MR. SCHROCK: I understood that. But what I don't understand is how the size of that beam compares with the gap between the rods here.

MR. DHIR: The rod gap is 3 millimeters here.

MR. SCHROCK: But you didn't tell me what the collumnated gamma densitometer beam is.

MR. DHIR: I said that initially we had 3 millimeter beam. Then we use a plug to reduce it to about 1.5 millimeter size.

MR. SCHROCK: So for the rod bundles it's 1.5 millimeters.

MR. DHIR: 1.5 millimeter size.

MR. SCHROCK: Compared to?

MR. DHIR: Three millimeter gap.

MR. SCHROCK: Three millimeter gaps. So it's -- and then it's shining through --

MR. DHIR: Shining through here a sequence of two phased fluid which is at one point only one -- only 3 millimeters thick and then essentially is unlimited halfway to the next row of rods.

MR. DHIR: Right.

MR. SCHROCK: So you're averaging kind of in --

MR. DHIR: In this cross section.

MR. SCHROCK: Yes. How do you interpret what it means I guess is the question I'm trying to get at, V.J.? It's an average longitudinally and axially with respect to the beam.

MR. DHIR: The question I would ask you that was raised, this is how distance -- here and here. And the question you would ask is how accurate it is. And --

MR. SCHROCK: No, not a question of accuracy, but just how do you interpret it? What does it mean after you've got the number?

MR. DHIR: Now, suppose I had a code model? Let's say that I had -- I was modeling this in a code and if I gave somebody this information, they should be at least validated to the --

CHAIRMAN WALLIS: This isn't the volume average in the rod bundle, because you're just taking a point --

MR. SCHROCK: That's my point I'm making. It's the volume average in a little one and half millimeter diameter tube running between the rod.

MR. DHIR: Exactly.

CHAIRMAN WALLIS: And that's not the same as the average to the bundle because you haven't counted the bit you couldn't see that's shelter by the rods.

MR. DHIR: That's right.

CHAIRMAN WALLIS: So it's not the average void fraction in the whole section.

MR. DHIR: Yes. But if you're going to validate some code which can do this kind configuration, you can test it --

CHAIRMAN WALLIS: Well, it isn't validate -- it isn't calculating void fraction in a strip. It's calculating an average void fraction for the whole works?

MR. DHIR: No, but if I had 3-D code, why I couldn't do it?

MR. BANERJEE: But you don't have a 3-D code.

MR. DHIR: But they have some 3-D code.

CHAIRMAN WALLIS: But the question is how does this related to what TRAC needs now? What it needs now is an average over everything.

MR. BAJOREK: You don't have it there, but you will have it in your flat plate.

MR. DHIR: Flat plate we have.

MR. BAJOREK: In that case we would be able to get --

MR. DHIR: But if you want to test the flat plate.

MR. BANERJEE: Well, I guess it's history now. But this geometry gamma densitometer is not ideal.

MR. DHIR: Yes.

MR. BANERJEE: Because we made rod bundle void fraction measurements about 25 years ago in neutrons capturing and --

MR. DHIR: But, again, how many things you want to do?

MR. BANERJEE: Right.

MR. DHIR: It's not only we --

CHAIRMAN WALLIS: What do you need to do to get the answers required?

MR. DHIR: WE didn't need --

CHAIRMAN WALLIS: What you want, and you may want all kinds of things.

MR. DHIR: Right. But we don't need -- we are just providing this additional information basically.

CHAIRMAN WALLIS: I think actually we both want and need to take a break fairly soon. Is this a good point to do it?

MR. DHIR: That's fine, yes.

CHAIRMAN WALLIS: We going to move on to a different topic?

MR. DHIR: Yes, right.

(Whereupon, at 3:39 p.m. off the record 3:53).

CHAIRMAN WALLIS: We're now on the record. Get ready to proceed.

Can you start to give us results, or did you not finish --

MR. DHIR: No, I just want to describe the flow loop a little bit, quickly.

CHAIRMAN WALLIS: Because we do want to finish up before 6:00, so we're going to have to move along here I think.

MR. DHIR: Okay.

CHAIRMAN WALLIS: If there's no argument about modeling concepts, it might move along much quicker.

MR. DHIR: Right. Okay. First, our graphic -- flat plate and this was followed by a 9-rod bundle and we did fabricate two 9-rod bundle. The first rod bundle got destroyed during the experimentation or got degraded in some sense, so we had to rebuild another one.

MR. KRESS: But you joined the nucleus -- your departure --

MR. DHIR: No. No. It was degraded -- instrumentation. Thermocouple failed and so forth.

And on the flat plat we carried out 125 flow boiling experiments. The pressure in our experiments was with one bar, Marked velocity was varied from about 124 to 898 kilograms per meter squared, liquids are cooling on t to 50, wall heat flux around 2 to 113, wall plus centimeter square and contact angle from 30 to 90 degrees.

MR. KRESS: A question about those. Do those correspond decay heat levels?

MR. DHIR: Those correspond to the operation levels, right. From 2 to 113.

MR. KRESS: And the flows are what's calculated to exist during the -- level --

MR. DHIR: Right. Right.

MR. KRESS: Okay.

MR. DHIR: This a schematic of the facility flow loop, so forth. We have two tanks of each one 1.25 cube in volume and the liquid is stored in one of these tanks and its conditioned to bring it desired temperature and then it's pumped through a preheating section so that we can correctly control the temperature of the liquid that enters the first section. And after that it is dumped into another second tank.

Now, this is a photograph of the facility. Basically you see those two tanks and preheater. And this is -- where the test section is placed, either rod bundle or the flat plate. And this is our power supply, and this power supply is rated at 40 volts and

2.225 amps, so it give you 100 kilowatts of power. And this we have gotten through our grants, and not NRC.

This is how the test chamber for the flat plate tests. A converging nozzle was followed by a flow technique section about 61 centimeters upstream of the trail section. Then 30 centimeters downstream there was a rectangular section and followed by a -- nozzle.

CHAIRMAN WALLIS: That's a --

MR. DHIR: Beg pardon? Centimeters.

CHAIRMAN WALLIS: The dimension are really in -- 30.5 is

MR. DHIR: One feet.

CHAIRMAN WALLIS: 61 is 2 feet.

MR. DHIR: Two feet, right.

CHAIRMAN WALLIS: Okay.

MR. DHIR: And the cross section -- it's a square cross section 4.2 centimeters each side and copper block faces the one side and three sides you have glass windows.

MR. SCHROCK: And at these low temperatures you have no problems with glass windows?

MR. DHIR: No.

MR. SCHROCK: No itching of the windows?

MR. DHIR: No. You have -- still using a

-- not different, but rod bundle up to 3.5 baud.

This is -- you are seeing the best block.

MR. RANSOM: Just out of curiosity, why didn't they use a transition region --

MR. DHIR: What do you mean transition?

MR. RANSOM: Well, normally you'd use like triangular section or something to minimize the distortion, you know --

MR. DHIR: Right. But this length was sufficiently long enough. This region was sufficiently long enough to give us --

MR. RANSOM: Well, you're measuring the gradient through that nose more region, right?

MR. DHIR: Right, this region.

MR. RANSOM: Well, didn't you tell me the cartilage heaters extend all the way through the block?

MR. DHIR: They go up through here, up to this length.

MR. RANSOM: Yes, that's what I thought.

MR. DHIR: And beyond that, then the heat has to flow through this section.

CHAIRMAN WALLIS: It's not a very one dimensional looking -- when you think of solving the conduction equation. Maybe all the differences -- the differences are small anyone.

MR. DHIR: Very small difference. You'll see that in a minute.

And this is for the rod bundle. Rod bundle, the rod tubing is Zircalloy-4 and the rod is only is 1.1 centimeter, the sheet thickness is 1.5 millimeter. And we arrange in a 3 x 3 grid. A total of 140 subcooled flow boiling experiments were performed. These experiments have been performed at 1 bar, 2 and 3 bar. And in the 1 bar case, we varied the marked velocity from 186 to 2800 which is 20 centimeters to 2.8 meters per second. So quite large range.

Subcooling from 2.7 to 69 degrees at inlet. Heat flux 1.6 to 25 bar per centimeters. Current contact angle was about 57, hydraulic -- was 1.23

CHAIRMAN WALLIS: You say P was 1.03 just because of the elevation of your lab or something?

MR. DHIR: Elevation of the lab and the pump is in -- you know, the well was there, so there's--

CHAIRMAN WALLIS: Is that sort of pressure?

MR. DHIR: Yes.

CHAIRMAN WALLIS: Because atmospheric pressure varies by ten percent or something anyway.

MR. DHIR: Right.

CHAIRMAN WALLIS: So did you adjust for variations in atmospheric pressure?

MR. DHIR: No. This is the pressure --

CHAIRMAN WALLIS: Oh, so this gauge pressure?

MR. DHIR: No, no, no. Absolute pressure, but the pressure constance is calibrated.

CHAIRMAN WALLIS: Absolute pressure?

MR. DHIR: Right.

CHAIRMAN WALLIS: And you adjust something to make it 103 when the outside pressure is 105?

MR. DHIR: We not have 105. I don't think we got 105.

CHAIRMAN WALLIS: You don't?

MR. DHIR: I don't think so. Usually it's below what it must be at most of the time. We are close to the ocean.

CHAIRMAN WALLIS: Yes, but the barometric pressure when storms come by varies by plus or minus 5 percent.

MR. DHIR: Right. Most cases it was 1.03 but again we can -- this is a trivial item.

MR. SCHROCK: Not on experimental days.

MR. DHIR: No, this is the measurement from the pressure -- I'm giving you 1.03, not all experiments would have 1.03.

CHAIRMAN WALLIS: Oh, okay. Okay.

MR. DHIR: So it's not --

CHAIRMAN WALLIS: It's a nominal pressure.

MR. DHIR: Right, nominal.

And this is how the rod bundle looks like. And I thought the rod bundle is 91 centimeters. Again, we have a -- flow welding section and this is the photograph, you can see how it looks like. And all four sides of the -- are glass windows.

CHAIRMAN WALLIS: It's prestressed. It looks like a prestressed thing with tables holding it together.

MR. DHIR: Right.

MR. DHIR: Yes, you have pins loaded actually, pins holding it together.

CHAIRMAN WALLIS: Bungee cords.

MR. DHIR: After you heat there is some expansion.

MR. KRESS: Where does the power come in at? Through the bottom?

MR. DHIR: Through the top and bottom.

MR. KRESS: Through the top and the bottom.

MR. DHIR: And in the rod bundle we have placed thermocouples about 18 centimeters apart and is about 6, 7 locations. And the filler material we used in two bundles. First one was lava and the second one we used G-10 insert and there was a slot cut in the G-10 on lava and the thermocouples were carried through to the slots. And this was filled with high thermoconductivity poxy and kind of pushed it to the surface.

MR. RANSOM: Were these fresh ZR-4 tubes?

MR. DHIR: ZR-4.

MR. RANSOM: You expect a high oxidation level to change the nucleation density or --

MR. DHIR: We had no change. But in -- water we did not see degradation. Copper it's much more.

MR. RANSOM: I was concerned about the reactor case where you probably have a high --

MR. DHIR: I'll show you something with boron.

MR. RANSOM: Oh, okay.

MR. DHIR: Boron does more than just --

MR. RANSOM: V.J., what is lava? Is that

the lava I know or --

MR. DHIR: Yes, that's the lava.

MR. RANSOM: Is that lava rock?

MR. DHIR: Right. You can get lava rock pieces. And it's a --

MR. SCHROCK: Isn't that a commercial product you --

MR. DHIR: Yes.

MR. SCHROCK: -- you make. It's sort of a ceramic after you cook it.

MR. DHIR: Cook it, right. A similar tool, you know.

MR. SCHROCK: But it's machineable.

MR. DHIR: Yes. You can get rod bundles and rods --

MR. RANSOM: A manmade product --

MR. DHIR: Yes. Okay. First with the flat plate, we looked at how uniform the heat flux was along the axial direction. And, as you can see, for this case about 42 -- this is at those several locations how the heat flux varied on the thermocouple.

CHAIRMAN WALLIS: Oh, you did the measures across the plane?

MR. DHIR: Across the plane. I don't have it here, but does not vary more than 5 percent. There was some drop on the edges, but it's fairly uniform.

CHAIRMAN WALLIS: Heat loss?

MR. DHIR: Heat loss, although it's insulated, but still there is a heat loss.

CHAIRMAN WALLIS: You think it'd be higher at the edges.

MR. DHIR: No, no. Temperature drops but the heat flux is higher. So if you look at the temperature uniform to along the surface, the temperature is lower on the outer side.

This is the wall temperature as a function of distance for one case. And you can see initially it's a subcooled flow, forced convection and then boiling starts somewhere around here. There's some temperature drop and then it stays fairly constant flow.

This is the temperature profile in the liquid with the thermocouple which -- outward. And most of the drop occurs very close to the wall, the laminar sublayer. And as we go farther downstream and the outer region of the thermal layer expands and you can see the thermal layer becomes quite thick.

CHAIRMAN WALLIS: The top one of this with the pinky triangle there.

MR. DHIR: 24?

CHAIRMAN WALLIS: What TRAC is doing is taking some average which you would say would be 88 or something, or is TRAC taking 85, or what does TRAC take as the temperature of the liquid.

MR. DHIR: Bulk 35.

CHAIRMAN WALLIS: No, that's not. The average is 87 or something.

MR. DHIR: Average if you look at over the whole cross section is close to 85.

CHAIRMAN WALLIS: Isn't that what TRAC uses, the average over the whole cross section?

MR. BAJOREK: It would know the 85 degrees in this point.

CHAIRMAN WALLIS: It wouldn't know that -- no, 85 at all. It would just know the average. TRAC doesn't calculate the peak or minimum. It just takes the average.

MR. RANSOM: Yes, it's the bulk --

MR. DHIR: The -- it would be 85.

CHAIRMAN WALLIS: No. The average is above 85.

MR. RANSOM: Well, it's got to increase as you flow down the --

MR. DHIR: Right. But how much -- you

know, if you integrate those over there, how much increase is going to --

CHAIRMAN WALLIS: But TRAC would say that the top, the average bulk temperature is 85, although there is water -- 85 --

MR. DHIR: Higher than --

CHAIRMAN WALLIS: Say the bulk temperature was 87. Now there is water 85 in the middle.

MR. BAJOREK: Yes, but most heat transfer correlations are using that bulk temperature, not an average temperature.

CHAIRMAN WALLIS: That's right. So it -- what's the difference? What's the difference?

MR. BAJOREK: Barring what little bit you have in the boundary layer.

CHAIRMAN WALLIS: What do you mean by bulk?

You mean -- is wider than that.

MR. DHIR: No, channel is -- channel is the 4 centimeters wide.

CHAIRMAN WALLIS: So the bulk temperature is something like 87 when you average over the whole thing. It's not 85, although there is liquid at 85 in the middle.

MR. DHIR: Right.

CHAIRMAN WALLIS: When you're doing your condensation on these bubbles --

MR. DHIR: Right.

CHAIRMAN WALLIS: -- you're something average. If they go out in the middle, they disappear.

MR. DHIR: No. Bubbles were declining only up to about here.

CHAIRMAN WALLIS: Only up to about there?

MR. DHIR: Right. So I'm making the local--

CHAIRMAN WALLIS: What you see is the -- temperature than the bulk?

MR. DHIR: Right.

CHAIRMAN WALLIS: Of course, these transverse things are not modeled in TRAC at all.

MR. SCHROCK: Now, M.J., this acts as a single phase set of measurements.

MR. DHIR: This is single phase.

MR. SCHROCK: Yes. So there's no --

MR. DHIR: Right.

MR. SCHROCK: Once you start getting two phase, this gets all changed.

MR. DHIR: Changed, but still outer regions you'll have still some cooling.

MR. RANSOM: I think you had some arguments in the paper that it's still measuring the liquid temperature or approximately the liquid temperature?

MR. DHIR: Right.

MR. RANSOM: But you do use that data to establish the temperature of the liquid where the bubble is at, right?

MR. DHIR: Exactly. That's what we do. And the first the exercise we made it was kind of a test how good experiments you're doing, we calculated from the wall side and if we take this gradient does it match. And this gradient we have much more uncertainty because its profile is so steep. But we didn't -- about 20 percent it matched with what we were putting in from the other side.

This is how the heat transfer coefficient looks along the copper block -- single phase flow. So you can see it's just developing flow. It's like -- flat plate and the -- is decreasing. In this case the narrow number at the end about -- less than the what we need for transition -- to turbulent flow.

MR. RANSOM: What did you say the Reynolds number is?

MR. DHIR: Based on the length.

MR. RANSOM: Yes, is what?

MR. DHIR: It's about close to 10 above 5.

MR. RANSOM: Ten to the 5th, right?

MR. DHIR: Right.

MR. BANERJEE: Based on the length.

MR. DHIR: Length. Right.

MR. RANSOM: Distance. REL?

MR. BANERJEE: Right.

MR. RANSOM: Is that a curve a model?

MR. DHIR: No, just to the data.

MR. RANSOM: It would have been helpful to -- how did it compare with --

MR. DHIR: This is a laminar -- flow -- we can apply, but you see I don't have it here. I think that it is one of the reports that was discussed, what difference. But we find the -- value is about 20 percent higher than what you'll get for laminar flow.

MR. RANSOM: Which is 20 percent higher?

MR. DHIR: This value here.

MR. RANSOM: Is 20 percent higher than what we would --

MR. DHIR: Then you'll get for -- profile for example is you calculate what --

MR. BANERJEE: So you should be able to trace the --

MR. DHIR: Oh, yes, right. We did that. I think it's in one of the reports. But the key point here was it looked --

CHAIRMAN WALLIS: In a square geometry.

MR. DHIR: Right, so there will be some difference. And also we have not taken any precaution to make the flow laminar exchange, so there will be some difference.

And this is on the rod bundle, single phase heat transfer coefficient.

CHAIRMAN WALLIS: That's length the axis there?

MR. DHIR: Axis? Yes, it's not missing here. It's these, distance from inlet.

CHAIRMAN WALLIS: It's also showing a big entrance length effect.

MR. DHIR: Yes. You see almost 50 hydraulic damages.

CHAIRMAN WALLIS: So how come it correlates so well on the right hand side?

MR. DHIR: Just one thing. I'll come to that.

This is -- flow and you see out 50 -- damage it becomes almost fully developed and the colored symbols are for the central rod. And the open symbols are for the rod which is at the corner and facing -- thermocouple is facing outside, outward direction. So it's in the quadrant. And that thermocouple -- reduced from that thermocouple is about 15 to 20 percent lower than the central rod.

MR. SCHROCK: So would you say again whether those numbers are X over D or just X?

MR. DHIR: This is rod bundle, this is just Z centimeters.

MR. SCHROCK: Z?

MR. DHIR: Z, centimeters.

MR. BANERJEE: And the hydraulic damage is about

MR. DHIR: 1.2

MR. BANERJEE: So this is in turbulent zone?

MR. DHIR: Yes, 11,000 yes.

CHAIRMAN WALLIS: So it's developing pretty slowly?

MR. DHIR: Yes.

CHAIRMAN WALLIS: It is, yes.

MR. SCHROCK: It should go faster than that for Reynolds of 11,000 I think.

MR. DHIR: 11,000 I don't know.

CHAIRMAN WALLIS: Anyway, what's the right hand side?

MR. DHIR: Okay. And the right hand side is we take these values here and we develop a case and we are plotting them for the Weisman number based on this fully heat transfer coefficient normalized the -- number to the .4 power we are plotting against -- to the .8 power. And these are all of our data covering a range of 8,000 to about 95,000

MR. RANSOM: Just a little bit of clarification. The heater rods continue on in unheated party? I mean --

MR. DHIR: No.

MR. RANSOM: What is the zero to 100, that's only the heated section?

MR. DHIR: Heated section ends here.

MR. RANSOM: And where does it begin?

MR. DHIR: Middle.

MR. RANSOM: And the leads and the other parts of the rods --

MR. DHIR: They're still longer -- so much longer, but our measurement start at --

MR. RANSOM: Okay. But what I'm wondering about, where does the viscous layer begin? You know, you have a viscous boundary layer and you have a thermal boundary layer.

MR. DHIR: Right.

MR. RANSOM: Obviously the thermal boundary layer begins at zero in this case at the heated point.

MR. DHIR: Right. Right.

MR. RANSOM: But presumably you must have a fully developed viscous layer.

MR. DHIR: No I've seen the other kind of grid to which these rods were sitting. So the floor is coming through holes.

MR. RANSOM: Through spaces?

MR. DHIR: Kind of spaces, but it's like a grid plate where the rod was sitting in.

MR. RANSOM: Where do the grid space start then?

MR. DHIR: Just above zero. You know, they're just sitting here.

MR. RANSOM: So you mean the flow comes in like jets?

MR. DHIR: That's right. The flow is coming in like through those holes, passages.

MR. RANSOM: So it's not a fully developed turbulent situation at the beginning of the heated section?

MR. DHIR: It's not fully developed flow, yes.

MR. RANSOM: It may be one reason the heat transfer coefficient is bigger at the beginning. It actually generates some extra -- with these jets.

MR. DHIR: Possible.

So what we find is the fully developed values for this range of -- number are about 16 percent higher than Dittus Boelter correlation. And the chain line is the Weisman correlation for a square grid type of arrangement and the rod bundle -- somewhat smaller than what we have, and his real number range was on the higher end. It was about from 30,000 to about 700,000 and yet on the low end about 8,000 to 95,000 but we are predicting lower heat transfers then will be predicted from Weisman's correlation.

MR. RANSOM: I'm back to Professor Sanjoy Banerjee's question, why do they agree down at the lower end, because they seem to be in quite a bit of disagreement, at least that is bolder on the left hand one, and yet the low Reynolds number on the right hand plot they seem to be in quite good agreement.

CHAIRMAN WALLIS: Well, it's actually quite close because there's a false origin on the left hand side.

MR. DHIR: Right.

MR. RANSOM: It's what?

CHAIRMAN WALLIS: It's a false origin. You know 2500 is the base and the left hand side the percent disagreement is not very big. Because go all the way down to zero.

MR. DHIR: Because zero is --

MR. SCHROCK: They're everywhere within 30 percent.

CHAIRMAN WALLIS: Anyway, the interesting problem is the two phrase, isn't it?

MR. DHIR: Right. But at least it gives you -- this range was not available in the literature and there's some interest in that range.

CHAIRMAN WALLIS: All right.

MR. DHIR: Because of application to -- and then we go to ONB. So now these are not -- if you see this 4, it's a 4 item which I showed you on the table, what phenomena we were trying to model or understand or measure and what the instrumentation was.

MR. BANERJEE: Claussius Clapeyron.

MR. DHIR: Yes.

MR. KRESS: I had a question about this, V.J.

MR. DHIR: Yes.

MR. KRESS: It seemed to me like that psia ought to be proper from the Claussius Clapeyron equation, the one on the right hand side of the first equation.

MR. DHIR: They put it at saturation temperature corresponding to the pressure in the vapor.

CHAIRMAN WALLIS: Which is the same as --

MR. DHIR: No. Liquid pressure is less. So the liquid is at saturation temperature. Pressure in the vapor bubble is higher than the pressure outside. And the temperature of the vapor has to be cooled to or at least the saturation temperature cause under the pressure in the bubble. So the temperature of the vapor is higher than the liquid, and that's the reasoning you made that the vapor bubble has to be surrounded by superheated liquid for it to go.

MR. SCHROCK: But the difference is small through bubbles of this size.

MR. DHIR: What size?

MR. SCHROCK: The average size they have when they're detached from the wall. There's not much--

MR. DHIR: Right.

MR. SCHROCK: Not much delta P involved once they've detached.

MR. DHIR: Well, how did we jump to detached bubbles. First I'm talking about onset of nucleate boiling.

MR. SCHROCK: Oh, yes. You're right. You're right. I'm not paying attention.

MR. DHIR: So we're talking about bubbles forming on the cavities --

MR. SCHROCK: Right, right.

MR. DHIR: -- which is a very, very small number.

MR. BANERJEE: So this is really right in the cavities?

MR. DHIR: Cavities, right. You know, as I said, initially we want to predict where the boiling starts. And if you're not going to predict that right and then you keep on adding the arrows you move downstream. So there are a number of correlations models that have been proposed since '60s. You all know them, know about them, I don't need to repeat them. But basically HSU it was proposed in '62, very simply matched or said the minimum superheat will be the case when temperature profile in the liquid is simply tangent to the superheat you need to -- because of the evaporation in the bubble.

And then Bergles & Rohsenow, Rohsenow followed that same idea, but they placed the properties of the fluid in terms of the pressure. And Sato and Matsumara again did a similar thing. Davis and Anderson added a constant C_1 to account for different contact angles.

MR. SCHROCK: Actually, the vapor inside the bubble is slightly superheated, but it's negligible importance.

MR. DHIR: Yes.

MR. SCHROCK: Rohsenow worked that out from a free energy argument years ago. And there's a physical model --

CHAIRMAN WALLIS: I think Maxwell did it, somebody like that.

MR. DHIR: It's higher than --

MR. SCHROCK: It's Helmholtz.

CHAIRMAN WALLIS: Helmholtz, somebody.

MR. DHIR: Right.

MR. SCHROCK: On a physical model.

MR. DHIR: Yes, Kandlikar.

And see those kind of correlations are modeled work when the liquids are partially breaking the surface. And Hahne, Spindler and Shen in 1990 looked at freons which -- the surface well and they found those correlations really under predicted the wall superheat at nucleation. And from experiments they found that -- could be written empirically like this, and if you use that corrective style you can calculate the 1B superheat and then you substitute that into the -- balance and this is a superheat and the liquid subcooling is there, multiple by heat transfer coefficient, that gives the heat flux and ONB. So you get delta to ONB and heat flux ONB both.

This is what -- see how our data looks like on the flat plate for different heat fluxs. This is the cover I show you earlier. This is a single phase heat transfer. And these are the plots we see as increase --

CHAIRMAN WALLIS: I didn't understand your little vertical things where you said this is where the two meet. I couldn't see. Either the numbers in the text don't agree with the position of those little vertical lines, and also I don't understand how they related to the curves.

MR. DHIR: Okay. Let me explain what we're blocking here.

Let's take this curve. Higher heat flux, okay. The heat transfer coefficient, it decreases and then increases and increases and then finally becomes almost constant. So we are plotting here is where we saw ONB and bubbles start to form on the surface. Visually and by noting the minimum in the curve.

CHAIRMAN WALLIS: Oh, the minimum.

MR. DHIR: Right.

CHAIRMAN WALLIS: I thought you said it was where there was departure from single phase, which would have put it way over to the left.

MR. DHIR: Right. That will be left. But minimum --

CHAIRMAN WALLIS: When you say minimum in the text, it says --

MR. DHIR: Minimum is the heat transfer improves after the bubbles start to form.

CHAIRMAN WALLIS: But it's really where it departs from single phase that's something's happened.

MR. DHIR: Right, but single phase it departed, visually we see single phase, and this is the two phased region.

CHAIRMAN WALLIS: I didn't understand this minimum idea at all. Because I was looking for where the one curve leaves the other one, which is actually further to the left. Okay. Now I think I understand it. No, I understand what you've done with the minimum part.

MR. DHIR: Right. But these are different heat flux curves.

CHAIRMAN WALLIS: Yes, but minimum wasn't clear to me that minimum meant anything.

MR. DHIR: What we find, if I set the heat flux and measure the heat transfer coefficient along the length of the plate, you will see heat transfer coefficient would decrease, a minimum value and then increase.

CHAIRMAN WALLIS: Okay.

MR. DHIR: And the minimum point almost coincided where the nucleation starts.

CHAIRMAN WALLIS: Well, why does it leave the single phase flow curve before that?

MR. DHIR: What do you mean before that?

CHAIRMAN WALLIS: Well, you dashed -- your colored red curves leaves the black curve way up at the left hand corner of the picture.

MR. DHIR: This one?

CHAIRMAN WALLIS: No, the curve.

MR. DHIR: This curve? This is single phase.

CHAIRMAN WALLIS: The red curve leaves the black curve at a point which is almost on the left hand corner up there at 6,000.

MR. DHIR: Right.

CHAIRMAN WALLIS: So really you shouldn't--

MR. DHIR: All these points should be one point.

CHAIRMAN WALLIS: No, no, the right curve. You put a curve in there. You've only really got red points, right? If you'd shown a straight line through the red points hitting the black line, then I would have believed something. But you fared in a curve. And the red curve leaves the black curve at about 2 in terms of Z. You see what I mean?

MR. DHIR: Right. This -- okay. The key point you want to make here is that you increase the heat flux, you leave this curve either later or earlier.

CHAIRMAN WALLIS: Come down the black curve.

MR. DHIR: Okay.

CHAIRMAN WALLIS: And then when you get into the right curve -- you're on the Metro, right?

MR. SCHROCK: Right.

CHAIRMAN WALLIS: When do you get onto the red curve?

MR. DHIR: That's true.

CHAIRMAN WALLIS: When does your finger go from the black curve to the red curve?

MR. DHIR: Okay. The question is here this represents some of uncertainty of measurement for each experiment. Actually this should be one curve.

MR. BANERJEE: Yes, the heat flux shouldn't have an effect.

MR. DHIR: Effect on the single phase heat transfer coefficient.

CHAIRMAN WALLIS: Okay. But you come down the black curve.

MR. DHIR: Yes.

CHAIRMAN WALLIS: Now put your finger on the black curve.

MR. DHIR: Right.

CHAIRMAN WALLIS: Come down there -- don't -- don't go so far.

MR. DHIR: Okay.

CHAIRMAN WALLIS: Now go up some more.

MR. DHIR: Okay.

CHAIRMAN WALLIS: Go up some more. Go up.

MR. DHIR: Okay.

CHAIRMAN WALLIS: And you still haven't met where the red line comes in.

MR. DHIR: Right.

CHAIRMAN WALLIS: The right line comes in up there somewhere.

MR. DHIR: This red line and this solid should be the same curve.

CHAIRMAN WALLIS: Well, then you should draw them the same.

MR. DHIR: Again, I have my data. The data says each time I take the experiment I have data difference. I'm not treating it.

MR. BANERJEE: --down there which is much closer.

MR. DHIR: Right. So that's what I'm saying --

MR. BANERJEE: So why do you draw it --

MR. DHIR: Oh, you could draw it here and then --

CHAIRMAN WALLIS: Then it would be clear where it left. You see, the thing is where do you leave one and hit the other? And you curved it like that, it looks as if it leaves at 6,000.

MR. BANERJEE: But there's no logic for you to miss that red point.

MR. DHIR: Again, it's just the line fell through the data. It's not specific. The key question is the idea we should be talking about does the minimum represent onset of nucleate boiling.

CHAIRMAN WALLIS: You see your text doesn't say anything about minimum. Your text says where the volume departs from the other line, and that is way up where I was trying to get you to go, and that's very misleading to the reader.

MR. SCHROCK: What is that inset ONB location, what's that legend mean?

MR. DHIR: ONB location, this is visual observation. Visually we see on the -- on the plate, you know the location --

MR. SCHROCK: So it's a vertical line not a horizontal line, but there's a solid one and a dashed one.

MR. BANERJEE: The first one is the minimum.

MR. DHIR: Dashed one represents the minimum, the heat transfer coefficient curve.

CHAIRMAN WALLIS: And the minimum should be ONB. But, anyway, we should probably move on. It's confusing to the reader when he sees these things.

MR. RANSOM: I can't even tell which one's dashed and which one's not because the dash is much different.

CHAIRMAN WALLIS: Which goes with which curve is also a question.

MR. DHIR: No, all these four curves should be one.

CHAIRMAN WALLIS: Up to some point

MR. DHIR: Up to some point. Because then they should diverge on each case.

CHAIRMAN WALLIS: Right.

MR. DHIR: But we are being honest to our data that what -- I plotted. So I could have plotted an average of these and then draw it, and then it will be clear, there'll be -- in that main curve.

MR. SCHROCK: So the visual and the observation from the HZ plot are sometimes one way in relationship, sometimes the other way.

MR. DHIR: Right. Because thermocouples are made indiscreetly so you're not exactly locating that -- from the plot you cannot exactly locate that.

MR. SCHROCK: Okay.

MR. KRESS: For the blue line the visual and the HZ are on top of each other?

MR. RANSOM: There's only one vertical line on that one.

MR. KRESS: Yes, it must be on top.

CHAIRMAN WALLIS: Anyway, you just have to clarify it.

MR. DHIR: Right.

MR. BANERJEE: What is the other one?

MR. DHIR: What? The other one is simply locating where the ONB occurs, how it's influenced by flow velocity and there are two different flow velocities, liquid subcooling and contact angle. Because the boiling has shifted. Superheat has shifted depending on the contact angle or your flow boiling -- flow velocity.

So basically this plot is telling you that if ONB to occur at this location, then delta $T_{W,ONB}$ would be higher if I have a high flow rate.

MR. SCHROCK: When they use this in their interpretation in the code evaluation, they're going to have to do something about the contact angle as a function of pressure. Are you providing those data for them or --

MR. DHIR: No, not as yet. Not as yet.

MR. SCHROCK: Has that come up in your discussion with the sponsor?

MR. DHIR: No.

CHAIRMAN WALLIS: It's a function --

MR. SCHROCK: But you agree, it depends on surface tension, which depends on temperature --

MR. DHIR: Surface tension is one really. It depends, you know, solid liquid also surface tension.

CHAIRMAN WALLIS: It depends on the age of the fuel.

MR. SCHROCK: It depends on a lot of stuff.

MR. DHIR: Lots of stuff.

MR. BANERJEE: So the contact angle has a larger effect than the flow velocity?

MR. DHIR: On what?

MR. BANERJEE: On the ONB.

MR. DHIR: That's true. In this situation you can see onset of nucleate boiling contact angle has more effect.

MR. BANERJEE: Lots more. I mean, the major effect.

MR. DHIR: Right.

MR. SCHROCK: See, at the outset I was a little puzzled by why the emphasis on low pressure for your experiment. In the TRAC code it seems tome they need the ability to solve this problem for any pressure that may occur during an accident transient or transient of any kind. And that would seem to cover a substantial range of pressure, not just low pressure.

MR. DHIR: Right.

MR. BANERJEE: But during the AP600 runs the low pressure behavior in the subcooled region, at least of RELAP-5 was --

MR. SCHROCK: That's what led them to think they had a problem.

MR. BANERJEE: Yes.

MR. DHIR: Right.

MR. BANERJEE: That's really what happened historically. We had a lot of trouble trying to interpret this result. Whether they are true or not, we don't know. Because maybe the -- for all we know. But that was the reason.

MR. DHIR: But at that time thinking was that these codes do well for at high pressures, but not at a low pressure.

CHAIRMAN WALLIS: We've got to five slides out of 83 in about --

MR. SCHROCK: How do we know? I mean, you've identified what the real dependence is here. And it's contact angle.

MR. DHIR: Right, it's quite important.

CHAIRMAN WALLIS: A elusive problem.

MR. SCHROCK: That means the code needs good contact angle information, and I don't think it has it.

MR. DHIR: You know, at least it's 30 points of whatever the key variable you need to know.

MR. SCHROCK: Right.

MR. DHIR: And then we come back and say rather what we want to do in the code.

MR. SCHROCK: Yes.

MR. BANERJEE: How did you vary the contact angle?

MR. DHIR: How did I vary the contact angle? For copper block we polished the surface and when you polish the copper block and use water, you get contact angle close to 90 degree.

Then we follow the standardized procedure where we put the copper block in the air and heat it so it gets oxidized. But controlling the oxidization, we can change the contact angle. And so we went down to about 30 degrees.

And generally when you like polished copper, after you're done with the experiment contact angle changes. And then we had to have some sort of an average for that run. But when you're at 30 degrees still same, for example, after you are done --

MR. SCHROCK: And for your Zircalloy you did it on a piece of flat Zircalloy with drops --

MR. DHIR: Right. Flat. And then we also put on the rod very small droplet and see what the contact angle was. And as you'll see in there, the contact angle is given for the --

MR. BANERJEE: 37 or something.

MR. DHIR: 57.

MR. BANERJEE: 57.

MR. SCHROCK: It's hard to measure well with little drops.

MR. DHIR: With the plates, yes, you can do that. Yes. But, again, the question is are you -- you know, we can spend all of our time on contact angles.

CHAIRMAN WALLIS: With little drops because the contact angle then varies over the surface?

MR. SCHROCK: Well, it's just very hard to see where the contact line is.

MR. DHIR: Again, then you can get in discussion of whether it's microscopic contact --

MR. SCHROCK: And you measure the --

MR. DHIR: So, but at least it gives you a perimeter which you can measure.

MR. SCHROCK: Yes.

CHAIRMAN WALLIS: That's an elusive perimeter. I remember going back to Burnstein's experiments and change the surface and a lot of things change.

MR. DHIR: But the question you ask yourself if it's elusive perimeter, it is an important perimeter. If it's not important, let's forget about it.

CHAIRMAN WALLIS: But it is important.

MR. DHIR: That's what I'm saying.

MR. BANERJEE: Well, your experiments show that it's --

MR. DHIR: It's extremely important.

MR. BANERJEE: -- the main perimeter.

MR. DHIR: That's right.

MR. BANERJEE: One of them.

MR. DHIR: In the past we have -- you know, somehow ignored it whenever the problem occurred or it's elusive problem and let's --

MR. SCHROCK: Because it's hard to measure and it's inconsistent.

MR. DHIR: Right.

MR. SCHROCK: We'd never get agreement.

MR. DHIR: But if you think of, again --

MR. SCHROCK: Subtle operational conditions.

MR. DHIR: That's true. But let's say I give you 57, you may come out at 52 or somebody comes with 62, it's not going to be 5 degrees.

MR. SCHROCK: Yes. That's right.

MR. DHIR: So that's what we should shoot for.

MR. SCHROCK: But the difference between 60 and 30 is big.

CHAIRMAN WALLIS: We need to move on.

MR. DHIR: So we have proposed our own correlation for predicting the onset of nucleate boiling. What we are saying is that the corrected size of the cavity is given by -- through the analysis, but the probability of finding this cavity diminishes as the contact angle decreases. This cavity may not be available to nucleate as at first it becomes more ready. So that's the --and this function F which corrects the cavity size we get empirically by correlating all the data that is available in the literature. And varying from the contact angle of one degree to almost 90 degree.

MR. BANERJEE: What is phi?

MR. DHIR: Phi contact angle.

MR. KRESS: And how are they supposed to know what that is in the code?

MR. DHIR: Code? If they're working with Zircalloy -- what's stated is they would know about 57 degrees.

MR. KRESS: That's the unoxidized state?

MR. DHIR: Right.

MR. KRESS: Okay.

MR. DHIR: So that's all I can say at the moment. But with the lowest thing it does effect -- it does depend on pressure as well.

CHAIRMAN WALLIS: -- contact angle is in the cavity, isn't this different? How do you measure that? It's not the same as on the surface.

MR. DHIR: I don't know. That's the proposal I have for research.

CHAIRMAN WALLIS: The cavities may be there because they are different.

MR. DHIR: Yes. We have looked at again, now we're going back to study. We did about 12 -- 12, 15 years ago and we looked at the shape of the cavities microscopically. And then for my polishing the surface and see what kind of cavity it really nucleated. And we were able to relate the trapment of gas in the cavity to a contact angle.

CHAIRMAN WALLIS: What I'm saying is suppose you have a clean surface, a clean line.

MR. DHIR: Yes.

CHAIRMAN WALLIS: And after a while it may develop cavities because of erosion and corrosion phenomenon.

MR. DHIR: Yes.

CHAIRMAN WALLIS: And because of this corrosive phenomenon what's in the cavity isn't the same as what's on the surface.

MR. DHIR: Yes.

CHAIRMAN WALLIS: And therefore what contact --

MR. DHIR: That's possible, yes. But again, that's possible but it's going to be -- first order of effect to second order effect.

MR. BANERJEE: I mean, BWI is adding zinc and its cobalt and all sorts of stuff was on the --

MR. DHIR: You know, again, we just did this -- now I'm jumping, but maybe -- Zircalloy, fresh Zircalloy and water we found contact angle was about 57. Then we did some experiments with boran and water. We put 7,000 ppm of boron in the water. And that water we used to measure the contact angle. It was about the same. Because the number's different with boron. And then we also, you know as you run that experiment, boron deposits from this cladding. And then we measured the contact angle again, it was not much different than 57. So that is what evidence we have. However, nucleation sites and then boron crust was on the surface, but much more, because you formed the porous structure --

CHAIRMAN WALLIS: Well, close around --

MR. DHIR: Right. So that increase the number.

CHAIRMAN WALLIS: You distill. You distill the water away and leave the crud behind.

MR. DHIR: Right. And then that crud is porous.

CHAIRMAN WALLIS: Yes.

MR. DHIR: And you form -- there. And in fact we found -- I'm giving -- with boron your nucleate boiling heat transfer was higher but single phase heat transfer was lower after the crud was formed.

So this is all of the data we have and which we put together in the literature. And you can see data varies from contact angle of 1 degree FC72 to about 90 degrees with copper, maybe 35 degrees with copper.

CHAIRMAN WALLIS: What's the pressure range?

MR. DHIR: Most of the data it's at -- of pressure but some is at high pressures as well. There the contact angle was given. I'll show you later on. I think it's -- it's Bergles & Rohsenow high pressure data.

MR. BANERJEE: And this is for flat plates, or does it also have rod bundles?

MR. DHIR: Rod bundles are there. It's the 57 8 points 2 bar, 6 points at 3 bar and about 7 points of water. With boron and about 19 points with rod bundle. So you have several data points.

MR. SCHROCK: These are all calculated from the equation on 34, the previous page?

MR. DHIR: The previous page, right.

CHAIRMAN WALLIS: This all assumes you have enough nucleation sites that you're not limited by the numbers, by Hsu criteria.

MR. DHIR: Hsu criteria gives us the -- sites. Then we are saying.

CHAIRMAN WALLIS: If they don't exist, then you won't get --

MR. DHIR: That's what -- nonexistence we call the F function. That's what accounts for nonexistence.

MR. SCHROCK: There's a note on that slide that says for small superheats. What's the one --

MR. DHIR: Oh, because we expanded the pressure difference to saturation temperature difference due to Clausius Clapeyron. And if you go to high superheats that doesn't work, you know. You can't expand like that.

MR. DHIR: Because of the Clausius Clapeyron?

MR. DHIR: Clausius Clapeyron, right.

MR. BANERJEE: But you could use it on Hahne's --

MR. DHIR: Right. On Hahne's equation you can use it. Go to steam table --

CHAIRMAN WALLIS: If you had a surface with no cavities in it and everything would be different?

MR. DHIR: Right.

CHAIRMAN WALLIS: Like boiling on mercury or something?

MR. DHIR: Mercury or, you know, again, but in the limit if you say contact angle goes to zero --

CHAIRMAN WALLIS: Well, no, just contact

angle. It's also a crunch of -- you have to have enough cavities for this --

MR. DHIR: That's true. You know, it's like glass. You can have one crystal, you won't get anything.

MR. KRESS: Why does the Hayes's alloy look different than all the others. It's those little crosses up to the top.

MR. DHIR: Cross, yes. That's the -- this that R11 which --

MR. KRESS: Freon.

MR. DHIR: Freon. Freon, and you know that was the number we got, but then we don't know what the reason is. Somebody's -- let's put all the data we have.

MR. SCHROCK: What is the largest superheat you had in your test tube?

MR. DHIR: My test the highest is about 15 degrees C.

MR. SCHROCK: 15C?

MR. DHIR: 15 to 20C, yes. That's how high we have gone. But there are others who have gone quite high. See, our data is mostly here, you can see. ONB's low superheat, but doing the experiments we have gone higher.

MR. SCHROCK: Okay.

CHAIRMAN WALLIS: Okay. The next one.

MR. DHIR: Okay. So we not able to only put it to ONB -- but also Q_{ONB} because it's a no single phase heat transfer coefficient --

CHAIRMAN WALLIS: If you don't put the -- effect in you get far more scatter?

MR. DHIR: That's right. But here you can see Q_{OMB}, we have gone about 4 orders of magnitude. Okay. This has all the high pressure data as well.

MR. BANERJEE: Which is the high pressure data.

MR. DHIR: That is this one, water and nickel. That -- high heat influx.

MR. KRESS: How is the contact angle determined in all these experiments?

MR. DHIR: Some are reported, but we have measured this to a droplet. We place a microdroplet --

MR. KRESS: Drop a droplet on there and as it --

MR. DHIR: Then take a photograph.

MR. KRESS: Okay.

MR. SCHROCK: Graham, how did you see the contact angle effect on his graphs? I didn't understand your point.

CHAIRMAN WALLIS: About what?

MR. BANERJEE: Well, it's collapsed through the --

CHAIRMAN WALLIS: I assume that if you don't put contact angle in -- they have a formula for using contact angle, f equals minus 6. But if you simply puts f equals 1 you get presumably much more scatter. It will be useful to show that, I think.

MR. DHIR: You should show that.

MR. BANERJEE: What happens if you put f--

MR. DHIR: You'll certainly underput.

MR. BANERJEE: Yes. If you take the exponential term out --

MR. DHIR: Right.

MR. BANERJEE: Phi is in radiance, right?

MR. DHIR: Right.

CHAIRMAN WALLIS: How much does f vary?

MR. DHIR: Oh, varies quite a bit. I'll show you next slide.

CHAIRMAN WALLIS: How much is quite a bit?

MR. DHIR: How you can go to -- you can go to as close as zero.

CHAIRMAN WALLIS: You said it varies. Does it vary --

MR. DHIR: Close to zero.

CHAIRMAN WALLIS: No, but isn't it from 4 degrees to 70 degrees?

MR. DHIR: Let me show you next group.

MR. BANERJEE: Well, if 5 goes to zero, then f is equal to zero?

MR. DHIR: Right.

CHAIRMAN WALLIS: But it is off the graph?

MR. DHIR: Right. You got to -- nucleation temperature, that's what we say.

This is one example how that f does it. They assume the correct size is 5 micron and then you see how -- when we would change. If I just kept 1, ONB would be only what you have here. Delta T_{ONB} over delta be homogeneous nucleus and we're plotting here. And you're close to .03 or .02. And because of the f function and eventually when this goes to zero, you're going to homogeneous nucleation temperature. That's how we effect the whole curve.

MR. BANERJEE: How well does Davis and Anderson correlation true?

MR. DHIR: It doesn't -- you know, if we account for their pressure -- it works okay. Not Davis and Anderson, but that was --

CHAIRMAN WALLIS: What happens if you use pi?

MR. DHIR: Beg pardon? Just one second.

CHAIRMAN WALLIS: You won't wet the surface at all?

MR. DHIR: Your question was -- Davis and Anderson doesn't do it that well with just pi.

Yes, what was the question?

CHAIRMAN WALLIS: If pi is pie, the liquid doesn't wet the surface at all?

MR. DHIR: Right. We have gone up to 9 degree even, now you can go on further. You go to zero.

CHAIRMAN WALLIS: Well, it goes to 1 and your correlation doesn't go to zero.

MR. DHIR: Right. One is just normalized. You know, with homogeneous nucleation. So your cluster size would determine.

MR. RANSOM: Isn't value T home?

MR. DHIR: Homogeneous nucleation.

MR. RANSOM: Oh, homogeneous nucleation.

CHAIRMAN WALLIS: So the water boiling on mercury or the contact angle is pi? It also has no nucleation centers.

MR. DHIR: Right. Right.

CHAIRMAN WALLIS: All right. We need to move along then.

MR. DHIR: Okay. Then we look at nucleation site density.

MR. KRESS: Now I want you to pronounce that. I want you to pronounce that name.

MR. DHIR: I can pronounce it. Kocamustafaogullari.

CHAIRMAN WALLIS: Yes, it's easy. Can you do it?

MR. KRESS: No.

MR. DHIR: Okay. Oh, maybe 18, 20 years ago these guys predicted that nucleation site density would correlate like this or the number density was normalized -- which was taken from FRIGG correlation. And now you can see FRIGG correlation is for pool boiling, not for flow boiling and the characteristic site would differ anyway.

So that is their model.

Wang at UCLA did theirs about 7, 8 years ago. We looked at pool boiling and we came up with number density like this. It depends on contact angle. And again, contact angle was very important variable, that's what we found.

CHAIRMAN WALLIS: What is D_c?

MR. DHIR: D_c is the captured --

CHAIRMAN WALLIS: Oh, that one. Okay.

MR. DHIR: And it's mostly proportionate to superheat.

CHAIRMAN WALLIS: It's surface perimeter.

MR. DHIR: And so the superheat, you see the power is 6 and very highly nonlinear. If you're now going to put it nucleation site, then see how do you hope to predict heat flux.

CHAIRMAN WALLIS: This is a very dimensional correlation.

MR. DHIR: Yes, it is dimensional. The cavity sizes and micron, and so forth.

So this is a picture we see, same surface, copper and two different contact angles. Same superheat. And left hand side 30 degree contact angles, right inside is 90 degrees.

CHAIRMAN WALLIS: Same history, too.

MR. KRESS: How did you vary the contact angle on it?

MR. DHIR: We discussed earlier, but what we have is the copper surface, we oxidize it.

MR. KRESS: You oxidize. Okay.

MR. BANERJEE: But nucleation site then simply is changed, too.

MR. DHIR: Yes, that's what I'm saying. Site density is strongly dependent on contact angle.

CHAIRMAN WALLIS: This is with all the cavities active because you know you can -- you know, snuff them out by boiling and then cooling down and pushing the liquid into the holes.

MR. DHIR: Yes. But when you go to higher superheat that effect is gone. But clearly you can see the difference. You know, I have refuted this with different number of students. So I may -- personally to ask them, give them a test score and then do it. And every time we see this --

CHAIRMAN WALLIS: You can do this snuffing experiment easily in there.

MR. DHIR: Yes, you can do it.

CHAIRMAN WALLIS: But there's no gas involved in the cavities. It's all just pure liquid?

MR. DHIR: There is always some trapped gas to start with, yes. And you can -- play games like that, you can have the cavities, then pressurize it or some you could subcool and then kill them. They will not come --

CHAIRMAN WALLIS: Right.

MR. SCHROCK: These equation that's per square centimeter from your graphs --

MR. DHIR: Right.

MR. SCHROCK: Per square centimeter.

MR. DHIR: In the literature you will see there are lots of -- and information with respect to flow boiling, especially in nucleation site density. Many people believe that all nucleation site density is effected by flow rate, it's effected by subcooling. But we find none of those effect it. The key variable is while superheat then contact angle. Okay.

And these are the data you see for our two flow velocities and fixed contact angle. And I can see there's hardly any effect of flow velocity.

MR. BANERJEE: And you just counted the sites?

MR. DHIR: Yes, photograph like I showed you and you can know the area and you can see how many are there.

MR. SCHROCK: You take multiple photographs and get the average then.

MR. DHIR: Repeat them.

MR. SCHROCK: Right. Yes.

MR. DHIR: It's a tedious job. And this is now flat plate, but two different subcoolings. So there's no, you know, there's a clear distinction on two subcoolings and we don't see any effect as long as your contact angle is fixed and -- so that will simply life some ways that you have only two variables.

And this is what we see from our experiments which we have done. And this is all of the data we got.

CHAIRMAN WALLIS: It's really four bounds, though.

MR. DHIR: Beg pardon?

CHAIRMAN WALLIS: Five. It's five groups.

MR. DHIR: Right. Five different contact angles.

MR. DHIR: Okay. This line and this dotted line is the correlation developed currently here from pool boiling data. And that correlation was for -- superheat greater than about 15 degrees corresponding to cavity size of about 5 --

CHAIRMAN WALLIS: So if you didn't control contact, you could go for a factor of thousands or so?

MR. DHIR: Yes, sure. That's why boiling curve shift all over the plate. That's the key ingredient. Although -- cavity size of cavities, side density doesn't play any role.

CHAIRMAN WALLIS: We should have both of you together.

MR. DHIR: We have. We were there in Illinois in May. We were there and there were -- so we had good discussion argument.

CHAIRMAN WALLIS: Was it a refereed discussion?

MR. DHIR: It will be published.

CHAIRMAN WALLIS: Okay. Let's move on.

MR. DHIR: Yes. Okay. So even you see there's a big variation in the number density with contact angle. And we would look at Kocamustafaogullari data and predict that, that's what he find.

So for all the data we have developed a correlation and -- less than 15 degrees number density varies as delta T to the square. But superheats it's delta T to the 5.3 power. Okay.

CHAIRMAN WALLIS: Next one shows the correlation.

MR. DHIR: And then the --

MR. SCHROCK: What happens to them at 15 degrees?

MR. DHIR: What 15 degrees.

MR. BANERJEE: They would discontinuous.

MR. SCHROCK: They're discontinuous.

MR. DHIR: Yes, they're the discontinuous. Because it depends on the -- you know, the surface. The superheat becomes large and then they just take off.

So you can see it's lots of data, lots of time was spent on this. And this is all the data we have and it's easy to get an error in this, especially when you're -- say over 1 centimeter square area, you have one cavity or two, you're a factor of 2 off. And so your densities and you see some scatter out here. And then high heat flux as the bubbles start to -- is very difficult to delineate how many cavities are there. So you see scatter out there.

This scatter is puzzling. This is the data we took very early and we found many more cavities than you will suspect from all the other data. And that's when we were just starting to experiment. My guess is that we had too much gas in the water, we had not aerated the water. We have not gone back and reproduced this data, so it's still there.

MR. BANERJEE: This is all your own data?

MR. DHIR: This is all our own data, but we -- as I showed you earlier, the data of Klausner -- we got.

See, most people don't give you contact angle or superheat. And if you don't have it, you can put it wherever you want to. And so we tried to void it.

CHAIRMAN WALLIS: Right. Right.

MR. DHIR: Next we go to bubble diameter and departure and lift off. And so what we find, here we plot bubble diameter departure. Departure means the bubbles start to slide on a nucleation site. Lift off is when it takes off, now going to the surface. Okay.

And here this typical data. We have more sets of data, but this for flat plate with velocity of about 35 centimeters per second and three different subcoolings.

As we increase the subcooling you can see the bubble diameter departure. As we increase the wall superheat it increases. We relate this to the inertia as bubble goes faster, there's more liquid inertia to be encountered. Bubble go to slow down with condensation, inertia is less.

And similar -- so we find bubble diameter departure going about square root of wall superheat and that's lift off diameter, which is larger than the departure diameter. So the bubbles grow as they move along the surface.

MR. BANERJEE: Why is this vertical line, like vertical scatter?

MR. DHIR: It's a measurement arrow you see once sometimes bubbles munch.

MR. BANERJEE: I see. It's not just the distribution of the --

MR. DHIR: Right. No, no. And this is from flat plate.

MR. BANERJEE: Right.

MR. DHIR: Now you could do some cavity experiments which you are doing also, you can get very clean data, the scatter won't be there.

CHAIRMAN WALLIS: Well, this next one you have a characteristic link which depends on G?

MR. DHIR: Yes.

CHAIRMAN WALLIS: Why is that?

MR. DHIR: Because even in the wall the bubble is attached there is a buoyancy actually.

CHAIRMAN WALLIS: So this is only for vertical upflow?

MR. DHIR: Vertical upflow.

CHAIRMAN WALLIS: If you have vertical downflow, it would be quite different.

MR. DHIR: Different, right.

CHAIRMAN WALLIS: Horizontal flow would be quite different?

MR. DHIR: That's right.

CHAIRMAN WALLIS: And we have done separate study which shows those things.

MR. BANERJEE: The microgravity?

MR. DHIR: Yes, different.

CHAIRMAN WALLIS: Yes. If you go to -- well, microgravity everything goes off scale here.

MR. DHIR: This one would go off scale, but you see that lift becomes extremely important, microgravity.

CHAIRMAN WALLIS: So this is a big correlation without much mechanism behind it?

MR. DHIR: That's right.

CHAIRMAN WALLIS: Right.

MR. DHIR: We have -- I'm not showing here. Where we're doing the medical simulations and data from that, we can do the correlation. But this is just from this work.

Basic physics is that superheat is important, subcooling is important and that's all I can say.

MR. BANERJEE: The length scale is surface tension to some sort of --

MR. DHIR: Buoyancy, yes. Typical end scale in boiling.

MR. BANERJEE: But if G is zero then you have a problem.

MR. DHIR: As I was saying again, it's

very specific to upflow, 1G.

CHAIRMAN WALLIS: And this surface tension comes in because the bubble is hanging onto the surface.

MR. DHIR: Hanging on to the surface and that's what is --

CHAIRMAN WALLIS: I think it would depend on the contact angle then.

MR. DHIR: Yes, this is -- again, this is a 30 degree contact angle. Contact angle is a variable.

CHAIRMAN WALLIS: Oh, you have a correlation involving --

MR. DHIR: Not as yet.

CHAIRMAN WALLIS: Okay.

MR. DHIR: We're not done with that. So this is only for 30 degree, one contact angle, although it should be stated there.

Now we just said okay, let's go to the -- and see what's out there, and this is what we find. This is the velocity we got from our previous graph which I showed you --

CHAIRMAN WALLIS: The curve is yours?

MR. DHIR: Yes. And this is all the data. You know, some people don't give you again, superheat, whole subcooling, local values and just see how much scatter is off all the data, but it seems like the velocity effect we're getting seems to be okay.

But to put all this data in perspective, we need to have, you know, information about what the superheat, local superheat was, subcooling was, contact angle was.

MR. BANERJEE: So this Unal's data is the one that Lahey used?

MR. DHIR: Right. Unal went to high pressures, too, you see. It's a larger angle pressure. And Unal's data is here.

CHAIRMAN WALLIS: It works best a high pressure.

MR. DHIR: Yes. But we don't know what the contact angle is for that case. Okay.

MR. SCHROCK: You probably could get a reasonable handle on it knowing the materials, huh, in Unal's data?

MR. DHIR: Approximate, yes. Contact angle, but not superheat.

MR. SCHROCK: Yes, not superheat.

MR. DHIR: Yes, and subcooling.

MR. BANERJEE: What is that curve you just fitted it?

MR. DHIR: We just plotted our curve, that velocity effect I got from the previous viewgraph, and that's just --

MR. BANERJEE: I see.

MR. DHIR: Without subcooling effect or superheat or contact angle. Just to see. If I had just done this, how far I could be off. And it also shows you that the velocity set probably is taken into--

CHAIRMAN WALLIS: Okay. I think we have to move on. There's a fantastic amount of information here.

MR. DHIR: So next is OSV. There's number of correlations starting with Bowring, '62 and it's kind of dimensional correlation. This delta $_{OSV}$ means that what is the liquid subcooling when the bubbles start to migrate into the bulk. So -- and they're relating -- flux and velocities.

And Levy did -- he accounted for the turbulent profile in the thermal boundary layer.

Dix correlated again delta $_{OSV}$, heat flux, single heat transfer coefficient.

Saha & Zuber which we were talking about earlier, this was basically telling you when OSV was not correct, but heat flux. It doesn't tell you heat transfer coefficient. People are using it.

And Zeitoun recently came up with a different correlation.

But you can see all of them somehow form a -- number which is --

CHAIRMAN WALLIS: D.J. I need to replace your battery. That's why it's clicking.

(Whereupon, at 5:01 p.m. off the record until 5:02 p.m.)

MR. DHIR: We are only halfway through.

MR. BANERJEE: But you've still got an hour.

CHAIRMAN WALLIS: You've got an hour.

MR. DHIR: Okay. I'll move quickly. But this is --

CHAIRMAN WALLIS: What we found is we didn't see this before. We saw some stuff, but most of this wasn't in it.

MR. DHIR: So we have looked at -- you know, from our data where the OSV occurs initially and those are the data you see here. And -- given location, this increase the flow rate, you need to have high heat flux. That's what it does.

And if you increase the subcooling, then also you need higher heat flux.

MR. BANERJEE: How are you defining onset of significant void here?

MR. DHIR: When the bubbles start to migrate.

MR. BANERJEE: Okay.

MR. DHIR: And some people have defined very differently, so there's always that struggle we had what did they mean. Some people may have the void fraction when they see some increase in void fraction, that's what -- they would call. But then that's way downstream you see.

MR. BANERJEE: Even if you have a millimeter bubble on a 1 centimeter diameter pipe on the wall, then you've got a void fraction of about 10 percent that's due to the millimeter bubble?

MR. DHIR: Right.

MR. BANERJEE: It's not trivial.

CHAIRMAN WALLIS: Because the ones on the wall are more significant?

MR. BANERJEE: Well, they appear --

CHAIRMAN WALLIS: That's right, so they're a lot --

MR. DHIR: So our proposal is based on this, that if the bubbles just before departure is smaller than the thermal layer thickness, then presumably the bubble will start to slide and eventually detach at high subcoolings. If on the other hand bubble is larger, then condensation occurring, then the liquid subcooling has to be less for bubble to grow to its desired size.

CHAIRMAN WALLIS: There's no further mechanics in this?

MR. DHIR: No. At the moment it's just--

CHAIRMAN WALLIS: I would think that motion of a bubble would depend on the mechanics.

MR. DHIR: Right. See, that's again -- it'll take some time. And we are looking at numerical simulation and then single bubble experiments. But that's funded through NASA. And that gives us this information. But right now it's amazing hypothesis. But we tested this hypothesis by taking all the data, Dix's data, we have all the water data and flat plate and rod bundle. And we found that this DD over delta correlated with the constant -- empirical constant C like this. So our correlation is very simple correlation, it's like dimension less wall superheat if we could do a constant, then OSV occurs.

And that constant we have gotten empirically. And it includes Dix's data, all data with freon and we have Bowring's data you'll see in the next viewgraph, and it seems to do for all of those data more reasonably well.

Bowring's data at high pressures and Dix's for the freon, and our data.

So up to now this is simply empirical.

Bubble release frequency, how do we get it. Here I think you can see what happens to the bubbles as they grow. I mark here this arrow. And here the bubble is growing, start to grow. This is the bubble which is growing. And now you see clearly this bubble is --

CHAIRMAN WALLIS: It looks like 2 bubbles.

MR. DHIR: Well, this a reflection.

CHAIRMAN WALLIS: Oh.

MR. DHIR: And growing, growing. You see the arrow is almost wanted -- cavity is. And now it start to slide. This is where the bubble was initially, now it's moved over. It continues to slide, slide and at that point it lifts off. And now after waiting --

CHAIRMAN WALLIS: And another one starts?

MR. DHIR: Beg pardon?

CHAIRMAN WALLIS: Then another one starts?

MR. DHIR: Another one starts.

MR. BANERJEE: But actually, you know as

soon as it slides it seems to be within the second slide already slightly lifted off if you look at the reflection.

MR. DHIR: Right. You know, that is a key question. Is there a layer underneath so create a liquid layer. And I don't know. That's a question I have myself. That is a bubble sliding on a thin film of liquid or is still in contact with the solid direct.

MR. BANERJEE: Well, if it is, then it's violating a no slide boundary condition.

MR. DHIR: Right.

MR. BANERJEE: Unless it's rolling.

MR. DHIR: Right. That's what Steve --

MR. BANERJEE: Steve Davis.

MR. DHIR: Steve Davis, he said that maybe the bubble is rolling, but it's very hard to --

MR. KRESS: It didn't look like the bubble changed sides much during the slide period.

MR. DHIR: Oh, it grows.

MR. KRESS: You think -- it didn't look like it grew.

MR. DHIR: No, it grows. And if you look at -- maybe it's not growing. It's bigger than the previous and then eventually --

MR. KRESS: It just maybe from here --

MR. DHIR: Yes, it grows. And I show you those lift off diameters, they're bigger. About 50 percent.

CHAIRMAN WALLIS: Okay. Okay. So you can get a frequency from this?

MR. DHIR: Yes, right.

MR. SCHROCK: You're comparing with Dix and Bowring, and as I remember both of them, they extrapolate the axial void profile to zero void and say that's the point.

MR. DHIR: Right.

MR. SCHROCK: So it's a little different meaning than yours.

MR. DHIR: Right, there is some difference. Right. That's the key issue we have.

And the waiting time at least you can see here. That is important because in some situations waiting time, transient conduction may have -- waiting time. And this is based on the data we got for different subcoolings and superheats.

Again, these -- what I'm going to show you here is not finalized. This is the stage where we are.

MR. BANERJEE: What do you mean by the waiting time here?

MR. DHIR: Once the bubble leaves the nucleation site, another bubble doesn't form right away.

MR. BANERJEE: Okay.

MR. DHIR: You wait for a while --

CHAIRMAN WALLIS: What's BG?

MR. DHIR: BG is the growth period of the bubble at the nucleation site.

CHAIRMAN WALLIS: Before it slides?

MR. DHIR: Slides, right.

So key thing we are saying is that with subcooling -- subcooling increases at a given superheat, you see the waiting times become longer. And at high superheats subcooling plays little role, waiting times become quite small in comparison to the growth time.

CHAIRMAN WALLIS: I presume where there's an intercept at one here where the waiting time is everything, that it's always waiting and there's this very occasional flip.

MR. DHIR: Right.

CHAIRMAN WALLIS: That's when there's a certain critical delta T for something to happen on the top there.

MR. DHIR: Top, right. That's what would

happen. I was hoping that this will go out to our OSV.

CHAIRMAN WALLIS: It doesn't?

MR. DHIR: I don't know. We have to test it. I asked them to test it. See if it goes to OSV condition. It should go to, you know, 1.

And this is the growth period -- growth period with -- subcooling and that's what the correlation we have so far. Again, one contact angle, 30 degrees.

You know, generally the bubbles slide -- can slide quite a while if you did not have any nucleation site on their part. This is a separate experiments we did where we had just a single bubble sliding over a surface, no other nucleation site. And--

CHAIRMAN WALLIS: This is still boiling, this isn't an air bubble or something like that?

MR. DHIR: No, no, no boiling. One on a single nucleation site.

And this bubble, as you can see, for 30 centimeter velocity has slide almost 18 millimeters before it lifted off.

MR. BANERJEE: How is the velocity defined here?

MR. DHIR: The distance the bubble travels from the nucleation site.

MR. BANERJEE: Is the velocity of the bubble?

MR. DHIR: Bubble.

MR. BANERJEE: Not the velocity of the fluid?

MR. DHIR: No, no. Velocity of the fluid is the parameter here. This is fluid velocity.

MR. BANERJEE: For which?

MR. DHIR: This is the fluid velocity.

MR. BANERJEE: Ah, so that's what I was asking.

MR. DHIR: No, no, this is liquid velocity.

MR. BANERJEE: How is that defined? What velocity is it? Is it bulk velocity?

MR. DHIR: Bulk velocity of the liquid. And I'm plotting the distance it slides as a function of the bulk velocity. But we can get the --

MR. BANERJEE: Is this for a specific bubble size, right?

MR. DHIR: Right, for these conditions. Single bubble -- liquid is saturated.

MR. BANERJEE: How big was the bubble?

MR. DHIR: To start with it's about 1.5

millimeter or so.

CHAIRMAN WALLIS: Well, now we get to a bit that we got a report on, the bubble collapsing.

MR. DHIR: Right.

CHAIRMAN WALLIS: That looks like a relative straight forward.

MR. DHIR: Straight forward, right. Should I go over it? No.

CHAIRMAN WALLIS: Quickly.

MR. DHIR: Everything is quick.

This is another picture of the bubbles are formed on the heater surface. You look at this bubble and this bubble detaching from the surface. It has detached. And now we are looking at its size and its position. And these are .8 milliseconds apart, these photographs.

And knowing the position we can calculate its velocity, local velocity and we also know from the photograph what the size of the bubble is.

CHAIRMAN WALLIS: So you know how far it is from the surface?

MR. DHIR: Oh, maybe a few millimeters, 2 or 3 millimeters.

CHAIRMAN WALLIS: You don't measure that, although the reflection probably tells you.

MR. DHIR: Reflection tells us where you start with.

CHAIRMAN WALLIS: It does tell you how far it is from the surface?

MR. DHIR: Yes, right, exactly. And that's how you calculate the distance.

CHAIRMAN WALLIS: Yes.

MR. DHIR: And knowing the bubble size as a function of time, you can deduce what the heat transfer coefficient should be.

And these are some of the correlations which are --

MR. BANERJEE: Provided you know the --

MR. DHIR: Liquid subcooling.

MR. BANERJEE: Yes, at that point.

MR. DHIR: At that point. But we have measured that liquid subcooling with a thermocouple. Not during that experiment, but with the same conditions we have measured what the temperature.

MR. BANERJEE: So the average?

MR. DHIR: Average.

MR. RANSOM: These are the overall heat transfer coefficient I guess. Now you could envisualize it as having a heat transfer coefficient on the interior of the bubble and, you know, heat transfer coefficient on the exterior, and some condition at the interface on interfacial temperature. And I gather this is from saturation temperature to whatever liquid temperature surrounds the bubble?

MR. DHIR: Right.

MR. RANSOM: Yes.

MR. DHIR: But the pressure in the bubble is, you know, not much different than outside. It's large bubble relatively, unless it becomes extremely small. But by that time we call it zero size.

MR. RANSOM: Are most of these limited by, say, the conduction in the liquid.

MR. DHIR: Yes, of course.

MR. RANSOM: Pretty much, I guess. What's the mechanism inside the bubble?

MR. BANERJEE: It's all pure steam, right?

MR. DHIR: Steam bubble.

MR. RANSOM: And rushing to the interface.

CHAIRMAN WALLIS: Rushing to the interface.

MR. DHIR: Right. So there is assumption there.

MR. SCHROCK: The heat transfer coefficient in the initial number has a delta T. I guess you've already responded to that. It's the bulk liquid temperature and --

MR. DHIR: Local temperature. Local liquid temperature.

MR. SCHROCK: And not a cross section?

MR. DHIR: No, where the bubble is.

CHAIRMAN WALLIS: That's the delta T --

MR. SCHROCK: I don't know what that means.

MR. DHIR: Okay.

MR. SCHROCK: You don't know what the temperature structure is in the cross section?

MR. DHIR: That's what I said, but we measured the temperature distribution in the liquid.

MR. BANERJEE: But it's only an average.

MR. DHIR: Average temperature we measure. I show you the temperature profiles in the liquid.

MR. SCHROCK: But that's axial.

MR. DHIR: Normal. No, no. That's normal to the surface. Very early I show you liquid temperature profiles normal to the surface.

MR. SCHROCK: I thought that was axial.

MR. DHIR: No, normal.

CHAIRMAN WALLIS: Anyway, there's a whole other correlations here.

MR. BANERJEE: What is data?

MR. DHIR: Data is the ratio of the bubble, instantaneous bubble diameter to its initial diameter.

CHAIRMAN WALLIS: And -- numbers are just surrogate for T, it's a dimensionless time.

MR. DHIR: Right. And the time starts when the bubble detaches.

CHAIRMAN WALLIS: Right.

MR. DHIR: So it's local liquid temperature, not average temperature.

See, the temperature is changing. Let's say this is the wall, the temperature's decreasing normal to the surface. So we have a thermocouple with which we get temperature distribution before we look specifically at one bubble here.

MR. SCHROCK: Okay. So the wall is the vertical boundary?

MR. DHIR: Vertical line here.

MR. SCHROCK: And you've measured temperature and function of --

MR. DHIR: Temperature distribution.

MR. SCHROCK: Y, for example, and --okay.

MR. KRESS: And we assume the bubble is always at the one position?

MR. DHIR: This is one bubble I'm showing.

There's another bubble at different position.

CHAIRMAN WALLIS: Moving relative to the fluid, that's where H comes from.

MR. KRESS: Right.

MR. SCHROCK: When you speak of thermocouple in this -- in this bubbly field --

MR. DHIR: Right.

MR. SCHROCK: -- you're getting time average of something --

MR. DHIR: Right.

MR. SCHROCK: -- sometimes the bubble is on it.

MR. DHIR: Bubble when we know the temperature goes up that the bubble crosses that thermocouple, we know that. That's not we don't take care of it.

MR. SCHROCK: No. But some fraction of the time the thermocouple is influenced by the vapor, not the liquid.

MR. DHIR: Right. Right.

MR. SCHROCK: Although it probably remains wet.

MR. DHIR: But this is, again, a time average.

MR. SCHROCK: Yes.

MR. DHIR: That's what we're using.

MR. SCHROCK: Anyway, that defines what it means.

CHAIRMAN WALLIS: And we're moving along. You have further mechanism you account for the thermal boundary layer effect?

MR. DHIR: Right. But keeping -- we are saying is that as the bubble is condensing, the thermal boundary layer thickens and that has to be counted for.

CHAIRMAN WALLIS: Right.

MR. DHIR: In the past it has not been done so. And our correlation, I'm going to jump to the correlation now.

CHAIRMAN WALLIS: It's much better than anybody else's except the Sideman ones.

MR. DHIR: Sideman, right.

CHAIRMAN WALLIS: Which doesn't have your corrections.

MR. DHIR: No, it does not.

CHAIRMAN WALLIS: It's much simpler, but it still works as well as yours.

MR. DHIR: Works almost, yes.

And the key premise we have is that as the bubble shrinks, the thermal boundary layer is actually thicker than it would be if it did not shrink. And so we think that -- number is effected by how long the condensation has been going on along the bubble surface.

MR. BANERJEE: The only thing is the exponent on the --

MR. DHIR: Right.

MR. BANERJEE: Of course, the bubbles are fairly small, but usually for a presurface problem that would be to the half.

MR. DHIR: Presurface, that's not presurface.

MR. BANERJEE: Well, it depends on how big the bubble. If you have circulation around the bubble, you wouldn't get -- that's a solid boundary condition.

MR. DHIR: Right. But see the range of numbers we have used --

CHAIRMAN WALLIS: The numbers, it doesn't vary very much.

MR. DHIR: Right.

MR. BAJOREK:

MR. BANERJEE: So you probably should not show that because that's surely something which any reviewer will jump on, including Gary Leedle and people like that.

CHAIRMAN WALLIS: Don't send it to him then.

MR. BANERJEE: Because it's a analytical solution for, you know --

MR. DHIR: You have something?

MR. BANERJEE: In the book even.

MR. SCHROCK: You have a correlation then which involves a transverse temperature profile which you have found from your data, but is a variable that the TRAC code doesn't have. So there's going to be a problem in applying that correlation until they're also given a basis for the transverse temperature variation.

MR. DHIR: In other words?

MR. SCHROCK: It's a catch 22.

MR. DHIR: Right. But the question is, firstly I want to know how with the physics we have. The next question you're asking how do I implement it.

MR. SCHROCK: Yes.

MR. DHIR: And as I said, we have not given that too much thought to that. That's all I can say.

CHAIRMAN WALLIS: So can we move to the next part, the void fraction?

MR. DHIR: Okay. So --

MR. BANERJEE: Well, one question.

MR. DHIR: Okay.

MR. BANERJEE: The correlation that involves the -- number and also the -- number.

MR. DHIR: Yes.

MR. BANERJEE: Now, that means that you're looking at some fluid motion due to the collapsing bubbles.

MR. DHIR: That's what we are seeing, right. That as this bubble is shrinking, it's carrying with this -- it's boundary layer around it is thicker than it would be if it was just a solid --

CHAIRMAN WALLIS: Compresses the layers of liquid around it.

MR. DHIR: Right.

MR. BANERJEE: But it's not moving very rapidly.

MR. DHIR: No, they're moving.

MR. BANERJEE: It's relative to the liquid.

MR. DHIR: The liquid, no. The velocity of the liquid is much higher than the bubble velocity.

MR. BANERJEE: So wouldn't it strip off--

MR. DHIR: Again, this is maybe some

mixing going on, but --

CHAIRMAN WALLIS: Can we move on to the next --

MR. SCHROCK: Is your -- number the same as Zeitoun?

MR. DHIR: Zeitoun. Where is Zeitoun? Number he based on -- on the diameter. No, it's different.

MR. SCHROCK: Different.

MR. DHIR: No, sorry -- number is same. Same. Same.

MR. SCHROCK: And you measure time in that from the onset of the bubble motion.

MR. DHIR: Motion, right. This zero is when the bubble leaves the surface and starts to roll.

CHAIRMAN WALLIS: Well, I'll ask again if we can move on to the next subject.

MR. BANERJEE: Trying to get on --

MR. DHIR: Get on what?

MR. BANERJEE: Never mind.

CHAIRMAN WALLIS: Well, there's so much here that we haven't seen before. That last subject was one we did get --

MR. DHIR: What did you see before? I don't -- I don't know. I don't know the context.

MR. BANERJEE: Well, the papers we have on this discuss void fraction.

MR. DHIR: Oh, I see. Because there is some papers you may not have seen.

MR. BANERJEE: Yes.

MR. DHIR: Okay. So I don't show you anything with respect condensation?

CHAIRMAN WALLIS: No, that was in -- no.

MR. DHIR: The correlation and stuff.

CHAIRMAN WALLIS: Now void fraction.

MR. DHIR: Next is the void fraction. And these are -- you see the photographs of boiling on the flat plate. And at different heights from bottom. Vapor film.

CHAIRMAN WALLIS: What's that got to do with bubbles?

MR. DHIR: No, it's a two phase mixture.

MR. BANERJEE: Is it full of bubbles or is it --

MR. DHIR: Bubbles, yes. Bubbles and liquid mixture.

MR. BANERJEE: But it's not a film yet?

CHAIRMAN WALLIS: It's the extent of the two phase --

MR. DHIR: Two phase mixture thickness

should be there. And it's not a vapor film, continuous vapor film. It's a mixture.

CHAIRMAN WALLIS: Oh, this is just a flash and very --

MR. DHIR: Yes, yes, sure. It's one time -- it changes. How this layer develops as you move down stream. That's our key point here. And it becomes thicker and thicker as you --

MR. BANERJEE: There's still subcooling of--

MR. DHIR: Yes.

CHAIRMAN WALLIS: It seems to have a structure, though. It's almost -- in all your models I'm assuming these bubbles go off and behave in some way, but you don't model this layer. So maybe the layer itself is doing something, has waves on it or whatever. Looks as if it's certainly not a smooth layer.

MR. DHIR: It's not.

CHAIRMAN WALLIS: So --

MR. DHIR: But it keeps -- time also.

CHAIRMAN WALLIS: Well, that probably effects things, too.

MR. DHIR: It's possible. Again, you can start somewhere.

CHAIRMAN WALLIS: Y is distance from the wall?

MR. DHIR: Y is the distance from the wall and alpha is the wall stretch, and average amount of the span, right. Span boils average of the flat plat.

And the right hand side is basically you see the edge of this two phased mixture layer as observed from the movie and the gamma densitometer. Gamma densitometer seems to correlate fairly okay.

MR. BANERJEE: From the movie how do you get this?

MR. DHIR: Your picture, you see the picture I showed you last time and now I look at the edge.

MR. BANERJEE: Oh, just looking at the edge?

MR. DHIR: Yes.

And this is the void fraction we talked about earlier in the rod bundle. And looking at one location. And how the rod bundle average basically was.

CHAIRMAN WALLIS: So shooting through?

MR. DHIR: Through, right.

MR. BANERJEE: It says qualitative stuff.

MR. DHIR: I don't know if it's

qualitative.

MR. BANERJEE: Qualitative in the sense that it gives indication, but it's sampling many different gaps, basically?

MR. DHIR: Gaps, yes. But it's average, as I said, across.

So basically what you would expect as we increase the heat flux at a given location, void fraction goes up. And if you increase the flow velocity, and even at a given heat flux the void fraction goes down, as you would expect.

Okay. And if you extrapolate those profiles, you see where -- would be and then we have measured, they're not too far off but there's a difference.

Next is kind of boiling curve, they're all random. And basically you see single phase forced convection stays there and then some point boiling starts. After boiling starts the temperature of the surface stays fairly constant. There's constant heat flux.

Then we come to last task. So procedure for calculating this wall heat flux and then coming back to this plate of heat flux. And we say, that okay you should give input, the geometry of the heater, marked velocity, contact angle, wall heat flux or wall superheat, liquid subcooling and pressure. If you give that kind of information and the fuel, whatever fuel there is, then from the correlation we have developed you can calculate ONB, OSV, bubble diameter departure, lift off diameter, number density of active sites. The sliding distance -- the force -- coefficient for force conduction. And then you look at whether your lift damage is less than the spacing between cavities. If it is, bubble damage is less than the spacing, then you are in partial nucleate boiling where the bubbles will slide and then lift off. Or if the bubbles depart and lift off damage is greater then the spacing, the bubbles will -- lift off size and then leave.

CHAIRMAN WALLIS: Where do you predict void fraction here?

MR. DHIR: Beg pardon?

CHAIRMAN WALLIS: Where do you predict void fraction?

MR. DHIR: You don't predict one. I can predict from the number density if you want what is the wall void fraction.

CHAIRMAN WALLIS: This is of interest, though, isn't it? Void fractions are interesting output from this?

MR. DHIR: No.

CHAIRMAN WALLIS: No.

MR. DHIR: Void fraction would be -- to calculate void fraction it will provide the source -- how much vapor is coming into the bulk.

CHAIRMAN WALLIS: Well, I'm just trying to make the bridge to the code. The code wants to predict a void fraction, doesn't it?

MR. DHIR: Right.

MR. RANSOM: It seems -- I can't quite put it together myself. But I mean it seems like it's an attempt to utilize variables available and calculate what regime you're in.

MR. DHIR: Right. You're in partial nucleate boiling or wall nucleate boiling. I'm still looking at the wall. I'm not looking at the flow. And the void fraction in the flow to calculate you need to know how much vapor I'm adding from the wall, what is local liquid subcooling, how much vapor is condensing and then you should be able to calculate how the void is building up as you move downstream. We are not doing that.

MR. RANSOM: That should be part of the code calculation.

MR. DHIR: Code calculation. Our part is only to tell what is happening at the wall.

CHAIRMAN WALLIS: Well, I think your next slide --

MR. RANSOM: They didn't ask you to get rid of the partition, you're saying it's more of a two step process now. You look at conditions based on namely bulk variables and what you think you know about the cladding, the contact angle, things like that and calculate conditions at the wall. And then from your other correlation or condensation we're going to be able to calculate the net to the cell.

I think most things are there minus some-- you know, good questions on what is that temperature profile which we need to the condensation, you know. Are we going to -- getting to the right contact angle and that higher pressure.

MR. DHIR: I don't -- first, I would say that I would build this in your code as, you know, a subroutine if you want to call it, and test it out as can you predict it. We tested it out and again our data and our correlation seems to work too good, I was surprised. But, again, we want to do more of this validation ourselves before I would say --

MR. BANERJEE: Yes, your correlation for

the heat transfer coefficient condensation if it was in the code would do difficulties because you have a Fourier number there, which means you'd have to track the bubbles to know what their lifetime is.

MR. DHIR: Right.

MR. BANERJEE: It would be much better if you could get a heat transfer correlation independent of your number.

MR. DHIR: You could -- average it out and do it.

MR. BANERJEE: But we have to look at the data in these cases and see.

MR. RANSOM:

MR. RANSOM: It would nice to fill this out because --

MR. DHIR: Actually the spacing between the cavities.

DR. MOODY: Like centimeters or --

MR. DHIR: They're really much smaller, millimeter or even less sometimes.

MR. RANSOM: Yes, quite a bit is missing from here like what you need to know about the velocity shield and if you do need this time in order to calculate the Fourier number --

MR. DHIR: mass velocity is there,

geometries there, you calculate it.

MR. RANSOM: Right. Those are fine.

MR. DHIR: Yes.

MR. RANSOM: But, again, like the Fourier number would be how do you evaluate it?

MR. BANERJEE: It doesn't track the bubbles.

MR. RANSOM: Well, there's no way of doing that in the codes at the present time.

MR. BAJOREK: You know the evaporation rate, so you can get effective bubble lifetime out of that. You can't integrate it down to zero. You're going to have to truncate it, but you should be able to get the --

MR. DHIR: It depends on your -- number, too, you know. Because that's a variable.

MR. RANSOM: This would be a great model for the old discon code that we wrote that you tracked all the bubbles. And you did know all this kind of information. But I doubt if you want to put that kind of model in TRAC.

MR. DHIR: We have to sometime -- that this is what we have developed and this is what TRAC would do. Now how do we transfer this -- we have to go through that part.

MR. RANSOM: Yes.

CHAIRMAN WALLIS: I've been looking at your slide. I think what you're doing in the next few slides is just a pulling together what you told us already --

MR. DHIR: Exactly.

CHAIRMAN WALLIS: -- into the pieces of this.

MR. DHIR: That's right. Exactly.

CHAIRMAN WALLIS: So maybe we don't need to go into the details.

MR. DHIR: Oh, but I can show that as well.

CHAIRMAN WALLIS: That's right, because you've sort of taken the relevant parts of your previous pieces.

MR. DHIR: Right. Now we have gone subprocesses, now we go to total processes.

CHAIRMAN WALLIS: Right. So how well does it work?

MR. DHIR: Too well.

CHAIRMAN WALLIS: Too well.

MR. RANSOM: Makes you suspicious.

CHAIRMAN WALLIS: Very suspicious.

MR. DHIR: That's what bothers me.

MR. BANERJEE: Must be plotting the same thing against --

MR. DHIR: I hope not. It works too well.

So let me just show you what we are calculating and how we are -- what is the transient conduction heat load and what is the forced convection contribution and how it changes at vault superheat. Okay. So as you're going from partial to fully developed nucleate boiling, the heat loads are changing. It's not a set variable, set number. The number is changing with superheat.

So here we plot the ratio of Q total, show a Q to Q total what the wall --

CHAIRMAN WALLIS: These are all predictions?

MR. DHIR: These are predictions, right.

CHAIRMAN WALLIS: There's no data to compare?

MR. DHIR: No. These are predictions.

MR. BANERJEE: What are those points then?

MR. DHIR: Points are predictions.

CHAIRMAN WALLIS: The line is just a line through.

MR. DHIR: Just a line through there predictions.

CHAIRMAN WALLIS: So there's no comparison with data here?

MR. DHIR: I'll show, yes, later on.

But this is the transient conduction contribution. I will say initially transient conduction is zero before the boiling starts, just as the boiling starts. Because transient conduction, this comes from the bubble motion.

CHAIRMAN WALLIS: Yes.

MR. DHIR: And as we continue to high superheat it wraps itself -- wall superheat divided by wall superheat at OSV. And as you go to high superheat about two times this delta OSV, very high heat flux. About 70 watts per centimetered square, now most of the heat is going through transient conduction. Very little from forced conduction.

MR. SCHROCK: Looks like you could have drawn a perfectly reasonable line to pick up that stray point in --

CHAIRMAN WALLIS: Oh, come on. It's just putting --

MR. SCHROCK: Isn't that a lot of nitpicking?

CHAIRMAN WALLIS: No. Let's move on. Line and points are the same.

MR. DHIR: Right. And then we do a different flow rate. And as you increase the flow rate or flow velocity and you can see the forced convection continues to persist for a longer superheat -- for higher superheats.

MR. RANSOM: What are the differences between the lines and the points?

MR. DHIR: Points are just -- points are predictions from the model.

MR. RANSOM: Yes.

MR. DHIR: And lines are just -- through the prediction.

MR. RANSOM: Why wouldn't you just draw straight lines and connect them all? I mean --

CHAIRMAN WALLIS: Okay.

MR. RANSOM: It's quite confusing.

CHAIRMAN WALLIS: Now the next curve is similar?

MR. DHIR: Yes. Next it's similar. These are flat plate. No -- but next is important one.

There we now having the total, we split it to how much is going into vapor production to the bulk and how much is going to the liquid either through condensation or just post convection, or some bubbles taking some hit liquid with them. And so you see the first diamonds are what is going into the liquid either through condensation or directly from the wall. The open triangles are what is going to the bulk as vapor. And the circles are condensation occurring at the bubbles either back to the surface or sliding along the surface.

MR. BANERJEE: That's the Qc sub atc.

MR. DHIR: ATC. Flow condensation attached bubbles which are either sliding or sitting.

So initially you start with all the heat is going into the liquid. And as you go to high superheat and for this particular case, 70 percent -- 60 percent is going into the liquid, about 30 -- about 40 percent is going into the vapor production. Out of that 60 percent for the liquid, about 15 percent is coming via condensation. Okay?

And a similar case we do it on the right hand side for higher flow rate and lower subcooling.

CHAIRMAN WALLIS: Then you do the same thing for the rod bundle?

MR. DHIR: For the rod bundle we do the same thing. And, again, transient conduction and forced convection at high superheats or upper -- sorry, not high superheats. Upper portion of the rod bundle because as the liquid heats up it becomes saturated and you see that most of the heat is again going through transient conduction. And very little goes through forced convection, but early on -- at the start you mostly by forced convection.

And, again, we have done two cases different flow rates and different subcoolings.

CHAIRMAN WALLIS: I'm a little concerned about all the heat going to transient conduction because that -- I'm not sure I can figure out how that would be modeled.

MR. DHIR: This model, because bubbles merge. I show you earlier, the bubbles merger model. We assume the bubbles when they are growing they merge with the neighboring bubbles, form a big bubble and leave.

CHAIRMAN WALLIS: They just leave the --

MR. DHIR: As the transient conduction is occurring before the bubbles form and new second bubbles form is the waiting period.

CHAIRMAN WALLIS: So the liquid's completely replaced when they leave?

MR. DHIR: Right. But there's no flow. This is another interesting thing. Here is that forced convection, that dies down. And that was the data I showed also. If you plot full boiling curve and forced convection, and you go to fully developed nucleate boiling, there's no effect of flow field. And that's what we are seeing.

And now to obtain --

MR. BANERJEE: This is a total?

MR. DHIR: Yes. Now we are breaking it up now into evaporation, condensation and going into the liquid in this graph. And as you can see what this particular set of conditions, rod bundle, contact angles would be 7 degrees. Initially the total heat flux was -- initially all of the heat is going into the liquid and has moved downstream. At about 70 centimeters downstream the liquid bulk becomes saturated in this case. And at -- of the bundle we see that now about -- only about, oh maybe 5 percent or 10 percent of the energy is going to the liquid and 90 percent is going into vapor.

Condensation play a very small role and it dies down just before the liquid becomes almost saturated.

MR. RANSOM: The point where the liquid becomes saturated in your case, though, the bulk is still subcooled, I guess?

MR. DHIR: No, bulk is saturated.

MR. RANSOM: The bulk is saturated?

MR. DHIR: Yes. Liquid is subcooled here.

MR. RANSOM: Well, by bulk you mean where the bubble is located, though --

MR. DHIR: No, no, no. The liquid is saturated.

MR. RANSOM: The entire --

MR. DHIR: Liquid, right. Right. That's the whole point of this. That you're describing --

MR. BANERJEE: So once it's saturated, then it's just split.

MR. DHIR: Right, to vapor production.

MR. BANERJEE: Was it heating up the liquid and --

MR. DHIR: Transient conduction and then it's --

MR. SCHROCK: When the bulk liquid is saturated, there is the possibility that part of it is subcollected and part of it's superheated? Liquid real close to the wall is superheated.

MR. DHIR: Right.

MR. SCHROCK: Liquid near the core is --

MR. DHIR: But I'm saying there's no subcooling. The liquid near the wall is always superheated.

MR. SCHROCK: Would this model then

transition to an accepted, say, saturated nucleate boiling model?

MR. DHIR: That's what it is. Beyond this point it's all saturated.

CHAIRMAN WALLIS: So now we get to the comparison with --

MR. DHIR: Right.

CHAIRMAN WALLIS: This is rather like the code assessment where no matter what's in the code --

MR. DHIR: This is just all the data we predicted and experiments.

MR. BANERJEE: But this is total, right?

MR. DHIR: Total. Wall heat flux.

CHAIRMAN WALLIS: And, of course, your model was itself deduced from the same experiments?

MR. DHIR: Yes, that's true. But the data -- the pieces were -- you know, developed for each --

MR. BANERJEE: They were all consistent and treated each of them well, the this is what you would expect?

MR. DHIR: That's true.

CHAIRMAN WALLIS: All right.

MR. DHIR: And that's what I said, it works too well. You need to test for different pressures and different data points and see if --

MR. BANERJEE: But you haven't --

MR. DHIR: No, not as yet. But you can see, you know it's -- I think NRC got its money's worth.

MR. BANERJEE: It's all self consistent.

MR. DHIR: Right. That's what it shows.

MR. KRESS: That's good, yes.

MR. BANERJEE: That's better.

MR. DHIR: So you can see all of the data is within about 20 percent of what we get from the model. And this is what is embarrassing in some sense, it's too good.

And these are the data for flat plate.

CHAIRMAN WALLIS: It's probably the same reason then, and you actually do experiments you used to develop the model, so --

MR. DHIR: Right, but the model has now bubble frequency, bubble diameter and now the transient conduction, force convection.

MR. BANERJEE: It all hangs together?

MR. DHIR: It all hangs together.

CHAIRMAN WALLIS: Right.

MR. DHIR: And this is the data and this is our prediction. And the good part was you see the media tracking it when it becomes -- nucleate boiling, the number densities is there and the model where the bubbles merge is included in there.

MR. BANERJEE: The only way to tell if it works really is to do an experiment in a slightly different diameter?

MR. DHIR: Right. Blind experiment, I would want to do it. And that's a standard problem, you do a blind experiment, give all that information. Somebody does the experiment, see how good it comes out. Maybe you should do the experiment.

MR. BANERJEE: I'll do the prediction.

CHAIRMAN WALLIS: I'll do the reviewing.

MR. DHIR: So this is for the rod bundle. We tried to put it -- the wall temperature for the heat flux is given. And these are the data which we measured for this particular set of conditions. And the triangles are the data and this is our prediction. And we marked out so where we put it to ONB to occur-- experiments where we saw ONB occurred. OSV where we occurred --

MR. SCHROCK: Is this the best comparison or is this --

MR. DHIR: This is one we did. We have not done many. These are the ones we have done. So there was no attempt to make the -- show you the best one. But I was kind of surprised. I thought there was much more difference, but it seems --

MR. BANERJEE: What if you did good job in correlating each piece, right?

MR. DHIR: Within the limitations we have, we did it. But, again, I'm not given credit --

MR. SCHROCK: Why does the rod bundle data expand a much smaller range of heat flux?

MR. DHIR: Because power input, see, we could not go too much power. We put in rod bundle, you know, enthalpy is increasing. Heat flux -- total heat input is about 60 kilowatts.

MR. SCHROCK: So it's just total surface area.

MR. DHIR: Is so large, right.

See, the flat plate we were putting only about 15 -- 10 to 15 kilowatts and here we are putting about 50 kilowatts.

And should I describe the boron?

CHAIRMAN WALLIS: Yes, I think you do, and we're doing very well.

MR. DHIR: So, you know, one of our -- as I said, the rod bundle has degraded. So we said let's -- why not use it before we discard it to study some effect of boron. So we added boron to water, about

7,000 ppm. Generally at the startup it's about close to 5,000 I think. And so we looked at different -- velocity, liquid subcooling was kind of fixed, but different heat flux. Up to 30 watts per centimeter squared. And as I said, contact angle with boron in the liquid we found was the same as was without boron.

CHAIRMAN WALLIS: And does boron get deposited on the wall in the reactor?

MR. DHIR: Yes, that's an issue.

CHAIRMAN WALLIS: So as crud thickness builds up on the --

MR. DHIR: On the rods, yes. That's an issue. Axial offset anomaly.

MR. BAJOREK: One of the big problems right now is called axial offset anomaly. And we believe what's going on is hot assemblies are up into the range where a good part of it is in subcooled boiling. The boron is platting out and then being such a good neutron grabber, that's causing some very oddball --

CHAIRMAN WALLIS: This is in normal operation of a reactor?

MR. BAJOREK: It's in normal operation.

CHAIRMAN WALLIS: Actually get boiling?

MR. BAJOREK: Yes. Oh, yes.

CHAIRMAN WALLIS: Oh, well --

MR. DHIR: So it's a very important piece of information which we got.

And this outer surface likes like it was kind of photographed.

CHAIRMAN WALLIS: 50 microns sounds to me like a lot.

MR. DHIR: Beg pardon?

CHAIRMAN WALLIS: 50 microns is a lot of degradation.

MR. DHIR: Yes, 40 -- 45 microns is developed in 12 hours about, 11 hours. And you see the surface, you see how it's structures, like a porous structure on the surface.

MR. BANERJEE: This was at low velocities or --

MR. DHIR: No, the velocities were as I showed you last viewgraph. I don't remember it.

CHAIRMAN WALLIS: You get enough of that in the reactor, it would shut it down.

MR. DHIR: Yes. Velocity was about 60 centimeters per second.

MR. BANERJEE: Okay. So it's low.

MR. DHIR: Low. But this boron concentration was high, at 7,000.

CHAIRMAN WALLIS: This is boric acid, this is boron --

MR. DHIR: Yes, boric acid.

CHAIRMAN WALLIS: So what's on the surface then? So it's not pure boron is it? What is it that's on the surface?

MR. DHIR: What do you mean? I don't -- boric acid, I guess.

CHAIRMAN WALLIS: It's boric acid?

MR. DHIR: Right.

MR. BANERJEE: It's tomoxide.

MR. DHIR: Tomoxide. We have not looked at the composition.

CHAIRMAN WALLIS: So what's the pH on the surface? Do you have a --

MR. DHIR: We measured the pH. I don't remember now. But not at the surface. But in the liquid what the pH was we measured it.

MR. SCHROCK: How do you measure that thickness?

MR. DHIR: That's a good question.

CHAIRMAN WALLIS: This is concentrated boric acid --

MR. DHIR: See that removable thermocouple? And thickness is very small. So we put a big dial so that when we move the micrometer close to the surface, made the contact and then backed off how much we back off from the clean surface and from that we deduced how much it was.

CHAIRMAN WALLIS: This is a Zircalloy surface?

MR. DHIR: Zircalloy.

CHAIRMAN WALLIS: Did you get zirconium borate or something formed on the surface?

MR. DHIR: No, no. I don't think it's a chemical reaction. It's just a deposition on the surface.

And we are doing now various detailed experiments, it's funded from DOE looking at a single bubble in boron and see how this deposition occurs during subcooled boiling. And we see very nice interesting patterns how it forms.

CHAIRMAN WALLIS: You get more nuclei with boron then?

MR. DHIR: Yes.

So next is nucleus and site density and you can see how it looks like. In the upper surface is clean surface and lower is with boron at same superheat.

CHAIRMAN WALLIS: And we should see more

nucleation sites?

MR. DHIR: Yes, that's what you should conclude. And that's where we plotted. These are all bundled with boron, 35 micron or 40 micron layer on it. And these are the clean surface.

MR. SCHROCK: Is the solution conducting then? Is this an electroplating process?

MR. DHIR: When it evaporates you taking only the liquid out and boron is left behind. With a concentration, local concentration exceeds the saturation limit.

MR. BANERJEE: It doesn't dissolve again, right?

MR. DHIR: It builds up with time, but it may be dissolving but the rate you are producing it more than it's dissolving back.

MR. BANERJEE: Well, but you know if there was a fluctuation of liquid over it --

MR. DHIR: Right.

MR. BANERJEE: -- it would tend to dissolve. So there's some irreversible process going on which doesn't allow it to go back.

CHAIRMAN WALLIS: If you stop boiling and flushed it with water, would it -- with just boric solution, would it dissolve the boron again?

MR. DHIR: If I flushed with clean water, I think so.

CHAIRMAN WALLIS: Or with the boric solution.

MR. BANERJEE: Do you think so or do you know so?

MR. DHIR: I think so I said.

CHAIRMAN WALLIS: You just --

MR. DHIR: It's a guess. I have not tested it.

MR. BANERJEE: It's not obvious that that happens.

MR. BAJOREK: I think it would behave very much like the calcium sulfate that you see in a lot of heat exchangers. And even if you have a flow going over, you still have the no slip condition at the boundary or near your surface. Even if it's flow, you're going to continually build up this crud.

MR. DHIR: But even if water --

MR. BANERJEE: I don't think it's going to dissolve.

MR. DHIR: No. Even water we have found -- clean water and you always have some contaminants. And when you do boiling after a while something is left on the surface.

And we have numerical simulation, too, to predict it. And basically some microlayer underneath where you evaporate and your concentrations go way beyond saturation limit and that's just there. Now how the -- I don't know the mechanism. But I'm just saying it's left behind.

MR. BANERJEE: There's not a soluble form then?

MR. DHIR: Right.

MR. BANERJEE: Something happens

MR. DHIR: So something has to happen.

CHAIRMAN WALLIS: Maybe it reacts with the zirconium?

MR. DHIR: Could be. But maybe we can take a sample and see how -- what it does.

MR. BANERJEE: Now in the reactor it's not the boron that's directly platting out. What they think it might be are other contaminants within the -- nickel coming out of the tube and iron platting out forming a crud and then the boron getting trapped in that matrix.

MR. DHIR: This is the boiling curve we got, like starting without boron in water. And that's what we had done earlier. And now then we tracked after every set of experiments. These are the three sets of experiment, like for 3 hours and 6 hours and 11 hours or something. And as you see with time, we find that single phase heat transfer goes down, but the boiling heat transfer goes up.

CHAIRMAN WALLIS: Well, this water's in the reactor for days and months, it's not just --

MR. DHIR: Right, but the concentration we used was higher than would be in the reactor. See, that's one other way. But the key thing was it's surprising result in some ways that in nucleate boiling your heat transfer is higher with boron because of the more -- nucleation sites. It also indirectly tells you that -- site density is important to know.

And in the single phase case is drops down because of the thermal resistance of this layer.

So what the future work, we are saying is that we still need to measure the void fraction in the rod bundle in a more detailed fashion, and especially also at higher pressures. And that's what we are doing now.

And then we have to generalize the models and correlations to other pressures.

CHAIRMAN WALLIS: Then are you going to take these models and apply them to some data which was taken at more --

MR. DHIR: Conditions, right. Right. That's my idea, to see how far we are predicting.

MR. SCHROCK: What do these negative delta T wall mean?

MR. DHIR: T wall is less than T_{sub}.

CHAIRMAN WALLIS: It's T minus T_{sub}?

MR. DHIR: Right. Okay.

CHAIRMAN WALLIS: Well, you're very courageous to do this. To give up all these complicated models for difficult looking phenomena and put together a way of predicting boiling heat transfer.

So I invite the Committee to send in comments and make comments now if there are any.

DR. MOODY: Just a question that goes way back to your page 55 where you made that correction.

Page 55 where you made a correction in that correlation.

MR. DHIR: Number?

DR. MOODY: Page 55. It's the label number 10, condensation heat transfer coefficient.

MR. DHIR: Just one second.

MR. BANERJEE: With the famous Fourier number.

MR. DHIR: Yes, 55. Okay. I don't have the transparency, but I have the sheet.

DR. MOODY: Yes, that's fine.

Now in the material we got --

MR. DHIR: Right.

DR. MOODY: -- there was a figure 7 that showed some spread in your data versus Fourier number.

MR. DHIR: Right.

DR. MOODY: And I'd just appreciate what you did. You took that data and brought it together with this correction?

MR. DHIR: Right.

DR. MOODY: And that's really a key --

MR. DHIR: Right.

DR. MOODY: In the whole contribution you have here?

MR. DHIR: At that part, not the total.

DR. MOODY: Yes, okay. And then you carried that over into the Nu number.

MR. DHIR: Nu number correction.

DR. MOODY: Okay. I just wanted a little--

MR. BANERJEE: That's how the Nu number correction was made?

MR. DHIR: Correction was made, right. And then that correct double diameter as well.

DR. MOODY: Just off hand, how do you make that correction? Is this a computer program that tells you what exponents --

MR. DHIR: No. It will be part of it. It also that I thought there should be some effect of the history. And then we went back and see how we could decide it. And that's how we put it --

DR. MOODY: So a little insight, understanding, a little -- yeah, yeah. Okay.

MR. DHIR: And it is published in international journal --

DR. MOODY: Yes.

MR. BANERJEE: But Eisenberg and Siesman correlation seems to do pretty well?

MR. DHIR: Right, pretty well, right.

MR. BANERJEE: Considering that they're not -- number there doesn't have a Fourier number.

MR. DHIR: Right, right. So it's kind of -- or whatever it does. That's the closest --

MR. BANERJEE: But effect is fairly small, right?

MR. DHIR: Right. But it becomes important when -- number is large. When high subcooling it's very important.

MR. BANERJEE: I guess because you get

rapid --

MR. DHIR: Rapid condensation, yes, that's what we see.

CHAIRMAN WALLIS: Any other words. Ready to close this session?

MR. BAJOREK: Just maybe to add from our office, we're very pleased with the work. We think it's really identified a lot of the fundamental physics and we think it gives us now a basis for developing and trying to come up with models that can put into the code.

Dr. Ransom's gone, but we wish we had been in a state where we could have started development on this sometime in the past. But it's in our model development plans and we hope to try to develop the routines, find exactly the ways to put this into TRAC-M over about the next year.

CHAIRMAN WALLIS: TRAC-M's going to calculate things like waiting time and --

MR. DHIR: There is no need for them.

CHAIRMAN WALLIS: No need for that?

MR. DHIR: They don't need to, we can give them recipes that this is what you do.

CHAIRMAN WALLIS: You mean boil this down into --

MR. DHIR: Right, into something which is manageable. They don't need to do it. Once we have validated our modeling, then other sets of data, we are confident, then we can give them what --

CHAIRMAN WALLIS: We still have to calculate all these things, don't they, in order to get your answers you have to calculate these waiting times and things?

MR. DHIR: They would need those in that information.

MR. BANERJEE: They're all phrased in terms of the parameters, right?

MR. DHIR: Right.

MR. BANERJEE: So that means you just make it a little black box and feed in these parameters--

CHAIRMAN WALLIS: Out comes a table lookup thing. If you have this, this flow rate, this temperature, this -- and you just --

MR. BANERJEE: A net it does it for them.

MR. SCHROCK: You're going to have to have a prescription for contact angle. You're going to have to have a prescription for transverse temperature distribution. And you're going to have to have a model for calculating alpha -- all those things.

MR. DHIR: Right.

MR. SCHROCK: I think they'll need some help getting that.

CHAIRMAN WALLIS: I think we're done. We are done.

(Whereupon, at 5:59 p.m. the meeting was adjourned.)