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1 UNITED STATES OF AMERICA

2 NUCLEAR REGULATORY COMMISSION

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4 MEETING

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6 ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

7 (ACRS)

8 SUBCOMMITTEE ON THERMAL-HYDRAULIC PHENOMENA

9 + + + + +

10 WEDNESDAY,

11 NOVEMBER 13, 2002

12 + + + + +

13 ROCKVILLE, MARYLAND

14 + + + + +

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

19 COMMITTEE MEMBERS PRESENT:

20 GRAHAM B. WALLIS, Chairman

21 SANJOY BANERJEE, Consultant

22 THOMAS S. KRESS, Member

23 FREDERICK MOODY, Consultant

24 VICTOR H. RANSOM, Member

25 VIRGIL E. SCHROCK, Consultant

1 ACRS STAFF PRESENT:

2 PAUL BOEHNERT, Staff Engineer

3

4 ALSO PRESENT:

5 KEN CARLSON, Framatome ANP

6 RALPH CARUSO, NRC

7 HUEIMING CHOW, Framatome ANP

8 JERRY HOLM, Framatome ANP

9 RALPH LANDRY, NRC

10 ROBERT MARTIN, Framatome

11 BILL NUTT, Framatome ANP

12 LARRY O'DELL, Framatome ANP

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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.

1 The rules for participation in this
2 meeting have been announced as part of the notice of
3 the meeting previously published in the Federal
4 Register on October 23, 2002.
5 A transcript of this meeting is being
6 kept, and the transcript will be made available as
7 stated in the Federal Register notice.
8 It is requested that speakers first
9 identify themselves and speak with sufficient clarity
10 and volume so that they can be readily heard.
11 We have received no written comments, nor
12 requests for time to make oral statements from members
13 of the public.
14 We will no proceed with the meeting, and
15 I call upon Mr. Jerry Holm from Framatome ANP-Richland
16 to begin.
17 MR. HOLM: Good morning. My name is Jerry
18 Holm. I'm manager of product licensing for Framatome.
19 Just one item of clarification maybe since
20 the last meeting. We've changed the company name.
21 It's now just Framatome ANP. The "Richland" has been
22 dropped. That was part of the initial merger
23 arrangement.
24 Today we're going to talk about the
25 Framatome ANP realistic large break LOCA methodology.

1 I'm going to start with a fairly detailed presentation
2 of the momentum equation which is going to be given by
3 Ken Carlson.
4 We need to target to finish this about
5 mid-afternoon so that we have time for the other
6 items. Then we'll go into the general RELAP5
7 questions, which came from Dr. Moody. Bob Martin is
8 going to do that. We'll talk about selection of node
9 size. Bob Martin is going to do that again.
10 We will then go into critical flow issues.
11 Ken Carlson is going to present that, and then
12 statistical analysis by Larry O'Dell.
13 We also received a request to talk about
14 the force and Rosenow equation correlation, and if we
15 have time, I'd like to stick that at the end of
16 today's presentation. If not, there should be time
17 after Larry O'Dell's tomorrow.
18 And then tomorrow we've got scheduled for
19 a summary of the methodology, and Larry O'Dell is
20 going to do that. He's going to go into the
21 requirements and capabilities, and then the response
22 to the request from the committee, and we're going to
23 talk about changes we made to RELAP5 to create the S-
24 RELAP5 code.
25 We're going to talk about assessment and

1 ranging parameters and sensitivity uncertainties.
2 This is an overview of the entire methodology. It
3 will be fairly similar to the presentation given
4 previously to the Subcommittee, but since we've got a
5 number of new members, I think it's worth going over
6 the information again.
7 With that, I'll turn it over to Ken
8 Carlson to start the momentum equation.
9 DR. WALLIS: I should remind you that you
10 have a list of 13 questions from me, some of which
11 have As and Bs and C parts, and you also have a
12 critique which asks you to respond to. And I think
13 we're looking for answers to these questions.
14 MR. HOLM: Right.
15 DR. WALLIS: So if you don't present them,
16 then you will have to provide them some other way.
17 MR. HOLM: Hopefully we have incorporated
18 those into the presentations. We structured this
19 presentation intending to respond to those questions,
20 but we thought it made more sense to have a more
21 functional presentation structure.
22 DR. WALLIS: Yes. Well, I hope it will
23 work out fine.
24 Thank you.
25 MR. BOEHNERT: Now, I understand we're

1 going to go into closed session; is that correct?

2 MR. CARLSON: Yes.

3 MR. BOEHNERT: Okay. So anyone here that

4 is not with Framatome ANP or doesn't have an agreement

5 with them to hear proprietary information, please

6 leave the room.

7 And, transcriber, we'll go to closed

8 session transcript. Thank you.

9 (Whereupon, at 8:35 a.m., the open meeting

10 was recessed, to reconvene immediately in closed

11 session and resume the open meeting at 3:17 p.m.)

12 MR. BOEHNERT: I want to remind everybody

13 we are in open session. So the transcriber should

14 have an open session transcript.

15 DR. WALLIS: Whose questions are we

16 answering now?

17 PARTICIPANT: Dr. Moody's questions.

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

19 MR. MARTIN: Okay. My name is Robert

20 Martin. Believe it or not, I work for Framatome ANP.

21 I can still claim that for a little bit longer, as

22 long as I pay my check every money.

23 Can you hear me now?

24 I'm addressing Dr. Moody's four questions

25 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.

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

2 And as far as our regular reload support,

3 it's not relevant. If we were building plants like

4 all the time, this would be important, and I guess we

5 have methodologies that are probably 20 years old that

6 they're collecting dust, and until we start building

7 plants we probably won't be pulling them up.

8 That was kind of my answer, and that was

9 it.

10 DR. WALLIS: If you were addressing it,

11 then you'd have a different kind of momentum equation

12 which had a force from the piping, including a normal

13 force.

14 MR. MARTIN: And then we'd have to go back

15 and think about this and prepare for --

16 DR. WALLIS: It would not look like what

17 we heard earlier today.

18 MR. MARTIN: Yeah, we'd have to prepare it

19 a little bit differently.

20 DR. MOODY: I'm going to assume if they

21 did address it and had to apply it, they'd do it

22 right. How's that?

23 MR. MARTIN: Okay. Your question was

24 this: please describe how the discharge mass flow

25 rate is obtained for the postulated instantaneous

1 rupture of a long pipe before the quasi-steady
2 blowdown rate is reached. The pipe is attached to a
3 pressure vessel.
4 And then you had some discussion there,
5 drawing on some early RELAP5 calculations for this
6 case, sometimes as a result of inflow rates exceeding
7 critical flows, too much massing.
8 Okay. Just a little background. Using
9 the fine nodalization and small time steps, RELAP5
10 codes have demonstrated the ability to mechanistically
11 capture the choke flow phenomenon. I think in Ken's
12 discussion Dr. Ransom made reference to the
13 calculations done in the '80s, and that's kind of what
14 I'm referring to here, that that exercise has been
15 done in the past with RELAP5 codes. It is not what
16 we're doing now because that would require very fine
17 nodes and small time steps.
18 Well, and as I say in the next sentence,
19 it's a complication.
20 To achieve fast execution speed, the
21 implicit evaluation is used for those terms
22 responsible for the sonic wave propagation time step,
23 and this allows for a maximum stable time step to
24 approach the current one.
25 Okay. So what do we do? And I'm not

1 force they apply to end plug to drive it off, and then
2 they measure the pressure at the time they see the
3 incipient flashing.
4 And so how does that enter into what
5 you've calculated here on this sudden drop in
6 recovery?
7 MR. MARTIN: Well, I'll yield a lot of
8 that to Ken's discussion at that point because he goes
9 into more detail.
10 MR. SCHROCK: Well, that's a little
11 different than the Jones use of that in a quasi-steady
12 prediction of the critical flow with subcooled liquid
13 stagnation condition.
14 MR. MARTIN: Okay.
15 MR. SCHROCK: That's what Jones did,
16 Abuoff (phonetic) and Jones.
17 MR. MARTIN: Okay.
18 DR. RANSOM: Well, I don't know, but
19 anyway, that's applied as a boundary condition. That
20 pressure, you know, is then the break pressure. It's
21 assumed that it can't go lower than that vaporization
22 pressure. So it becomes a boundary condition.
23 MR. SCHROCK: But it can go lower than
24 that vaporization pressure. That's what Alamgir and
25 Lienard measured

1 going to go into all of the details because that's
2 really a lot of your questions a little bit later, and
3 since Ken likes to suffer more than others, we'll just
4 let him get back up there and be the whipping boy.
5 But where we go is when the break opens
6 up, we start doing the calculation just like Ken's
7 talking about, basically Bernoulli calculated
8 velocities and pressures.
9 There's this Alamgir-Lienard-Jones model
10 for subcooled choke flow that were used to determine
11 the throat pressure and choke velocity, and then I
12 make here the reference to where it is in the
13 documentation.
14 Here we're using local conditions, and
15 when this Bernoulli calculated velocity exceeds choke
16 velocity, then the velocity just sets.
17 Now, that still begs your question. You
18 still have the problem that you have once upon a time
19 early with the calculations, and I'm going to say,
20 well, yeah, to some extent you do.
21 This figure here is just taken from one of
22 our large break analyses that we've provided for this
23 methodology submission, except for you'll never see it
24 because it's only for the first 20th of a second.
25 What we have here is flow rate out the

1 break on the pressure vessel side. You can see the
2 flow rate taken off, and then I have drawn these
3 vertical lines here to indicate when the junction
4 choke.
5 Okay. You can see it takes off and then,
6 wham, the model kicks in. It backs off a little bit,
7 and then it goes up again.
8 DR. MOODY: This is like a liquid line
9 that opens up in that first heavy curve going up with
10 the Bernoulli flow?
11 MR. MARTIN: Yeah, I'll tell you. The
12 time step here is .000 --
13 DR. MOODY: Whatever it is.
14 MR. MARTIN: -- four.
15 DR. MOODY: That's Bernoulli flow?
16 MR. MARTIN: Bernoulli flow up until --
17 DR. MOODY: Of liquid, yeah.
18 MR. MARTIN: -- up to here and then the
19 choke model comes on. The criteria is met and then we
20 just lock in based on the calculation of the sonic
21 calculation there.
22 DR. MOODY: So that's a --
23 DR. WALLIS: -- type of wave that
24 propagates along the pipe. You open the break and a
25 decompression wave runs down this pipe and comes back

1 again. Is that the model?

2 MR. MARTIN: You know, we don't rack that

3 way, and I can't say that I've really looked at that,

4 but maybe this --

5 DR. WALLIS: That's not what --

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

7 DR. WALLIS: No, no, you're not addressing

8 the sort of acoustic transient in the pipeline.

9 MR. MARTIN: No, we're not looking at

10 that. I mean, we're going to say it's in there, and

11 that's the history of the numerics, the sonic

12 component. That's why there's all of the discussion

13 on, you know, code stability, convergence.

14 The concern was, you know, you want to go

15 as fast as you can when you run this. You want to run

16 it at the Courant choke out limit. You don't want to

17 be stuck here at addressing the sonic -- the wave

18 propagation from these sorts of things, and --

19 DR. WALLIS: So when you --

20 MR. MARTIN: -- the formulation equation

21 for --

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

23 open the break --

24 MR. MARTIN: -- address that implicitly.

25 DR. WALLIS: When you break open the

1 break, does the -- a scan to the long pipe of the
2 vessel -- does the vessel immediately know that the
3 break is open? Does the mathematics --
4 MR. MARTIN: No, it --
5 DR. WALLIS: -- all along these nodes and
6 get to the vessel at no time?
7 It does propagate from node to node.
8 MR. MARTIN: Yes. I mean, you'll see
9 it -- let me explain this slide here a little bit.
10 This is the vessel side break node pressure. You open
11 it up here at 2250.
12 DR. RANSOM: That was the original
13 pressure, 2250?
14 MR. MARTIN: That was the steady state
15 pressure.
16 DR. RANSOM: Okay.
17 MR. MARTIN: Okay? And then this opens,
18 and it immediately drops.
19 DR. RANSOM: It decompresses.
20 MR. MARTIN: And you'll see that. You can
21 look down a little bit farther. This will dissipate.
22 You won't see this drop. At least numerically you
23 won't see that.
24 Spin, once that choke model comes on you
25 no longer see the downstream pressure effect, and then

1 it basically resettles, and so there's a period here
2 of, you know, maybe, let's see, that would be ten, 20,
3 20 time steps before you just rebalance that.
4 MR. SCHROCK: What is it that's predicting
5 this sudden drop in pressure?
6 MR. MARTIN: Excuse me?
7 MR. SCHROCK: What is it that is
8 predicting that sudden drop in pressure?
9 MR. MARTIN: Well, now you're going from
10 2250 to 14.
11 MR. SCHROCK: Right.
12 MR. MARTIN: Fourteen, point, seven, and
13 you know, you're applying basically -- and I don't
14 want to say momentum equation -- but basically from
15 the Bernoulli standpoint you've got to balance that,
16 and so air you're sucking right up. So it just
17 happens to be very rapid.
18 MR. SCHROCK: But you're treating it as
19 incompressible flow.
20 MR. MARTIN: Incompressible flow, that's
21 correct.
22 PARTICIPANT: That's for the first time
23 step or so?
24 MR. SCHROCK: Well, I don't know. That's
25 why I'm asking him what it is. More than one thing it

1 could be.

2 MR. MARTIN: Well, I mean, the vessel,

3 when you open at that break, I mean, obviously if I'm

4 going down to 14.7, in which case if this volume does

5 not get down that far you would saturate at some

6 point.

7 DR. RANSOM: I think you'll find that it's

8 not incompressible, that it's liquid, you know, which

9 is only slightly compressible, but the methods that

10 are being used there, you are propagating a

11 decompression wave back into the pipe, and the

12 recompression, of course, is when it reaches the

13 vessel and is reflected back as a compression wave

14 again, and then eventually the pressure stabilizes.

15 DR. BANERJEE: And the minimum pressure

16 probably is determined by Alamgir-Lienard-Jones for

17 nucleation.

18 DR. RANSOM: Right. It's the vapor, you

19 know, whatever, where vaporization begins.

20 DR. BANERJEE: Right.

21 DR. RANSOM: Or cavitation, you know.

22 MR. SCHROCK: See, they did that in a

23 tube. They use a forced off end of the tube. they

24 drive the end off the tube very, very rapidly, and

25 they get a rate of decompression by the amount of

1 DR. RANSOM: Well, lower than the vapor
2 pressure, yes. The undershoot is --
3 MR. SCHROCK: The amount of undershoot.
4 So is it the Alamgir-Lienard that determines that
5 minimum pressure?
6 MR. MARTIN: I would say they do not case
7 (phonetic).
8 DR. CHOW: This is Hueiming Chow.
9 I think that one is really because you
10 discharge too much mass. So your pressure -- I mean,
11 you have a volume, and your flow is too high. So
12 instantaneously you lost mass. That's why pressure is
13 brought out. This volume pressure is not a junction
14 pressure.
15 MR. MARTIN: I wouldn't say this is real
16 necessarily.
17 DR. CHOW: Yeah, but that's because you're
18 starting to discharge so much mass.
19 MR. MARTIN: Right. Maybe what I was
20 going to get to Moody's question is that other
21 problems that you can probably solve in the old one
22 are still there, but it really doesn't matter.
23 DR. MOODY: You've protected it.
24 MR. MARTIN: I'm protected it.
25 DR. MOODY: You've limited it so that it

1 doesn't go to infinite velocity or --

2 MR. MARTIN: Right, right. I mean, the

3 model kicks in and prevents it from going any further.

4 Yeah, maybe you have, you know, some unphysical

5 response here.

6 DR. RANSOM: But the question is what --

7 DR. MOODY: What model kicks in?

8 DR. RANSOM: -- what boundary condition or

9 what model do you use?

10 DR. MOODY: Apply then, yeah.

11 MR. MARTIN: Well, at that time it's the

12 Alamgir-Lienard-Jones model.

13 DR. RANSOM: For the pressure undershoot,

14 right?

15 DR. CHOW: Yeah, for the undershoot.

16 MR. MARTIN: But you turn around and use

17 it for the velocity.

18 DR. RANSOM: Right, and you calculate the

19 velocity that would correspond to that, and if it

20 exceeds the -- I mean, if that's less than the

21 velocity, I guess, that you calculated, then you say

22 it's true.

23 MR. MARTIN: Yes.

24 DR. RANSOM: And then apply that as the

25 boundary condition.

1 MR. MARTIN: It is just pretty algebraic.
2 There's nothing special about it.
3 Anyway, I guess my point was going to be
4 the problem is still there. It's just that it doesn't
5 really matter.
6 DR. MOODY: Okay.
7 DR. RANSOM: You feel that you do
8 calculate too great a mass flow rate; is that right?
9 MR. MARTIN: Yeah, initially, but --
10 DR. RANSOM: Too great relative to what?
11 MR. MARTIN: Well, basically based on the
12 discontinuity, I say that because it's not a smooth
13 result. It comes up above --
14 DR. RANSOM: Is it that first peak that
15 you're talking about?
16 MR. MARTIN: -- and then comes back down,
17 and of course, it progresses on. You know, I'm not
18 going to say that I've quantified, that it's
19 overshoot. But just based on those two slides.
20 DR. RANSOM: What are you going to do then
21 to qualify your model?
22 MR. MARTIN: Well, there's two -- to do an
23 application, there's a code; there's a methodology,
24 and maybe this gets into some of what Larry will talk
25 about tomorrow. That's why we have a CSAU process,

1 and a lot of that is where you address what's
2 important first.
3 And I think we'll look at that and say,
4 well, this is a break flow phenomenon, and we have an
5 approach for that that we'll go over a little bit
6 tomorrow. We went over it a lot last year.
7 And something like this would be a very
8 minor component to all of that.
9 MR. SCHROCK: Very minor what?
10 MR. MARTIN: So we would sweep it under
11 the rug maybe.
12 MR. SCHROCK: I'm sorry. I couldn't quite
13 understand what you said.
14 MR. MARTIN: At what point?
15 MR. SCHROCK: You said it would be a minor
16 something to that.
17 MR. MARTIN: A minor component to the
18 phenomenon of rate flow.
19 MR. SCHROCK: Okay.
20 MR. MARTIN: You know, we have a code
21 which provides us a certain utility, and the next step
22 is engineering the methodology on how to use it, and
23 that's where you have addressed a lot of these
24 uncertainties, uncertainties in modeling and
25 uncertainties in phenomena, and that's why I think the

1 CSAU methodology is very powerful, because it takes
2 something that's going to be less than desirable that
3 will get through a committee like this hopefully.
4 MR. SCHROCK: I interpreted Fred's
5 question to be an inquiry as to how the flow is
6 initiated from the stagnant condition at the outset,
7 stagnant with respect to the break, and I guess I
8 haven't understood what your explanation for that is.
9 MR. MARTIN: Well, it's all driven by the
10 large pressure drop which then suddenly appears.
11 MR. SCHROCK: Well, I know it's driven by
12 a large pressure drop, but what is done in a
13 calculation to establish the progression of the
14 velocity at the discharge --
15 DR. MOODY: What I think I -- what I think
16 I heard him say was the very first node after the
17 break is accelerated by upstream pressure all of a
18 sudden looking at one atmosphere or whatever the
19 ambient is, and with that DVDT, which showed up in
20 that first drawing or sketch you gave, trace. It goes
21 up very fast, but when it exceeds the choke flow point
22 of Alamgir-Lienard-Jones, that's when you artificially
23 just cut it off and say, "No further. We're going to
24 bring this down now."
25 So you're talking about the discharge of

1 what's coming out of that end node, and the other part
2 of that is what about node number two, node number
3 three?
4 You're probably just treating those all as
5 inflows and outflow nodes so that the propagation may
6 or may not be real, but at least it's going from one
7 node to the next based on your timing step.
8 You're controlling the discharge flow, and
9 so it doesn't exceed anything real.
10 MR. MARTIN: The boundary condition to
11 that point.
12 DR. MOODY: And now, let's see. If you're
13 on the outside of that, you're concerned about what's
14 coming into the room to pressurize. If you're
15 concerned about -- you know, actually with the --
16 well, maybe you're going to get into it a little bit
17 later about decompression moving up and moving back.
18 Maybe you're going to discuss that when you come to
19 the critical flow, but Virgil has asked about that.
20 I think what you've answered for me is the
21 discharge during that first little bit at least is
22 limited so that you don't over exceed.
23 MR. MARTIN: Okay.
24 MR. SCHROCK: But you're imagining that
25 it's calculated as a plug which has a force acting on

1 it, F equals MA, and you get an acceleration.

2 DR. MOODY: Yeah.

3 MR. SCHROCK: That gives you a velocity at

4 the end of the time step. It doesn't tell you what

5 the pressure is there, and the Alamgir-Lienard

6 correlation, the correlation that gave the amount of

7 pressure undershoot at the inception of flashing.

8 DR. MOODY: Yeah, okay.

9 MR. SCHROCK: And so I don't know how to

10 translate that velocity into the pressure under shoot.

11 DR. MOODY: That may be a dangling

12 question. At least you've answered what you do on the

13 discharge. Right or wrong it limits the outflow. We

14 eagerly wait for the rest of the discussion, I guess.

15 Your conclusion there was what?

16 MR. MARTIN: It may for a short period of

17 time over predict the flow. You still use small time

18 steps to limited that kind of problem. I'm assuming

19 you're looking at a time scale from zero to 300 or 800

20 seconds, whatever it happens to be the length of a

21 transient. You'll never see, you know, the symptom,

22 and a short period of over prediction is

23 inconsequential and conservative.

24 And I wouldn't put a lot of stake in

25 conservative because you're talking about so little

1 anyway.

2 DR. MOODY: I guess the only place that

3 could be vulnerable is if it was a very long pipe and

4 it took maybe the order of a second or two for a

5 pressure wave or decompression wave to go down and

6 come back, and maybe it has to do that a few times

7 before you get to a steady discharge critical flow and

8 whether or not that might be conservative or not,

9 depending on which side of the fence you're on, I

10 guess.

11 DR. KRESS: Your equations calculate the

12 pressure of that first node. Could it be that you set

13 that pressure at this undershoot pressure? Is that

14 the way that works or do you actually put a cap on the

15 flow rate?

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

17 DR. KRESS: It's on the flow.

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

19 Okay. Propagative flows. Please describe

20 how moving pressure or velocity services can be

21 tracked by the code. Do they propagate through a

22 subsystem either as sonic, water hammer, or shock

23 waves, the concern being volume, time average

24 properties made to distort spatial gradients that

25 drive propagation or pressure wave?

1 The statements on that. Pressure wave
2 velocities will typically range, and it's -- looking
3 at relap output actually can be quite broad. I just
4 say below 1,000, although most of the LOCA
5 calculations spends most of his time before 400, I
6 guess, meters per second.
7 Most of the RCS loops are less than 50
8 meters. So it's moving pretty quick around the loop.
9 S-RELAP5 simulates the dynamics of short
10 wavelength phenomena, pressure raise disturbances. So
11 we're not Courant limited.
12 No formal effort is made to track
13 propagation of pressure waves in S-RELAP5 because the
14 fact that sound C (phonetic) is calculated. So --
15 DR. MOODY: If I can translate that, I
16 guess, into other terms you'd say that the time
17 response of a pipe that breaks is very short compared
18 to the time duration of the transient, overall
19 transient that you're trying to analyze. So you can
20 basically skip over it, and it isn't too important.
21 MR. MARTIN: Right. You know, we don't
22 really look at it and don't really see it. I think a
23 lot of these systems are so complex that after the
24 initial shock wave you don't see anything. I mean,
25 things start reflecting all over the place.

1 I did this calculation years ago, and I
2 think you can see, you know, some effect of a pressure
3 wave like that, and this is a gravity wave problem
4 where you have a stack of water up. It's a
5 hypothetical problem where you have a stack of water
6 up, a stack of water down, and you let go and while it
7 does this bouncing, initially you see this little --
8 this echo up here, and it corresponds to that pressure
9 disturbance. It ends up getting amplified. You go
10 back again, and it starts damping out for that reason
11 or for other reasons probably.
12 But that's the only time you see it. I've
13 seen this. It's been obvious. Outside of that, you
14 know, you just can't look at the hydraulic parameters
15 and recognize a pressure wave kind of going through
16 there.
17 DR. MOODY: The little wave length or the
18 little --
19 MR. MARTIN: A very short one, and this
20 one has a relatively low sonic wave speed.
21 DR. RANSOM: Is that a stratified flow or
22 what is that?
23 MR. MARTIN: Yeah, it's just a pipe.
24 DR. RANSOM: The pipe with stratified --
25 MR. MARTIN: You've seen this problem

1 before. I did it back when you were there.

2 DR. RANSOM: This is the horizontal

3 rhonometer (phonetic) problem?

4 MR. MARTIN: Yes.

5 DR. RANSOM: So really it's the

6 propagation of the void wave back and forth in the

7 pipe.

8 DR. BANERJEE: Sixteen meters per second.

9 That's a fast void rate. It's almost like a two phase

10 pressure wave, two phase.

11 MR. SCHROCK: It is two phase.

12 MR. MARTIN: Yeah, this is -- maybe I

13 didn't talk about it. This is the void fractions at

14 half and --

15 DR. BANERJEE: That's right.

16 MR. MARTIN: So it is.

17 The Framatome experience, small pressure

18 disturbances are imperceptible in plant analysis.

19 Break provides the singular large disturbance in LOCAs

20 and begins to dampen when the choke model is applied

21 quick, very early.

22 The only other time I can think of when

23 you might see something is when maybe ECC came on and

24 you had the cold water coming and you might get an

25 effect in condensation.

1 Again, I can't pick up on that, but maybe
2 there's not enough variables in RELAP5 that address
3 that. You know, we always have those in our hip
4 pocket. It's generally not important for LOCA
5 application.
6 DR. BANERJEE: Is it true always that
7 these pressure waves don't give you effects on core
8 structures and things? These are not considered
9 important for LOCA?
10 MR. MARTIN: That's the next one.
11 DR. BANERJEE: I see.
12 MR. MARTIN: Yeah, it's something I can't
13 say I've previously thought about, you know, at least
14 not for a long time, but I know a lot of effort has
15 gone into looking at water hammer and pressurized
16 thermoshock.
17 DR. BANERJEE: They seem to worry about
18 this in Europe quite a bit.
19 MR. MARTIN: Yeah, I think the rules in
20 Europe are a little bit different.
21 DR. BANERJEE: Yeah.
22 DR. MOODY: Maybe it would be a little bit
23 more palatable if you say for the transients that are
24 analyzed at the S-RELAP5, that your propagation
25 effects are negligible or nonconsequential.

1 I'm thinking some years ago that the NRC
2 actually used one of the RELAP programs to determine
3 what happens when a pipe broke within a biological
4 shield. The short side of the break, immediately
5 steady state or quasi-steady state, critical
6 discharge, but the long side of the break, with the
7 way it was calculated without limited the Bernoulli
8 fall, it shot out of there and the doggone thing about
9 -- it reached an overturning moment on that biological
10 shield, and that, of course, made a lot of eyebrows go
11 up until it was shown that wait a minute; that other
12 side of the pipe, the long side, there was a
13 propagation effect that slowed things down while the
14 pressure waves moved around the biological shield, and
15 that's a propagation effect.
16 And when you balance the two, by the time
17 the pressure wave returned from the pipe, the pressure
18 wave had reached the back side on the biological
19 shield and was beginning to even up. So it was
20 nonconsequential.
21 But still it was a case where propagation
22 effects were important. Can we say that there is no
23 need for propagation effects then in whatever S-RELAP5
24 is going to do?
25 MR. MARTIN: I think we can say that

1 safely.

2 DR. MOODY: Will some user down the road

3 ever been tempted to use it for -- well, maybe you had

4 better put a big statement in front.

5 MR. CARUSO: Dr. Moody, this is Ralph

6 Caruso from the staff.

7 I believe that this issue used to known as

8 an issue called asymmetric LOCA loads.

9 DR. MOODY: Yeah.

10 MR. CARUSO: Does that phrase ring a bell?

11 DR. MOODY: I think so.

12 MR. CARUSO: It was resolved when we went

13 to leak before break.

14 DR. MOODY: Okay.

15 MR. CARUSO: That was one of the reasons

16 why we accepted the leak before break concept, was to

17 resolve the asymmetric LOCA loads issue because this

18 is extremely difficult to calculate.

19 There were some other reasons, but based

20 on acceptance of leak before break asymmetric LOCA

21 loads are not considered anymore.

22 Now, someone might ask: well, why do we

23 consider large break LOCAs? And you would have to ask

24 the Commission that. They acknowledged that there was

25 a bit of an incongruity in regulations when they did

1 that.

2 So we still consider them, but we don't

3 consider the asymmetric LOCA loads.

4 MR. MARTIN: And that's basically what

5 we're talking about here with that, and I'll just skip

6 on over to the selection of the nodes.

7 To some extent you brought this up in

8 Ken's discussion. I'll just overview it here, kind of

9 emphasize the priorities we had in this step.

10 Please describe how various node sizes are

11 selected at a given system, providing assurance that

12 the dominant phenomena are predicted, are presented of

13 the actual system response being analyzed. Of course

14 nodalization may mask important phenomena.

15 Okay. This is kind of where we get

16 into -- we move beyond code and talk about

17 methodology, and then I've thrown up these quotes from

18 the CSAU bible quantifying reactor safety margins.

19 The plant model must be nodalized finally enough to

20 represent both the important phenomena and design

21 characteristics of a nuclear power plant, but coarsely

22 enough to remain economical.

23 Number two, thus, the preferred path is to

24 establish a standard nuclear power plant nodalization

25 for the subsequent analysis; minimizes or removes

1 nodalization and the freedom to manipulate noding as
2 a contributor to uncertainty.
3 MR. BOEHNERT: Excuse me. Are those
4 emphases yours or from the --
5 MR. MARTIN: This is from the Technical
6 Program Group.
7 MR. BOEHNERT: Okay, okay.
8 MR. MARTIN: That did this 13 years ago or
9 whatever.
10 Therefore, a nodalization selection
11 procedure defines the minimum noding needed to capture
12 the important phenomena.
13 DR. WALLIS: That's interesting. This
14 fixing the standard nodalization doesn't really remove
15 uncertainty. What it does is it removes flexibility
16 in prediction. It simply forces you to use one
17 nodalization.
18 There may be uncertainty associated with
19 how well that represents the real thing.
20 MR. MARTIN: The idea being that it
21 would --
22 DR. WALLIS: It's a fixed uncertainty.
23 MR. MARTIN: -- move the uncertainty to
24 your analysis, you uncertainty analysis later.
25 DR. WALLIS: Yes, but you know, it means

1 that you don't actually investigate uncertainty.

2 MR. MARTIN: Well, and that's not

3 completely true either because, you know, like has

4 been said, the result can be very sensitive to how you

5 nodalize things, and so you want to be careful that

6 you do a reasonably good job.

7 DR. WALLIS: I think what's done is to get

8 a nodalization that drives you to some extremes so

9 that you're conservative. It's a departure from the

10 best estimate code. You nodalize so that your PCT is

11 the maximum rather than doing a whole lot

12 nodalizations doing a spectrum of PCTs and saying,

13 "Well, we'll take some sort of average nodalization,

14 and then we'll look at sensitivities about it."

15 I think the tendency is to say, "Well,

16 look at the extreme case and nodalize that one."

17 Isn't that what's done?

18 MR. MARTIN: Yeah. You know, again, if

19 you're fitting this into CSAU type methodology, all

20 along you've got to be thinking we need to address

21 uncertainty in particular phenomena, and in many cases

22 you can just go to data and take care of it.

23 Sometimes, and maybe they could test

24 examples of ECC bypass, it becomes difficult to get

25 the right kind of data to cover that, and nodalization

1 in this case can be used, you know, to demonstrate
2 it's conservative against the test data because you
3 have a certain nodalization. It can then be used to
4 argue that you're covering that uncertainty.
5 And in fact, that's what we're doing as
6 far as the ECC pipe.
7 DR. WALLIS: -- philosophically about
8 saying, "Let's just be conservative."
9 Let's nodalize and get a conservative
10 result rather than saying let's nodalize different
11 ways and look at a spectrum of results in order to
12 define that uncertainty. That's a different
13 philosophy.
14 One is the old, conservative approach.
15 The new one is perhaps the best estimate with
16 uncertainty approach. So removing noding is -- you're
17 essentially saying, "Let's be conservative about
18 noding based on experience," rather than use noding as
19 a way of looking at uncertainty due to noding.
20 MR. MARTIN: We do that sometimes. You
21 know, where you can, you want to do best estimate, but
22 you know, with limited data, ultimately I think you
23 have to show against data that you do okay, and that
24 really is -- from the outside I'm looking at this
25 stuff. If I see that, you know, one, either you don't

1 have any data to support it or that you don't do a
2 very good job against data, then I have no confidence.
3 If you have a situation where you don't
4 have data or you have limited data, I mean, that's --
5 we have a lot of data out there, but in some cases,
6 maybe such as these, you have a limited amount of
7 data, and there you might want to say, "I'm covering
8 the uncertainty of the fact there is a limited amount
9 of data in this area by noding up this way."
10 DR. WALLIS: I think with CFD what you try
11 to do is you try to nodalize more finally until it
12 doesn't make any difference anymore. So it's
13 asymptotically approaching what you believe is, let's
14 say, the right answer.
15 I suspect in this case there are just so
16 many games you can play with how you nodalize
17 different places. You're not really converging
18 unannounced on nodalization. There's always
19 uncertainty associated with how you nodalize.
20 MR. MARTIN: And again, the downcomer is
21 a good example because when you get finer nodes,
22 you'll get a different answer, and before we talk
23 about the RAI, so if you have a chance to look at that
24 to some extent, but coarse nodes, you have to think,
25 you know, what does that do. You're modeling this

1 downcomer or this big, round thing, and I have a point
2 here and a point there and a point there, and that
3 describes my azimuthal. What's my mean free path from
4 break or from intact loop to broken loop?
5 Well, if I have to leap over a hot leg,
6 then it's a bit -- well, the coarse node is shorter
7 than if you have more detail. If you have to go
8 through, you know, six points versus three points, my
9 mean free path is a lot shorter with a coarse node,
10 and that's why more water is flying out in the coarser
11 node.
12 You've got to think of what do you get
13 when you model like you do, and certainly with fine
14 nodes you get closer to reality.
15 DR. RANSOM: I'd like to make a comment
16 along those lines. I think that sort of in general
17 that I've argued against this idea of convergence or
18 finer and finer nodalizations, ad infinitum, with
19 these methods for several reasons, and the most
20 fundamental one is these are average models, and some
21 people like to look at them as area average, but in
22 reality they have their genesis in volume averaging
23 methods and time average.
24 And so all of the parameters are finite
25 parameters having to do with things like flow regime,

1 frictional factors and things like that.

2 So it isn't true that necessarily finer

3 and finer nodalization make much sense. In fact, my

4 philosophy is that you could go down to as small as,

5 say, a pipe diameter, and that might make some sense

6 for axial nodes.

7 And these kind of studies have been made

8 and these do, indeed, more or less converge, but going

9 a little bit further to the assessment that evolved

10 over the years from application of the law of

11 semiscale and other experiments like that, is you can

12 see some of these plenum models don't make a whole lot

13 of sense from a physical point of view.

14 And so the question could be raise, you

15 know, how well do they work actually, and so they

16 applied those to the different experiments and found

17 nodalizations that agreed with the data and were

18 satisfactory, especially within efficiency, I guess,

19 in the old days.

20 Now, today we could afford more nodes. So

21 that's not quite as big an issue, but then the

22 philosophy was that if it worked in that case, a

23 prototypic experiment, you'd better no change it if

24 you're going to go model a plant and, you know,

25 include something about the peak clad temperature.

1 And so I think even a CSAU methodology
2 spells this out, that the nodalization should be
3 developed at the prototypic experiments and justified
4 on that basis, and then that same philosophy used to
5 model the plant.
6 DR. WALLIS: You mean it should be based
7 on the system experiments.
8 DR. RANSOM: Yes.
9 DR. WALLIS: As a scaled system.
10 DR. RANSOM: Yes.
11 DR. WALLIS: But you don't mean full scale
12 when you say prototypic.
13 DR. RANSOM: Well, if we had full scale,
14 but unfortunately -- and I think the uncertainty has
15 to be, you know, derived from those experiments.
16 DR. WALLIS: You mean a scaled experiment.
17 So the APEX --
18 DR. RANSOM: Semiscale or LOFT or APEX or
19 PUMA or any of the other experiments.
20 DR. BANERJEE: The problem is the idea
21 that you're putting forward is difficult to apply to
22 scale-up since you don't know what is the appropriate
23 scaling parameters. You have really not done a lot of
24 work on nondimensional groups with scale.
25 There is no similitude theory for these

1 things. So the only thing you can appeal to in some
2 way is that the results don't change very much with
3 the nodalization scheme that we've got.
4 If the results are very sensitive to
5 nodalization, then you've got a whole new set of
6 parameters which you can adjust, and one comment was
7 that all he needed was three parameters, and he could
8 fit anything. And here you've got what? Including
9 the nodes about 500 or something.
10 So I don't think that argument really
11 holds water like --
12 DR. RANSOM: Well, I've never heard a
13 better argument so far.
14 DR. BANERJEE: Well, the argument should
15 be that your nodalization schemes should not affect
16 the results very much.
17 DR. RANSOM: Well, I think in general
18 that's --
19 DR. BANERJEE: You don't have to converge
20 in the normal sense of mathematical convergence, but
21 nonetheless, each time you change a nodal subdivide
22 one, your results change a whole lot and there's
23 something totally wrong.
24 DR. RANSOM: Well, I think you've even
25 heard today that small changes in nodalizations have

1 not changed the results very much, but I think the
2 loss philosophically -- let's say you model a T
3 differently when you go from the prototypic experiment
4 to the full scale plant safety calculation. I'd say
5 that's kind of dangerous without knowing, you know,
6 what the effect of that nodalization change is.
7 And in terms of scale, now geometrically,
8 of course, the scale is contained within, you know,
9 the structure of a code. The correlations are based
10 on Reynolds number, Weber number, Prandial number, you
11 know, on and on, and an attempt to make these things
12 at least dimensionally independent, and so those
13 become the basis of scale.
14 And I don't know that there's any reason
15 to believe extremely suspicious, you know, of the
16 scaling argument.
17 DR. BANERJEE: I think there is. If
18 you've got a T junction pulling liquid out of an ADS-4
19 valve, and you do this experiment in a one inch pipe
20 or a four inch pipe, and then you have this huge thing
21 which is what, 14 or whatever inches it is? I think
22 the phenomenon is going to not scale that way, but
23 there, again, you can examine that from, say, boundary
24 layer point of view and see what effect the boundaries
25 would make on that sort of thing.

1 You know, there are extremes of scale that
2 certainly you're going to find effects and you're
3 going to have to be careful.
4 DR. BANERJEE: I can probably think of ten
5 examples where you have scaling arguments which need
6 to be made then in a detailed way and actually applied
7 to look at the full scale plant compared to the
8 experiments, maybe 20 areas.
9 DR. RANSOM: I don't profess to be an
10 expert in this area, and a group of experts put
11 together CSAU, and that seems to be the methodology
12 that's being followed, you know.
13 DR. BANERJEE: But Graham and I were on
14 the peer review group unfortunately.
15 DR. RANSOM: Well, I guess you guys can
16 explain it then.
17 DR. WALLIS: The topic is node size and
18 node scaling. I think scaling is a separate question
19 from the node size.
20 DR. BANERJEE: Well, the nodes, what
21 you're saying when Vic says fix it for these
22 experiments and then hope for the best for the
23 reactor, the scale is factor of ten or 20 or something
24 different in certain areas, you know. So --
25 DR. WALLIS: I can see if something like

1 this was scaling. If what's happening in some device
2 is, say, being governed by, say, the rate at which
3 bubbles can be released from the stratified layer of
4 liquid, and so it takes a certain time; if that
5 becomes -- if the scale for that to happen relative to
6 the scale at which things are happening on this big
7 node size is different when you change the scale of
8 the big node, then you've got something different
9 happening.
10 DR. BANERJEE: I'll give you a classical
11 example of this. They were doing small scale
12 experiments on chemical reactors, which is somehow
13 related to these reactors, and they found that
14 emergency relief was fine.
15 They went to the big reactors. They
16 continued to blow up. Okay? And it's a very simple
17 reason that was found. It was found that the level
18 swell in a small reactor when they do this doesn't get
19 to the vent because the level swell depends on how
20 much liquid there is to begin with, which scales as
21 eight.
22 But when you go to something 30 feet high,
23 the level reaches the top and you get two phase
24 flowout. It's a very simple example of where the
25 small scale experiment is completely wrong compared to

1 the full scale stuff.

2 DR. WALLIS: -- was saying the rate of --

3 if you have a small scale experiment, the bubbles can

4 detach from the liquid layer at a certain rate. If

5 you change the scale of things, things are moving up

6 more rapidly. The bubbles can't detach. So you

7 entrain them because these two phenomena don't scale

8 the same way.

9 DR. BANERJEE: Right.

10 DR. WALLIS: One happens on a scale of a

11 foot or something, and the other one takes the node

12 and sweeps it up too quickly for the bubbles to

13 detect. You've got to look at these two effects.

14 One of them changes the scale and the other one

15 doesn't. Therefore, there may be a change in scale.

16 DR. MOODY: I missed something. We were

17 talking about scale, and we were talking about

18 nodalization. How did we make the transition?

19 DR. BANERJEE: Well, he was saying you can

20 fix the scale -- nodalization based on small scale

21 experiments and just carry it over to the reactor, and

22 I was arguing you cannot.

23 DR. RANSOM: Well, there are examples

24 where you have to be reasonable. For example, in the

25 old Semiscale experiment, they used a pipe downcomer,

1 and you'd say, okay, a pipe was fine for that, and
2 then you went to a large break LOCA with an annular
3 downcomer in a plant, and you'll find that the bypass
4 is quite different, and so the nodalization has to be
5 changed.
6 I mean, you have to temper that with some
7 reason, I would say. After good scaling arguments, I
8 think these kinds of things are done, but some of
9 these things you bring up are a reason for continued
10 research in this area that the NRC if they are safety
11 issues should be concerned with.
12 DR. MOODY: Is the bottom line scaling
13 really depends on the application? I don't mean
14 scaling. I meant nodalization depends on the
15 application largely.
16 DR. RANSOM: I don't know. I think that's
17 true.
18 DR. BANERJEE: I mean, in a sense what Vic
19 was saying was -- I recall what CSAU, the methodology
20 was agreed on that this was a good starting point;
21 that this sort of gives you the right sort of
22 nodalization because it works somewhere. We don't
23 know where, and things are relatively insensitive, but
24 then you've got to look at phenomena, you know, which
25 are highly ranked and make sure they're properly

1 scaled, you've got enough nodalization, and everything
2 is properly documented.
3 Now, that's not quite the same thing
4 that's being said there though.
5 DR. MOODY: Go back to the earlier example
6 where he was getting immediate discharge out of the
7 end of a broken pipe. If you had very small nodes or
8 very large nodes, I think that would be handled the
9 same way. You still -- in one case a large node would
10 take a little longer to get up in the Bernoulli flow
11 to exceed the Alamgir-Lienard-Jones criteria, whereas
12 if you had a very short node, it would just be a
13 millisecond or less to get up there, but you would
14 still limit the flow, and you are saying what really
15 happens in a transient sense doesn't matter too much.
16 So we might as well use about any convenient
17 nodalization in the piping and also other parts of the
18 system.
19 You must look at those and see what's
20 happening in each part of the system. How fine do I
21 need to know this, some property?
22 MR. MARTIN: Yeah, ultimately there are a
23 number of measures when it comes to how good your
24 nodalization is. At least, you know, I did the work,
25 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

1 phenomena themselves, which isn't captured by just
2 legislating that you have the same geometrically
3 shaped nodes in the two scales.
4 MR. MARTIN: Right, and that's why we have
5 a rather large suite of assessments. We did do the
6 uncertainty analysis on what, a six, eight R-BEACON
7 tests, and they had some variation in scale there.
8 It's now whole scale, and that was where our
9 certificates came from, but at the same time we did do
10 LOFT, and we did do the Semiscale thing where it would
11 have a blowdown, and while that wasn't included in
12 uncertainty, you can look at that and say, "Well, it
13 was pretty damned close," and you move on.
14 Again, and also break load is something
15 special because we also arrange break area to address
16 that portion of the regulation. So it's somewhat
17 unique.
18 DR. KRESS: What does that last sentence
19 on that --
20 MR. MARTIN: Oh, I didn't read the last
21 sentence.
22 DR. KRESS: What does it mean?
23 MR. MARTIN: This procedure starts with
24 the analyst experience in previous code assessment and
25 application studies and any document nodalization

1 studies.

2 Next, nodalization studies are performed

3 during the simulation of separate and integral

4 effects, code data comparison, and finally, a

5 derivative process using the nuclear power plant

6 models is employed to determine sufficiency of the

7 nuclear power plant model nodalization.

8 Is that the last --

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

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

11 on.

12 DR. KRESS: Yeah.

13 MR. MARTIN: That's what I call a

14 shakedown. The model shakedown, in the previous code

15 assessment, documentation of what people have done

16 for, you know, the last 30 years. That's out there

17 already.

18 You know, then you can, of course, play

19 with that nodalization on the small scales, you know,

20 for data, and then I'm just saying you can play with

21 that on the big scale because you have other things --

22 DR. KRESS: You're looking for sensitivity

23 on that last one there?

24 MR. MARTIN: Right. You're also looking,

25 you know -- that's when you have to pull in the

1 drawings of the actual plant you're actually doing,
2 and there are structural differences, you know. The
3 separate and integral tests are somewhat idealized at
4 the real plant, and you know we spent a lot of time on
5 upper plenum.
6 You have a certain amount of asymmetry
7 just structurally that you have to address, and that's
8 shakedown. You know, you want to address the
9 important phenomena, again. You know, up there
10 because of the asymmetry of flow --
11 DR. KRESS: But the problem with the
12 sentence though is I couldn't figure out how to
13 determine what was meant by sufficiency.
14 MR. MARTIN: Oh, okay. Sufficiency?
15 That's an engineering judgment.
16 DR. KRESS: Okay.
17 MR. MARTIN: That's budget, too.
18 DR. KRESS: Okay.
19 MR. MARTIN: But we did spend an awful lot
20 of time and Larry was in my office. "Are you done
21 yet?"
22 And I would say Monday every week.
23 DR. WALLIS: Engineering judgment.
24 Sufficiency is determined by engineering judgment,
25 which is a very hard thing to quantify.

1 MR. MARTIN: It is a hard thing to
2 quantify when it comes to --
3 DR. WALLIS: How do I know how good your
4 engineering judgment is?
5 MR. MARTIN: One of the big things that I
6 focused on was even though it's not nodalization, but
7 time step sensitivities because that is something you
8 can't play with to some extent.
9 And of course, what I ended up doing is I
10 went down to basically what was a tolerable limit. As
11 you know, we run like 59 cases, and for this to be a
12 practical methodology we need to have a turnaround
13 within a week, you know, throw in the calculations,
14 and so you know, with the three loop sample problem we
15 had in there, those are taking between three and four
16 hours to run right now, and you multiply that by 60
17 and you get a pretty large number, and that's where I
18 came out.
19 But anyway, I played with the time
20 sensitivities a little bit using the same statistical
21 approach that we talk about to quantify, by randomly
22 varying time steps, to quantify what is, you know, the
23 certainty related to these time steps, and to some
24 extent that also translates to nodalization because
25 that's part of it.

1 DR. WALLIS: What I think you do is the
2 following. You determine sufficiency of a
3 nodalization by thinking of ways in which it could be
4 insufficient and exploring those and satisfying
5 yourself that you haven't found those and that you've
6 resolved what look like insufficiencies, and you say
7 it's now sufficient. That's the way you do it.
8 We have to sort of rely on your integrity
9 to explore all of these possible reasonable ways in
10 which it could be insufficient and then to conclude
11 that I haven't found any that really make it
12 sufficient. Therefore it's okay.
13 MR. MARTIN: Well, that would come out of
14 our PIRT review teams. They'd come out and, you know,
15 I would stand up there and somebody like Mark
16 Thorogood or Larry Hochreiter would say, "You can't do
17 that," or, "Why don't you do this?"
18 The same kind of form as we have here, and
19 you know, half the time they would have a point and go
20 back and play with it. You know, this is a long time
21 coming to get to this point.
22 DR. WALLIS: Yes.
23 MR. MARTIN: So it has gone through some
24 fire to get to this point.
25 MR. O'DELL: This is Larry O'Dell with

1 Framatome. I guess I would say, too, that we did
2 really do quite a few iterations on that in the CSAU
3 process because we started with a nodalization that
4 was put together based on sort of our previous
5 experience using the code and, you know, industry
6 experience. We did a series of comparisons of the
7 plant calculations to see how the analysis went.
8 We modified it, the nodalization. Then we
9 went back, ran a series of assessments primarily LOFT,
10 CCTF, UPTF, and FLECHT SEASET experiments covering
11 ranges of scales and looked at the nodalization there,
12 made nodalization changes, went back through it again.
13 So this was a real iterative process that,
14 you know, we ran an awful lot of cases in this to get
15 this final nodalization.
16 MR. MARTIN: Okay. Just some of the
17 necessary conditions that I would apply on
18 nodalization. Number one, discriminate key
19 structures' characteristics. This is going to the
20 drawings. You've really got to match the drawings
21 first.
22 Attain acceptable steady state agreement
23 with the plant. Okay. There's a ton of art there.
24 Some of that is the form losses, and we have to
25 validate those form losses.

1 The reality is that Crane does a really
2 good job. Crane and IDLECHEK do a really good job,
3 and there's not a lot of tweaking that has to go on,
4 and that's fortunate.
5 Of course, we observed the phenomenon in
6 this, and that's really the point of your question,
7 and that's where we apply the CSU philosophy, where we
8 identify the important phenomena and focus on that and
9 try to identify what --
10 DR. WALLIS: That's where scaling might
11 make a difference; that if you've got, you know, a
12 small scale, you've got more velocities, and they're
13 small relative to relative loss to bubbles. So the
14 relative loss to bubbles is important. In the big
15 scale everything scales up, including the velocities.
16 And maybe the bubble slip is relatively
17 unimportant so that the dominant phenomena have
18 changed by changing the scales, and this is an
19 important bullet.
20 MR. MARTIN: It's an important bullet.
21 You know, there's not a lot of full scale data, right?
22 So I guess in many ways the scale you address by
23 getting the scale you have, and there is a broad scale
24 up there. I mean somebody scaling to LOFT is a pretty
25 big range, and there's things in between.

1 You can develop a lot of confidence by at
2 least covering that range, and then if there is full
3 scale data, then obviously that goes a long way, too.
4 But I think we've covered that and documented that
5 pretty well in EM-2150.
6 Always in the back of my mind is maintain
7 reasonable computational economics, and on some level
8 like the downcomer nodalization is what it is, you
9 know, for that reason.
10 DR. RANSOM: How many nodes are you using
11 in the large break LOCA in total now?
12 MR. MARTIN: Total?
13 DR. RANSOM: Yeah.
14 MR. MARTIN: I can't count it up, but
15 just look at the core. We have 24 times the four
16 rings. There's 100 there. Gosh, probably at least
17 double that.
18 DR. RANSOM: Two hundred nodes?
19 MR. MARTIN: Two hundred.
20 DR. RANSOM: And that's what, for your
21 steam generators?
22 MR. MARTIN: Yeah. It's probably more
23 than 200. It's probably 300 because you multiply it
24 by, you know, each loop.
25 DR. RANSOM: Now, those are pretty

1 computationally intensive still?

2 MR. MARTIN: Yes, they are still. You

3 know, we're current generation minus one or two, you

4 know. We have 200 megahertz machines, and you know,

5 maybe we have a few processors so we can run some of

6 these parallel, and then we have turf wars on who gets

7 computer time, and that's normal engineering

8 environment.

9 But you know, the three loop sample

10 problem we provide is probably the quickest running

11 we've got, and that's unfortunate because I thought

12 that was the bleeding limit, you know, three and a

13 half hour calculations, but we have -- you know, we've

14 learned in the last year when we address some of these

15 low containment pressure issues that you can have this

16 large break LOCA go out 1,000 seconds before you get

17 quench.

18 And that calculation is taken closer to

19 six-plus hours.

20 DR. RANSOM: How much?

21 MR. MARTIN: Six-plus hours. so it gets

22 a little painful, but we're kind of -- you know.

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

24 those.

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

1 Obviously that's what we said we were going to do, and
2 you know, we'll just charge the customer a little bit
3 more for it.
4 (Laughter.)
5 DR. KRESS: With the increased
6 computational capabilities, as computers get better
7 and better, that bullet on maintain reasonable
8 computational economics might disappear some day. If
9 it did, I think it would be worthwhile thinking about
10 what Vic Ransom said, that there's probably some node
11 size where it doesn't make any sense to go finer than
12 anyway for other reasons.
13 Now, what I'd like to know is what are
14 those other reasons, and it might be worthwhile
15 thinking about that because I think that economics may
16 go away one of these days.
17 MR. MARTIN: Wasn't there a paper that I
18 believe Art Shay wrote about the lower limit on node
19 sizes where you may be unable or something like that?
20 DR. RANSOM: I wrote some notes up about
21 12 years ago that argued about this averaging, you
22 know, and what's consistent with the average model,
23 and roughly it's like one L over D, and going beyond
24 that, unless you're treating shocks in a shock tube or
25 something where you know physically that it can be

1 captured by a much smaller control volume, it doesn't
2 make much sense.
3 And we apply these methods to things that
4 involve flow regimes, for example, slugs that even
5 with the coarse nodalizations they have extend over
6 more than one volume, and the physics is not there,
7 you know, in terms of how do these flow regimes change
8 and how are they propagated from volume to volume.
9 I mean, there are a lot of areas like that
10 that really are more uncertain I would guess than some
11 of the things we're dealing with. I mean, it's got to
12 get right, you know, and the onus is on Framatome to
13 get it right so that it can be understood.
14 DR. BANERJEE: In the approximation -- I
15 mean, I don't really want to argue this because I
16 think it's fairly clear that you have an
17 interpenetrating continuum model here. So when you do
18 that from any other field of polymers or whatever,
19 this has to go down to a mathematical convergence
20 equations. There are two fluid equations which are
21 written in many fields. This is not the only field,
22 and they all converge. This is the only field that
23 they don't, in fact.
24 And the reason they don't most of the time
25 is some physics is left out.

1 DR. RANSOM: Well,f or example the
2 nodalization studies that have been made with these
3 methods are like Edward's pipe problem, you know,
4 fairly simple things where you go to maybe 1,000 or
5 2,000 nodes, and indeed, they converge. I mean,
6 there's no question about it. They get to the point
7 where they don't change anymore even probably at 30,
8 40 to 100 nodes.
9 But I'm not sure that makes any sense, and
10 those are, incidentally, down at where the L over D is
11 much less than what I'd recommend that these things be
12 applied to.
13 DR. BANERJEE: Well, I think they should,
14 but leaving that aside, the days when you were running
15 the code and so on, one could defend 300 nodes as
16 being sort of a computational problem. We routinely
17 run problems with ten to the sixth to ten to the nine
18 nodes now in some of the big machines.
19 DR. RANSOM: Right.
20 DR. BANERJEE: Ten to the nine, of course,
21 is the outer limit, but ten to the six is very common.
22 And I don't understand what the big
23 problem is. CFD people run this all the time.
24 MR. MARTIN: Let me give some perspective.
25 We have -- I go to these RELAP5 3D meetings

1 occasionally, and Bettis has been playing with linking
2 RECAP to CFD, and they note basically they've done a
3 large break LOCA where they've taken CFD and handled
4 the core with, you know, I'm sure ten to the sixth
5 order type thing, and then the rest of it is RELAP and
6 LOCA.
7 It took them eight weeks to complete. So
8 I think we're still a ways away.
9 DR. BANERJEE: It depends on how you run
10 it. The CFD codes are run on clusters. We run CFD
11 codes on 32 node clusters with 64 processes. They run
12 fast.
13 MR. MARTIN: And you get a good deal from
14 the manufacturers at universities and stuff.
15 DR. BANERJEE: You can build a 64 process
16 cluster for $50,000. I'll build you one. Give me the
17 money.
18 DR. RANSOM: Santa Barbara is ahead of its
19 time.
20 (Laughter.)
21 DR. BANERJEE: It's just like neighborhood
22 clusters. It's not that big a deal.
23 MR. MARTIN: In time, you know, we'll
24 improve these things. I mean, we are --
25 DR. BANERJEE: So are you running these on

1 sort of a two gigahertz processors or what?

2 MR. MARTIN: Two hundred, 200 megahertz.

3 Well, like I say, we're current generation minus one

4 or two, you know, and these are relatively new

5 machines to me, and so -- but that cost us 20K, you

6 know, something. You might get four heads or

7 something like that, and we still pay 20K for that.

8 DR. BANERJEE: These machines are costing

9 you 20K?

10 MR. MARTIN: That's what they charge, you

11 know, the old companies.

12 DR. BANERJEE: I can go out and buy a PC

13 which does --

14 MR. MARTIN: Yes, we know that. We get

15 mad every time they come back with a quote, and maybe

16 one day we'll just move everything to a PC platform

17 and do it ourselves, but that's a big effort, too,

18 because we have our own qualification procedures that

19 are required.

20 DR. BANERJEE: So you run these on what

21 machines?

22 MR. MARTIN: On Hewlett-Packard.

23 DR. BANERJEE: Hewlett-Packard what?

24 MR. MARTIN: K -- it's called K-box. K-

25 500 or something like that.

1 DR. BANERJEE: And you can't run them on
2 a Linux machine?
3 MR. MARTIN: Our codes aren't qualified
4 for Linux machines, and again, we have our own --
5 DR. BANERJEE: Oh, they're not?
6 MR. MARTIN: -- out own -- I don't know
7 who actually determines our qualifications for
8 platforms, but we got on HP ten-plus years ago, and
9 that's where we're at, and a migration is not a
10 trivial task.
11 DR. BANERJEE: These are not reportable to
12 machines which are like Fortran, Linux machines?
13 MR. MARTIN: We can do -- I mean, you
14 know, we have enough hacks around. I've ported to a
15 Mac, you know. Chow has ported it to Linux, and you
16 know, we play our games at home, but when we do
17 production runs, we've got to keep a standard, and
18 we've chosen the Hewlett-Packard platform for that
19 DR. BANERJEE: So the result change when
20 you run it on different machines?
21 MR. O'DELL: No. This is Larry O'Dell of
22 Framatome.
23 DR. BANERJEE: I would really like to know
24 this.
25 MR. O'DELL: Well, no. It's not a matter

1 of the results changing. We have a process we have to
2 go through to qualify the codes, and we move them
3 from, you know, one compiler to another compiler or
4 one operating system to another operating system on
5 the HP, and if you move to another computer you end up
6 having to go through this full qualification process
7 of a code because we're allowed to use it in licensing
8 analysis.
9 Now, we can just port the code over and
10 play with it and stuff. That's not an issue, but we
11 have to go through this qualification process, you
12 know, in order to have an Appendix B qualified code,
13 and that is not a minor process. It's costly, and it
14 takes a lot of time to do that.
15 So there's some resistance built into the
16 system to being able to move to a code, to another
17 platform and then use it.
18 DR. BANERJEE: So do the results --
19 MR. LANDRY: Sanjoy.
20 DR. BANERJEE: -- or not?
21 MR. LANDRY: Sanjoy, if I may, this is
22 Ralph Landry from the staff.
23 Our regulations require configuration
24 control of a licensing code. That means it must be
25 frozen. It's approved for a particular machine,

1 particular compiler. We don't allow changing that
2 without rereviewing the work of the vendor.
3 That is not to tie their hands, but it was
4 intended to keep people from making changes in codes
5 without our knowing those changes were made.
6 Today -- now that was written back in the
7 mid-'70s. Today you can go out and get a slew of
8 machines, all of them compilers, and run the codes,
9 yes. But you're not maintaining configuration control
10 when you do that.
11 But there has been a history of different
12 machines, whether they are using big ended or little
13 ended CPUs in giving different results. So a code is
14 compiled and run on a particular platform. It's
15 proven on that platform. To change it you have to get
16 permission, and you have to go through the entire
17 requalification program.
18 So it's not not a matter of whether they
19 can go out and buy a Linux box for $900 versus an HP
20 for $20,000. It may cost them more to requalify the
21 code to go to that $900 box than to buy another HP
22 when you consider the cost of what it takes to go
23 through the QA process.
24 So we're not trying to tie their hands
25 with that. This is to maintain control of a code that

1 is regulated under the Code of Federal Regulations.

2 DR. BANERJEE: But if they qualified the

3 code, what is involved, that the code gets the same

4 results or that its results change depending on the

5 platform?

6 MR. LANDRY: There's an enormous --

7 MR. CARUSO: It would be like a new

8 submission.

9 MR. LANDRY: There's an enormous amount of

10 paper attached to this, The manpower, the staff

11 loading, the paper work.

12 MR. CARUSO: The documentation is just

13 enormous.

14 DR. BANERJEE: So they're sort of frozen

15 to one platform or what?

16 MR. CARUSO: Pretty much.

17 MR. LANDRY: When we say a frozen code, we

18 mean that that code cannot have anything changed in it

19 without notifying the NRC. That means they can't

20 change a light in the coding without telling us. They

21 cannot change the compiler without telling us. If

22 they change the platform, they have to change the

23 compiler.

24 DR. BANERJEE: So they're the process of

25 qualifying this code right now or it's already

1 qualified?

2 MR. LANDRY: This code is a frozen code.

3 We have the version number. We have the version

4 numbers for the codes that are interfaced within S-

5 RELAP5, and that defines which platform, which

6 compiler, which operating system, which we happen to

7 have one to match up with.

8 DR. BANERJEE: But this is a new code,

9 right, that they're qualifying?

10 MR. LANDRY: Well, it's the sort of code

11 that's been under development for a decade.

12 DR. BANERJEE: Right, right, but this has

13 to still be qualified, S-RELAP5?

14 MR. LANDRY: They've already qualified

15 before they come in here.

16 DR. BANERJEE: It's already qualified?

17 MR. LANDRY: It has to be. We won't

18 review a copy -- and I'll talk about this tomorrow --

19 we don't review a code that is not a frozen code. It

20 must be frozen, and it must be under configuration

21 control before we will review it.

22 DR. MOODY: Well, that should solve all of

23 the problems really, shouldn't it? If somewhere along

24 the line someone wants to put it on another system,

25 why, then there had better not be any variation in

1 answers because the code is just making
2 mathematical -- it's counting stuff.
3 MR. LANDRY: It's not very easy to move
4 the code to different platforms. One of the other
5 vendors is still running on the VAX, on the VMS
6 platforms, VMS operating system, because they don't
7 want to transfer to UNIX. It's too much trouble.
8 DR. WALLIS: That's truly remarkable.
9 DR. BANERJEE: I didn't know this, This
10 is a discovery.
11 (Laughter.)
12 MR. LANDRY: If I could make one other
13 comment, we were getting pretty far afield from --
14 DR. WALLIS: Yes.
15 MR. LANDRY: -- what Dr. Martin was trying
16 to talk about here with nodalization. The CSAU --
17 DR. WALLIS: I'd like to move on.
18 MR. LANDRY: Huh?
19 DR. WALLIS: I'd like to move on, but why
20 don't you see if we can wrap up this one?
21 MR. LANDRY: Can I make it real quick?
22 The nodalization concept that was put
23 forth in CSAU was to try to get a consistency
24 nodalization approach to the different code modelers.
25 At the time CSAU was written, everybody and their

1 brother was using a different concept and different
2 approach to nodalization.
3 What this was trying to do was to get
4 everybody to a consistent approach that would be
5 consistent with the nodalization that was used on the
6 experiment evaluations and experimental assessment
7 programs.
8 The nodalization that has been used in the
9 past, somebody's wife was using a nodalization on
10 AP600 of 1200 nodes. We had them run a simplified
11 version of that code with under 600 nodes and got very
12 much the same answers.
13 You can get ridiculous in this, and what
14 this whole process is trying to do is say put some
15 rationality, put some sensibility and put some
16 consistency in the approach you take in nodalizing
17 experimental programs and the nuclear power plant.
18 MR. MARTIN: You're referring to my wife.
19 DR. WALLIS: Can we go on?
20 MR. MARTIN: Sure. My last bullet, I
21 guess we can stop here. We've already hit these
22 things: maintain scalability, important; and
23 accuracy; numerical stability; and convergence.
24 And then the conclusions and you can get
25 onto, I guess, the next thing, but initial

1 nodalization is based on experience. Then we go
2 revised based on plant studies and assessments, and
3 then we validated to the performance of final
4 assessments.
5 That's kind of the end there of that.
6 DR. KRESS: How would you start out
7 nodalizing ESBWR?
8 MR. MARTIN: What do you mean? Our SWR-
9 1000 that we've kind of thrown on the docket here?
10 DR. KRESS: Yeah. How would you do that?
11 MR. MARTIN: Well, first, there isn't
12 really a lot of WR experience.
13 DR. KRESS: Not much experience there.
14 MR. MARTIN: And RELAP5. There is some.
15 I believe Brown's Ferry. We do have an old Decker
16 (phonetic) there. So it might be a beginning. I
17 can't tell you I've looked at the design.
18 Probably we start throwing something
19 together initially, and we've only got to capture the
20 phenomenon, you know.
21 DR. KRESS: It would probably build on
22 your experience you've had with the --
23 MR. MARTIN: Sure. We have building
24 experience here. We did EMF-2102, does have a couple
25 of GE tests, you know, 1,000 psi tests, and it's

1 touchy-feely in the beginning, and --

2 DR. KRESS: That's where some tests would

3 be helpful.

4 MR. MARTIN: Exactly. Tests are paramount

5 definitely. No, it will be a tough process, a long

6 process to get that down. You know, the phenomenon

7 and the question itself is key. In my opinion, you

8 know, as far as large break LOCA and BWRs, the big

9 players are break flow, and of course, our treatment

10 is pretty broad and it covers a whole break spectrum

11 in the same process.

12 You have heat transfers is important, you

13 know, the important one, and ECC bypass is important,

14 and then everything else kind of tapers off real quick

15 as being, you know, important for this application.

16 BWR, I'm not quite so sure you can just

17 have a few dominant things and win that way. I mean,

18 you talk about the ADS stuff and maybe the AP600 test.

19 You know, that's a phenomenon there.

20 Obviously Ralph referred to AP600 work

21 that my wife worked on in Idaho, and the code didn't

22 always work. More often than not, it didn't get the

23 right result. There was a lot of code versions that

24 we went through, and I would anticipate that we'll do

25 the same with the BWR work, as well as SWR-1000 once

1 we get down that path.

2 I mean, it's been ten years, 15 years to

3 develop this methodology, and it won't be trivial to

4 go to the next step, but we have a lot of experience

5 now, and we can build on that and be more efficient.

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

7 Framatome again.

8 I would also say that, you know, our

9 counterparts in Germany have been using S-RELAP5 to do

10 BWR plants in Germany already, and we have been, you

11 know, interacting with them in Germany to get their at

12 least initial nodalizations for these types of plants.

13 So we're using that pretty much as a

14 starting point.

15 DR. BANERJEE: Ralph, let me ask you: is

16 this rule also used for reactor physics codes and

17 everything?

18 MR. LANDRY: No. Ralph Landry, staff.

19 Ten CFR 50.46 applies to loss of coolant

20 accident analysis programs only. It is specifically

21 written and applies to light water cooled zirconium or

22 Zircaloy clad uranium dioxide fuel reactors. It's

23 only for calculating LOCAs. It does not apply to

24 physics.

25 We have other regulatory guides and

1 standard review plans that we use to apply to other
2 modeling techniques, physics, transient analyses, et
3 cetera. But there's only one Code of Federal
4 Regulations statement with regard to analysis, and
5 that is with LOCA.
6 MR. CARUSO: I would make the observation
7 that Appendix B, the quality assurance standard, has
8 all of the safety related methods.
9 MR. HOLM: right. This is Jerry Holm with
10 Framatome.
11 I would say that it's the Appendix B
12 requirements that are making us spend all of this time
13 and effort validating the code. Fifty, forty-six
14 requires that we inform the NRC on the LOCA codes. So
15 that's what we would do for LOCA codes.
16 I would change the physics code without
17 telling the NRC, but I still have to validate it under
18 Appendix B if I move from one platform to another. So
19 I'll rerun a whole suite of test cases to verify and
20 get the same answers.
21 And we've had the same experience the NRC
22 has had, that we've moved from one platform. In fact,
23 we've moved from one compiler to another compiler and
24 got different answers. Sometimes we've discovered
25 errors in the compilers that were provided to us.

1 MR. CARUSO: And there are NRC codes that
2 will give you different answers on different
3 platforms.
4 MR. HOLM: So, you know, we have about 160
5 codes, and we move from one platform to another. We
6 have a very big job ahead of us.
7 DR. BANERJEE: So they compile, but they
8 give you different answers.
9 MR. CARUSO: Give you different answers.
10 DR. WALLIS: It's just very strange to me
11 because we will have sort of students running fluent
12 on different platforms for homework, and we accept any
13 of the answers, and we haven't run into problems that
14 we're dependent on the platform or the compiler.
15 MR. CARUSO: There's one NRC code -- I
16 won't say which one it is -- but it had a standard run
17 time of 100 seconds with a standard problem, and
18 that's how you check the installation. You ran it for
19 100 seconds, and if you got the same answer as the
20 standard problem, then you declared that you had
21 installed it successfully.
22 Well, one foreign user decided to run it
23 past 100 seconds on two different platforms, and the
24 problems diverged.
25 DR. WALLIS: Okay. It's an interesting

1 world.

2 (Laughter.)

3 DR. BANERJEE: It will pose problems.

4 DR. WALLIS: Can we move on to the

5 critical flow model? I think these were questions

6 raised by Professor Schrock. Is that what you're

7 going to address now?

8 MR. CARUSO: Yes. If I may, you have one

9 slide with proprietary material on it, and it's a very

10 small piece here. Is it possible for you to just talk

11 around those numbers so we don't have to go into

12 closed session?

13 I mean, people can look at the numbers.

14 Is that acceptable to you guys?

15 DR. KRESS: Yes.

16 MR. CARUSO: Okay. Let's do that then,

17 and I'll make sure that that does not show up in the

18 open portion of the transcript.

19 DR. WALLIS: Now, Virgil, since these are

20 your questions, I think you should have real priority

21 in asking them.

22 DR. RANSOM: Thank you.

23 DR. WALLIS: And being satisfied or not by

24 the answers.

25 MR. CARLSON: Yes, and so --

1 MR. SCHROCK: I looked at your first
2 viewgraph, and I have to conclude for this to make any
3 sense to other people, you need to either paraphrase
4 or read the question that was put to you.
5 MR. CARLSON: Right. Yes, I believe that
6 should have been, I believe, on this slide, and I
7 apologize for that.
8 MR. SCHROCK: Well, it's a little bit
9 lengthy, but self-choking discussion as you have it in
10 three bullets is out of context for the question
11 posed.
12 MR. CARLSON: Oh, okay.
13 MR. SCHROCK: So I don't know if you
14 misunderstand the question or --
15 PARTICIPANT: What is the question?
16 MR. CARLSON: Well, let's see. "A
17 numerical computation of critical flow in pipes,
18 therefore, necessarily requires very fine nodalization
19 as the critical flow location is approached. These
20 realities are not reflected in S-RELAP5 critical flow
21 model, which should be applicable to real geometries
22 where friction often plays a role. Please provide a
23 rationale for answering that . . . model in the
24 context of the above discussion."
25 There's also more discussion about

1 convergent nozzle geometry and other appropriate
2 geometries.
3 MR. SCHROCK: Well, it begins with a
4 statement that it's based or inspired by the Ransom-
5 Trapp model.
6 MR. CARLSON: Right.
7 MR. SCHROCK: Which is a model based on
8 the assumption of thermal equilibrium slip flow at
9 constant entropy, and constant entropy assumption
10 limits the application to convergent nozzles. That's
11 a key statement in the preamble to the question.
12 The geometry of the break in general is
13 not a convergent nozzle.
14 MR. CARLSON: The geometry of the break --
15 MR. SCHROCK: In order to achieve a
16 constant entropy flow you need that specific geometry.
17 MR. CARLSON: Well, I believe the
18 rationale was that the model was developed assuming
19 constant entropy conditions to develop the model, but
20 then it is applied at any time where there is a large
21 pressure difference between an upstream and a
22 downstream node.
23 And the process of using that model would
24 limit the velocities and considered to be choke or a
25 choke point where -- and for the critical flow, it was

1 assumed that wave propagation information downstream
2 does not propagate upstream to the --
3 MR. SCHROCK: That's not an issue in my
4 question.
5 MR. CARLSON: Right.
6 MR. SCHROCK: The issue has to do with
7 what is it that can cause the flow to be accelerated
8 in the channel. Three --
9 MR. CARLSON: Right.
10 MR. SCHROCK: -- physical factors that are
11 involved are area change, okay, area change --
12 MR. CARLSON: Friction and volume --
13 MR. SCHROCK: -- high friction and heat
14 addition.
15 MR. CARLSON: Yes.
16 MR. SCHROCK: And you're ending up with a
17 statement down here in which you are saying friction
18 and heat addition play no role in LOCA. So it's not
19 consistent.
20 MR. CARLSON: Yes.
21 MR. SCHROCK: You don't have a convergent
22 nozzle.
23 MR. CARLSON: You don't have a convergent
24 nozzle.
25 MR. SCHROCK: You do have a change of

1 state which moves the fluid towards the point of
2 choking.
3 MR. CARLSON: Yes.
4 MR. CARLSON: But you don't have an answer
5 as to what causes that change of state to move it to
6 choking. You can't do that in a straight pipe if you
7 do not have friction or heat addition or both.
8 MR. CARLSON: Would Dr. Chow like to help
9 me out on this?
10 (Laughter.)
11 MR. CARLSON: The answer is no, apparently
12 not.
13 DR. CHOW: Originally what I tried to say,
14 that the equation like the constant entropy, that's
15 for the wave disturbance, for the -- I mean, basically
16 that's -- you cannot say that's for the -- remember
17 when you derive the sun speed (phonetic)? You always
18 use constant entropy. That's because you are leading
19 with wave disturbance for very small distance, very
20 small distance and no entropy change.
21 So basically that answers the question
22 about when you try to provide the wave equation for
23 the choke, that's not really the full equation for the
24 flow. That's the full equation for the wave
25 disturbance.

1 So for the wave disturbance, it's a
2 constant entropy process. So in terms of the friction
3 choke, you are talking about compression drop. In the
4 gas, you have a long, very long pipe. Okay? Then
5 your pressure will be decreased because of friction.
6 And in this case your pressure is a very,
7 very long pipe, but that's done like this in the
8 reactor. We don't get over 300 or 400 long pipe, like
9 that. Okay? So --
10 MR. SCHROCK: Three hundred or 400 what?
11 DR. CHOW: Feet, 300 or 400 foot long
12 pipe, like a very, very long pipe. Okay? That's
13 basically -- and you have to have a compression flow.
14 So you have a density change. Basically in order to
15 be a friction choking, you basically have to have a
16 very, very long pipe. Along the pipe the pressure
17 drop-in, and your density for that, the density will
18 be decreased. So because you have constant flow, so
19 your velocity will increase.
20 At a certain point you will reach a choke
21 point where the speed will be equal to the sun speed
22 (phonetic). Okay? So in the compressible flow, that
23 exact phenomenon of the friction choking is there, but
24 I don't think in the reactor system you can find
25 account the friction choking at all.

1 That's what probably says that it's really
2 in the gas dynamics. In the reactor system, for
3 example, the flow is based with more incompressible.
4 I don't know that word. There is no idea that
5 compressible, I know. So you don't have NCI
6 (phonetic) say the density will be decreased along the
7 long pipe like that. so --
8 MR. SCHROCK: I'm having difficulty
9 hearing you well, and I certainly don't understand the
10 point, but I do seem to be hearing that you're making
11 some distinction between compressible flow and some
12 other kind of flow that you imagine exists in the
13 reactor application. Am I correct?
14 DR. CHOW: Yeah. I'm talking about --
15 MR. SCHROCK: The fluid is, in fact, a
16 compressible fluid in the two phase state.
17 DR. CHOW: Yeah, I understand that.
18 MR. SCHROCK: Okay.
19 DR. CHOW: Yeah. From choice --
20 MR. SCHROCK: And all of its behavior is
21 characterized by the gas dynamics arguments that are
22 developed in Shapiro's text. It's not as though when
23 you go to two phase flow you've created some different
24 kinds of processes that lead to choking. It's the
25 same processes.

1 DR. CHOW: Yeah, I understand that. I'm
2 just --
3 MR. SCHROCK: And so I'm not able to
4 understand what your argument is.
5 DR. CHOW: Yeah. I'm trying to say that
6 when you depend on your -- in order to have friction
7 choke, you have to -- pressure have to be decreased
8 along the pipe. Okay? The pressure --
9 MR. SCHROCK: Yes, and in any choking
10 process, the pressure is decreased along the flow
11 direction --
12 DR. CHOW: Yeah, that's right.
13 MR. SCHROCK: -- as you approach the point
14 of choking, and so the issue that I raise is simply
15 that there are three possible ways that this can occur
16 independently or in concert that will lead a one
17 dimensional flow to choking, and those phenomena are
18 the change in cross-sectional area, reduction in the
19 cross-sectional area, the effect of friction, and the
20 effect of heat addition.
21 The Ransom-Trapp model, which is said to
22 be the basis for the RELAP5 critical flow model, has
23 as its initial assumption that the two phase flow is,
24 in fact, an equilibrium flow with slip and constant
25 entropy.

1 But to achieve constant entropy, you need
2 area reduction. So you cannot have the approach to
3 critical flow as described by the Ransom-Trapp model
4 for the case of a straight pipe. Okay?
5 Now, there are a whole range of geometric
6 possibilities from a straight pipe to a convergent
7 nozzle, depending on the rate of reduction in the
8 area. You look at the Marviken geometry, and it's
9 nearly a straight pipe. It's not quite a straight
10 pipe, but it's nearly a straight pipe.
11 You have to have very high mach numbers as
12 you come into the discharge pipe in Marviken. It's
13 not as though you come in with a low mach number and
14 you accelerate to a very high mach number at the
15 outlet of the discharge pipe. In fact, it has to come
16 in at a very high -- because there's very little
17 distance left for the friction to act, very little
18 area reduction to drive it to the critical state.
19 Okay. So in the real geometry of a broken
20 pipe guillotine break presumably you have two straight
21 pipes, short, admittedly short straight pipes, but
22 they are straight pipes, and in order to get to the
23 critical state in those straight pipes, you have to
24 account for it through friction.
25 DR. WALLIS: Why doesn't RELAP do that?

1 RELAP has friction and all of that.

2 DR. RANSOM: Let me -- there's a big

3 misconception here. The Ransom-Trapp model was just

4 a method of characteristics to derive what is the

5 choking criterion, you know, V plus or minus the speed

6 of sound equal to zero, which is a stationary wave.

7 And so we came up with an expression for the speed of

8 sound that would apply.

9 It's a local criterion. It's not an

10 integrated criterion that you would apply all the way

11 down a pipe.

12 In terms of what properties that speed is

13 then based on is the nearest node, you know, the

14 nearest node to the break. And so in that section,

15 indeed, if there is heat transfer, area change, it has

16 to be taken into account there.

17 And you know, certainly in the classical

18 sense area change, heat transfer, although Shapiro

19 only deals with in that section, I think, steady flow

20 process that like in a rocket nozzle or something of

21 that type, but --

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

23 steady?

24 DR. RANSOM: At that last node it is a

25 quasi-steady.

1 MR. SCHROCK: Quasi-steady. So it's not
2 an issue.
3 DR. RANSOM: It's derived from transient
4 equations. So I don't think there's --
5 DR. CHOW: Yeah. I mean, in terms of the
6 code, the code take care of the friction. It is
7 called Adam, the friction. So the phenomenon is out.
8 It may take care if there is their friction there.
9 So while all they try to say that the
10 criteria is just only for the wave disturbance, so --
11 DR. RANSOM: You say on the next
12 viewgraph, "Friction and heat addition mechanisms are
13 important for gas dynamics, but do not play a role in
14 LOCA."
15 DR. CHOW: What we --
16 DR. RANSOM: That is absolutely wrong.
17 DR. CHOW: What we try to say, that that's
18 the -- just we -- basically all of these frictions
19 still in there. Okay? And trying to say in order to
20 achieve that kind of friction choke defined by the
21 Shapiro and that kind of classical case, it doesn't
22 appear in the from (phonetic). Trying to say that the
23 Shapiro, the classical case, to have a friction
24 constant flow in the sense of that does not appear in
25 the actor (phonetic) because you have to have a long

1 pipe in order to get this friction going down along
2 the pipe. That's all I am saying.
3 DR. WALLIS: Well, isn't Ransom-Trapp just
4 used as the choking criterion?
5 DR. CHOW: Yeah, just --
6 DR. WALLIS: Just like M equals one.
7 You're saying what's the effect if M equals one
8 criterion that's used in Shapiro for both the friction
9 and the added heat addition and the area change.
10 DR. CHOW: Yeah, yeah.
11 DR. WALLIS: It's the same. You use an
12 isotropic M equals one as a criterion at the very end
13 of the pipe no matter how you got there.
14 DR. CHOW: Yeah, that's right.
15 DR. WALLIS: Isn't that what you're doing
16 here?
17 DR. CHOW: Right.
18 DR. WALLIS: You're simply saying no
19 matter how S-RELAP5 gets there, when it gets to the
20 Ransom-Trapp criterion we'll say it's choked even
21 though RELAP5 itself isn't running into any kind of an
22 infinite pressure gradient or anything.
23 So you're imposing a different kind of M
24 equals one than RELAP5 itself would predict, but
25 you're using that and saying, "Ah, ha, it's choked."

1 DR. CHOW: Yeah, that's right. That's
2 right, yeah.
3 DR. BANERJEE: Is that because the
4 equations for RELAP5 doesn't contain your
5 characteristics or what?
6 DR. RANSOM: Well, as you well know, the
7 equations are ill posed supposedly, and if you look at
8 it in a differential sense, they've got complex
9 characteristics, but --
10 DR. BANERJEE: So how did you --
11 DR. RANSOM: -- what Trapp and I, we
12 factored the equations, and we threw away the
13 imaginary part of the characteristic groups and then
14 only looked at the real part, which was presumably the
15 real space propagation rate and show that that comes
16 out to be very near the homogeneous equilibrium speed
17 of sound, and it varies with void fraction, of course,
18 and you know, the density ratio.
19 But I don't know. It's an approximation.
20 I mean, it's -- but I haven't seen anything better, I
21 guess, at this point.
22 DR. WALLIS: So S-RELAP5 itself isn't
23 predicting that there's some kind of critical event
24 occurring, that you can't get anymore flowout.
25 DR. RANSOM: Well, generally the idea was

1 that you would let RELAP5 calculate until it
2 calculated a velocity that exceeded the speed of
3 sound, and then you'd say, well, this implied a
4 boundary condition.
5 DR. WALLIS: It seems to me the S-RELAP5
6 might itself have some characteristics which would
7 lead to a prediction of choking before you reach the
8 Ransom-Trapp criteria, in which case you'd be
9 predicting infinite pressure gradients in that last
10 node --
11 DR. RANSOM: That's possible.
12 DR. WALLIS: -- before you've reached the
13 Ransom-Trapp model.
14 I don't know what you do then if Ransom-
15 Trapp is your criterion for choking and you haven't
16 been able to get there because S-RELAP5 won't let you
17 get there.
18 Not taking it away from -- that was the
19 problem I had here, was if you're imposing a choking
20 criterion which doesn't naturally follow from your own
21 equations, you could get into some problems knowing
22 which one to use under some circumstances.
23 DR. BANERJEE: Well, but you know that
24 many people like the French and a lot of people put
25 physical effects in to make the characteristics real,

1 not artificially, but by adding a bubble or whatever.

2 DR. RANSOM: Well, I would argue that

3 their methods are as artificial as putting in

4 numerical viscosity. I mean, they added things which

5 were artifacts and --

6 DR. BANERJEE: -- based on physics.

7 DR. RANSOM: And I have a paper I've

8 written that goes into that subject, but what you show

9 is they artificially stabilize the solution long

10 before you'd see stabilization as a result of, say,

11 turbulent phenomena and, you know, the real dissipated

12 mechanisms.

13 So it's as artificial as --

14 DR. BANERJEE: Well, without arguing that,

15 you know, that's a very detailed argument. The issue

16 would be more whether imposing something on the

17 outside when it doesn't arise naturally in the

18 equations might lead to certain -- well, we know that

19 it leads to sometimes on physical effects when you try

20 to choke things.

21 For example, if your choking went above

22 the sound speed of the homogeneous equilibrium model

23 and the situation is such that the flows were closely

24 coupled, you'd get choking in the pipe at multiple

25 points perhaps.

1 DR. RANSOM: That physically could happen.
2 And in fact, when we originally did this, we used to
3 allow checking for choking at every point within the
4 pipe, and I think over time they've gotten away from
5 doing that because it tended to cause a lot of
6 numerical difficulty, you know, choking, unchoking,
7 choking, and unchoking type of thing.
8 And I'm not sure I'd recommend that
9 because, in general, in the LOCA type of problem, you
10 know where it chokes, you know, at the exit.
11 DR. MOODY: Let me try to help.
12 DR. RANSOM: Or unless there's a
13 contraction upstream somewhere where it might choke
14 like Virgil has brought up.
15 DR. MOODY: But going back to Virgil's
16 original concern, I think the thing he was asking was,
17 first of all, choke flow, constant entropy flow,
18 critical flow at the end of a converging nozzle
19 enables you to go from a stagnation condition to a
20 state of mach equals one for whatever kind of fluid
21 you're using.
22 And, in fact, that can happen anywhere,
23 can't it? In a pipe where you take the local
24 stagnation pressure, which may have experienced a lot
25 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.

1 DR. WALLIS: Then it predicts that PDZ is
2 infinite, and you can't get any further.
3 DR. MOODY: Okay. That's a standing
4 pressure --
5 DR. WALLIS: I think we're concerned that
6 S-RELAP5 might predict DPDZ as infinite in a way which
7 is inconsistent with Ransom-Trapp, and which one do
8 you pick. Isn't that part of the problem being raised
9 here?
10 DR. MOODY: Excuse me?
11 (Laughter.)
12 DR. MOODY: I didn't follow your --
13 DR. WALLIS: Oh.
14 DR. MOODY: Well, I think your concern was
15 real, and just piecing together some of the things
16 that have been said, it sounds to me like you're not
17 assuming isentropic (phonetic) flow through the entire
18 pipe.
19 DR. WALLIS: No, no.
20 DR. MOODY: That's the condition for sound
21 speed, is DPD rho or constant entropy, right?
22 DR. WALLIS: Right, at local condition.
23 DR. MOODY: But that local condition means
24 the local entropy, which may have been really
25 butchered up by friction all the way down the pipe.

1 MR. SCHROCK: Yeah. Well, your rationale
2 is, to answer my own question, that they used the
3 Ransom-Trapp model only as a characterization of
4 choking based on local conditions.
5 DR. CHOW: That's right. That's right.
6 MR. SCHROCK: And then the code has to
7 calculate the approach to that. I think there's some
8 difficulties in the numerical work which is done in
9 approaching the critical point.
10 DR. CHOW: I think your question probably
11 is that when you do that, you have a long pipe, very
12 long pipe, and you may have a choking point, which is
13 actually before the choke --
14 MR. SCHROCK: Well, you have a very long
15 node just upstream of the location where the gradient
16 is extremely strong, and so how you can establish any
17 degree of accuracy in that computation is a problem.
18 DR. CHOW: Yeah, we don't have a very,
19 very long node, and basically you have a few hundred
20 feet, you know, to adhere --
21 MR. SCHROCK: Are you disagreeing, Graham?
22 DR. WALLIS: I just think they can't have
23 a long node. They must have some fine noding near the
24 critical --
25 MR. SCHROCK: No, they don't.

1 DR. WALLIS: They don't?

2 MR. SCHROCK: They don't.

3 Well, what is your noding in the

4 assessment calculations? On Marviken I think you've

5 got about three nodes in the discharge pipe.

6 MR. SCHROCK: If you plot the pressure

7 profile --

8 DR. CHOW: -- about five, I think, five or

9 six nodes under discharge pipe.

10 MR. SCHROCK: You think what?

11 DR. CHOW: Yeah, the Marviken is actually

12 choking in the throat, in the nozzle, not in the

13 discharge pipe. They have a very long discharge pipe,

14 and choking is not happening on the discharge pipe.

15 MR. SCHROCK: Well, I think you need to

16 show the Marviken geometry again if you're going to

17 talk about a throat. It goes into a section which is

18 straight, and then it goes into a section which is

19 small constriction, very small constriction, and then

20 it has a section of divergence; is that correct?

21 DR. CHOW: The vessel --

22 MR. SCHROCK: And the variations are all

23 very gentle.

24 DR. CHOW: Yeah.

25 MR. SCHROCK: Very little change, very

1 little change, not very different from a straight
2 pipe.
3 DR. CHOW: That's right, yeah, and in
4 this case --
5 MR. SCHROCK: And if it's not very
6 different from a straight pipe, then it does mean that
7 you're calculating on the last node upstream of the
8 point of choking over a very wide range of
9 thermodynamic conditions, a very wide range of
10 thermodynamic conditions, and you're not going to
11 capture the condition at the minimum area point with
12 very satisfactory accuracy.
13 DR. CHOW: Well, it means that the
14 choking, where there is actual choke in the nozzle is
15 -- that's what in terms of that we don't know where is
16 actual choke in the -- we just basically say that
17 apply the choking criteria at another pipe. So that's
18 what, and so we did calculate from the base to the
19 discharge pipe to the nozzle. These all -- everything
20 is calculate at that, and you are talking about maybe
21 the choking will occur in some other place other than
22 the nozzle, but I don't think that's --
23 MR. SCHROCK: I don't think I said that.
24 I haven't talked about choking occurring at some place
25 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 --

1 DR. WALLIS: That's right.

2 MR. CARLSON: Yes.

3 DR. WALLIS: A bit clearer there.

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

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

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

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

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

18 MR. CARLSON: Right.

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

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

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

25 MR. SCHROCK: If you keep going down --

1 MR. CARLSON: Yeah, yeah. What you're
2 referring to is the A, Jones supply.
3 MR. SCHROCK: That's right.
4 MR. CARLSON: Okay. And I think Jones
5 describes it as the upstream flow area, and we set it
6 to the volume flow area, what we refer to as the
7 volume flow area that's at the center of the volume
8 length.
9 DR. WALLIS: It's at the center of the
10 volume length?
11 MR. CARLSON: Well, it's the center. It's
12 the -- the area is constant throughout the volume, and
13 so whatever you define to be the volume flow area
14 is --
15 MR. SCHROCK: When you say Jones defined
16 it as the upstream flow area, with reference to what?
17 The experiment of Alamgir and Lienhard?
18 MR. CARLSON: I believe it was to the
19 throat. I believe it was to the throat.
20 DR. WALLIS: Well, there's an At over --
21 MR. SCHROCK: The throat is in the
22 numerator, At divided by A.
23 MR. CARLSON: Right. And so --
24 MR. SCHROCK: See, Alamgir and Lienhard
25 did this in a straight pipe and used an explosive

1 charge to blow the end off a straight pipe very
2 rapidly so that they would get a very high rate of
3 decompression.
4 So I looked at your numerical work, and
5 then I looked back at this equation, and I couldn't
6 understand how you match what you're doing in your
7 numerical work with what Jones had here.
8 And then it raised the question in my mind
9 what did Jones mean by A. He's got an At, which
10 presumably means throat, and then there's an A.
11 MR. CARLSON: Well, I believe Jones
12 defined A as the upstream flow of --
13 MR. SCHROCK: Upstream in what geometry?
14 MR. CARLSON: In terms of a converging or
15 the -- well, he assumes a converging/diverging nozzle,
16 and so the area at the -- the unscripted area is said
17 to be the -- the interest area.
18 MR. SCHROCK: So you're talking about the
19 interpretation that Jones described this in terms of
20 the geometry of the Brookhaven experiments in
21 convergent/divergent nozzles, which is different from
22 Alamgir and Lienhard.
23 DR. WALLIS: But if I have a small --
24 MR. CARLSON: I wasn't aware of that.
25 DR. WALLIS: -- break in a big pipe, I've

1 got a 30 inch pipe and I've got a two inch hole in it.
2 Is At two inches and A 30 inches?
3 MR. CARLSON: Yes.
4 DR. WALLIS: That's a huge area change.
5 MR. CARLSON: Is that right? Is that
6 right?
7 PARTICIPANT: Yes.
8 MR. SCHROCK: Well, if you use the large
9 break guillotine break of cold leg, for example,
10 you're going to have no change in the area.
11 MR. CARLSON: That's right.
12 DR. WALLIS: So that's one.
13 MR. SCHROCK: So that term essentially
14 washes out.
15 DR. WALLIS: It's one. At over A is one.
16 MR. CARLSON: Is one.
17 MR. SCHROCK: It's one.
18 MR. CARLSON: Well, in the large break
19 LOCA there's a break spectrum that is run, and this
20 would vary from one to --
21 MR. SCHROCK: But when I look at your
22 numerical solution, I conclude that what you're going
23 to substitute in there, in general, is going to depend
24 upon the noding choice --
25 MR. CARLSON: That's right.

1 MR. SCHROCK: -- that you've made for the
2 application to your critical flow problem.
3 MR. CARLSON: That's right.
4 MR. SCHROCK: And so how does that relate
5 to what Jones did, and how is it justified?
6 MR. CARLSON: Well, if I was more familiar
7 with what Jones did. Dr. Chow, would you help me
8 address this, please?
9 DR. CHOW: I think the A is actually
10 upstream of the nozzle area. So basically the A is
11 the other requirement to the RELAP5, the 480 and
12 upstream, you know, nozzle, and the At is the area of
13 nozzle, lowest area of nozzle.
14 MR. SCHROCK: Well, since you've thought
15 that it's related to Jones' experiments in convergent
16 and divergent nozzles, it would seem that upstream
17 area is essentially undefined.
18 DR. CHOW: One, seventy-five is the area
19 of the upstream flow area. Why is it undefined? It's
20 just the upstream of the nozzle. Basically --
21 basically, in the additive, that's the area that --
22 because area of the nozzle, that's basically --
23 MR. CARLSON: Well, I assume that Jones
24 wrote the area as a small distance upstream of the
25 throat as maybe an entrance effect or an entrance

1 area.

2 MR. SCHROCK: A small distance upstream of

3 the throat either puts it in the convergent part of it

4 or upstream of the --

5 MR. CARLSON: Upstream of the converging

6 section would be the entrance to the nozzle. I think

7 there was a term that I thought he was looking for,

8 that VADX, which is in 5.23, and he was expecting a

9 certain range where this was the area change with

10 respect to distance from the entrance or the throat

11 entrance to actually what the throat area was, but

12 anyway, I think that was --

13 DR. WALLIS: This depressurization rate

14 looks a bit odd, too.

15 MR. CARLSON: This is the depressurization

16 rate, yes.

17 DR. WALLIS: this is the rate at which a

18 given piece of fluid is changing its pressure as it

19 flows through the nozzle. It's not the rate at which

20 the system is depressurizing. You're looking at a

21 piece of fluid and saying, "How rapidly is it changing

22 its pressure?"

23 MR. CARLSON: Right.

24 MR. SCHROCK: Or how rapidly the

25 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

1 equation.

2 MR. SCHROCK: They have a dimensional

3 relationship, and it requires specific units.

4 MR. CARLSON: Yes.

5 MR. SCHROCK: And those units were not

6 Pascals per second.

7 MR. CARLSON: They were some of that mega

8 atmospheres per second.

9 MR. SCHROCK: Right.

10 MR. CARLSON: And I think that was

11 converted to Pascals per second to use from the code.

12 MR. SCHROCK: Well, that's very dangerous.

13 I don't know that you've used it consistently then

14 because, as I just said, the Alamgir/Lienhard

15 correlation is not dimensionless. It's a dimensional

16 relationship, and it requires those specific units,

17 not any others, mega Pascals per second.

18 MR. CARLSON: Mega Pascals. Oh, okay.

19 MR. SCHROCK: So I suspect then that you

20 do not use consistent units.

21 DR. WALLIS: Well, mega Pascals per second

22 is a million times Pascal per second. Have you got an

23 error of a million?

24 MR. SCHROCK: Mega atmospheres per second.

25 MR. CARLSON: Mega atmospheres.

1 MR. SCHROCK: Even more than a million.

2 MR. CARLSON: Well, yeah.

3 MR. SCHROCK: Yeah.

4 MR. CARLSON: Well, actually that's a mega

5 times -- well, the internal units of Pascal.

6 MR. SCHROCK: Oh, come on. You can't play

7 fast and loose with --

8 MR. CARLSON: I'm not trying to.

9 MR. SCHROCK: Are your units consistent in

10 the code?

11 MR. CARLSON: And the answer is yes.

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

13 MR. SCHROCK: Not if you've used in that

14 equation Pascals per second.

15 MR. O'DELL: No, I think what the

16 statement means about proper unit conversion has done

17 in the coding, basically it has converted to the mega

18 atmospheres per second where they've got the --

19 unfortunately on the previous slide shows that that

20 term -- acknowledges that term was in mega atmospheres

21 per second, and the statement there on the viewgraph

22 is that the proper unit conversion is done in the

23 coding.

24 And I think if you go back to our code

25 verification where we went through that, that was one

1 of the things that was being checked in the code
2 verification process, was to make sure that we were
3 using these things in the proper --
4 DR. WALLIS: So you had to look at the
5 coding and see if there's a suitable factor in there.
6 MR. O'DELL: Right.
7 DR. WALLIS: With a mega atmosphere, it's
8 ten to the 11th Pascal? You get a factor of ten to
9 the 11th? Something.
10 MR. CARLSON: Everything in the code is
11 converted to Pascals.
12 DR. WALLIS: So someone has looked at the
13 actual lines in the code and found that there is
14 conversion of units in there somewhere?
15 MR. O'DELL: Yes, in the verification
16 process we went through the actual coding on these
17 models to make sure that in fact we're using them
18 properly.
19 DR. WALLIS: That's the difficulty the
20 reader has. The reader reads the documentation and
21 sees some units there and what's actually encoded
22 might be something else, but he has no way of checking
23 that without looking at the source code.
24 MR. CARLSON: That's true.
25 MR. O'DELL: That's true.

1 DR. WALLIS: And this is true of many
2 correlations. Many heat transfer correlations are
3 written in weird units, and you have to convert them
4 to your code units hopefully.
5 MR. CARLSON: Well, we --
6 MR. SCHROCK: So the answer is that you've
7 checked it and you assure us that the units are
8 correct.
9 MR. CARLSON: I personally have not
10 checked it, but other -- it has been checked.
11 MR. O'DELL: It's been checked in the
12 code verification process.
13 MR. SCHROCK: Okay.
14 DR. WALLIS: Can we move on then?
15 MR. SCHROCK: Yeah.
16 MR. CARLSON: Let's see. Equation 5.25
17 and 5.26. This factor depends on the system geometry
18 and noding choice. Explain the basis for these
19 equations, the background and reasons for arbitrary
20 choices, 5.26 and 5.27.
21 Five, twenty-six and --
22 DR. WALLIS: It would really help if we
23 had Section 5. I don't know how much progress we can
24 make without knowing to what use this is put.
25 MR. CARLSON: Section 5, equation 5.26, is

1 -- well, Section 5.25, I don't happen to have that.
2 MR. SCHROCK: It's used in the calculation
3 of sigma.
4 MR. CARLSON: Oh, there it is.
5 MR. SCHROCK: Of Alamgir and Lienhard's
6 correlation.
7 DR. WALLIS: Oh, it's used, okay, in the
8 sigma.
9 MR. CARLSON: Yeah, all right.
10 DR. WALLIS: Is this what Lienhard told
11 you to do or did you just make an assumption? This is
12 your assumption that seemed reasonable. Is that what
13 it is?
14 MR. CARLSON: Yes.
15 DR. WALLIS: It may be okay. I can't
16 really tell without looking at data or something to
17 see if it works.
18 MR. O'DELL: And in fact, you know, in
19 the process of going through the Marviken test, I
20 mean, we have gone through the Marviken test with
21 these models and determined, you know, a bias, which
22 was one as I recall, and I don't recall what the
23 uncertainty was, but in the application of these, we
24 have done comparisons, you know, to the Marviken tests
25 to determine the bias and uncertainty for the flow.

1 MR. SCHROCK: Well, there is in the
2 methodology a need to determine the depressurization
3 rate in the experiment, and to do that you use an
4 equation which appears to depend upon the noding, and
5 so presumably you'd get a different depressurization
6 rate if you used a different noding. If you used a
7 different depressurization rate, the criterion for the
8 onset of flashing would be changed.
9 So it all comes out in the assessment, but
10 there are too many things involved in the assessment.
11 To begin with, the Marviken experiment itself has a
12 complication that there is flashing within the vessel,
13 and you know only what RELAP tells you about what the
14 void fraction is entering the flow channel.
15 MR. CARLSON: Right.
16 MR. SCHROCK: Okay, and that's determined
17 by RELAP. It's not determined by experiment.
18 MR. CARLSON: Right.
19 MR. SCHROCK: You have no independent
20 check on that. So there's an uncertainty there.
21 Then you need this depressurization rate,
22 which is -- well, I'm sorry I'm mixing that with the
23 later problem of saturated blowdown.
24 MR. CARLSON: Yes.
25 DR. CHOW: This is Hueiming Chow.

1 I think that in terms of R-BEACON they
2 don't -- they really have measurement of the pressure
3 and the temperature at the discharge, at just in front
4 of the pipe. So you cannot say we don't have any
5 information about the step in front of the nozzle.
6 They have mentioned.
7 So in our calculation, we have to compare
8 the temperature and the pressure in the form of
9 nozzle. So we did have data on that. So basically it
10 said we have to continue calculation from the base to
11 the -- before the nozzle that stays about the same as
12 the experiment. So they did have measurement data in
13 there. So it's no say they don't have measurement
14 data.
15 And they used the same kind of equation in
16 the exact extent. I mean, this equation basically the
17 Monica (phonetic) approach mentioned. The only thing,
18 the only choice that you have, you have the volume
19 area; you have junction area. That's only choice.
20 You define it in the code to get the approachment.
21 What's that? The area changing nature.
22 So this is an approach mentioned, and how
23 good is that compared to the experiment data?
24 That's all I can say.
25 MR. SCHROCK: Okay. Would you go ahead?

1 MR. CARLSON: Moving on --

2 DR. WALLIS: Are you going to answer all

3 of the questions that Dr. Schrock has here?

4 MR. CARLSON: If Dr. Schrock wants me to.

5 There's a whole string of them.

6 DR. WALLIS: Well, I'm not sure we have

7 time to do that. I think all we're going here is

8 establishing your credibility. Maybe we should look

9 at the questions which are most relevant for

10 establishing credibility of your approach rather than

11 everything. I think most of us are going to be

12 completely lost with all of these without seeing what

13 the equations actually are.

14 Well, how about something like this, using

15 HEM, which mysteriously obtained from choke but then

16 applied to slip equilibrium calculations?

17 MR. CARLSON: Pardon me?

18 DR. WALLIS: Which one of these questions

19 do you think we ought to focus on in order to get an

20 idea as to whether these folks know what they're

21 doing? Do we need to go through every one of them?

22 MR. CARLSON: No.

23 DR. WALLIS: Should we require a written

24 reply or something or what?

25 MR. CARLSON: In the --

1 MR. SCHROCK: Let's take the one in the
2 middle of the second page. Equation 5.28 is said to
3 be obtained from equation 2.85 with gravity and
4 friction omitted, and this is said to be done for
5 clarity and the derivation to follow, but the omitted
6 terms "friction" and "gravity" are in the code.
7 Nothing is said here about -- it's my
8 parenthetic note that nothing is said about the
9 flashing term which is also committed in that
10 equation.
11 DR. WALLIS: And what is equation 5.28
12 representing?
13 MR. CARLSON: Five, twenty-eight, that is
14 the simplified steady state.
15 MR. SCHROCK: Five, twenty-eight is a
16 simplified momentum equation.
17 DR. WALLIS: Oh, dear.
18 (Laughter.)
19 MR. CARLSON: Yeah, that's what I thought
20 when I first saw that.
21 MR. SCHROCK: Simplified steady state
22 momentum equation.
23 DR. BANERJEE: Bernoulli's question would
24 also be choking.
25 MR. CARLSON: Right, and --

1 MR. SCHROCK: No phase change accounted
2 for.
3 MR. CARLSON: Well, the phase change is
4 accounted for, would probably be -- well, is accounted
5 for in the void fraction.
6 MR. SCHROCK: I don't know. I'm looking
7 at equation 5.28, which is on page 5-14 --
8 MR. CARLSON: Right.
9 MR. SCHROCK: -- of Rev. 4.
10 MR. CARLSON: Right.
11 MR. SCHROCK: It has none of these last
12 three terms.
13 MR. CARLSON: That's right, and these were
14 just added to complete, to satisfy your question that
15 could have been in --
16 MR. SCHROCK: My question is about the
17 equation 5.28 that's in the report.
18 MR. CARLSON: Yes.
19 MR. SCHROCK: Not the equation that's on
20 the board.
21 MR. CARLSON: Right, about the flashing
22 term, and I'm assuming, you know, when you say that
23 the flashing term is not in the 5.28, I believe --
24 MR. SCHROCK: Let me read the complete
25 question.

1 MR. CARLSON: Simplify --

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

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

12 MR. CARLSON: Right.

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

15 MR. CARLSON: Explaining how --

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

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

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

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

1 transfer term.

2 DR. WALLIS: It's gamma, G, VF1, SVG term

3 which is in 2.85.

4 MR. CARLSON: Right.

5 DR. WALLIS: Except that in choking that

6 change of phase is important.

7 MR. CARLSON: And when it's equal

8 velocity, that particular term is not there.

9 DR. WALLIS: If it's homogeneous

10 equilibrium?

11 MR. CARLSON: Yeah, for a homogeneous flow

12 or homogeneous equilibrium.

13 MR. SCHROCK: Well, wait a minute. In

14 homogeneous equilibrium model, flashing occurs, mass

15 transfer.

16 DR. BANERJEE: The flashing term is very

17 important.

18 MR. CARLSON: I have a hard time -- I

19 don't understand what you mean by the flashing term.

20 DR. BANERJEE: Gamma.

21 MR. CARLSON: Gamma? Gamma in the

22 momentum equation is basically the mass generated, the

23 mass generation rate, and it's applied as a mass

24 transfer due to or momentum due to mass transfer, and

25 it wasn't applied in this assumption or in this

1 equation because we're assuming equal velocity. So
2 there would be no momentum transfer between the phases
3 if there was flashing, that is, condensation or
4 evaporation.
5 MR. SCHROCK: Well, equal velocity simply
6 means that the liquid and vapor are moving at the same
7 speed.
8 MR. CARLSON: That's right.
9 MR. SCHROCK: But both of their speeds are
10 changing, and one of the contributing factors to their
11 changing speed is the fact that some of the liquid may
12 be evaporating or in the high quality region may be
13 condensing.
14 MR. CARLSON: Yes, but we -- but that
15 flashing term, like I say, well, it was not applied.
16 It was assumed that it wasn't needed for equal
17 velocity.
18 MR. SCHROCK: Well, you've gone through
19 some strange things to establish what you regard as
20 suitable average values of the product of alpha times
21 rho --
22 MR. CARLSON: Yes.
23 MR. SCHROCK: -- in the last half node --
24 MR. CARLSON: Yes.
25 MR. SCHROCK: -- upstream of the point

1 of --

2 MR. CARLSON: Right.

3 DR. WALLIS: The alpha-rho?

4 MR. SCHROCK: What I've asked you to do

5 here is to explain how this works. What is the

6 significance of those average values? Why are they

7 going to serve adequately when, in fact, you have

8 friction and you have flashing involved in the

9 process?

10 Friction and gravity.

11 MR. CARLSON: Is this -- I'm not sure what

12 terms you're referring to. I think you're referring

13 to the adjustments to density from the center to the

14 throat.

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

16 that.

17 MR. SCHROCK: I guess you need to read

18 your own report because what you've done in this

19 report is to develop a rationale for how you will

20 choose average values --

21 MR. CARLSON: Yes.

22 MR. SCHROCK: -- for integration purposes

23 of the product of rho times alpha. Okay? And to do

24 that, you've used only a part of the problem. You

25 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

1 this integration of mass difference. We never say
2 that in the past we have a vaporization. We say at
3 this end of the vaporization, we have so much
4 vaporization, at the other end so much vaporization.
5 So just around the past we don't consider
6 the vaporization. That's all it says.
7 MR. CARLSON: But you're really just doing
8 the integration from the south center just to the
9 throat.
10 DR. CHOW: To the throat.
11 MR. CARLSON: And this is the replacement
12 of the momentum equation; is that correct?
13 DR. CHOW: Yeah, yeah.
14 MR. CARLSON: Yes.
15 DR. CHOW: Because that RELAP5 doesn't
16 solve the equation of state at the junction, and
17 that's why these state is needed.
18 MR. CARLSON: This is, again, that
19 linearization or that integration. If you get a
20 better property for alpha, rho at the throat --
21 DR. CHOW: Yeah, at the throat, yeah,
22 that's it, and --
23 MR. CARLSON: What we found was that when
24 we looked or compare RELAP5 to, say, homogeneous
25 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

1 necessary to keep them separate like that?

2 MR. CARLSON: Not really. We thought for

3 clarity possibly.

4 DR. MOODY: Okay. Then you could

5 essentially factor out the D by DX or V squared from

6 the left-hand side.

7 DR. WALLIS: So you're assuming these

8 velocities are equal?

9 MR. CARLSON: Well, this is for the HEM,

10 critical flow. We would leave the two velocities

11 there if we were going to do two phase critical flow.

12 DR. MOODY: Of HEM? If it's homogeneous,

13 they are both traveling at the same velocity.

14 MR. CARLSON: Right, but the same coding

15 is used for another model option, that is, to not use

16 a --

17 DR. MOODY: Okay. That's just the way the

18 code would change.

19 MR. CARLSON: That's just the way the code

20 works. We should have factored it out because they're

21 equal.

22 DR. MOODY: I just want to be sure I

23 understand what you've got, and so, yeah, they are the

24 same value velocity in the HEM model on both sides of

25 the equation. Vf is equal to Vg.

1 MR. CARLSON: Yes.

2 DR. MOODY: Okay. Thank you.

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

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

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

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

15 MR. CARLSON: I apologize.

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

18 DR. WALLIS: Shall we drop the --

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

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

25 Can we take a break until quarter till

1 six, if everyone can stay for the statistical
2 resolution, which I notice doesn't have many
3 transparencies, but may still require some discussion?
4 What is I think very interesting and
5 before we get to that is I thought that you had a very
6 interesting graph of PCT versus percent oxidation,
7 which showed they were very closely correlated. That
8 was a useful piece of information for me. I don't
9 know if you're going to show that in the presentation,
10 but if we can find it in the break, that would be
11 useful, I think.
12 Let's come back at quarter to six.
13 (Whereupon, the foregoing matter went off
14 the record at 5:36 p.m. and went back on
15 the record at 5:47 p.m.)
16 DR. WALLIS: Let's come back into session.
17 MR. O'DELL: Okay. What I would talk
18 about quickly here was basically the statistical issue
19 that came up during the review of the methodology and
20 what the ultimate resolution of that issue was.
21 The issue came up during the review was
22 that the NRC request, report to PCT maximum nodal
23 oxidation and the total oxidation as a joint
24 probability statement.
25 In the original proposed realistic large

1 break LOCA methodology, we reported an individual 95-
2 95 value for each of the criteria; PCT, maximum nodal
3 oxidation and total oxidation, and we never made any
4 kind of a joint probability statement with respect to
5 the three criteria.
6 DR. WALLIS: Now, can I get clear what
7 you're saying here? In order to satisfy these three
8 criteria, you need to have enough code runs so that
9 you can get 95-95 value for all three. That's what's
10 meant by joint probability, not just --
11 MR. O'DELL: Right.
12 DR. WALLIS: -- how many do you need to
13 get either one or the other or the other. I mean, you
14 need 59 maybe to get PCT if that's all you care about,
15 and you need 59 to get total oxidation if that's all
16 you care about.
17 But if you want to get both of them with
18 this 95 percent certainty, then you might need a
19 different number of runs.
20 MR. O'DELL: Exactly.
21 DR. WALLIS: That's what you mean by this
22 joint probability question.
23 MR. O'DELL: Right. The resolution of
24 this is based on Regulatory Guide 1.157, which
25 indicates that the revised Paragraph 50.46(a)(1)(i)

1 requires that it be shown with a high probability that
2 none of the criteria of Paragraph 50.46 will be
3 exceeded and is not limited to the peak cladding
4 temperature criterion.
5 However, since the other criteria are
6 strongly dependent on peak cladding temperature
7 explicit consideration of the probability exceeding
8 the other criteria may not be required if it can be
9 demonstrated that meeting the temperature criterion at
10 the 95 percent probability level insures with an
11 equal or greater probability that the other criteria
12 will not be exceeded.
13 So this basically allows you to report the
14 95-95 PCT and the associated oxidation with that 95-95
15 PCT case.
16 DR. WALLIS: This is very different from
17 requiring you to have this 95 percent probability
18 level with all three of the criteria, which this seems
19 to give you a way out if you can show getting the PCT
20 with 95 percent probability and assures that the other
21 criteria will be met with greater probability.
22 MR. O'DELL: Right.
23 DR. WALLIS: In which case you have to
24 look at how these different criteria are correlated.
25 MR. CARLSON: Exactly. And you know, we

1 did that, and unfortunately I don't have the slides
2 for that particular one, but what has been agreed is
3 that we would change our methodology to amend proposed
4 methodology to only report the 95-95 PCT case and its
5 associated maximum total oxidation and total
6 oxidation.
7 DR. KRESS: Now, how will you demonstrate
8 though that that meets the criteria of the high
9 probability to meet the other two?
10 MR. O'DELL: Well, we've gone through and
11 looked at a series of cases, including the three loop,
12 four loop, and then we look the three loop case and
13 ran the power up in the three loop case to drive it to
14 a PCT of around 2,200. So we have a series of cases,
15 and what I do have in the way of some back-up figures,
16 this shows -- and unfortunately this one doesn't have
17 the high temperature ones. We ran some up to where
18 they're a little over 2,200.
19 But what we were trying to show in this
20 particular slide is this is the frequency distribution
21 that we got on PCT from our cases that we ran. Okay?
22 And if you look at --
23 DR. RANSOM: How many independent
24 variables went into this, I mean, that you varied
25 statistically?

1 MR. O'DELL: I think there's 39 roughly,
2 something; somewhere around 35 to 40.
3 DR. RANSOM: Are they listed somewhere
4 where a list of all the ones and their associated
5 frequency distribution that you use?
6 MR. O'DELL: That's provided in the
7 sample problem on the four loop in EMF-2103.
8 DR. RANSOM: And that's in the
9 documentation that we have?
10 MR. O'DELL: Yes.
11 DR. RANSOM: Which volume is that?
12 MR. BOEHNERT: Larry, I notice that stuff
13 is labeled proprietary. I assume we're talking around
14 the numbers because we are in open session, and I just
15 wanted to remind everyone of that.
16 MR. O'DELL: But, anyway, what this was
17 showing is the distribution of PCTs from the cases,
18 and indicating, you know, we're up in the high
19 temperature range there, approaching the 2,200.
20 And then if you look at that in
21 conjunction -- and I guess that one was the three loop
22 PCT, and here's four loop PCT case --
23 DR. KRESS: Are all of these with 59 runs?
24 MR. O'DELL: Yes, these cases are for
25 actually the sample problems that we've provided in

1 the documentation, and it shows, again, the
2 temperature distribution for the PCT versus the
3 frequency of the calculation in the 59 cases.
4 DR. RANSOM: How many calculations were
5 made to establish that?
6 MR. O'DELL: The 59 calculations.
7 DR. RANSOM: Fifty-nine. So that came out
8 of the 59 calculations?
9 MR. O'DELL: right.
10 DR. WALLIS: And the 59 is an assurance
11 that you've actually captured the 95th percentile.
12 MR. O'DELL: Right.
13 DR. WALLIS: Just looking at that, it
14 looks as if you're sort of in the 95th percentile,
15 just sketching out a probable distribution roughly.
16 MR. O'DELL: Right.
17 DR. WALLIS: But there could be some
18 values in the tail up to 1850 or whatever presumably.
19 MR. O'DELL: Right. I mean, 95-95 is a
20 five percent probability that you --
21 DR. RANSOM: Well, in your methodology, do
22 you have a way of tracing back what parameters were
23 most influential and the change, I mean, in causing --
24 MR. O'DELL: Yeah. We go through and
25 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

1 that seems to be inconsistent with the Appendix K
2 requirement that you study a spectrum of break sizes
3 and determine the worst case, and so you're sort of
4 mixing the spectrum of break sizes with the statistics
5 of the code prediction for a given break size based on
6 other variations.
7 MR. O'DELL: Right, and I understand in
8 the Appendix K the way that they've handled the break
9 spectrum issue is that they do, in fact, run a set of
10 break sizes in determining the limiting break. Okay?
11 They report that then as basically your limiting break
12 PCT, but they're running all of the calculations on
13 all of the break sizes in a deterministic fashion to
14 find that. So once you find the limited break in an
15 Appendix K analysis, you basically have the limiting
16 calculation.
17 In the statistical analysis, we're
18 treating the break size as one of the statistical
19 parameters, and we're varying that along with the
20 other parameters, but not --
21 MR. SCHROCK: But I would think that once
22 you've established what the limiting break size is,
23 that you would then look at the uncertainty in the
24 prediction at that break size.
25 MR. O'DELL: The problem --

1 MR. SCHROCK: And that's what you're
2 trying to address statistically now, isn't it?
3 MR. O'DELL: Well, the problem with
4 trying to do that is what exactly is the values you
5 use for the other parameters, the other strong
6 parameters. Okay?
7 I mean is it a top P taxing (phonetic)
8 shape that I run through all of that, and how do I
9 know which one I select? Okay?
10 In order to do what you're requesting, I
11 either have to be able to do a deterministic analysis
12 in some fashion where I set again, you know, basically
13 doing Appendix K type calculation to find the limiting
14 break and then vary everything around that. You know,
15 that's not as simple a calculation from a statistical
16 perspective as it first appears because what is the
17 limiting break relative to all the other parameters
18 you're varying?
19 So the only real way to treat the break
20 size in a statistical methodology is to, in fact,
21 include that as one of the statistical parameters.
22 DR. RANSOM: What was the range of break
23 sizes that you considered?
24 MR. O'DELL: I'll go through that
25 tomorrow when I go through the overall methodology,

1 but we looked at both, you know, split break from a
2 ten percent up to a 1.0 and double ended guillotines
3 from the 1.0 up to a 2.0.
4 DR. KRESS: Now, you don't ascribe any
5 significance to this distribution?
6 MR. O'DELL: Not with 59 cases. Okay?
7 DR. KRESS: I mean, it just happened to be
8 the way it turned out with 59 cases, right? But you
9 probably ascribe some significance to that 1,700
10 thing.
11 MR. O'DELL: Right. I mean, what you're
12 -- there's basically three calculations that gave us
13 the temperatures in that 1,700 or the range here that
14 we're reporting around the 1,700.
15 DR. WALLIS: And you're binning here. So
16 it could be --
17 MR. O'DELL: Exactly.
18 DR. WALLIS: -- a different binning might
19 put some in the 1,650, 1,750.
20 MR. O'DELL: Right.
21 DR. WALLIS: Those three points might be
22 spreading to three.
23 MR. O'DELL: Yeah.
24 DR. WALLIS: It's an interesting idea to
25 put in the break sizes the statistical problem.

1 DR. KRESS: Yeah.

2 DR. WALLIS: I think that may be

3 justified.

4 MR. O'DELL: It makes an interesting

5 approach to a statistical analysis if you don't.

6 DR. WALLIS: Yes, it does.

7 DR. KRESS: That may impact the ranges and

8 distribution of the other parameters.

9 MR. O'DELL: Potentially. Potentially it

10 could, yes.

11 DR. WALLIS: I think that makes sense. We

12 can then begin to argue about whether the 95-95 is

13 good enough because it depends on if you're looking at

14 all of these things in a statistical way. How certain

15 do we really need to be?

16 There's nothing magic in my mind about 95

17 percent.

18 MR. O'DELL: Well, the 95 percent

19 obviously is the one that was picked because it shows

20 up in the reg. guide as being the --

21 DR. WALLIS: How certain you want to be

22 must depend on the risk and consequence and things

23 like that, the risk of being wrong. I'm not sure that

24 the staff has really worked out a good rationale for

25 95-95. It sounded good at the time, but now it's

1 going to be used perhaps for a more important decision
2 making process that more thought needs to be given to
3 what it should be.
4 DR. KRESS: It depends on what you want to
5 use it for.
6 DR. WALLIS: That's right.
7 MR. O'DELL: Now, these are the same 59
8 runs.
9 MR. O'DELL: Right. These are the same
10 59 runs, again, at three peak local oxidation, and as
11 you can see, they're all clustered down here for those
12 59 cases where the criteria, 17 percent criteria,
13 they're significantly away from that criteria.
14 DR. WALLIS: And if I'm betting 1,000
15 bucks on on the outlook from RELAP, I might be
16 satisfied with a 95-95 percent certainty of not losing
17 it all or something.
18 But if I'm risking my life, I might want
19 to have a 99-99. It depends on what's at stake,
20 doesn't it?
21 DR. KRESS: That's a good question.
22 MR. O'DELL: Well, that's true.
23 DR. KRESS: But that looks like sufficient
24 justification to say that this is captured in 95 PCT.
25 MR. O'DELL: Right, and again --

1 DR. KRESS: The same thing on the total
2 oxidation.
3 MR. O'DELL: Yeah, and if you look the
4 four loop again confirms the same sort of analysis.
5 DR. WALLIS: Well, what's peak local
6 oxidation criterion in the regulation?
7 MR. O'DELL: Seventeen percent.
8 DR. WALLIS: Way over there.
9 DR. KRESS: Yeah. Now, I'm not sure
10 that's way over there because I don't know what your
11 scale is.
12 MR. O'DELL: Well, it basically goes from
13 zero to 17, and we do have some --
14 PARTICIPANT: It's a linear scale.
15 DR. KRESS: It's linear.
16 MR. O'DELL: Yeah. We have some cases,
17 you know, where we didn't get any oxidation on some of
18 the lower temperature cases.
19 MR. HOLM: This is Jerry Holm.
20 I think one of the things we looked at is
21 if you look at the standard deviation of that, and we
22 realize it's not normal, but just as a figure of
23 merit, that top peak there is 30 standard deviations
24 away from the criteria.
25 DR. WALLIS: Well, the 95-95 as an

1 estimate here would be around one.

2 PARTICIPANTS: Yes.

3 DR. WALLIS: Whereas you're out 17.

4 You're way far away from the criteria.

5 MR. O'DELL: Right.

6 DR. WALLIS: So I think that you might

7 have a really good argument here.

8 DR. KRESS: Might have a really good

9 argument.

10 MR. O'DELL: And, again, I've got the

11 same set of slides showing for the three loop total

12 core oxidation. Again, for the total core oxidation

13 the limit is one percent, and we're back in here

14 around the .05 percent, and seeing basically a result

15 for the four loop.

16 We chose, again, the criteria of one

17 percent and back in here around .03 on this particular

18 transient.

19 DR. KRESS: Now, the only reason this

20 works out is because of the strong correlation.

21 MR. O'DELL: Because of the strong

22 correlation.

23 DR. WALLIS: But also it's more than

24 strong correlation. I mean, it also shows that no

25 matter what you have for peak clad temperature, all of

1 these other parameters are so low --

2 DR. KRESS: That's right

3 DR. WALLIS: -- that with enormous

4 certainty they're going to be way far away from the

5 criteria.

6 DR. KRESS: Yeah, it's more than just the

7 correlation.

8 MR. BOEHNERT: Larry, I'll need copies of

9 those slides for the record.

10 MR. O'DELL: Okay.

11 DR. WALLIS: Yeah, I think they're very

12 interesting. Good to end the day on a persuasive

13 argument.

14 DR. RANSOM: Mr. Chairman, I'm assigned to

15 Bill Nutt, and there were some aspects of this. He

16 made more than 59 runs. Why is that? Less

17 conservative?

18 And he said he has some slides that he

19 wouldn't mind discussing if you want to take a few

20 minutes.

21 DR. WALLIS: I would agree to that if it

22 doesn't take too long.

23 Would you like to do that?

24 MR. NUTT: Sure.

25 DR. WALLIS: It's much easier to

1 understand data than it is these A1s and A2s.

2 MR. NUTT: Well, it depends if you have

3 the abstract mind, right? Which, by the way, I don't.

4 Andy and I were sitting there, "What's a real good

5 concrete example of what I'm talking about here?"

6 That means -- can you hear me okay?

7 MR. BOEHNERT: Identify yourself for the

8 record, please.

9 MR. NUTT: This is Bill Nutt from

10 Framatome.

11 And actually I've presented these slides

12 to you before. So I guess I'd better put my glasses

13 down here because they'll get in the way.

14 DR. WALLIS: Did you present them or did

15 they come back in a --

16 MR. NUTT: I think I presented them one.

17 DR. WALLIS: -- RAI reply to the --

18 MR. NUTT: These were back-up slides once

19 for when we -- well, suppose someone did ask this

20 question, and I think we did at one point when Uri was

21 asking questions during a meeting, and I'm not sure it

22 was in front of the ACRS. It may have been in front

23 of the staff, and we presented this question.

24 And what I did was this. I did a little

25 sampling test. I took a normal distribution. I

1 actually did this with several different
2 distributions. I'll just talk about the normal.
3 I took a normal distribution that has an
4 upper 95 percent limit of 275 so I could go back and
5 say standard deviation of 167 and, you know, a mean of
6 zero.
7 Then I calculated the upper 95-95 using a
8 nonparametric approach for sample sizes from 59 to
9 410. So I take 59 and I say, you know, what's the top
10 one, and I do this 2,000 times.
11 Then I calculate the number of times that
12 that fraction was below the 95-95 limit, the
13 probability it was below the 95 limit, the ten percent
14 change, you know, that it was below the 95 percent
15 limit, and so on.
16 And I did this. Then I removed one step
17 inside, and I'd say, okay, now let me take the second
18 point in and I'll do 92 cases, and I did those 2,000
19 times. So I'm getting my frequency with which I
20 exceed my limit. Okay?
21 So that's what shows up on this picture.
22 Now, if we look at this curve, this is 59.
23 The interesting thing is all of them come to five
24 percent, right? Because it is 95-95. So they all go
25 to five percent.

1 But if I had 59 points, you see, there is
2 a reasonable chance that I could be clear up there,
3 which would be at 450 rather than the 275 that we
4 said.
5 And don't take these numbers too seriously
6 because if I had used a uniform distribution, this
7 picture would look the same, but the scale would b
8 compressed. Okay? So the actual values are dependent
9 on distribution.
10 But the conclusion here, if I take 59,
11 right, if I take 93, this improves quite a bit. My
12 probability of ever getting over 400 has all of a
13 sudden gotten a lot smaller, right?
14 And by the time I get up to I think it's
15 410 points where I can take the 13th point in, the
16 chances of me getting more than 50 above my -- you
17 know, of having my sample that I select as my 95-95,
18 my chance of having it be over 300 are really very,
19 very slim.
20 DR. WALLIS: So what is the probability on
21 the axis there? The probability of what?
22 MR. NUTT: Oh, this is the probability
23 that the number that you pick as the 95-95 will be
24 here. Okay? So you all can take your sample and say
25 I got a sample. What's --

1 DR. KRESS: When in reality it's really
2 down there.
3 MR. NUTT: Here's the real one, and
4 there's a five percent change it will be here, but
5 what's the chance that I'll pick that value as my 95?
6 DR. KRESS: Which could happen.
7 MR. NUTT: Which could happen.
8 DR. KRESS: In any given run.
9 MR. NUTT: Yeah. So one basically says
10 all I get when I take more points is I don't really
11 improve the 95-95 point, right? It's still a 95-95.
12 That's all.
13 But what happens is I tend to get rid of
14 the possibility of having one of those flyers way out
15 there.
16 Now, as you saw from the data that we have
17 taken, we're not getting a lot of flyers. We took one
18 corrupt -- I think if we take the one at -- we did one
19 set of cases that went to 2274, had same sets of
20 plots, but since it wasn't QAed we didn't put it in a
21 picture.
22 DR. WALLIS: It depends a bit on the shape
23 of the tail of the distribution.
24 MR. NUTT: Yes. And when you get up and
25 you take off in the metal water reaction, it leans a

1 little bit more out, and you do tend to get them
2 spread.
3 When we did the case that had 2274, there
4 was -- as a highest PCT -- there weren't any others up
5 there. It was up there by itself, but when you step
6 down, you know, in the 2100s, there was another one,
7 but in this one we had a 100 degree spread in it.
8 In these cases that we looked out where we
9 were down lower, they're all stacked in closer
10 because, you know, the upper tail on that distribution
11 is not so high.
12 Do you know what I'm thinking, what I'm
13 referring to?
14 DR. RANSOM: I think I do.
15 MR. NUTT: Yeah, if you --
16 DR. RANSOM: To the fact that if you take
17 more cases and run more samples, you'll actually
18 increase your chances of getting one of the higher
19 values out in the 90 -- beyond the 95th percentile.
20 MR. NUTT: And if I allow myself to move
21 inside now, that scatter out there in the tail, I'm
22 moving further inside, and I have less chance of
23 getting out in that tail.
24 But what does happen when you get up
25 closer -- and the tail doesn't really exist too much

1 at these lower -- the tail is not too wide at these
2 lower pieces, but the tail can get larger when your
3 distribution gets really skewed, you know, because
4 some cases will hit runaway metal water, and they'll
5 pop way up here, and others won't.
6 So instead of what looks close to a normal
7 distribution, you start to lean a little bit higher,
8 but we've taken it up to 2274 on a 95-95, which is
9 probably -- we'll never submit anything like that, you
10 know, and it still looped. You know, the conclusions
11 that Larry presented were, you know, still valid,
12 still looked very much the same.
13 But it did also have -- the 2274 was
14 basically almost an outlier. Had we done more cases,
15 we might have been able to reduce that. Okay?
16 DR. KRESS: Now, as you increase the
17 number of runs, your output distribution that you
18 could plot comes closer and closer to the real.
19 MR. NUTT: Yes.
20 DR. KRESS: So you have additional
21 information you could use to say whether or not
22 that -- you've got a higher probability of getting
23 those numbers, but you had more information. You
24 could use that to maybe rule it out or something --
25 MR. NUTT: Yes.

1 DR. KRESS: -- because you're getting
2 closer to the real distribution.
3 MR. NUTT: Yes. You're paying more money,
4 and you're getting a better answer. It is a very bad
5 way to get a distribution.
6 DR. KRESS: Yeah, it's not a good way.
7 MR. NUTT: If I wanted the distribution,
8 and I think I was looking at response services because
9 that's a better way to get a distribution.
10 DR. KRESS: Yeah, it would be much better.
11 MR. NUTT: You know, you have virtually
12 like a one over square root of N.
13 DR. KRESS: Yeah. So it's 10,000 runs or
14 something like that.
15 MR. NUTT: Oh, yeah. It's ridiculous.
16 DR. WALLIS: But if you had it, then you
17 could look at it and say, "Ah." The 2,200 is the
18 point -- is the 99.9 percent percentile --
19 MR. NUTT: Right.
20 DR. WALLIS: -- beyond that. You could
21 actually make that.
22 MR. NUTT: but the problem with the
23 response service is the biggest error they make is out
24 on the tails, and so if you're looking out on the
25 tail, that's where the biggest error is in the

1 response services.

2 Response services are really good in the

3 middle. So I like to use them when I'm doing

4 statistical work; I like to use them in the middle of

5 a process where I put something else on the end to

6 finish it up and be more accurate because they're a

7 little bad on the tails. Okay?

8 DR. RANSOM: By a response surface, you

9 mean you would just do parametric runs and construct

10 a surface for the PCT as a function of all the

11 independent variables.

12 MR. NUTT: Exactly. I'd run a set of

13 constructed variations in each of the parameters

14 designed, you know, to get rid of all of the --

15 designed to give me a sufficient number of points to

16 fit a polynomial of some kind. Usually it's a

17 polynomial. A polynomial of some kind over, you know,

18 say over the --

19 DR. RANSOM: But would that require more

20 than the 59 runs that you're making to do this from a

21 statistical point of view?

22 MR. NUTT: If I do three variables, no.

23 But, see, we're doing 40, 39, and I think some of

24 those are multiple variables. So I think the real

25 number is higher than 39, and we're actually treating

1 anything. We're treating anything that we could think
2 of that had any contribution to the large break LOCA.
3 DR. RANSOM: Have you looked into all --
4 some people are proposing linearized methods for
5 response surface.
6 MR. NUTT: I don't like linearized
7 methods. Linearized methods are equivalent of a root
8 sum squares, and a root sum squares is okay as long as
9 two things occur: one, normal distribution; and, two,
10 you add or subtract.
11 If you don't add or subtract, they're not
12 valid, and if they're not normal distributions,
13 they're meaningless. They're still valid, but they're
14 meaningless.
15 (Laughter.)
16 MR. NUTT: No, I mean, you could still get
17 a valid calculation of the standard deviation if you
18 add or subtract, but if your distribution is not a
19 normal distribution, what do you do with the standard
20 deviation?
21 DR. KRESS: That's right.
22 MR. NUTT: Yeah, it doesn't do you any
23 good. So okay?
24 DR. KRESS: That was very interesting.
25 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

1 that way is that particular fit was a fit where I fit
2 it with the PCT and with the local oxidation, and the
3 local oxidation is a random variable.
4 So if you make a nice, neat polynomial on
5 a random variable, it doesn't plot very well as a
6 nice, straight line.
7 If I plot it purely as -- if I do core
8 wide oxidation purely as a function of PCT only, and
9 you can do ALMOST as well, and later on I did it to
10 get it, then you get a nice, smooth curver because
11 this is a nice, smooth function.
12 And all those really are is step-wise
13 linear segments just to show you, you know, where the
14 mean goes.
15 DR. WALLIS: So there are really two
16 arguments you use. One is that these variables are
17 correlated so that if you know one of the extremes,
18 you should know the other one.
19 And the other is that the PCT is far
20 closer to its criterion than the other one. The total
21 oxidation criterion is off the map.
22 MR. NUTT: Exactly. In fact, I think I
23 was mentioning to someone the other day that this
24 really is, too, and I think it's an important point,
25 and I'm going to repeat your point because I think it

1 is important.

2 I think there's two things that we

3 demonstrate. One, that if you meet PCT, if you can

4 meet the PCT criterion, there's no possibility that

5 you'll fail either of the oxidation criteria.

6 Secondly, because they're very highly

7 correlated, if you take the highest PCT case and

8 report the oxidation from it, that those two oxidation

9 numbers are very close to being the maximum oxidation.

10 DR. WALLIS: All the 95th percentile.

11 MR. NUTT: Right, sir. They're very high

12 probability. So simply taking one case and reporting

13 both answers is quite sufficient, but again, it takes

14 the two reasons together.

15 DR. WALLIS: Now, do you want us to -- is

16 there a matter of debate between you and the staff or

17 are you agreed to this?

18 MR. NUTT: The staff is here.

19 MR. LANDRY: The staff accepts that

20 argument. You'll hear more about this tomorrow, but

21 I --

22 DR. WALLIS: We will hear about it?

23 MR. LANDRY: As far as I know. I don't

24 have my statistical person here.

25 DR. WALLIS: We'll have a statistical

1 expert here tomorrow?

2 MR. LANDRY: But as far as I know, he

3 accepts this argument.

4 DR. WALLIS: He accepts this argument.

5 That is remarkable.

6 Okay. Anything else today? Anything else

7 today? The probability of our getting to dinner is

8 increasing.

9 DR. KRESS: Is that 95 percent?

10 DR. RANSOM: Now, what kind of statistical

11 method are we going to use to select where we're

12 going?

13 DR. KRESS: A random walk.

14 DR. WALLIS: Shall we come off the record

15 at this point since we're getting a little erratic?

16 Yes?

17 Thank you very much.

18 (Whereupon, at 6:18 p.m., the meeting was

19 adjourned.)

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CERTIFICATE

This is to certify that the attached proceedings before the United States Nuclear Regulatory Commission in the matter of:

Name of Proceeding: Advisory Committee on Reactor Safeguards Thermal-Hydraulic Phenomena Subcommittee - Open Session

Docket Number: N/A

Location: Rockville, Maryland

were held as herein appears, and that this is the original transcript thereof for the file of the United States Nuclear Regulatory Commission taken by me and, thereafter reduced to typewriting by me or under the direction of the court reporting company, and that the transcript is a true and accurate record of the foregoing proceedings.

/s/ Rebecca Davis

Rebecca Davis
Official Reporter
Neal R. Gross & Co., Inc.

INTRODUCTORY STATEMENT BY THE CHAIRMAN OF THE
SUBCOMMITTEE ON THERMAL-HYDRAULIC PHENOMENA
11545 ROCKVILLE PIKE, ROOM T-2B1
ROCKVILLE, MARYLAND
NOVEMBER 13, 2002

The meeting will now come to order. This is a continuation of the meeting of the ACRS Subcommittee on Thermal-Hydraulic Phenomena. I am 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.

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 or requests for time to make oral statements from members of the public.

(Chairman's Comments-if any)

We will now proceed with the meeting and I call upon Mr. Jerry Holm, from Framatome ANP-Richland, to begin.