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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

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MEETING

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

(ACRS)

SUBCOMMITTEE ON THERMAL-HYDRAULIC PHENOMENA

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WEDNESDAY,

NOVEMBER 13, 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., Dr. Graham Wallis, Chairman, presiding.

COMMITTEE MEMBERS PRESENT:

GRAHAM B. WALLIS, Chairman

SANJOY BANERJEE, Consultant

THOMAS S. KRESS, Member

FREDERICK MOODY, Consultant

VICTOR H. RANSOM, Member

VIRGIL E. SCHROCK, Consultant

ACRS STAFF PRESENT:

PAUL BOEHNERT, Staff Engineer

ALSO PRESENT:

KEN CARLSON, Framatome ANP

RALPH CARUSO, NRC

HUEIMING CHOW, Framatome ANP

JERRY HOLM, Framatome ANP

RALPH LANDRY, NRC

ROBERT MARTIN, Framatome

BILL NUTT, Framatome ANP

LARRY O'DELL, Framatome ANP

C O N T E N T S

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

(8:30 a.m.)

DR. WALLIS: The meeting will now come to order.

This is a continuation of the 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, Fred Moody, and Virgil Schrock.

For today and tomorrow's sessions, the subcommittee will continue review of the Framatome ANP-Richland S-RELAP5 realistic code version and its application to PWR large break LOCA analyses.

Portions of this meeting will be closed to the public for discussion of information considered proprietary to Framatome ANP-Richland, Incorporated.

And, Jerry, would you please let us know when that's the case, when you think it's proprietary information?

MR. HOLM: Yes.

DR. WALLIS: Mr. Paul Boehnert is the cognizant ACRS staff engineer for this meeting.

The rules for participation in this meeting have been announced as part of the notice of the meeting previously published in the Federal Register on October 23, 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 requests for time to make oral statements from members of the public.

We will no proceed with the meeting, and I call upon Mr. Jerry Holm from Framatome ANP-Richland to begin.

MR. HOLM: Good morning. My name is Jerry Holm. I'm manager of product licensing for Framatome.

Just one item of clarification maybe since the last meeting. We've changed the company name. It's now just Framatome ANP. The "Richland" has been dropped. That was part of the initial merger arrangement.

Today we're going to talk about the Framatome ANP realistic large break LOCA methodology.

I'm going to start with a fairly detailed presentation of the momentum equation which is going to be given by Ken Carlson.

We need to target to finish this about mid-afternoon so that we have time for the other items. Then we'll go into the general RELAP5 questions, which came from Dr. Moody. Bob Martin is going to do that. We'll talk about selection of node size. Bob Martin is going to do that again.

We will then go into critical flow issues. Ken Carlson is going to present that, and then statistical analysis by Larry O'Dell.

We also received a request to talk about the force and Rosenow equation correlation, and if we have time, I'd like to stick that at the end of today's presentation. If not, there should be time after Larry O'Dell's tomorrow.

And then tomorrow we've got scheduled for a summary of the methodology, and Larry O'Dell is going to do that. He's going to go into the requirements and capabilities, and then the response to the request from the committee, and we're going to talk about changes we made to RELAP5 to create the S-RELAP5 code.

We're going to talk about assessment and ranging parameters and sensitivity uncertainties. This is an overview of the entire methodology. It will be fairly similar to the presentation given previously to the Subcommittee, but since we've got a number of new members, I think it's worth going over the information again.

With that, I'll turn it over to Ken Carlson to start the momentum equation.

DR. WALLIS: I should remind you that you have a list of 13 questions from me, some of which have As and Bs and C parts, and you also have a critique which asks you to respond to. And I think we're looking for answers to these questions.

MR. HOLM: Right.

DR. WALLIS: So if you don't present them, then you will have to provide them some other way.

MR. HOLM: Hopefully we have incorporated those into the presentations. We structured this presentation intending to respond to those questions, but we thought it made more sense to have a more functional presentation structure.

DR. WALLIS: Yes. Well, I hope it will work out fine.

Thank you.

MR. BOEHNERT: Now, I understand we're

going to go into closed session; is that correct?

MR. CARLSON: Yes.

MR. BOEHNERT: Okay. So anyone here that is not with Framatome ANP or doesn't have an agreement with them to hear proprietary information, please leave the room.

And, transcriber, we'll go to closed session transcript. Thank you.

(Whereupon, at 8:35 a.m., the open meeting was recessed, to reconvene immediately in closed session and resume the open meeting at 3:17 p.m.)

MR. BOEHNERT: I want to remind everybody we are in open session. So the transcriber should have an open session transcript.

DR. WALLIS: Whose questions are we answering now?

PARTICIPANT: Dr. Moody's questions.

DR. WALLIS: Dr. Moody's questions. Okay.

MR. MARTIN: Okay. My name is Robert Martin. Believe it or not, I work for Framatome ANP. I can still claim that for a little bit longer, as long as I pay my check every money.

Can you hear me now?

I'm addressing Dr. Moody's four questions he sent about a month or so ago. These topics, first they are somewhat related: transient discharge of mass and energy -- I'm just using your titles here -- propagation flows, forces on piping and structure. I think in the agenda Laurie had separated out the last one.

I only have a total of like 17 slides here. So I just stuck it here as one presentation, but that's the last one, selection of node sizes, and for that actually I just pulled out slides from last year, which of course you will hear.

DR. WALLIS: I don't think you're going to address the third bullet, forces on piping.

MR. MARTIN: Yeah. That's --

DR. WALLIS: You're not going to address it. It says it, but it's not part of --

MR. MARTIN: Basically our answer is -- I mean, if you want to get to the punch line ahead of time --

DR. WALLIS: You're not addressing it.

MR. MARTIN: -- we're not addressing it for realistic LOCA applications --

DR. WALLIS: Yes.

MR. MARTIN: I guess for LOCA applications in general, forces on piping --

PARTICIPANT: You don't care.

MR. MARTIN: Yeah, we don't care.

And as far as our regular reload support, it's not relevant. If we were building plants like all the time, this would be important, and I guess we have methodologies that are probably 20 years old that they're collecting dust, and until we start building plants we probably won't be pulling them up.

That was kind of my answer, and that was it.

DR. WALLIS: If you were addressing it, then you'd have a different kind of momentum equation which had a force from the piping, including a normal force.

MR. MARTIN: And then we'd have to go back and think about this and prepare for --

DR. WALLIS: It would not look like what we heard earlier today.

MR. MARTIN: Yeah, we'd have to prepare it a little bit differently.

DR. MOODY: I'm going to assume if they did address it and had to apply it, they'd do it right. How's that?

MR. MARTIN: Okay. Your question was this: please describe how the discharge mass flow rate is obtained for the postulated instantaneous rupture of a long pipe before the quasi-steady blowdown rate is reached. The pipe is attached to a pressure vessel.

And then you had some discussion there, drawing on some early RELAP5 calculations for this case, sometimes as a result of inflow rates exceeding critical flows, too much massing.

Okay. Just a little background. Using the fine nodalization and small time steps, RELAP5 codes have demonstrated the ability to mechanistically capture the choke flow phenomenon. I think in Ken's discussion Dr. Ransom made reference to the calculations done in the '80s, and that's kind of what I'm referring to here, that that exercise has been done in the past with RELAP5 codes. It is not what we're doing now because that would require very fine nodes and small time steps.

Well, and as I say in the next sentence, it's a complication.

To achieve fast execution speed, the implicit evaluation is used for those terms responsible for the sonic wave propagation time step, and this allows for a maximum stable time step to approach the current one.

Okay. So what do we do? And I'm not

going to go into all of the details because that's really a lot of your questions a little bit later, and since Ken likes to suffer more than others, we'll just let him get back up there and be the whipping boy.

But where we go is when the break opens up, we start doing the calculation just like Ken's talking about, basically Bernoulli calculated velocities and pressures.

There's this Alamgir-Lienard-Jones model for subcooled choke flow that were used to determine the throat pressure and choke velocity, and then I make here the reference to where it is in the documentation.

Here we're using local conditions, and when this Bernoulli calculated velocity exceeds choke velocity, then the velocity just sets.

Now, that still begs your question. You still have the problem that you have once upon a time early with the calculations, and I'm going to say, well, yeah, to some extent you do.

This figure here is just taken from one of our large break analyses that we've provided for this methodology submission, except for you'll never see it because it's only for the first 20th of a second.

What we have here is flow rate out the

break on the pressure vessel side. You can see the flow rate taken off, and then I have drawn these vertical lines here to indicate when the junction choke.

Okay. You can see it takes off and then, wham, the model kicks in. It backs off a little bit, and then it goes up again.

DR. MOODY: This is like a liquid line that opens up in that first heavy curve going up with the Bernoulli flow?

MR. MARTIN: Yeah, I'll tell you. The time step here is .000 --

DR. MOODY: Whatever it is.

MR. MARTIN: -- four.

DR. MOODY: That's Bernoulli flow?

MR. MARTIN: Bernoulli flow up until --

DR. MOODY: Of liquid, yeah.

MR. MARTIN: -- up to here and then the choke model comes on. The criteria is met and then we just lock in based on the calculation of the sonic calculation there.

DR. MOODY: So that's a --

DR. WALLIS: -- type of wave that propagates along the pipe. You open the break and a decompression wave runs down this pipe and comes back again. Is that the model?

MR. MARTIN: You know, we don't rack that way, and I can't say that I've really looked at that, but maybe this --

DR. WALLIS: That's not what --

MR. MARTIN: -- is part of the answer.

DR. WALLIS: No, no, you're not addressing the sort of acoustic transient in the pipeline.

MR. MARTIN: No, we're not looking at that. I mean, we're going to say it's in there, and that's the history of the numerics, the sonic component. That's why there's all of the discussion on, you know, code stability, convergence.

The concern was, you know, you want to go as fast as you can when you run this. You want to run it at the Courant choke out limit. You don't want to be stuck here at addressing the sonic -- the wave propagation from these sorts of things, and --

DR. WALLIS: So when you --

MR. MARTIN: -- the formulation equation for --

DR. WALLIS: -- open the break -- when you open the break --

MR. MARTIN: -- address that implicitly.

DR. WALLIS: When you break open the

break, does the -- a scan to the long pipe of the vessel -- does the vessel immediately know that the break is open? Does the mathematics --

MR. MARTIN: No, it --

DR. WALLIS: -- all along these nodes and get to the vessel at no time?

It does propagate from node to node.

MR. MARTIN: Yes. I mean, you'll see it -- let me explain this slide here a little bit. This is the vessel side break node pressure. You open it up here at 2250.

DR. RANSOM: That was the original pressure, 2250?

MR. MARTIN: That was the steady state pressure.

DR. RANSOM: Okay.

MR. MARTIN: Okay? And then this opens, and it immediately drops.

DR. RANSOM: It decompresses.

MR. MARTIN: And you'll see that. You can look down a little bit farther. This will dissipate. You won't see this drop. At least numerically you won't see that.

Spin, once that choke model comes on you no longer see the downstream pressure effect, and then it basically resettles, and so there's a period here of, you know, maybe, let's see, that would be ten, 20, 20 time steps before you just rebalance that.

MR. SCHROCK: What is it that's predicting this sudden drop in pressure?

MR. MARTIN: Excuse me?

MR. SCHROCK: What is it that is predicting that sudden drop in pressure?

MR. MARTIN: Well, now you're going from 2250 to 14.

MR. SCHROCK: Right.

MR. MARTIN: Fourteen, point, seven, and you know, you're applying basically -- and I don't want to say momentum equation -- but basically from the Bernoulli standpoint you've got to balance that, and so air you're sucking right up. So it just happens to be very rapid.

MR. SCHROCK: But you're treating it as incompressible flow.

MR. MARTIN: Incompressible flow, that's correct.

PARTICIPANT: That's for the first time step or so?

MR. SCHROCK: Well, I don't know. That's why I'm asking him what it is. More than one thing it could be.

MR. MARTIN: Well, I mean, the vessel, when you open at that break, I mean, obviously if I'm going down to 14.7, in which case if this volume does not get down that far you would saturate at some point.

DR. RANSOM: I think you'll find that it's not incompressible, that it's liquid, you know, which is only slightly compressible, but the methods that are being used there, you are propagating a decompression wave back into the pipe, and the recompression, of course, is when it reaches the vessel and is reflected back as a compression wave again, and then eventually the pressure stabilizes.

DR. BANERJEE: And the minimum pressure probably is determined by Alamgir-Lienard-Jones for nucleation.

DR. RANSOM: Right. It's the vapor, you know, whatever, where vaporization begins.

DR. BANERJEE: Right.

DR. RANSOM: Or cavitation, you know.

MR. SCHROCK: See, they did that in a tube. They use a forced off end of the tube. they drive the end off the tube very, very rapidly, and they get a rate of decompression by the amount of force they apply to end plug to drive it off, and then they measure the pressure at the time they see the incipient flashing.

And so how does that enter into what you've calculated here on this sudden drop in recovery?

MR. MARTIN: Well, I'll yield a lot of that to Ken's discussion at that point because he goes into more detail.

MR. SCHROCK: Well, that's a little different than the Jones use of that in a quasi-steady prediction of the critical flow with subcooled liquid stagnation condition.

MR. MARTIN: Okay.

MR. SCHROCK: That's what Jones did, Abuoff (phonetic) and Jones.

MR. MARTIN: Okay.

DR. RANSOM: Well, I don't know, but anyway, that's applied as a boundary condition. That pressure, you know, is then the break pressure. It's assumed that it can't go lower than that vaporization pressure. So it becomes a boundary condition.

MR. SCHROCK: But it can go lower than that vaporization pressure. That's what Alamgir and Lienard measured

DR. RANSOM: Well, lower than the vapor pressure, yes. The undershoot is --

MR. SCHROCK: The amount of undershoot. So is it the Alamgir-Lienard that determines that minimum pressure?

MR. MARTIN: I would say they do not case (phonetic).

DR. CHOW: This is Hueiming Chow.

I think that one is really because you discharge too much mass. So your pressure -- I mean, you have a volume, and your flow is too high. So instantaneously you lost mass. That's why pressure is brought out. This volume pressure is not a junction pressure.

MR. MARTIN: I wouldn't say this is real necessarily.

DR. CHOW: Yeah, but that's because you're starting to discharge so much mass.

MR. MARTIN: Right. Maybe what I was going to get to Moody's question is that other problems that you can probably solve in the old one are still there, but it really doesn't matter.

DR. MOODY: You've protected it.

MR. MARTIN: I'm protected it.

DR. MOODY: You've limited it so that it

doesn't go to infinite velocity or --

MR. MARTIN: Right, right. I mean, the model kicks in and prevents it from going any further. Yeah, maybe you have, you know, some unphysical response here.

DR. RANSOM: But the question is what --

DR. MOODY: What model kicks in?

DR. RANSOM: -- what boundary condition or what model do you use?

DR. MOODY: Apply then, yeah.

MR. MARTIN: Well, at that time it's the Alamgir-Lienard-Jones model.

DR. RANSOM: For the pressure undershoot, right?

DR. CHOW: Yeah, for the undershoot.

MR. MARTIN: But you turn around and use it for the velocity.

DR. RANSOM: Right, and you calculate the velocity that would correspond to that, and if it exceeds the -- I mean, if that's less than the velocity, I guess, that you calculated, then you say it's true.

MR. MARTIN: Yes.

DR. RANSOM: And then apply that as the boundary condition.

MR. MARTIN: It is just pretty algebraic. There's nothing special about it.

Anyway, I guess my point was going to be the problem is still there. It's just that it doesn't really matter.

DR. MOODY: Okay.

DR. RANSOM: You feel that you do calculate too great a mass flow rate; is that right?

MR. MARTIN: Yeah, initially, but --

DR. RANSOM: Too great relative to what?

MR. MARTIN: Well, basically based on the discontinuity, I say that because it's not a smooth result. It comes up above --

DR. RANSOM: Is it that first peak that you're talking about?

MR. MARTIN: -- and then comes back down, and of course, it progresses on. You know, I'm not going to say that I've quantified, that it's overshoot. But just based on those two slides.

DR. RANSOM: What are you going to do then to qualify your model?

MR. MARTIN: Well, there's two -- to do an application, there's a code; there's a methodology, and maybe this gets into some of what Larry will talk about tomorrow. That's why we have a CSAU process, and a lot of that is where you address what's important first.

And I think we'll look at that and say, well, this is a break flow phenomenon, and we have an approach for that that we'll go over a little bit tomorrow. We went over it a lot last year.

And something like this would be a very minor component to all of that.

MR. SCHROCK: Very minor what?

MR. MARTIN: So we would sweep it under the rug maybe.

MR. SCHROCK: I'm sorry. I couldn't quite understand what you said.

MR. MARTIN: At what point?

MR. SCHROCK: You said it would be a minor something to that.

MR. MARTIN: A minor component to the phenomenon of rate flow.

MR. SCHROCK: Okay.

MR. MARTIN: You know, we have a code which provides us a certain utility, and the next step is engineering the methodology on how to use it, and that's where you have addressed a lot of these uncertainties, uncertainties in modeling and uncertainties in phenomena, and that's why I think the

CSAU methodology is very powerful, because it takes something that's going to be less than desirable that will get through a committee like this hopefully.

MR. SCHROCK: I interpreted Fred's question to be an inquiry as to how the flow is initiated from the stagnant condition at the outset, stagnant with respect to the break, and I guess I haven't understood what your explanation for that is.

MR. MARTIN: Well, it's all driven by the large pressure drop which then suddenly appears.

MR. SCHROCK: Well, I know it's driven by a large pressure drop, but what is done in a calculation to establish the progression of the velocity at the discharge --

DR. MOODY: What I think I -- what I think I heard him say was the very first node after the break is accelerated by upstream pressure all of a sudden looking at one atmosphere or whatever the ambient is, and with that DVDT, which showed up in that first drawing or sketch you gave, trace. It goes up very fast, but when it exceeds the choke flow point of Alamgir-Lienard-Jones, that's when you artificially just cut it off and say, "No further. We're going to bring this down now."

So you're talking about the discharge of what's coming out of that end node, and the other part of that is what about node number two, node number three?

You're probably just treating those all as inflows and outflow nodes so that the propagation may or may not be real, but at least it's going from one node to the next based on your timing step.

You're controlling the discharge flow, and so it doesn't exceed anything real.

MR. MARTIN: The boundary condition to that point.

DR. MOODY: And now, let's see. If you're on the outside of that, you're concerned about what's coming into the room to pressurize. If you're concerned about -- you know, actually with the -- well, maybe you're going to get into it a little bit later about decompression moving up and moving back. Maybe you're going to discuss that when you come to the critical flow, but Virgil has asked about that.

I think what you've answered for me is the discharge during that first little bit at least is limited so that you don't over exceed.

MR. MARTIN: Okay.

MR. SCHROCK: But you're imagining that it's calculated as a plug which has a force acting on it, F equals MA, and you get an acceleration.

DR. MOODY: Yeah.

MR. SCHROCK: That gives you a velocity at the end of the time step. It doesn't tell you what the pressure is there, and the Alamgir-Lienard correlation, the correlation that gave the amount of pressure undershoot at the inception of flashing.

DR. MOODY: Yeah, okay.

MR. SCHROCK: And so I don't know how to translate that velocity into the pressure under shoot.

DR. MOODY: That may be a dangling question. At least you've answered what you do on the discharge. Right or wrong it limits the outflow. We eagerly wait for the rest of the discussion, I guess.

Your conclusion there was what?

MR. MARTIN: It may for a short period of time over predict the flow. You still use small time steps to limited that kind of problem. I'm assuming you're looking at a time scale from zero to 300 or 800 seconds, whatever it happens to be the length of a transient. You'll never see, you know, the symptom, and a short period of over prediction is inconsequential and conservative.

And I wouldn't put a lot of stake in conservative because you're talking about so little anyway.

DR. MOODY: I guess the only place that could be vulnerable is if it was a very long pipe and it took maybe the order of a second or two for a pressure wave or decompression wave to go down and come back, and maybe it has to do that a few times before you get to a steady discharge critical flow and whether or not that might be conservative or not, depending on which side of the fence you're on, I guess.

DR. KRESS: Your equations calculate the pressure of that first node. Could it be that you set that pressure at this undershoot pressure? Is that the way that works or do you actually put a cap on the flow rate?

MR. MARTIN: No, it's on the flow.

DR. KRESS: It's on the flow.

MR. MARTIN: It's not on the pressure.

Okay. Propagative flows. Please describe how moving pressure or velocity services can be tracked by the code. Do they propagate through a subsystem either as sonic, water hammer, or shock waves, the concern being volume, time average properties made to distort spatial gradients that drive propagation or pressure wave?

The statements on that. Pressure wave velocities will typically range, and it's -- looking at relap output actually can be quite broad. I just say below 1,000, although most of the LOCA calculations spends most of his time before 400, I guess, meters per second.

Most of the RCS loops are less than 50 meters. So it's moving pretty quick around the loop.

S-RELAP5 simulates the dynamics of short wavelength phenomena, pressure raise disturbances. So we're not Courant limited.

No formal effort is made to track propagation of pressure waves in S-RELAP5 because the fact that sound C (phonetic) is calculated. So --

DR. MOODY: If I can translate that, I guess, into other terms you'd say that the time response of a pipe that breaks is very short compared to the time duration of the transient, overall transient that you're trying to analyze. So you can basically skip over it, and it isn't too important.

MR. MARTIN: Right. You know, we don't really look at it and don't really see it. I think a lot of these systems are so complex that after the initial shock wave you don't see anything. I mean, things start reflecting all over the place.

I did this calculation years ago, and I think you can see, you know, some effect of a pressure wave like that, and this is a gravity wave problem where you have a stack of water up. It's a hypothetical problem where you have a stack of water up, a stack of water down, and you let go and while it does this bouncing, initially you see this little -- this echo up here, and it corresponds to that pressure disturbance. It ends up getting amplified. You go back again, and it starts damping out for that reason or for other reasons probably.

But that's the only time you see it. I've seen this. It's been obvious. Outside of that, you know, you just can't look at the hydraulic parameters and recognize a pressure wave kind of going through there.

DR. MOODY: The little wave length or the little --

MR. MARTIN: A very short one, and this one has a relatively low sonic wave speed.

DR. RANSOM: Is that a stratified flow or what is that?

MR. MARTIN: Yeah, it's just a pipe.

DR. RANSOM: The pipe with stratified --

MR. MARTIN: You've seen this problem

before. I did it back when you were there.

DR. RANSOM: This is the horizontal rhonometer (phonetic) problem?

MR. MARTIN: Yes.

DR. RANSOM: So really it's the propagation of the void wave back and forth in the pipe.

DR. BANERJEE: Sixteen meters per second. That's a fast void rate. It's almost like a two phase pressure wave, two phase.

MR. SCHROCK: It is two phase.

MR. MARTIN: Yeah, this is -- maybe I didn't talk about it. This is the void fractions at half and --

DR. BANERJEE: That's right.

MR. MARTIN: So it is.

The Framatome experience, small pressure disturbances are imperceptible in plant analysis. Break provides the singular large disturbance in LOCAs and begins to dampen when the choke model is applied quick, very early.

The only other time I can think of when you might see something is when maybe ECC came on and you had the cold water coming and you might get an effect in condensation.

Again, I can't pick up on that, but maybe there's not enough variables in RELAP5 that address that. You know, we always have those in our hip pocket. It's generally not important for LOCA application.

DR. BANERJEE: Is it true always that these pressure waves don't give you effects on core structures and things? These are not considered important for LOCA?

MR. MARTIN: That's the next one.

DR. BANERJEE: I see.

MR. MARTIN: Yeah, it's something I can't say I've previously thought about, you know, at least not for a long time, but I know a lot of effort has gone into looking at water hammer and pressurized thermoshock.

DR. BANERJEE: They seem to worry about this in Europe quite a bit.

MR. MARTIN: Yeah, I think the rules in Europe are a little bit different.

DR. BANERJEE: Yeah.

DR. MOODY: Maybe it would be a little bit more palatable if you say for the transients that are analyzed at the S-RELAP5, that your propagation effects are negligible or nonconsequential.

I'm thinking some years ago that the NRC actually used one of the RELAP programs to determine what happens when a pipe broke within a biological shield. The short side of the break, immediately steady state or quasi-steady state, critical discharge, but the long side of the break, with the way it was calculated without limited the Bernoulli fall, it shot out of there and the doggone thing about -- it reached an overturning moment on that biological shield, and that, of course, made a lot of eyebrows go up until it was shown that wait a minute; that other side of the pipe, the long side, there was a propagation effect that slowed things down while the pressure waves moved around the biological shield, and that's a propagation effect.

And when you balance the two, by the time the pressure wave returned from the pipe, the pressure wave had reached the back side on the biological shield and was beginning to even up. So it was nonconsequential.

But still it was a case where propagation effects were important. Can we say that there is no need for propagation effects then in whatever S-RELAP5 is going to do?

MR. MARTIN: I think we can say that

safely.

DR. MOODY: Will some user down the road ever been tempted to use it for -- well, maybe you had better put a big statement in front.

MR. CARUSO: Dr. Moody, this is Ralph Caruso from the staff.

I believe that this issue used to known as an issue called asymmetric LOCA loads.

DR. MOODY: Yeah.

MR. CARUSO: Does that phrase ring a bell?

DR. MOODY: I think so.

MR. CARUSO: It was resolved when we went to leak before break.

DR. MOODY: Okay.

MR. CARUSO: That was one of the reasons why we accepted the leak before break concept, was to resolve the asymmetric LOCA loads issue because this is extremely difficult to calculate.

There were some other reasons, but based on acceptance of leak before break asymmetric LOCA loads are not considered anymore.

Now, someone might ask: well, why do we consider large break LOCAs? And you would have to ask the Commission that. They acknowledged that there was a bit of an incongruity in regulations when they did that.

So we still consider them, but we don't consider the asymmetric LOCA loads.

MR. MARTIN: And that's basically what we're talking about here with that, and I'll just skip on over to the selection of the nodes.

To some extent you brought this up in Ken's discussion. I'll just overview it here, kind of emphasize the priorities we had in this step.

Please describe how various node sizes are selected at a given system, providing assurance that the dominant phenomena are predicted, are presented of the actual system response being analyzed. Of course nodalization may mask important phenomena.

Okay. This is kind of where we get into -- we move beyond code and talk about methodology, and then I've thrown up these quotes from the CSAU bible quantifying reactor safety margins. The plant model must be nodalized finally enough to represent both the important phenomena and design characteristics of a nuclear power plant, but coarsely enough to remain economical.

Number two, thus, the preferred path is to establish a standard nuclear power plant nodalization for the subsequent analysis; minimizes or removes nodalization and the freedom to manipulate noding as a contributor to uncertainty.

MR. BOEHNERT: Excuse me. Are those emphases yours or from the --

MR. MARTIN: This is from the Technical Program Group.

MR. BOEHNERT: Okay, okay.

MR. MARTIN: That did this 13 years ago or whatever.

Therefore, a nodalization selection procedure defines the minimum noding needed to capture the important phenomena.

DR. WALLIS: That's interesting. This fixing the standard nodalization doesn't really remove uncertainty. What it does is it removes flexibility in prediction. It simply forces you to use one nodalization.

There may be uncertainty associated with how well that represents the real thing.

MR. MARTIN: The idea being that it would --

DR. WALLIS: It's a fixed uncertainty.

MR. MARTIN: -- move the uncertainty to your analysis, you uncertainty analysis later.

DR. WALLIS: Yes, but you know, it means

that you don't actually investigate uncertainty.

MR. MARTIN: Well, and that's not completely true either because, you know, like has been said, the result can be very sensitive to how you nodalize things, and so you want to be careful that you do a reasonably good job.

DR. WALLIS: I think what's done is to get a nodalization that drives you to some extremes so that you're conservative. It's a departure from the best estimate code. You nodalize so that your PCT is the maximum rather than doing a whole lot nodalizations doing a spectrum of PCTs and saying, "Well, we'll take some sort of average nodalization, and then we'll look at sensitivities about it."

I think the tendency is to say, "Well, look at the extreme case and nodalize that one." Isn't that what's done?

MR. MARTIN: Yeah. You know, again, if you're fitting this into CSAU type methodology, all along you've got to be thinking we need to address uncertainty in particular phenomena, and in many cases you can just go to data and take care of it.

Sometimes, and maybe they could test examples of ECC bypass, it becomes difficult to get the right kind of data to cover that, and nodalization in this case can be used, you know, to demonstrate it's conservative against the test data because you have a certain nodalization. It can then be used to argue that you're covering that uncertainty.

And in fact, that's what we're doing as far as the ECC pipe.

DR. WALLIS: -- philosophically about saying, "Let's just be conservative."

Let's nodalize and get a conservative result rather than saying let's nodalize different ways and look at a spectrum of results in order to define that uncertainty. That's a different philosophy.

One is the old, conservative approach. The new one is perhaps the best estimate with uncertainty approach. So removing noding is -- you're essentially saying, "Let's be conservative about noding based on experience," rather than use noding as a way of looking at uncertainty due to noding.

MR. MARTIN: We do that sometimes. You know, where you can, you want to do best estimate, but you know, with limited data, ultimately I think you have to show against data that you do okay, and that really is -- from the outside I'm looking at this stuff. If I see that, you know, one, either you don't have any data to support it or that you don't do a very good job against data, then I have no confidence.

If you have a situation where you don't have data or you have limited data, I mean, that's -- we have a lot of data out there, but in some cases, maybe such as these, you have a limited amount of data, and there you might want to say, "I'm covering the uncertainty of the fact there is a limited amount of data in this area by noding up this way."

DR. WALLIS: I think with CFD what you try to do is you try to nodalize more finally until it doesn't make any difference anymore. So it's asymptotically approaching what you believe is, let's say, the right answer.

I suspect in this case there are just so many games you can play with how you nodalize different places. You're not really converging unannounced on nodalization. There's always uncertainty associated with how you nodalize.

MR. MARTIN: And again, the downcomer is a good example because when you get finer nodes, you'll get a different answer, and before we talk about the RAI, so if you have a chance to look at that to some extent, but coarse nodes, you have to think, you know, what does that do. You're modeling this downcomer or this big, round thing, and I have a point here and a point there and a point there, and that describes my azimuthal. What's my mean free path from break or from intact loop to broken loop?

Well, if I have to leap over a hot leg, then it's a bit -- well, the coarse node is shorter than if you have more detail. If you have to go through, you know, six points versus three points, my mean free path is a lot shorter with a coarse node, and that's why more water is flying out in the coarser node.

You've got to think of what do you get when you model like you do, and certainly with fine nodes you get closer to reality.

DR. RANSOM: I'd like to make a comment along those lines. I think that sort of in general that I've argued against this idea of convergence or finer and finer nodalizations, ad infinitum, with these methods for several reasons, and the most fundamental one is these are average models, and some people like to look at them as area average, but in reality they have their genesis in volume averaging methods and time average.

And so all of the parameters are finite parameters having to do with things like flow regime, frictional factors and things like that.

So it isn't true that necessarily finer and finer nodalization make much sense. In fact, my philosophy is that you could go down to as small as, say, a pipe diameter, and that might make some sense for axial nodes.

And these kind of studies have been made and these do, indeed, more or less converge, but going a little bit further to the assessment that evolved over the years from application of the law of semiscale and other experiments like that, is you can see some of these plenum models don't make a whole lot of sense from a physical point of view.

And so the question could be raise, you know, how well do they work actually, and so they applied those to the different experiments and found nodalizations that agreed with the data and were satisfactory, especially within efficiency, I guess, in the old days.

Now, today we could afford more nodes. So that's not quite as big an issue, but then the philosophy was that if it worked in that case, a prototypic experiment, you'd better no change it if you're going to go model a plant and, you know, include something about the peak clad temperature.

And so I think even a CSAU methodology spells this out, that the nodalization should be developed at the prototypic experiments and justified on that basis, and then that same philosophy used to model the plant.

DR. WALLIS: You mean it should be based on the system experiments.

DR. RANSOM: Yes.

DR. WALLIS: As a scaled system.

DR. RANSOM: Yes.

DR. WALLIS: But you don't mean full scale when you say prototypic.

DR. RANSOM: Well, if we had full scale, but unfortunately -- and I think the uncertainty has to be, you know, derived from those experiments.

DR. WALLIS: You mean a scaled experiment. So the APEX --

DR. RANSOM: Semiscale or LOFT or APEX or PUMA or any of the other experiments.

DR. BANERJEE: The problem is the idea that you're putting forward is difficult to apply to scale-up since you don't know what is the appropriate scaling parameters. You have really not done a lot of work on nondimensional groups with scale.

There is no similitude theory for these

things. So the only thing you can appeal to in some way is that the results don't change very much with the nodalization scheme that we've got.

If the results are very sensitive to nodalization, then you've got a whole new set of parameters which you can adjust, and one comment was that all he needed was three parameters, and he could fit anything. And here you've got what? Including the nodes about 500 or something.

So I don't think that argument really holds water like --

DR. RANSOM: Well, I've never heard a better argument so far.

DR. BANERJEE: Well, the argument should be that your nodalization schemes should not affect the results very much.

DR. RANSOM: Well, I think in general that's --

DR. BANERJEE: You don't have to converge in the normal sense of mathematical convergence, but nonetheless, each time you change a nodal subdivide one, your results change a whole lot and there's something totally wrong.

DR. RANSOM: Well, I think you've even heard today that small changes in nodalizations have not changed the results very much, but I think the loss philosophically -- let's say you model a T differently when you go from the prototypic experiment to the full scale plant safety calculation. I'd say that's kind of dangerous without knowing, you know, what the effect of that nodalization change is.

And in terms of scale, now geometrically, of course, the scale is contained within, you know, the structure of a code. The correlations are based on Reynolds number, Weber number, Prandial number, you know, on and on, and an attempt to make these things at least dimensionally independent, and so those become the basis of scale.

And I don't know that there's any reason to believe extremely suspicious, you know, of the scaling argument.

DR. BANERJEE: I think there is. If you've got a T junction pulling liquid out of an ADS-4 valve, and you do this experiment in a one inch pipe or a four inch pipe, and then you have this huge thing which is what, 14 or whatever inches it is? I think the phenomenon is going to not scale that way, but there, again, you can examine that from, say, boundary layer point of view and see what effect the boundaries would make on that sort of thing.

You know, there are extremes of scale that certainly you're going to find effects and you're going to have to be careful.

DR. BANERJEE: I can probably think of ten examples where you have scaling arguments which need to be made then in a detailed way and actually applied to look at the full scale plant compared to the experiments, maybe 20 areas.

DR. RANSOM: I don't profess to be an expert in this area, and a group of experts put together CSAU, and that seems to be the methodology that's being followed, you know.

DR. BANERJEE: But Graham and I were on the peer review group unfortunately.

DR. RANSOM: Well, I guess you guys can explain it then.

DR. WALLIS: The topic is node size and node scaling. I think scaling is a separate question from the node size.

DR. BANERJEE: Well, the nodes, what you're saying when Vic says fix it for these experiments and then hope for the best for the reactor, the scale is factor of ten or 20 or something different in certain areas, you know. So --

DR. WALLIS: I can see if something like

this was scaling. If what's happening in some device is, say, being governed by, say, the rate at which bubbles can be released from the stratified layer of liquid, and so it takes a certain time; if that becomes -- if the scale for that to happen relative to the scale at which things are happening on this big node size is different when you change the scale of the big node, then you've got something different happening.

DR. BANERJEE: I'll give you a classical example of this. They were doing small scale experiments on chemical reactors, which is somehow related to these reactors, and they found that emergency relief was fine.

They went to the big reactors. They continued to blow up. Okay? And it's a very simple reason that was found. It was found that the level swell in a small reactor when they do this doesn't get to the vent because the level swell depends on how much liquid there is to begin with, which scales as eight.

But when you go to something 30 feet high, the level reaches the top and you get two phase flowout. It's a very simple example of where the small scale experiment is completely wrong compared to the full scale stuff.

DR. WALLIS: -- was saying the rate of -- if you have a small scale experiment, the bubbles can detach from the liquid layer at a certain rate. If you change the scale of things, things are moving up more rapidly. The bubbles can't detach. So you entrain them because these two phenomena don't scale the same way.

DR. BANERJEE: Right.

DR. WALLIS: One happens on a scale of a foot or something, and the other one takes the node and sweeps it up too quickly for the bubbles to detect. You've got to look at these two effects. One of them changes the scale and the other one doesn't. Therefore, there may be a change in scale.

DR. MOODY: I missed something. We were talking about scale, and we were talking about nodalization. How did we make the transition?

DR. BANERJEE: Well, he was saying you can fix the scale -- nodalization based on small scale experiments and just carry it over to the reactor, and I was arguing you cannot.

DR. RANSOM: Well, there are examples where you have to be reasonable. For example, in the old Semiscale experiment, they used a pipe downcomer, and you'd say, okay, a pipe was fine for that, and then you went to a large break LOCA with an annular downcomer in a plant, and you'll find that the bypass is quite different, and so the nodalization has to be changed.

I mean, you have to temper that with some reason, I would say. After good scaling arguments, I think these kinds of things are done, but some of these things you bring up are a reason for continued research in this area that the NRC if they are safety issues should be concerned with.

DR. MOODY: Is the bottom line scaling really depends on the application? I don't mean scaling. I meant nodalization depends on the application largely.

DR. RANSOM: I don't know. I think that's true.

DR. BANERJEE: I mean, in a sense what Vic was saying was -- I recall what CSAU, the methodology was agreed on that this was a good starting point; that this sort of gives you the right sort of nodalization because it works somewhere. We don't know where, and things are relatively insensitive, but then you've got to look at phenomena, you know, which are highly ranked and make sure they're properly scaled, you've got enough nodalization, and everything is properly documented.

Now, that's not quite the same thing that's being said there though.

DR. MOODY: Go back to the earlier example where he was getting immediate discharge out of the end of a broken pipe. If you had very small nodes or very large nodes, I think that would be handled the same way. You still -- in one case a large node would take a little longer to get up in the Bernoulli flow to exceed the Alamgir-Lienard-Jones criteria, whereas if you had a very short node, it would just be a millisecond or less to get up there, but you would still limit the flow, and you are saying what really happens in a transient sense doesn't matter too much. So we might as well use about any convenient nodalization in the piping and also other parts of the system.

You must look at those and see what's happening in each part of the system. How fine do I need to know this, some property?

MR. MARTIN: Yeah, ultimately there are a number of measures when it comes to how good your nodalization is. At least, you know, I did the work, and I was looking for the sensitivity piece as number one, and if something had -- I mean that's what we're being measured on in regulatory space, and you know, you have to have your priorities.

Now, this next slide here --

DR. WALLIS: Well, let's look at another example in the case of the R-BEACON experiments. If you have a geometrically similar nozzle discharging critical flow and you go to the big size and you've got similar velocities and it takes longer for fluid to go through those nodes; if there's a relaxation process which takes a certain time, then that relaxation will occur differently relative to the node length in the big scale than it does in the small scale.

And so the assumptions you make at one scale are not necessarily -- it's like this proposal. It's a similar thing, and I think it was shown in that case that the relaxation -- that the nonequilibrium in the small nozzle is much more likely to be important than in a great big nozzle because in a great big nozzle the fluid has a long time to go through the nozzle and adjust itself to go through the same geometrical shape, and therefore, the equilibrium is less in the big nozzle.

So there's a scaling effect on the

phenomena themselves, which isn't captured by just legislating that you have the same geometrically shaped nodes in the two scales.

MR. MARTIN: Right, and that's why we have a rather large suite of assessments. We did do the uncertainty analysis on what, a six, eight R-BEACON tests, and they had some variation in scale there. It's now whole scale, and that was where our certificates came from, but at the same time we did do LOFT, and we did do the Semiscale thing where it would have a blowdown, and while that wasn't included in uncertainty, you can look at that and say, "Well, it was pretty damned close," and you move on.

Again, and also break load is something special because we also arrange break area to address that portion of the regulation. So it's somewhat unique.

DR. KRESS: What does that last sentence on that --

MR. MARTIN: Oh, I didn't read the last sentence.

DR. KRESS: What does it mean?

MR. MARTIN: This procedure starts with the analyst experience in previous code assessment and application studies and any document nodalization studies.

Next, nodalization studies are performed during the simulation of separate and integral effects, code data comparison, and finally, a derivative process using the nuclear power plant models is employed to determine sufficiency of the nuclear power plant model nodalization.

Is that the last --

DR. KRESS: It's the very last sentence.

MR. MARTIN: -- that you have a question on.

DR. KRESS: Yeah.

MR. MARTIN: That's what I call a shakedown. The model shakedown, in the previous code assessment, documentation of what people have done for, you know, the last 30 years. That's out there already.

You know, then you can, of course, play with that nodalization on the small scales, you know, for data, and then I'm just saying you can play with that on the big scale because you have other things --

DR. KRESS: You're looking for sensitivity on that last one there?

MR. MARTIN: Right. You're also looking, you know -- that's when you have to pull in the drawings of the actual plant you're actually doing, and there are structural differences, you know. The separate and integral tests are somewhat idealized at the real plant, and you know we spent a lot of time on upper plenum.

You have a certain amount of asymmetry just structurally that you have to address, and that's shakedown. You know, you want to address the important phenomena, again. You know, up there because of the asymmetry of flow --

DR. KRESS: But the problem with the sentence though is I couldn't figure out how to determine what was meant by sufficiency.

MR. MARTIN: Oh, okay. Sufficiency? That's an engineering judgment.

DR. KRESS: Okay.

MR. MARTIN: That's budget, too.

DR. KRESS: Okay.

MR. MARTIN: But we did spend an awful lot of time and Larry was in my office. "Are you done yet?"

And I would say Monday every week.

DR. WALLIS: Engineering judgment. Sufficiency is determined by engineering judgment, which is a very hard thing to quantify.

MR. MARTIN: It is a hard thing to quantify when it comes to --

DR. WALLIS: How do I know how good your engineering judgment is?

MR. MARTIN: One of the big things that I focused on was even though it's not nodalization, but time step sensitivities because that is something you can't play with to some extent.

And of course, what I ended up doing is I went down to basically what was a tolerable limit. As you know, we run like 59 cases, and for this to be a practical methodology we need to have a turnaround within a week, you know, throw in the calculations, and so you know, with the three loop sample problem we had in there, those are taking between three and four hours to run right now, and you multiply that by 60 and you get a pretty large number, and that's where I came out.

But anyway, I played with the time sensitivities a little bit using the same statistical approach that we talk about to quantify, by randomly varying time steps, to quantify what is, you know, the certainty related to these time steps, and to some extent that also translates to nodalization because that's part of it.

DR. WALLIS: What I think you do is the following. You determine sufficiency of a nodalization by thinking of ways in which it could be insufficient and exploring those and satisfying yourself that you haven't found those and that you've resolved what look like insufficiencies, and you say it's now sufficient. That's the way you do it.

We have to sort of rely on your integrity to explore all of these possible reasonable ways in which it could be insufficient and then to conclude that I haven't found any that really make it sufficient. Therefore it's okay.

MR. MARTIN: Well, that would come out of our PIRT review teams. They'd come out and, you know, I would stand up there and somebody like Mark Thorogood or Larry Hochreiter would say, "You can't do that," or, "Why don't you do this?"

The same kind of form as we have here, and you know, half the time they would have a point and go back and play with it. You know, this is a long time coming to get to this point.

DR. WALLIS: Yes.

MR. MARTIN: So it has gone through some fire to get to this point.

MR. O'DELL: This is Larry O'Dell with

Framatome. I guess I would say, too, that we did really do quite a few iterations on that in the CSAU process because we started with a nodalization that was put together based on sort of our previous experience using the code and, you know, industry experience. We did a series of comparisons of the plant calculations to see how the analysis went.

We modified it, the nodalization. Then we went back, ran a series of assessments primarily LOFT, CCTF, UPTF, and FLECHT SEASET experiments covering ranges of scales and looked at the nodalization there, made nodalization changes, went back through it again.

So this was a real iterative process that, you know, we ran an awful lot of cases in this to get this final nodalization.

MR. MARTIN: Okay. Just some of the necessary conditions that I would apply on nodalization. Number one, discriminate key structures' characteristics. This is going to the drawings. You've really got to match the drawings first.

Attain acceptable steady state agreement with the plant. Okay. There's a ton of art there. Some of that is the form losses, and we have to validate those form losses.

The reality is that Crane does a really good job. Crane and IDLECHEK do a really good job, and there's not a lot of tweaking that has to go on, and that's fortunate.

Of course, we observed the phenomenon in this, and that's really the point of your question, and that's where we apply the CSU philosophy, where we identify the important phenomena and focus on that and try to identify what --

DR. WALLIS: That's where scaling might make a difference; that if you've got, you know, a small scale, you've got more velocities, and they're small relative to relative loss to bubbles. So the relative loss to bubbles is important. In the big scale everything scales up, including the velocities.

And maybe the bubble slip is relatively unimportant so that the dominant phenomena have changed by changing the scales, and this is an important bullet.

MR. MARTIN: It's an important bullet. You know, there's not a lot of full scale data, right? So I guess in many ways the scale you address by getting the scale you have, and there is a broad scale up there. I mean somebody scaling to LOFT is a pretty big range, and there's things in between.

You can develop a lot of confidence by at least covering that range, and then if there is full scale data, then obviously that goes a long way, too. But I think we've covered that and documented that pretty well in EM-2150.

Always in the back of my mind is maintain reasonable computational economics, and on some level like the downcomer nodalization is what it is, you know, for that reason.

DR. RANSOM: How many nodes are you using in the large break LOCA in total now?

MR. MARTIN: Total?

DR. RANSOM: Yeah.

MR. MARTIN: I can't count it up, but just look at the core. We have 24 times the four rings. There's 100 there. Gosh, probably at least double that.

DR. RANSOM: Two hundred nodes?

MR. MARTIN: Two hundred.

DR. RANSOM: And that's what, for your steam generators?

MR. MARTIN: Yeah. It's probably more than 200. It's probably 300 because you multiply it by, you know, each loop.

DR. RANSOM: Now, those are pretty

computationally intensive still?

MR. MARTIN: Yes, they are still. You know, we're current generation minus one or two, you know. We have 200 megahertz machines, and you know, maybe we have a few processors so we can run some of these parallel, and then we have turf wars on who gets computer time, and that's normal engineering environment.

But you know, the three loop sample problem we provide is probably the quickest running we've got, and that's unfortunate because I thought that was the bleeding limit, you know, three and a half hour calculations, but we have -- you know, we've learned in the last year when we address some of these low containment pressure issues that you can have this large break LOCA go out 1,000 seconds before you get quench.

And that calculation is taken closer to six-plus hours.

DR. RANSOM: How much?

MR. MARTIN: Six-plus hours. so it gets a little painful, but we're kind of -- you know.

DR. RANSOM: And you've got to run 60 of those.

MR. MARTIN: We've got to, yeah.

Obviously that's what we said we were going to do, and you know, we'll just charge the customer a little bit more for it.

(Laughter.)

DR. KRESS: With the increased computational capabilities, as computers get better and better, that bullet on maintain reasonable computational economics might disappear some day. If it did, I think it would be worthwhile thinking about what Vic Ransom said, that there's probably some node size where it doesn't make any sense to go finer than anyway for other reasons.

Now, what I'd like to know is what are those other reasons, and it might be worthwhile thinking about that because I think that economics may go away one of these days.

MR. MARTIN: Wasn't there a paper that I believe Art Shay wrote about the lower limit on node sizes where you may be unable or something like that?

DR. RANSOM: I wrote some notes up about 12 years ago that argued about this averaging, you know, and what's consistent with the average model, and roughly it's like one L over D, and going beyond that, unless you're treating shocks in a shock tube or something where you know physically that it can be captured by a much smaller control volume, it doesn't make much sense.

And we apply these methods to things that involve flow regimes, for example, slugs that even with the coarse nodalizations they have extend over more than one volume, and the physics is not there, you know, in terms of how do these flow regimes change and how are they propagated from volume to volume.

I mean, there are a lot of areas like that that really are more uncertain I would guess than some of the things we're dealing with. I mean, it's got to get right, you know, and the onus is on Framatome to get it right so that it can be understood.

DR. BANERJEE: In the approximation -- I mean, I don't really want to argue this because I think it's fairly clear that you have an interpenetrating continuum model here. So when you do that from any other field of polymers or whatever, this has to go down to a mathematical convergence equations. There are two fluid equations which are written in many fields. This is not the only field, and they all converge. This is the only field that they don't, in fact.

And the reason they don't most of the time is some physics is left out.

DR. RANSOM: Well,f or example the nodalization studies that have been made with these methods are like Edward's pipe problem, you know, fairly simple things where you go to maybe 1,000 or 2,000 nodes, and indeed, they converge. I mean, there's no question about it. They get to the point where they don't change anymore even probably at 30, 40 to 100 nodes.

But I'm not sure that makes any sense, and those are, incidentally, down at where the L over D is much less than what I'd recommend that these things be applied to.

DR. BANERJEE: Well, I think they should, but leaving that aside, the days when you were running the code and so on, one could defend 300 nodes as being sort of a computational problem. We routinely run problems with ten to the sixth to ten to the nine nodes now in some of the big machines.

DR. RANSOM: Right.

DR. BANERJEE: Ten to the nine, of course, is the outer limit, but ten to the six is very common.

And I don't understand what the big problem is. CFD people run this all the time.

MR. MARTIN: Let me give some perspective. We have -- I go to these RELAP5 3D meetings occasionally, and Bettis has been playing with linking RECAP to CFD, and they note basically they've done a large break LOCA where they've taken CFD and handled the core with, you know, I'm sure ten to the sixth order type thing, and then the rest of it is RELAP and LOCA.

It took them eight weeks to complete. So I think we're still a ways away.

DR. BANERJEE: It depends on how you run it. The CFD codes are run on clusters. We run CFD codes on 32 node clusters with 64 processes. They run fast.

MR. MARTIN: And you get a good deal from the manufacturers at universities and stuff.

DR. BANERJEE: You can build a 64 process cluster for $50,000. I'll build you one. Give me the money.

DR. RANSOM: Santa Barbara is ahead of its time.

(Laughter.)

DR. BANERJEE: It's just like neighborhood clusters. It's not that big a deal.

MR. MARTIN: In time, you know, we'll improve these things. I mean, we are --

DR. BANERJEE: So are you running these on sort of a two gigahertz processors or what?

MR. MARTIN: Two hundred, 200 megahertz. Well, like I say, we're current generation minus one or two, you know, and these are relatively new machines to me, and so -- but that cost us 20K, you know, something. You might get four heads or something like that, and we still pay 20K for that.

DR. BANERJEE: These machines are costing you 20K?

MR. MARTIN: That's what they charge, you know, the old companies.

DR. BANERJEE: I can go out and buy a PC which does --

MR. MARTIN: Yes, we know that. We get mad every time they come back with a quote, and maybe one day we'll just move everything to a PC platform and do it ourselves, but that's a big effort, too, because we have our own qualification procedures that are required.

DR. BANERJEE: So you run these on what machines?

MR. MARTIN: On Hewlett-Packard.

DR. BANERJEE: Hewlett-Packard what?

MR. MARTIN: K -- it's called K-box. K-500 or something like that.

DR. BANERJEE: And you can't run them on a Linux machine?

MR. MARTIN: Our codes aren't qualified for Linux machines, and again, we have our own --

DR. BANERJEE: Oh, they're not?

MR. MARTIN: -- out own -- I don't know who actually determines our qualifications for platforms, but we got on HP ten-plus years ago, and that's where we're at, and a migration is not a trivial task.

DR. BANERJEE: These are not reportable to machines which are like Fortran, Linux machines?

MR. MARTIN: We can do -- I mean, you know, we have enough hacks around. I've ported to a Mac, you know. Chow has ported it to Linux, and you know, we play our games at home, but when we do production runs, we've got to keep a standard, and we've chosen the Hewlett-Packard platform for that

DR. BANERJEE: So the result change when you run it on different machines?

MR. O'DELL: No. This is Larry O'Dell of Framatome.

DR. BANERJEE: I would really like to know this.

MR. O'DELL: Well, no. It's not a matter

of the results changing. We have a process we have to go through to qualify the codes, and we move them from, you know, one compiler to another compiler or one operating system to another operating system on the HP, and if you move to another computer you end up having to go through this full qualification process of a code because we're allowed to use it in licensing analysis.

Now, we can just port the code over and play with it and stuff. That's not an issue, but we have to go through this qualification process, you know, in order to have an Appendix B qualified code, and that is not a minor process. It's costly, and it takes a lot of time to do that.

So there's some resistance built into the system to being able to move to a code, to another platform and then use it.

DR. BANERJEE: So do the results --

MR. LANDRY: Sanjoy.

DR. BANERJEE: -- or not?

MR. LANDRY: Sanjoy, if I may, this is Ralph Landry from the staff.

Our regulations require configuration control of a licensing code. That means it must be frozen. It's approved for a particular machine, particular compiler. We don't allow changing that without rereviewing the work of the vendor.

That is not to tie their hands, but it was intended to keep people from making changes in codes without our knowing those changes were made.

Today -- now that was written back in the mid-'70s. Today you can go out and get a slew of machines, all of them compilers, and run the codes, yes. But you're not maintaining configuration control when you do that.

But there has been a history of different machines, whether they are using big ended or little ended CPUs in giving different results. So a code is compiled and run on a particular platform. It's proven on that platform. To change it you have to get permission, and you have to go through the entire requalification program.

So it's not not a matter of whether they can go out and buy a Linux box for $900 versus an HP for $20,000. It may cost them more to requalify the code to go to that $900 box than to buy another HP when you consider the cost of what it takes to go through the QA process.

So we're not trying to tie their hands with that. This is to maintain control of a code that is regulated under the Code of Federal Regulations.

DR. BANERJEE: But if they qualified the code, what is involved, that the code gets the same results or that its results change depending on the platform?

MR. LANDRY: There's an enormous --

MR. CARUSO: It would be like a new submission.

MR. LANDRY: There's an enormous amount of paper attached to this, The manpower, the staff loading, the paper work.

MR. CARUSO: The documentation is just enormous.

DR. BANERJEE: So they're sort of frozen to one platform or what?

MR. CARUSO: Pretty much.

MR. LANDRY: When we say a frozen code, we mean that that code cannot have anything changed in it without notifying the NRC. That means they can't change a light in the coding without telling us. They cannot change the compiler without telling us. If they change the platform, they have to change the compiler.

DR. BANERJEE: So they're the process of qualifying this code right now or it's already qualified?

MR. LANDRY: This code is a frozen code. We have the version number. We have the version numbers for the codes that are interfaced within S-RELAP5, and that defines which platform, which compiler, which operating system, which we happen to have one to match up with.

DR. BANERJEE: But this is a new code, right, that they're qualifying?

MR. LANDRY: Well, it's the sort of code that's been under development for a decade.

DR. BANERJEE: Right, right, but this has to still be qualified, S-RELAP5?

MR. LANDRY: They've already qualified before they come in here.

DR. BANERJEE: It's already qualified?

MR. LANDRY: It has to be. We won't review a copy -- and I'll talk about this tomorrow -- we don't review a code that is not a frozen code. It must be frozen, and it must be under configuration control before we will review it.

DR. MOODY: Well, that should solve all of the problems really, shouldn't it? If somewhere along the line someone wants to put it on another system, why, then there had better not be any variation in answers because the code is just making mathematical -- it's counting stuff.

MR. LANDRY: It's not very easy to move the code to different platforms. One of the other vendors is still running on the VAX, on the VMS platforms, VMS operating system, because they don't want to transfer to UNIX. It's too much trouble.

DR. WALLIS: That's truly remarkable.

DR. BANERJEE: I didn't know this, This is a discovery.

(Laughter.)

MR. LANDRY: If I could make one other comment, we were getting pretty far afield from --

DR. WALLIS: Yes.

MR. LANDRY: -- what Dr. Martin was trying to talk about here with nodalization. The CSAU --

DR. WALLIS: I'd like to move on.

MR. LANDRY: Huh?

DR. WALLIS: I'd like to move on, but why don't you see if we can wrap up this one?

MR. LANDRY: Can I make it real quick?

The nodalization concept that was put forth in CSAU was to try to get a consistency nodalization approach to the different code modelers. At the time CSAU was written, everybody and their brother was using a different concept and different approach to nodalization.

What this was trying to do was to get everybody to a consistent approach that would be consistent with the nodalization that was used on the experiment evaluations and experimental assessment programs.

The nodalization that has been used in the past, somebody's wife was using a nodalization on AP600 of 1200 nodes. We had them run a simplified version of that code with under 600 nodes and got very much the same answers.

You can get ridiculous in this, and what this whole process is trying to do is say put some rationality, put some sensibility and put some consistency in the approach you take in nodalizing experimental programs and the nuclear power plant.

MR. MARTIN: You're referring to my wife.

DR. WALLIS: Can we go on?

MR. MARTIN: Sure. My last bullet, I guess we can stop here. We've already hit these things: maintain scalability, important; and accuracy; numerical stability; and convergence.

And then the conclusions and you can get onto, I guess, the next thing, but initial nodalization is based on experience. Then we go revised based on plant studies and assessments, and then we validated to the performance of final assessments.

That's kind of the end there of that.

DR. KRESS: How would you start out nodalizing ESBWR?

MR. MARTIN: What do you mean? Our SWR-1000 that we've kind of thrown on the docket here?

DR. KRESS: Yeah. How would you do that?

MR. MARTIN: Well, first, there isn't really a lot of WR experience.

DR. KRESS: Not much experience there.

MR. MARTIN: And RELAP5. There is some. I believe Brown's Ferry. We do have an old Decker (phonetic) there. So it might be a beginning. I can't tell you I've looked at the design.

Probably we start throwing something together initially, and we've only got to capture the phenomenon, you know.

DR. KRESS: It would probably build on your experience you've had with the --

MR. MARTIN: Sure. We have building experience here. We did EMF-2102, does have a couple of GE tests, you know, 1,000 psi tests, and it's touchy-feely in the beginning, and --

DR. KRESS: That's where some tests would be helpful.

MR. MARTIN: Exactly. Tests are paramount definitely. No, it will be a tough process, a long process to get that down. You know, the phenomenon and the question itself is key. In my opinion, you know, as far as large break LOCA and BWRs, the big players are break flow, and of course, our treatment is pretty broad and it covers a whole break spectrum in the same process.

You have heat transfers is important, you know, the important one, and ECC bypass is important, and then everything else kind of tapers off real quick as being, you know, important for this application.

BWR, I'm not quite so sure you can just have a few dominant things and win that way. I mean, you talk about the ADS stuff and maybe the AP600 test. You know, that's a phenomenon there.

Obviously Ralph referred to AP600 work that my wife worked on in Idaho, and the code didn't always work. More often than not, it didn't get the right result. There was a lot of code versions that we went through, and I would anticipate that we'll do the same with the BWR work, as well as SWR-1000 once we get down that path.

I mean, it's been ten years, 15 years to develop this methodology, and it won't be trivial to go to the next step, but we have a lot of experience now, and we can build on that and be more efficient.

MR. O'DELL: This is Larry O'Dell with Framatome again.

I would also say that, you know, our counterparts in Germany have been using S-RELAP5 to do BWR plants in Germany already, and we have been, you know, interacting with them in Germany to get their at least initial nodalizations for these types of plants.

So we're using that pretty much as a starting point.

DR. BANERJEE: Ralph, let me ask you: is this rule also used for reactor physics codes and everything?

MR. LANDRY: No. Ralph Landry, staff.

Ten CFR 50.46 applies to loss of coolant accident analysis programs only. It is specifically written and applies to light water cooled zirconium or Zircaloy clad uranium dioxide fuel reactors. It's only for calculating LOCAs. It does not apply to physics.

We have other regulatory guides and

standard review plans that we use to apply to other modeling techniques, physics, transient analyses, et cetera. But there's only one Code of Federal Regulations statement with regard to analysis, and that is with LOCA.

MR. CARUSO: I would make the observation that Appendix B, the quality assurance standard, has all of the safety related methods.

MR. HOLM: right. This is Jerry Holm with Framatome.

I would say that it's the Appendix B requirements that are making us spend all of this time and effort validating the code. Fifty, forty-six requires that we inform the NRC on the LOCA codes. So that's what we would do for LOCA codes.

I would change the physics code without telling the NRC, but I still have to validate it under Appendix B if I move from one platform to another. So I'll rerun a whole suite of test cases to verify and get the same answers.

And we've had the same experience the NRC has had, that we've moved from one platform. In fact, we've moved from one compiler to another compiler and got different answers. Sometimes we've discovered errors in the compilers that were provided to us.

MR. CARUSO: And there are NRC codes that will give you different answers on different platforms.

MR. HOLM: So, you know, we have about 160 codes, and we move from one platform to another. We have a very big job ahead of us.

DR. BANERJEE: So they compile, but they give you different answers.

MR. CARUSO: Give you different answers.

DR. WALLIS: It's just very strange to me because we will have sort of students running fluent on different platforms for homework, and we accept any of the answers, and we haven't run into problems that we're dependent on the platform or the compiler.

MR. CARUSO: There's one NRC code -- I won't say which one it is -- but it had a standard run time of 100 seconds with a standard problem, and that's how you check the installation. You ran it for 100 seconds, and if you got the same answer as the standard problem, then you declared that you had installed it successfully.

Well, one foreign user decided to run it past 100 seconds on two different platforms, and the problems diverged.

DR. WALLIS: Okay. It's an interesting

world.

(Laughter.)

DR. BANERJEE: It will pose problems.

DR. WALLIS: Can we move on to the critical flow model? I think these were questions raised by Professor Schrock. Is that what you're going to address now?

MR. CARUSO: Yes. If I may, you have one slide with proprietary material on it, and it's a very small piece here. Is it possible for you to just talk around those numbers so we don't have to go into closed session?

I mean, people can look at the numbers. Is that acceptable to you guys?

DR. KRESS: Yes.

MR. CARUSO: Okay. Let's do that then, and I'll make sure that that does not show up in the open portion of the transcript.

DR. WALLIS: Now, Virgil, since these are your questions, I think you should have real priority in asking them.

DR. RANSOM: Thank you.

DR. WALLIS: And being satisfied or not by the answers.

MR. CARLSON: Yes, and so --

MR. SCHROCK: I looked at your first viewgraph, and I have to conclude for this to make any sense to other people, you need to either paraphrase or read the question that was put to you.

MR. CARLSON: Right. Yes, I believe that should have been, I believe, on this slide, and I apologize for that.

MR. SCHROCK: Well, it's a little bit lengthy, but self-choking discussion as you have it in three bullets is out of context for the question posed.

MR. CARLSON: Oh, okay.

MR. SCHROCK: So I don't know if you misunderstand the question or --

PARTICIPANT: What is the question?

MR. CARLSON: Well, let's see. "A numerical computation of critical flow in pipes, therefore, necessarily requires very fine nodalization as the critical flow location is approached. These realities are not reflected in S-RELAP5 critical flow model, which should be applicable to real geometries where friction often plays a role. Please provide a rationale for answering that . . . model in the context of the above discussion."

There's also more discussion about

convergent nozzle geometry and other appropriate geometries.

MR. SCHROCK: Well, it begins with a statement that it's based or inspired by the Ransom-Trapp model.

MR. CARLSON: Right.

MR. SCHROCK: Which is a model based on the assumption of thermal equilibrium slip flow at constant entropy, and constant entropy assumption limits the application to convergent nozzles. That's a key statement in the preamble to the question.

The geometry of the break in general is not a convergent nozzle.

MR. CARLSON: The geometry of the break --

MR. SCHROCK: In order to achieve a constant entropy flow you need that specific geometry.

MR. CARLSON: Well, I believe the rationale was that the model was developed assuming constant entropy conditions to develop the model, but then it is applied at any time where there is a large pressure difference between an upstream and a downstream node.

And the process of using that model would limit the velocities and considered to be choke or a choke point where -- and for the critical flow, it was assumed that wave propagation information downstream does not propagate upstream to the --

MR. SCHROCK: That's not an issue in my question.

MR. CARLSON: Right.

MR. SCHROCK: The issue has to do with what is it that can cause the flow to be accelerated in the channel. Three --

MR. CARLSON: Right.

MR. SCHROCK: -- physical factors that are involved are area change, okay, area change --

MR. CARLSON: Friction and volume --

MR. SCHROCK: -- high friction and heat addition.

MR. CARLSON: Yes.

MR. SCHROCK: And you're ending up with a statement down here in which you are saying friction and heat addition play no role in LOCA. So it's not consistent.

MR. CARLSON: Yes.

MR. SCHROCK: You don't have a convergent nozzle.

MR. CARLSON: You don't have a convergent nozzle.

MR. SCHROCK: You do have a change of

state which moves the fluid towards the point of choking.

MR. CARLSON: Yes.

MR. CARLSON: But you don't have an answer as to what causes that change of state to move it to choking. You can't do that in a straight pipe if you do not have friction or heat addition or both.

MR. CARLSON: Would Dr. Chow like to help me out on this?

(Laughter.)

MR. CARLSON: The answer is no, apparently not.

DR. CHOW: Originally what I tried to say, that the equation like the constant entropy, that's for the wave disturbance, for the -- I mean, basically that's -- you cannot say that's for the -- remember when you derive the sun speed (phonetic)? You always use constant entropy. That's because you are leading with wave disturbance for very small distance, very small distance and no entropy change.

So basically that answers the question about when you try to provide the wave equation for the choke, that's not really the full equation for the flow. That's the full equation for the wave disturbance.

So for the wave disturbance, it's a constant entropy process. So in terms of the friction choke, you are talking about compression drop. In the gas, you have a long, very long pipe. Okay? Then your pressure will be decreased because of friction.

And in this case your pressure is a very, very long pipe, but that's done like this in the reactor. We don't get over 300 or 400 long pipe, like that. Okay? So --

MR. SCHROCK: Three hundred or 400 what?

DR. CHOW: Feet, 300 or 400 foot long pipe, like a very, very long pipe. Okay? That's basically -- and you have to have a compression flow. So you have a density change. Basically in order to be a friction choking, you basically have to have a very, very long pipe. Along the pipe the pressure drop-in, and your density for that, the density will be decreased. So because you have constant flow, so your velocity will increase.

At a certain point you will reach a choke point where the speed will be equal to the sun speed (phonetic). Okay? So in the compressible flow, that exact phenomenon of the friction choking is there, but I don't think in the reactor system you can find account the friction choking at all.

That's what probably says that it's really in the gas dynamics. In the reactor system, for example, the flow is based with more incompressible. I don't know that word. There is no idea that compressible, I know. So you don't have NCI (phonetic) say the density will be decreased along the long pipe like that. so --

MR. SCHROCK: I'm having difficulty hearing you well, and I certainly don't understand the point, but I do seem to be hearing that you're making some distinction between compressible flow and some other kind of flow that you imagine exists in the reactor application. Am I correct?

DR. CHOW: Yeah. I'm talking about --

MR. SCHROCK: The fluid is, in fact, a compressible fluid in the two phase state.

DR. CHOW: Yeah, I understand that.

MR. SCHROCK: Okay.

DR. CHOW: Yeah. From choice --

MR. SCHROCK: And all of its behavior is characterized by the gas dynamics arguments that are developed in Shapiro's text. It's not as though when you go to two phase flow you've created some different kinds of processes that lead to choking. It's the same processes.

DR. CHOW: Yeah, I understand that. I'm just --

MR. SCHROCK: And so I'm not able to understand what your argument is.

DR. CHOW: Yeah. I'm trying to say that when you depend on your -- in order to have friction choke, you have to -- pressure have to be decreased along the pipe. Okay? The pressure --

MR. SCHROCK: Yes, and in any choking process, the pressure is decreased along the flow direction --

DR. CHOW: Yeah, that's right.

MR. SCHROCK: -- as you approach the point of choking, and so the issue that I raise is simply that there are three possible ways that this can occur independently or in concert that will lead a one dimensional flow to choking, and those phenomena are the change in cross-sectional area, reduction in the cross-sectional area, the effect of friction, and the effect of heat addition.

The Ransom-Trapp model, which is said to be the basis for the RELAP5 critical flow model, has as its initial assumption that the two phase flow is, in fact, an equilibrium flow with slip and constant entropy.

But to achieve constant entropy, you need area reduction. So you cannot have the approach to critical flow as described by the Ransom-Trapp model for the case of a straight pipe. Okay?

Now, there are a whole range of geometric possibilities from a straight pipe to a convergent nozzle, depending on the rate of reduction in the area. You look at the Marviken geometry, and it's nearly a straight pipe. It's not quite a straight pipe, but it's nearly a straight pipe.

You have to have very high mach numbers as you come into the discharge pipe in Marviken. It's not as though you come in with a low mach number and you accelerate to a very high mach number at the outlet of the discharge pipe. In fact, it has to come in at a very high -- because there's very little distance left for the friction to act, very little area reduction to drive it to the critical state.

Okay. So in the real geometry of a broken pipe guillotine break presumably you have two straight pipes, short, admittedly short straight pipes, but they are straight pipes, and in order to get to the critical state in those straight pipes, you have to account for it through friction.

DR. WALLIS: Why doesn't RELAP do that?

RELAP has friction and all of that.

DR. RANSOM: Let me -- there's a big misconception here. The Ransom-Trapp model was just a method of characteristics to derive what is the choking criterion, you know, V plus or minus the speed of sound equal to zero, which is a stationary wave. And so we came up with an expression for the speed of sound that would apply.

It's a local criterion. It's not an integrated criterion that you would apply all the way down a pipe.

In terms of what properties that speed is then based on is the nearest node, you know, the nearest node to the break. And so in that section, indeed, if there is heat transfer, area change, it has to be taken into account there.

And you know, certainly in the classical sense area change, heat transfer, although Shapiro only deals with in that section, I think, steady flow process that like in a rocket nozzle or something of that type, but --

MR. SCHROCK: Well, isn't this quasi-steady?

DR. RANSOM: At that last node it is a quasi-steady.

MR. SCHROCK: Quasi-steady. So it's not an issue.

DR. RANSOM: It's derived from transient equations. So I don't think there's --

DR. CHOW: Yeah. I mean, in terms of the code, the code take care of the friction. It is called Adam, the friction. So the phenomenon is out. It may take care if there is their friction there.

So while all they try to say that the criteria is just only for the wave disturbance, so --

DR. RANSOM: You say on the next viewgraph, "Friction and heat addition mechanisms are important for gas dynamics, but do not play a role in LOCA."

DR. CHOW: What we --

DR. RANSOM: That is absolutely wrong.

DR. CHOW: What we try to say, that that's the -- just we -- basically all of these frictions still in there. Okay? And trying to say in order to achieve that kind of friction choke defined by the Shapiro and that kind of classical case, it doesn't appear in the from (phonetic). Trying to say that the Shapiro, the classical case, to have a friction constant flow in the sense of that does not appear in the actor (phonetic) because you have to have a long pipe in order to get this friction going down along the pipe. That's all I am saying.

DR. WALLIS: Well, isn't Ransom-Trapp just used as the choking criterion?

DR. CHOW: Yeah, just --

DR. WALLIS: Just like M equals one. You're saying what's the effect if M equals one criterion that's used in Shapiro for both the friction and the added heat addition and the area change.

DR. CHOW: Yeah, yeah.

DR. WALLIS: It's the same. You use an isotropic M equals one as a criterion at the very end of the pipe no matter how you got there.

DR. CHOW: Yeah, that's right.

DR. WALLIS: Isn't that what you're doing here?

DR. CHOW: Right.

DR. WALLIS: You're simply saying no matter how S-RELAP5 gets there, when it gets to the Ransom-Trapp criterion we'll say it's choked even though RELAP5 itself isn't running into any kind of an infinite pressure gradient or anything.

So you're imposing a different kind of M equals one than RELAP5 itself would predict, but you're using that and saying, "Ah, ha, it's choked."

DR. CHOW: Yeah, that's right. That's right, yeah.

DR. BANERJEE: Is that because the equations for RELAP5 doesn't contain your characteristics or what?

DR. RANSOM: Well, as you well know, the equations are ill posed supposedly, and if you look at it in a differential sense, they've got complex characteristics, but --

DR. BANERJEE: So how did you --

DR. RANSOM: -- what Trapp and I, we factored the equations, and we threw away the imaginary part of the characteristic groups and then only looked at the real part, which was presumably the real space propagation rate and show that that comes out to be very near the homogeneous equilibrium speed of sound, and it varies with void fraction, of course, and you know, the density ratio.

But I don't know. It's an approximation. I mean, it's -- but I haven't seen anything better, I guess, at this point.

DR. WALLIS: So S-RELAP5 itself isn't predicting that there's some kind of critical event occurring, that you can't get anymore flowout.

DR. RANSOM: Well, generally the idea was that you would let RELAP5 calculate until it calculated a velocity that exceeded the speed of sound, and then you'd say, well, this implied a boundary condition.

DR. WALLIS: It seems to me the S-RELAP5 might itself have some characteristics which would lead to a prediction of choking before you reach the Ransom-Trapp criteria, in which case you'd be predicting infinite pressure gradients in that last node --

DR. RANSOM: That's possible.

DR. WALLIS: -- before you've reached the Ransom-Trapp model.

I don't know what you do then if Ransom-Trapp is your criterion for choking and you haven't been able to get there because S-RELAP5 won't let you get there.

Not taking it away from -- that was the problem I had here, was if you're imposing a choking criterion which doesn't naturally follow from your own equations, you could get into some problems knowing which one to use under some circumstances.

DR. BANERJEE: Well, but you know that many people like the French and a lot of people put physical effects in to make the characteristics real, not artificially, but by adding a bubble or whatever.

DR. RANSOM: Well, I would argue that their methods are as artificial as putting in numerical viscosity. I mean, they added things which were artifacts and --

DR. BANERJEE: -- based on physics.

DR. RANSOM: And I have a paper I've written that goes into that subject, but what you show is they artificially stabilize the solution long before you'd see stabilization as a result of, say, turbulent phenomena and, you know, the real dissipated mechanisms.

So it's as artificial as --

DR. BANERJEE: Well, without arguing that, you know, that's a very detailed argument. The issue would be more whether imposing something on the outside when it doesn't arise naturally in the equations might lead to certain -- well, we know that it leads to sometimes on physical effects when you try to choke things.

For example, if your choking went above the sound speed of the homogeneous equilibrium model and the situation is such that the flows were closely coupled, you'd get choking in the pipe at multiple points perhaps.

DR. RANSOM: That physically could happen. And in fact, when we originally did this, we used to allow checking for choking at every point within the pipe, and I think over time they've gotten away from doing that because it tended to cause a lot of numerical difficulty, you know, choking, unchoking, choking, and unchoking type of thing.

And I'm not sure I'd recommend that because, in general, in the LOCA type of problem, you know where it chokes, you know, at the exit.

DR. MOODY: Let me try to help.

DR. RANSOM: Or unless there's a contraction upstream somewhere where it might choke like Virgil has brought up.

DR. MOODY: But going back to Virgil's original concern, I think the thing he was asking was, first of all, choke flow, constant entropy flow, critical flow at the end of a converging nozzle enables you to go from a stagnation condition to a state of mach equals one for whatever kind of fluid you're using.

And, in fact, that can happen anywhere, can't it? In a pipe where you take the local stagnation pressure, which may have experienced a lot of friction loss along the way and come up with a mach of one, and how RELAP may do it -- I know how Shapiro does it. He integrates over a pipe with friction setting as a boundary condition the mach equals one at the exit and then determining what length pipe is going to take you from stagnation state to that condition.

DR. WALLIS: But he's very lucky in that the mach one is also inherent in the equations he's using. So he's going to find it one way or the other.

Here we've got the equations that are used not being consistent with the mach one Ransom-Trapp model.

DR. RANSOM: You've got pressure --

DR. WALLIS: Or it could be the Mood model, for instance. Any model

DR. RANSOM: Any model.

DR. WALLIS: Any model which is not S-RELAP5.

(Laughter.)

DR. MOODY: But you do have pressure, velocity, and density varying along the pipe by friction, and at some point your pressure, velocity, and density are going to reach a state where the sound speed which is a function of pressure and density will match the velocity.

DR. WALLIS: Then it predicts that PDZ is infinite, and you can't get any further.

DR. MOODY: Okay. That's a standing pressure --

DR. WALLIS: I think we're concerned that S-RELAP5 might predict DPDZ as infinite in a way which is inconsistent with Ransom-Trapp, and which one do you pick. Isn't that part of the problem being raised here?

DR. MOODY: Excuse me?

(Laughter.)

DR. MOODY: I didn't follow your --

DR. WALLIS: Oh.

DR. MOODY: Well, I think your concern was real, and just piecing together some of the things that have been said, it sounds to me like you're not assuming isentropic (phonetic) flow through the entire pipe.

DR. WALLIS: No, no.

DR. MOODY: That's the condition for sound speed, is DPD rho or constant entropy, right?

DR. WALLIS: Right, at local condition.

DR. MOODY: But that local condition means the local entropy, which may have been really butchered up by friction all the way down the pipe.

MR. SCHROCK: Yeah. Well, your rationale is, to answer my own question, that they used the Ransom-Trapp model only as a characterization of choking based on local conditions.

DR. CHOW: That's right. That's right.

MR. SCHROCK: And then the code has to calculate the approach to that. I think there's some difficulties in the numerical work which is done in approaching the critical point.

DR. CHOW: I think your question probably is that when you do that, you have a long pipe, very long pipe, and you may have a choking point, which is actually before the choke --

MR. SCHROCK: Well, you have a very long node just upstream of the location where the gradient is extremely strong, and so how you can establish any degree of accuracy in that computation is a problem.

DR. CHOW: Yeah, we don't have a very, very long node, and basically you have a few hundred feet, you know, to adhere --

MR. SCHROCK: Are you disagreeing, Graham?

DR. WALLIS: I just think they can't have a long node. They must have some fine noding near the critical --

MR. SCHROCK: No, they don't.

DR. WALLIS: They don't?

MR. SCHROCK: They don't.

Well, what is your noding in the assessment calculations? On Marviken I think you've got about three nodes in the discharge pipe.

MR. SCHROCK: If you plot the pressure profile --

DR. CHOW: -- about five, I think, five or six nodes under discharge pipe.

MR. SCHROCK: You think what?

DR. CHOW: Yeah, the Marviken is actually choking in the throat, in the nozzle, not in the discharge pipe. They have a very long discharge pipe, and choking is not happening on the discharge pipe.

MR. SCHROCK: Well, I think you need to show the Marviken geometry again if you're going to talk about a throat. It goes into a section which is straight, and then it goes into a section which is small constriction, very small constriction, and then it has a section of divergence; is that correct?

DR. CHOW: The vessel --

MR. SCHROCK: And the variations are all very gentle.

DR. CHOW: Yeah.

MR. SCHROCK: Very little change, very

little change, not very different from a straight pipe.

DR. CHOW: That's right, yeah, and in this case --

MR. SCHROCK: And if it's not very different from a straight pipe, then it does mean that you're calculating on the last node upstream of the point of choking over a very wide range of thermodynamic conditions, a very wide range of thermodynamic conditions, and you're not going to capture the condition at the minimum area point with very satisfactory accuracy.

DR. CHOW: Well, it means that the choking, where there is actual choke in the nozzle is -- that's what in terms of that we don't know where is actual choke in the -- we just basically say that apply the choking criteria at another pipe. So that's what, and so we did calculate from the base to the discharge pipe to the nozzle. These all -- everything is calculate at that, and you are talking about maybe the choking will occur in some other place other than the nozzle, but I don't think that's --

MR. SCHROCK: I don't think I said that. I haven't talked about choking occurring at some place other than a nozzle. What I'm saying is that Marviken geometry has very little area change from the beginning to the outlet. Okay?

DR. CHOW: That's not -- the vessel is quite big. Then the discharging pipe, so in thermal that is not really true. You have a big vessel, then you have a discharging pipe. Then the discharging pipe and not the nozzle is about the same, but from the base to the discharging pipe is probably different area.

MR. SCHROCK: Okay. I think we're getting nowhere with this one.

DR. WALLIS: Yeah, I'm puzzled, too, because it mentions the Ransom-Trapp model and then there's some kind of another empirical criterion in this equation 520, and then there's something about setting the apparent mass coefficient to infinity, and then there's the homogeneous equilibrium model invoked.

These are all different models for choking.

MR. CARLSON: Right. It's --

DR. WALLIS: I'm not sure which one is being used.

MR. CARLSON: Well, we should have put in the questions and then the response would be --

DR. WALLIS: That's right.

MR. CARLSON: Yes.

DR. WALLIS: A bit clearer there.

MR. CARLSON: So I guess the special case for subcooled upstream stagnation states is treated by our methods developed by Abdaf, Jones and Wu (phonetic) for flow converging nozzles. In this case, flashing inception is thought to occur at the throat and pressure below the saturation pressure.

Pressure is predicated by a critical correlation by Alamgir and Lienhard, and modifications due to Jones. The S-RELAP5 documentation is unclear.

Question A, how does Jones define A in equation 522?

Well, of course, I don't have --

MR. SCHROCK: In S-RELAP5 it appears to depend upon noding choice.

MR. CARLSON: Right.

MR. SCHROCK: It's part of the question actually.

MR. CARLSON: I can't see. I can't read this.

Let's see. The liquid fluid at the throat is calculated as 524, and I think --

MR. SCHROCK: If you keep going down --

MR. CARLSON: Yeah, yeah. What you're referring to is the A, Jones supply.

MR. SCHROCK: That's right.

MR. CARLSON: Okay. And I think Jones describes it as the upstream flow area, and we set it to the volume flow area, what we refer to as the volume flow area that's at the center of the volume length.

DR. WALLIS: It's at the center of the volume length?

MR. CARLSON: Well, it's the center. It's the -- the area is constant throughout the volume, and so whatever you define to be the volume flow area is --

MR. SCHROCK: When you say Jones defined it as the upstream flow area, with reference to what? The experiment of Alamgir and Lienhard?

MR. CARLSON: I believe it was to the throat. I believe it was to the throat.

DR. WALLIS: Well, there's an At over --

MR. SCHROCK: The throat is in the numerator, At divided by A.

MR. CARLSON: Right. And so --

MR. SCHROCK: See, Alamgir and Lienhard did this in a straight pipe and used an explosive charge to blow the end off a straight pipe very rapidly so that they would get a very high rate of decompression.

So I looked at your numerical work, and then I looked back at this equation, and I couldn't understand how you match what you're doing in your numerical work with what Jones had here.

And then it raised the question in my mind what did Jones mean by A. He's got an At, which presumably means throat, and then there's an A.

MR. CARLSON: Well, I believe Jones defined A as the upstream flow of --

MR. SCHROCK: Upstream in what geometry?

MR. CARLSON: In terms of a converging or the -- well, he assumes a converging/diverging nozzle, and so the area at the -- the unscripted area is said to be the -- the interest area.

MR. SCHROCK: So you're talking about the interpretation that Jones described this in terms of the geometry of the Brookhaven experiments in convergent/divergent nozzles, which is different from Alamgir and Lienhard.

DR. WALLIS: But if I have a small --

MR. CARLSON: I wasn't aware of that.

DR. WALLIS: -- break in a big pipe, I've got a 30 inch pipe and I've got a two inch hole in it. Is At two inches and A 30 inches?

MR. CARLSON: Yes.

DR. WALLIS: That's a huge area change.

MR. CARLSON: Is that right? Is that right?

PARTICIPANT: Yes.

MR. SCHROCK: Well, if you use the large break guillotine break of cold leg, for example, you're going to have no change in the area.

MR. CARLSON: That's right.

DR. WALLIS: So that's one.

MR. SCHROCK: So that term essentially washes out.

DR. WALLIS: It's one. At over A is one.

MR. CARLSON: Is one.

MR. SCHROCK: It's one.

MR. CARLSON: Well, in the large break LOCA there's a break spectrum that is run, and this would vary from one to --

MR. SCHROCK: But when I look at your numerical solution, I conclude that what you're going to substitute in there, in general, is going to depend upon the noding choice --

MR. CARLSON: That's right.

MR. SCHROCK: -- that you've made for the application to your critical flow problem.

MR. CARLSON: That's right.

MR. SCHROCK: And so how does that relate to what Jones did, and how is it justified?

MR. CARLSON: Well, if I was more familiar with what Jones did. Dr. Chow, would you help me address this, please?

DR. CHOW: I think the A is actually upstream of the nozzle area. So basically the A is the other requirement to the RELAP5, the 480 and upstream, you know, nozzle, and the At is the area of nozzle, lowest area of nozzle.

MR. SCHROCK: Well, since you've thought that it's related to Jones' experiments in convergent and divergent nozzles, it would seem that upstream area is essentially undefined.

DR. CHOW: One, seventy-five is the area of the upstream flow area. Why is it undefined? It's just the upstream of the nozzle. Basically -- basically, in the additive, that's the area that -- because area of the nozzle, that's basically --

MR. CARLSON: Well, I assume that Jones wrote the area as a small distance upstream of the throat as maybe an entrance effect or an entrance area.

MR. SCHROCK: A small distance upstream of the throat either puts it in the convergent part of it or upstream of the --

MR. CARLSON: Upstream of the converging section would be the entrance to the nozzle. I think there was a term that I thought he was looking for, that VADX, which is in 5.23, and he was expecting a certain range where this was the area change with respect to distance from the entrance or the throat entrance to actually what the throat area was, but anyway, I think that was --

DR. WALLIS: This depressurization rate looks a bit odd, too.

MR. CARLSON: This is the depressurization rate, yes.

DR. WALLIS: this is the rate at which a given piece of fluid is changing its pressure as it flows through the nozzle. It's not the rate at which the system is depressurizing. You're looking at a piece of fluid and saying, "How rapidly is it changing its pressure?"

MR. CARLSON: Right.

MR. SCHROCK: Or how rapidly the undershoot. I'm sure that's what --

DR. WALLIS: This is an approximation for it.

MR. CARLSON: Yes.

DR. WALLIS: Well, where do we go from here?

MR. SCHROCK: I guess move on to the next question.

DR. WALLIS: What's the next question?

MR. CARLSON: All right. Question A, that was S-RELAP5 appears to begin on a noding choice,. Please explain equation 5.23. Why does the text say this is the use of Pascals per second rather than units required in the Alamgir and Lienhard correlation?

Note that no units are specified in terms on the right-hand side of the equation of 5.23. Please show that S-RELAP5 uses consistent units.

I think this was the term that you had in question, and it's the units of Pascals per second, and this is just, you know, it was converted in --

DR. WALLIS: Is that what Jones said it should be, Pascals per second?

MR. SCHROCK: It's not Jones' choice. It's Alamgir and Lienhard.

DR. WALLIS: Well, they have a funny

equation.

MR. SCHROCK: They have a dimensional relationship, and it requires specific units.

MR. CARLSON: Yes.

MR. SCHROCK: And those units were not Pascals per second.

MR. CARLSON: They were some of that mega atmospheres per second.

MR. SCHROCK: Right.

MR. CARLSON: And I think that was converted to Pascals per second to use from the code.

MR. SCHROCK: Well, that's very dangerous. I don't know that you've used it consistently then because, as I just said, the Alamgir/Lienhard correlation is not dimensionless. It's a dimensional relationship, and it requires those specific units, not any others, mega Pascals per second.

MR. CARLSON: Mega Pascals. Oh, okay.

MR. SCHROCK: So I suspect then that you do not use consistent units.

DR. WALLIS: Well, mega Pascals per second is a million times Pascal per second. Have you got an error of a million?

MR. SCHROCK: Mega atmospheres per second.

MR. CARLSON: Mega atmospheres.

MR. SCHROCK: Even more than a million.

MR. CARLSON: Well, yeah.

MR. SCHROCK: Yeah.

MR. CARLSON: Well, actually that's a mega times -- well, the internal units of Pascal.

MR. SCHROCK: Oh, come on. You can't play fast and loose with --

MR. CARLSON: I'm not trying to.

MR. SCHROCK: Are your units consistent in the code?

MR. CARLSON: And the answer is yes.

MR. O'DELL: I think -- I think --

MR. SCHROCK: Not if you've used in that equation Pascals per second.

MR. O'DELL: No, I think what the statement means about proper unit conversion has done in the coding, basically it has converted to the mega atmospheres per second where they've got the -- unfortunately on the previous slide shows that that term -- acknowledges that term was in mega atmospheres per second, and the statement there on the viewgraph is that the proper unit conversion is done in the coding.

And I think if you go back to our code verification where we went through that, that was one of the things that was being checked in the code verification process, was to make sure that we were using these things in the proper --

DR. WALLIS: So you had to look at the coding and see if there's a suitable factor in there.

MR. O'DELL: Right.

DR. WALLIS: With a mega atmosphere, it's ten to the 11th Pascal? You get a factor of ten to the 11th? Something.

MR. CARLSON: Everything in the code is converted to Pascals.

DR. WALLIS: So someone has looked at the actual lines in the code and found that there is conversion of units in there somewhere?

MR. O'DELL: Yes, in the verification process we went through the actual coding on these models to make sure that in fact we're using them properly.

DR. WALLIS: That's the difficulty the reader has. The reader reads the documentation and sees some units there and what's actually encoded might be something else, but he has no way of checking that without looking at the source code.

MR. CARLSON: That's true.

MR. O'DELL: That's true.

DR. WALLIS: And this is true of many correlations. Many heat transfer correlations are written in weird units, and you have to convert them to your code units hopefully.

MR. CARLSON: Well, we --

MR. SCHROCK: So the answer is that you've checked it and you assure us that the units are correct.

MR. CARLSON: I personally have not checked it, but other -- it has been checked.

MR. O'DELL: It's been checked in the code verification process.

MR. SCHROCK: Okay.

DR. WALLIS: Can we move on then?

MR. SCHROCK: Yeah.

MR. CARLSON: Let's see. Equation 5.25 and 5.26. This factor depends on the system geometry and noding choice. Explain the basis for these equations, the background and reasons for arbitrary choices, 5.26 and 5.27.

Five, twenty-six and --

DR. WALLIS: It would really help if we had Section 5. I don't know how much progress we can make without knowing to what use this is put.

MR. CARLSON: Section 5, equation 5.26, is

-- well, Section 5.25, I don't happen to have that.

MR. SCHROCK: It's used in the calculation of sigma.

MR. CARLSON: Oh, there it is.

MR. SCHROCK: Of Alamgir and Lienhard's correlation.

DR. WALLIS: Oh, it's used, okay, in the sigma.

MR. CARLSON: Yeah, all right.

DR. WALLIS: Is this what Lienhard told you to do or did you just make an assumption? This is your assumption that seemed reasonable. Is that what it is?

MR. CARLSON: Yes.

DR. WALLIS: It may be okay. I can't really tell without looking at data or something to see if it works.

MR. O'DELL: And in fact, you know, in the process of going through the Marviken test, I mean, we have gone through the Marviken test with these models and determined, you know, a bias, which was one as I recall, and I don't recall what the uncertainty was, but in the application of these, we have done comparisons, you know, to the Marviken tests to determine the bias and uncertainty for the flow.

MR. SCHROCK: Well, there is in the methodology a need to determine the depressurization rate in the experiment, and to do that you use an equation which appears to depend upon the noding, and so presumably you'd get a different depressurization rate if you used a different noding. If you used a different depressurization rate, the criterion for the onset of flashing would be changed.

So it all comes out in the assessment, but there are too many things involved in the assessment. To begin with, the Marviken experiment itself has a complication that there is flashing within the vessel, and you know only what RELAP tells you about what the void fraction is entering the flow channel.

MR. CARLSON: Right.

MR. SCHROCK: Okay, and that's determined by RELAP. It's not determined by experiment.

MR. CARLSON: Right.

MR. SCHROCK: You have no independent check on that. So there's an uncertainty there.

Then you need this depressurization rate, which is -- well, I'm sorry I'm mixing that with the later problem of saturated blowdown.

MR. CARLSON: Yes.

DR. CHOW: This is Hueiming Chow.

I think that in terms of R-BEACON they don't -- they really have measurement of the pressure and the temperature at the discharge, at just in front of the pipe. So you cannot say we don't have any information about the step in front of the nozzle. They have mentioned.

So in our calculation, we have to compare the temperature and the pressure in the form of nozzle. So we did have data on that. So basically it said we have to continue calculation from the base to the -- before the nozzle that stays about the same as the experiment. So they did have measurement data in there. So it's no say they don't have measurement data.

And they used the same kind of equation in the exact extent. I mean, this equation basically the Monica (phonetic) approach mentioned. The only thing, the only choice that you have, you have the volume area; you have junction area. That's only choice. You define it in the code to get the approachment. What's that? The area changing nature.

So this is an approach mentioned, and how good is that compared to the experiment data?

That's all I can say.

MR. SCHROCK: Okay. Would you go ahead?

MR. CARLSON: Moving on --

DR. WALLIS: Are you going to answer all of the questions that Dr. Schrock has here?

MR. CARLSON: If Dr. Schrock wants me to. There's a whole string of them.

DR. WALLIS: Well, I'm not sure we have time to do that. I think all we're going here is establishing your credibility. Maybe we should look at the questions which are most relevant for establishing credibility of your approach rather than everything. I think most of us are going to be completely lost with all of these without seeing what the equations actually are.

Well, how about something like this, using HEM, which mysteriously obtained from choke but then applied to slip equilibrium calculations?

MR. CARLSON: Pardon me?

DR. WALLIS: Which one of these questions do you think we ought to focus on in order to get an idea as to whether these folks know what they're doing? Do we need to go through every one of them?

MR. CARLSON: No.

DR. WALLIS: Should we require a written reply or something or what?

MR. CARLSON: In the --

MR. SCHROCK: Let's take the one in the middle of the second page. Equation 5.28 is said to be obtained from equation 2.85 with gravity and friction omitted, and this is said to be done for clarity and the derivation to follow, but the omitted terms "friction" and "gravity" are in the code.

Nothing is said here about -- it's my parenthetic note that nothing is said about the flashing term which is also committed in that equation.

DR. WALLIS: And what is equation 5.28 representing?

MR. CARLSON: Five, twenty-eight, that is the simplified steady state.

MR. SCHROCK: Five, twenty-eight is a simplified momentum equation.

DR. WALLIS: Oh, dear.

(Laughter.)

MR. CARLSON: Yeah, that's what I thought when I first saw that.

MR. SCHROCK: Simplified steady state momentum equation.

DR. BANERJEE: Bernoulli's question would also be choking.

MR. CARLSON: Right, and --

MR. SCHROCK: No phase change accounted for.

MR. CARLSON: Well, the phase change is accounted for, would probably be -- well, is accounted for in the void fraction.

MR. SCHROCK: I don't know. I'm looking at equation 5.28, which is on page 5-14 --

MR. CARLSON: Right.

MR. SCHROCK: -- of Rev. 4.

MR. CARLSON: Right.

MR. SCHROCK: It has none of these last three terms.

MR. CARLSON: That's right, and these were just added to complete, to satisfy your question that could have been in --

MR. SCHROCK: My question is about the equation 5.28 that's in the report.

MR. CARLSON: Yes.

MR. SCHROCK: Not the equation that's on the board.

MR. CARLSON: Right, about the flashing term, and I'm assuming, you know, when you say that the flashing term is not in the 5.28, I believe --

MR. SCHROCK: Let me read the complete question.

MR. CARLSON: Simplify --

MR. SCHROCK: Equation 5.28 is said to be obtained from equation 2.85 with gravity and friction omitted, and this is said to be done for clarity and the derivation to follow, but the omitted terms, "friction" and "gravity," are in the code.

So it's saying that the code contains something that's not in this equation. This simplified form is used to establish suitable average values of rho-alpha products used in the numerical integration.

MR. CARLSON: Right.

MR. SCHROCK: Okay. Please explain how this works when friction and flashing are included.

MR. CARLSON: Explaining how --

MR. SCHROCK: You go through this strange gyration --

MR. CARLSON: This is the question that's in the code. I mean that's, I think, what part of your question was.

DR. WALLIS: So it doesn't have a flashing term?

MR. CARLSON: But it doesn't have a flashing term on it. Okay? And what I assumed you mean by flashing is that you're talking about a mass transfer term.

DR. WALLIS: It's gamma, G, VF1, SVG term which is in 2.85.

MR. CARLSON: Right.

DR. WALLIS: Except that in choking that change of phase is important.

MR. CARLSON: And when it's equal velocity, that particular term is not there.

DR. WALLIS: If it's homogeneous equilibrium?

MR. CARLSON: Yeah, for a homogeneous flow or homogeneous equilibrium.

MR. SCHROCK: Well, wait a minute. In homogeneous equilibrium model, flashing occurs, mass transfer.

DR. BANERJEE: The flashing term is very important.

MR. CARLSON: I have a hard time -- I don't understand what you mean by the flashing term.

DR. BANERJEE: Gamma.

MR. CARLSON: Gamma? Gamma in the momentum equation is basically the mass generated, the mass generation rate, and it's applied as a mass transfer due to or momentum due to mass transfer, and it wasn't applied in this assumption or in this equation because we're assuming equal velocity. So there would be no momentum transfer between the phases if there was flashing, that is, condensation or evaporation.

MR. SCHROCK: Well, equal velocity simply means that the liquid and vapor are moving at the same speed.

MR. CARLSON: That's right.

MR. SCHROCK: But both of their speeds are changing, and one of the contributing factors to their changing speed is the fact that some of the liquid may be evaporating or in the high quality region may be condensing.

MR. CARLSON: Yes, but we -- but that flashing term, like I say, well, it was not applied. It was assumed that it wasn't needed for equal velocity.

MR. SCHROCK: Well, you've gone through some strange things to establish what you regard as suitable average values of the product of alpha times rho --

MR. CARLSON: Yes.

MR. SCHROCK: -- in the last half node --

MR. CARLSON: Yes.

MR. SCHROCK: -- upstream of the point

of --

MR. CARLSON: Right.

DR. WALLIS: The alpha-rho?

MR. SCHROCK: What I've asked you to do here is to explain how this works. What is the significance of those average values? Why are they going to serve adequately when, in fact, you have friction and you have flashing involved in the process?

Friction and gravity.

MR. CARLSON: Is this -- I'm not sure what terms you're referring to. I think you're referring to the adjustments to density from the center to the throat.

DR. BANERJEE: I think it's simpler than that.

MR. SCHROCK: I guess you need to read your own report because what you've done in this report is to develop a rationale for how you will choose average values --

MR. CARLSON: Yes.

MR. SCHROCK: -- for integration purposes of the product of rho times alpha. Okay? And to do that, you've used only a part of the problem. You haven't used the whole problem. My question then can be restated as: why do those average values serve adequately when the entire problem is considered rather than a part of the problem?

DR. WALLIS: I'm not sure that --

MR. SCHROCK: I'm trying to figure out what you're saying in this document, and I can't. I can't understand what it's doing.

MR. CARLSON: Dr. Chow, can you get --

DR. CHOW: I think these just momentum equations. These exactly the momentum equation and the sum momentum equation, except that for the mass transfer term is not there. But these, they will be just quasi-steady state integral from the boring center to the junction.

MR. SCHROCK: Okay.

DR. CHOW: Yeah, you can't use the -- in order to get a LOCA condition, you have to determine the state, equation or state of the junction. Okay? And it is just a steady state integration from the boring center to the junction.

And during this integral part, I mean, we always assumed that vaporization occurs at the end of integration, not in between integration, and even in our volume center, the vaporization is associated with volume center. We never say that when doing all of this integration of mass difference. We never say that in the past we have a vaporization. We say at this end of the vaporization, we have so much vaporization, at the other end so much vaporization.

So just around the past we don't consider the vaporization. That's all it says.

MR. CARLSON: But you're really just doing the integration from the south center just to the throat.

DR. CHOW: To the throat.

MR. CARLSON: And this is the replacement of the momentum equation; is that correct?

DR. CHOW: Yeah, yeah.

MR. CARLSON: Yes.

DR. CHOW: Because that RELAP5 doesn't solve the equation of state at the junction, and that's why these state is needed.

MR. CARLSON: This is, again, that linearization or that integration. If you get a better property for alpha, rho at the throat --

DR. CHOW: Yeah, at the throat, yeah, that's it, and --

MR. CARLSON: What we found was that when we looked or compare RELAP5 to, say, homogeneous equilibrium table values generated from RELAP4 that we had maybe not quite the correct mass flow that, say, RELAP4 would predict for HEM. So we included a correction term for the densities so that we get exactly what HEM specifies under stagnation conditions.

We assume that there's no change in state or from the center to the throat. We only assume that there was a change in pressure going from the center, cell center pressure to the throat pressure, and it turns out and we assume there's no slip in that assumption or the slip didn't change, and that assumption was not necessarily sufficient. So we had to go in and make an adjustment to how the density changes from the center to the throat.

DR. CHOW: I see your question about friction is explained in the next one, in the integration from the volume center to the floor, the alpha lower factor.

PARTICIPANT: No, no, no.

DR. CHOW: On approachment by the volume center value. Okay?

So basically the alpha and the DF if daily use of volume center value, and because we can see the friction, this is very short distance. So the friction and the gravitational force, the contribution very small. The momentum contribution is much, much more dominant.

So that's why the different approach mentioned used for the friction terms.

DR. WALLIS: So you're looking at the note just beside the choking point where gravity and friction really don't play much of a role?

DR. CHOW: Yeah, we just --

DR. WALLIS: You're just accelerating everything?

DR. CHOW: Yeah, we just only half cell, only half cell, because we don't have any junction property. That's why we have to do this half cell integration

DR. MOODY: Maybe I'm not necessarily for having any confusion, but I just wondered in the line below the equation you say, well, certainly when you add the two equations, the mass transfer term, the momentum transfer from vaporization cancels. Then you say the term is not present due to the assumption of equal velocity.

I think that term cancels anyway, doesn't it? And yet you've got two velocities in that equation. I'm a little confused. Why are they not reduced to one value like V? If they're equal, is it necessary to keep them separate like that?

MR. CARLSON: Not really. We thought for clarity possibly.

DR. MOODY: Okay. Then you could essentially factor out the D by DX or V squared from the left-hand side.

DR. WALLIS: So you're assuming these velocities are equal?

MR. CARLSON: Well, this is for the HEM, critical flow. We would leave the two velocities there if we were going to do two phase critical flow.

DR. MOODY: Of HEM? If it's homogeneous, they are both traveling at the same velocity.

MR. CARLSON: Right, but the same coding is used for another model option, that is, to not use a --

DR. MOODY: Okay. That's just the way the code would change.

MR. CARLSON: That's just the way the code works. We should have factored it out because they're equal.

DR. MOODY: I just want to be sure I understand what you've got, and so, yeah, they are the same value velocity in the HEM model on both sides of the equation. Vf is equal to Vg.

MR. CARLSON: Yes.

DR. MOODY: Okay. Thank you.

DR. WALLIS: Well, Virgil, what did you want to do? Should we -- I'm wondering how long we should spend on these questions, if it would be fruitful in our responses.

MR. SCHROCK: I think the documentation is extremely unclear about that is done and how it's justified.

DR. WALLIS: So it's a big like the momentum story.

MR. SCHROCK: The questions that I raised could have been answered in a more reasonable way, and I don't think we're getting at --

MR. CARLSON: I apologize.

MR. SCHROCK: -- reasonable answers to the questions. So --

DR. WALLIS: Shall we drop the --

MR. SCHROCK: I'm willing to leave it that I'm not satisfied with that section.

DR. WALLIS: All right. We drop the critical flow thing. I would like to get to the statistical matters, but I do think we should have a break before we do that.

Can we take a break until quarter till

six, if everyone can stay for the statistical resolution, which I notice doesn't have many transparencies, but may still require some discussion?

What is I think very interesting and before we get to that is I thought that you had a very interesting graph of PCT versus percent oxidation, which showed they were very closely correlated. That was a useful piece of information for me. I don't know if you're going to show that in the presentation, but if we can find it in the break, that would be useful, I think.

Let's come back at quarter to six.

(Whereupon, the foregoing matter went off the record at 5:36 p.m. and went back on the record at 5:47 p.m.)

DR. WALLIS: Let's come back into session.

MR. O'DELL: Okay. What I would talk about quickly here was basically the statistical issue that came up during the review of the methodology and what the ultimate resolution of that issue was.

The issue came up during the review was that the NRC request, report to PCT maximum nodal oxidation and the total oxidation as a joint probability statement.

In the original proposed realistic large break LOCA methodology, we reported an individual 95-95 value for each of the criteria; PCT, maximum nodal oxidation and total oxidation, and we never made any kind of a joint probability statement with respect to the three criteria.

DR. WALLIS: Now, can I get clear what you're saying here? In order to satisfy these three criteria, you need to have enough code runs so that you can get 95-95 value for all three. That's what's meant by joint probability, not just --

MR. O'DELL: Right.

DR. WALLIS: -- how many do you need to get either one or the other or the other. I mean, you need 59 maybe to get PCT if that's all you care about, and you need 59 to get total oxidation if that's all you care about.

But if you want to get both of them with this 95 percent certainty, then you might need a different number of runs.

MR. O'DELL: Exactly.

DR. WALLIS: That's what you mean by this joint probability question.

MR. O'DELL: Right. The resolution of this is based on Regulatory Guide 1.157, which indicates that the revised Paragraph 50.46(a)(1)(i) requires that it be shown with a high probability that none of the criteria of Paragraph 50.46 will be exceeded and is not limited to the peak cladding temperature criterion.

However, since the other criteria are strongly dependent on peak cladding temperature explicit consideration of the probability exceeding the other criteria may not be required if it can be demonstrated that meeting the temperature criterion at the 95 percent probability level insures with an equal or greater probability that the other criteria will not be exceeded.

So this basically allows you to report the 95-95 PCT and the associated oxidation with that 95-95 PCT case.

DR. WALLIS: This is very different from requiring you to have this 95 percent probability level with all three of the criteria, which this seems to give you a way out if you can show getting the PCT with 95 percent probability and assures that the other criteria will be met with greater probability.

MR. O'DELL: Right.

DR. WALLIS: In which case you have to look at how these different criteria are correlated.

MR. CARLSON: Exactly. And you know, we

did that, and unfortunately I don't have the slides for that particular one, but what has been agreed is that we would change our methodology to amend proposed methodology to only report the 95-95 PCT case and its associated maximum total oxidation and total oxidation.

DR. KRESS: Now, how will you demonstrate though that that meets the criteria of the high probability to meet the other two?

MR. O'DELL: Well, we've gone through and looked at a series of cases, including the three loop, four loop, and then we look the three loop case and ran the power up in the three loop case to drive it to a PCT of around 2,200. So we have a series of cases, and what I do have in the way of some back-up figures, this shows -- and unfortunately this one doesn't have the high temperature ones. We ran some up to where they're a little over 2,200.

But what we were trying to show in this particular slide is this is the frequency distribution that we got on PCT from our cases that we ran. Okay? And if you look at --

DR. RANSOM: How many independent variables went into this, I mean, that you varied statistically?

MR. O'DELL: I think there's 39 roughly, something; somewhere around 35 to 40.

DR. RANSOM: Are they listed somewhere where a list of all the ones and their associated frequency distribution that you use?

MR. O'DELL: That's provided in the sample problem on the four loop in EMF-2103.

DR. RANSOM: And that's in the documentation that we have?

MR. O'DELL: Yes.

DR. RANSOM: Which volume is that?

MR. BOEHNERT: Larry, I notice that stuff is labeled proprietary. I assume we're talking around the numbers because we are in open session, and I just wanted to remind everyone of that.

MR. O'DELL: But, anyway, what this was showing is the distribution of PCTs from the cases, and indicating, you know, we're up in the high temperature range there, approaching the 2,200.

And then if you look at that in conjunction -- and I guess that one was the three loop PCT, and here's four loop PCT case --

DR. KRESS: Are all of these with 59 runs?

MR. O'DELL: Yes, these cases are for actually the sample problems that we've provided in the documentation, and it shows, again, the temperature distribution for the PCT versus the frequency of the calculation in the 59 cases.

DR. RANSOM: How many calculations were made to establish that?

MR. O'DELL: The 59 calculations.

DR. RANSOM: Fifty-nine. So that came out of the 59 calculations?

MR. O'DELL: right.

DR. WALLIS: And the 59 is an assurance that you've actually captured the 95th percentile.

MR. O'DELL: Right.

DR. WALLIS: Just looking at that, it looks as if you're sort of in the 95th percentile, just sketching out a probable distribution roughly.

MR. O'DELL: Right.

DR. WALLIS: But there could be some values in the tail up to 1850 or whatever presumably.

MR. O'DELL: Right. I mean, 95-95 is a five percent probability that you --

DR. RANSOM: Well, in your methodology, do you have a way of tracing back what parameters were most influential and the change, I mean, in causing --

MR. O'DELL: Yeah. We go through and produce scatter plots. Okay? And one of the points in some of those scatter plots is if you look at the scatter plot, you can see the ones that actually show a trend, and then the ones that don't show a trend.

The ones where you can physically see a trend within those scatter plots are the dominant, important parameter, and --

DR. RANSOM: What were those dominant parameters in the large break LOCA?

MR. O'DELL: Well, the break size is important. The heat transfer is one of the important parameters.

DR. RANSOM: Heat transfer and --

MR. O'DELL: Heat powers and axial shapes being used, particularly the axial shape.

MR. SCHROCK: One of the requirements is that you find what is the worst break size, and so once you have found what you think is the worst break size, then shouldn't this statistical evaluation hold that fixed and look at all of the other variations instead?

MR. O'DELL: the way we've treated the break sizes, we've treated this statistically. Okay? We go through and we randomly vary the break size throughout the --

MR. SCHROCK: Yeah, but my point is that

that seems to be inconsistent with the Appendix K requirement that you study a spectrum of break sizes and determine the worst case, and so you're sort of mixing the spectrum of break sizes with the statistics of the code prediction for a given break size based on other variations.

MR. O'DELL: Right, and I understand in the Appendix K the way that they've handled the break spectrum issue is that they do, in fact, run a set of break sizes in determining the limiting break. Okay? They report that then as basically your limiting break PCT, but they're running all of the calculations on all of the break sizes in a deterministic fashion to find that. So once you find the limited break in an Appendix K analysis, you basically have the limiting calculation.

In the statistical analysis, we're treating the break size as one of the statistical parameters, and we're varying that along with the other parameters, but not --

MR. SCHROCK: But I would think that once you've established what the limiting break size is, that you would then look at the uncertainty in the prediction at that break size.

MR. O'DELL: The problem --

MR. SCHROCK: And that's what you're trying to address statistically now, isn't it?

MR. O'DELL: Well, the problem with trying to do that is what exactly is the values you use for the other parameters, the other strong parameters. Okay?

I mean is it a top P taxing (phonetic) shape that I run through all of that, and how do I know which one I select? Okay?

In order to do what you're requesting, I either have to be able to do a deterministic analysis in some fashion where I set again, you know, basically doing Appendix K type calculation to find the limiting break and then vary everything around that. You know, that's not as simple a calculation from a statistical perspective as it first appears because what is the limiting break relative to all the other parameters you're varying?

So the only real way to treat the break size in a statistical methodology is to, in fact, include that as one of the statistical parameters.

DR. RANSOM: What was the range of break sizes that you considered?

MR. O'DELL: I'll go through that tomorrow when I go through the overall methodology, but we looked at both, you know, split break from a ten percent up to a 1.0 and double ended guillotines from the 1.0 up to a 2.0.

DR. KRESS: Now, you don't ascribe any significance to this distribution?

MR. O'DELL: Not with 59 cases. Okay?

DR. KRESS: I mean, it just happened to be the way it turned out with 59 cases, right? But you probably ascribe some significance to that 1,700 thing.

MR. O'DELL: Right. I mean, what you're -- there's basically three calculations that gave us the temperatures in that 1,700 or the range here that we're reporting around the 1,700.

DR. WALLIS: And you're binning here. So it could be --

MR. O'DELL: Exactly.

DR. WALLIS: -- a different binning might put some in the 1,650, 1,750.

MR. O'DELL: Right.

DR. WALLIS: Those three points might be spreading to three.

MR. O'DELL: Yeah.

DR. WALLIS: It's an interesting idea to put in the break sizes the statistical problem.

DR. KRESS: Yeah.

DR. WALLIS: I think that may be justified.

MR. O'DELL: It makes an interesting approach to a statistical analysis if you don't.

DR. WALLIS: Yes, it does.

DR. KRESS: That may impact the ranges and distribution of the other parameters.

MR. O'DELL: Potentially. Potentially it could, yes.

DR. WALLIS: I think that makes sense. We can then begin to argue about whether the 95-95 is good enough because it depends on if you're looking at all of these things in a statistical way. How certain do we really need to be?

There's nothing magic in my mind about 95 percent.

MR. O'DELL: Well, the 95 percent obviously is the one that was picked because it shows up in the reg. guide as being the --

DR. WALLIS: How certain you want to be must depend on the risk and consequence and things like that, the risk of being wrong. I'm not sure that the staff has really worked out a good rationale for 95-95. It sounded good at the time, but now it's going to be used perhaps for a more important decision making process that more thought needs to be given to what it should be.

DR. KRESS: It depends on what you want to use it for.

DR. WALLIS: That's right.

MR. O'DELL: Now, these are the same 59 runs.

MR. O'DELL: Right. These are the same 59 runs, again, at three peak local oxidation, and as you can see, they're all clustered down here for those 59 cases where the criteria, 17 percent criteria, they're significantly away from that criteria.

DR. WALLIS: And if I'm betting 1,000 bucks on on the outlook from RELAP, I might be satisfied with a 95-95 percent certainty of not losing it all or something.

But if I'm risking my life, I might want to have a 99-99. It depends on what's at stake, doesn't it?

DR. KRESS: That's a good question.

MR. O'DELL: Well, that's true.

DR. KRESS: But that looks like sufficient justification to say that this is captured in 95 PCT.

MR. O'DELL: Right, and again --

DR. KRESS: The same thing on the total oxidation.

MR. O'DELL: Yeah, and if you look the four loop again confirms the same sort of analysis.

DR. WALLIS: Well, what's peak local oxidation criterion in the regulation?

MR. O'DELL: Seventeen percent.

DR. WALLIS: Way over there.

DR. KRESS: Yeah. Now, I'm not sure that's way over there because I don't know what your scale is.

MR. O'DELL: Well, it basically goes from zero to 17, and we do have some --

PARTICIPANT: It's a linear scale.

DR. KRESS: It's linear.

MR. O'DELL: Yeah. We have some cases, you know, where we didn't get any oxidation on some of the lower temperature cases.

MR. HOLM: This is Jerry Holm.

I think one of the things we looked at is if you look at the standard deviation of that, and we realize it's not normal, but just as a figure of merit, that top peak there is 30 standard deviations away from the criteria.

DR. WALLIS: Well, the 95-95 as an

estimate here would be around one.

PARTICIPANTS: Yes.

DR. WALLIS: Whereas you're out 17. You're way far away from the criteria.

MR. O'DELL: Right.

DR. WALLIS: So I think that you might have a really good argument here.

DR. KRESS: Might have a really good argument.

MR. O'DELL: And, again, I've got the same set of slides showing for the three loop total core oxidation. Again, for the total core oxidation the limit is one percent, and we're back in here around the .05 percent, and seeing basically a result for the four loop.

We chose, again, the criteria of one percent and back in here around .03 on this particular transient.

DR. KRESS: Now, the only reason this works out is because of the strong correlation.

MR. O'DELL: Because of the strong correlation.

DR. WALLIS: But also it's more than strong correlation. I mean, it also shows that no matter what you have for peak clad temperature, all of these other parameters are so low --

DR. KRESS: That's right

DR. WALLIS: -- that with enormous certainty they're going to be way far away from the criteria.

DR. KRESS: Yeah, it's more than just the correlation.

MR. BOEHNERT: Larry, I'll need copies of those slides for the record.

MR. O'DELL: Okay.

DR. WALLIS: Yeah, I think they're very interesting. Good to end the day on a persuasive argument.

DR. RANSOM: Mr. Chairman, I'm assigned to Bill Nutt, and there were some aspects of this. He made more than 59 runs. Why is that? Less conservative?

And he said he has some slides that he wouldn't mind discussing if you want to take a few minutes.

DR. WALLIS: I would agree to that if it doesn't take too long.

Would you like to do that?

MR. NUTT: Sure.

DR. WALLIS: It's much easier to

understand data than it is these A1s and A2s.

MR. NUTT: Well, it depends if you have the abstract mind, right? Which, by the way, I don't. Andy and I were sitting there, "What's a real good concrete example of what I'm talking about here?"

That means -- can you hear me okay?

MR. BOEHNERT: Identify yourself for the record, please.

MR. NUTT: This is Bill Nutt from Framatome.

And actually I've presented these slides to you before. So I guess I'd better put my glasses down here because they'll get in the way.

DR. WALLIS: Did you present them or did they come back in a --

MR. NUTT: I think I presented them one.

DR. WALLIS: -- RAI reply to the --

MR. NUTT: These were back-up slides once for when we -- well, suppose someone did ask this question, and I think we did at one point when Uri was asking questions during a meeting, and I'm not sure it was in front of the ACRS. It may have been in front of the staff, and we presented this question.

And what I did was this. I did a little sampling test. I took a normal distribution. I actually did this with several different distributions. I'll just talk about the normal.

I took a normal distribution that has an upper 95 percent limit of 275 so I could go back and say standard deviation of 167 and, you know, a mean of zero.

Then I calculated the upper 95-95 using a nonparametric approach for sample sizes from 59 to 410. So I take 59 and I say, you know, what's the top one, and I do this 2,000 times.

Then I calculate the number of times that that fraction was below the 95-95 limit, the probability it was below the 95 limit, the ten percent change, you know, that it was below the 95 percent limit, and so on.

And I did this. Then I removed one step inside, and I'd say, okay, now let me take the second point in and I'll do 92 cases, and I did those 2,000 times. So I'm getting my frequency with which I exceed my limit. Okay?

So that's what shows up on this picture.

Now, if we look at this curve, this is 59. The interesting thing is all of them come to five percent, right? Because it is 95-95. So they all go to five percent.

But if I had 59 points, you see, there is a reasonable chance that I could be clear up there, which would be at 450 rather than the 275 that we said.

And don't take these numbers too seriously because if I had used a uniform distribution, this picture would look the same, but the scale would b compressed. Okay? So the actual values are dependent on distribution.

But the conclusion here, if I take 59, right, if I take 93, this improves quite a bit. My probability of ever getting over 400 has all of a sudden gotten a lot smaller, right?

And by the time I get up to I think it's 410 points where I can take the 13th point in, the chances of me getting more than 50 above my -- you know, of having my sample that I select as my 95-95, my chance of having it be over 300 are really very, very slim.

DR. WALLIS: So what is the probability on the axis there? The probability of what?

MR. NUTT: Oh, this is the probability that the number that you pick as the 95-95 will be here. Okay? So you all can take your sample and say I got a sample. What's --

DR. KRESS: When in reality it's really down there.

MR. NUTT: Here's the real one, and there's a five percent change it will be here, but what's the chance that I'll pick that value as my 95?

DR. KRESS: Which could happen.

MR. NUTT: Which could happen.

DR. KRESS: In any given run.

MR. NUTT: Yeah. So one basically says all I get when I take more points is I don't really improve the 95-95 point, right? It's still a 95-95. That's all.

But what happens is I tend to get rid of the possibility of having one of those flyers way out there.

Now, as you saw from the data that we have taken, we're not getting a lot of flyers. We took one corrupt -- I think if we take the one at -- we did one set of cases that went to 2274, had same sets of plots, but since it wasn't QAed we didn't put it in a picture.

DR. WALLIS: It depends a bit on the shape of the tail of the distribution.

MR. NUTT: Yes. And when you get up and you take off in the metal water reaction, it leans a little bit more out, and you do tend to get them spread.

When we did the case that had 2274, there was -- as a highest PCT -- there weren't any others up there. It was up there by itself, but when you step down, you know, in the 2100s, there was another one, but in this one we had a 100 degree spread in it.

In these cases that we looked out where we were down lower, they're all stacked in closer because, you know, the upper tail on that distribution is not so high.

Do you know what I'm thinking, what I'm referring to?

DR. RANSOM: I think I do.

MR. NUTT: Yeah, if you --

DR. RANSOM: To the fact that if you take more cases and run more samples, you'll actually increase your chances of getting one of the higher values out in the 90 -- beyond the 95th percentile.

MR. NUTT: And if I allow myself to move inside now, that scatter out there in the tail, I'm moving further inside, and I have less chance of getting out in that tail.

But what does happen when you get up closer -- and the tail doesn't really exist too much at these lower -- the tail is not too wide at these lower pieces, but the tail can get larger when your distribution gets really skewed, you know, because some cases will hit runaway metal water, and they'll pop way up here, and others won't.

So instead of what looks close to a normal distribution, you start to lean a little bit higher, but we've taken it up to 2274 on a 95-95, which is probably -- we'll never submit anything like that, you know, and it still looped. You know, the conclusions that Larry presented were, you know, still valid, still looked very much the same.

But it did also have -- the 2274 was basically almost an outlier. Had we done more cases, we might have been able to reduce that. Okay?

DR. KRESS: Now, as you increase the number of runs, your output distribution that you could plot comes closer and closer to the real.

MR. NUTT: Yes.

DR. KRESS: So you have additional information you could use to say whether or not that -- you've got a higher probability of getting those numbers, but you had more information. You could use that to maybe rule it out or something --

MR. NUTT: Yes.

DR. KRESS: -- because you're getting closer to the real distribution.

MR. NUTT: Yes. You're paying more money, and you're getting a better answer. It is a very bad way to get a distribution.

DR. KRESS: Yeah, it's not a good way.

MR. NUTT: If I wanted the distribution, and I think I was looking at response services because that's a better way to get a distribution.

DR. KRESS: Yeah, it would be much better.

MR. NUTT: You know, you have virtually like a one over square root of N.

DR. KRESS: Yeah. So it's 10,000 runs or something like that.

MR. NUTT: Oh, yeah. It's ridiculous.

DR. WALLIS: But if you had it, then you could look at it and say, "Ah." The 2,200 is the point -- is the 99.9 percent percentile --

MR. NUTT: Right.

DR. WALLIS: -- beyond that. You could actually make that.

MR. NUTT: but the problem with the response service is the biggest error they make is out on the tails, and so if you're looking out on the tail, that's where the biggest error is in the response services.

Response services are really good in the middle. So I like to use them when I'm doing statistical work; I like to use them in the middle of a process where I put something else on the end to finish it up and be more accurate because they're a little bad on the tails. Okay?

DR. RANSOM: By a response surface, you mean you would just do parametric runs and construct a surface for the PCT as a function of all the independent variables.

MR. NUTT: Exactly. I'd run a set of constructed variations in each of the parameters designed, you know, to get rid of all of the -- designed to give me a sufficient number of points to fit a polynomial of some kind. Usually it's a polynomial. A polynomial of some kind over, you know, say over the --

DR. RANSOM: But would that require more than the 59 runs that you're making to do this from a statistical point of view?

MR. NUTT: If I do three variables, no. But, see, we're doing 40, 39, and I think some of those are multiple variables. So I think the real number is higher than 39, and we're actually treating anything. We're treating anything that we could think of that had any contribution to the large break LOCA.

DR. RANSOM: Have you looked into all -- some people are proposing linearized methods for response surface.

MR. NUTT: I don't like linearized methods. Linearized methods are equivalent of a root sum squares, and a root sum squares is okay as long as two things occur: one, normal distribution; and, two, you add or subtract.

If you don't add or subtract, they're not valid, and if they're not normal distributions, they're meaningless. They're still valid, but they're meaningless.

(Laughter.)

MR. NUTT: No, I mean, you could still get a valid calculation of the standard deviation if you add or subtract, but if your distribution is not a normal distribution, what do you do with the standard deviation?

DR. KRESS: That's right.

MR. NUTT: Yeah, it doesn't do you any good. So okay?

DR. KRESS: That was very interesting.

MR. NUTT: Mr. Ransom, does that answer

your questions?

DR. RANSOM: Yeah. Can I get copies of the slides?

MR. NUTT: Sure. I'll give them to Jerry and he'll make sure.

DR. WALLIS: Do you have a write-up?

DR. BANERJEE: What are these slides?

DR. KRESS: Oh, that's to show how well correlated the peak clad temperature is to the total oxidation.

MR. O'DELL: This is Larry O'Dell.

In those particular slides there, we didn't have clear ones, but what they show, as I mentioned, we ran three sets, and we drove the temperatures up to 23-something. That shows all of those cases.

MR. NUTT: If you would notice, I think you would notice I was about to make the same point, that there is a 2,274 degree case in there, and I think you can see that the local oxidations and the core wide oxidations still behave very well.

DR. WALLIS: I think something is wrong with the fit being the crosses. The fit should be a curve, shouldn't it?

MR. NUTT: The reason that that's done

that way is that particular fit was a fit where I fit it with the PCT and with the local oxidation, and the local oxidation is a random variable.

So if you make a nice, neat polynomial on a random variable, it doesn't plot very well as a nice, straight line.

If I plot it purely as -- if I do core wide oxidation purely as a function of PCT only, and you can do ALMOST as well, and later on I did it to get it, then you get a nice, smooth curver because this is a nice, smooth function.

And all those really are is step-wise linear segments just to show you, you know, where the mean goes.

DR. WALLIS: So there are really two arguments you use. One is that these variables are correlated so that if you know one of the extremes, you should know the other one.

And the other is that the PCT is far closer to its criterion than the other one. The total oxidation criterion is off the map.

MR. NUTT: Exactly. In fact, I think I was mentioning to someone the other day that this really is, too, and I think it's an important point, and I'm going to repeat your point because I think it is important.

I think there's two things that we demonstrate. One, that if you meet PCT, if you can meet the PCT criterion, there's no possibility that you'll fail either of the oxidation criteria.

Secondly, because they're very highly correlated, if you take the highest PCT case and report the oxidation from it, that those two oxidation numbers are very close to being the maximum oxidation.

DR. WALLIS: All the 95th percentile.

MR. NUTT: Right, sir. They're very high probability. So simply taking one case and reporting both answers is quite sufficient, but again, it takes the two reasons together.

DR. WALLIS: Now, do you want us to -- is there a matter of debate between you and the staff or are you agreed to this?

MR. NUTT: The staff is here.

MR. LANDRY: The staff accepts that argument. You'll hear more about this tomorrow, but I --

DR. WALLIS: We will hear about it?

MR. LANDRY: As far as I know. I don't have my statistical person here.

DR. WALLIS: We'll have a statistical

expert here tomorrow?

MR. LANDRY: But as far as I know, he accepts this argument.

DR. WALLIS: He accepts this argument. That is remarkable.

Okay. Anything else today? Anything else today? The probability of our getting to dinner is increasing.

DR. KRESS: Is that 95 percent?

DR. RANSOM: Now, what kind of statistical method are we going to use to select where we're going?

DR. KRESS: A random walk.

DR. WALLIS: Shall we come off the record at this point since we're getting a little erratic? Yes?

Thank you very much.

(Whereupon, at 6:18 p.m., the meeting was adjourned.)