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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS (ACRS)

SUBCOMMITTEE ON THERMAL-HYDRAULIC PHENOMENA

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THURSDAY,

NOVEMBER 20, 2003

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ROCKVILLE, MARYLAND

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The meeting was convened in Room T2B3 of Two White Flint North, 11545 Rockville Pike, Rockville, Maryland, at 8:30 a.m., Dr. Graham B. Wallis, Chairman, presiding.

MEMBERS PRESENT:

GRAHAM B. WALLIS

Chairman

F. PETER FORD

ACRS Member

THOMAS S, KRESS

ACRS Member

VICTOR H. RANSOM

ACRS Member

JOHN D. SEIBER

ACRS Member

ACRS STAFF PRESENT:

RALPH CARUSO

Staff

SANJOY BANERJEE

ACRS Consultant

ALSO PRESENT:

Joseph Staudenmeier

RES

John Mahaffy

Penn State

Ken Jones

APT

Christopher Murray

RES

Jack Rosenthal

AEOD/ROAB

Stephen M. Bajorek

RES

Joe Kelly

RES

Ralph Landry

NRR/DSSA/SRXB

Chester Gingrich
RES/DSARE/SMSAB
Weidong Wang
RES/DSARE/SMSAB
William Krotiuk
RES/DSARE/SMSAB
Phil Reed
RES/DSARE/RPERWMD
David Ebert
ADSTM, Inc.
Birol Akdtas
ISL, Inc.
Shandai Lu
NRC/NRR/SXRB
Yue Guan
ADSTM, Inc.
Zena Abdullahi
NRR/DSSA/SRXB

A-G-E-N-D-A

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

(8:20 a.m.)

MR. CARUSO: Good morning. We will resume this meeting of the Thermal Hydraulic Subcommittee of the ACRS, and we will hear some more about the trace code. Looking forward to it. Steve Bajorek is here from RES, and he's going to get us started.

MR. BAJOREK: Okay, good morning. My name is Steve Bajorek from the Office of Research. What we would like to do today is start moving into assessments. First we're going to talk a little bit about the assessments and some of the work that we've done in 2003 in order to complete the code consolidation.

Then I'd like to start talking about what we feel is a heck of a lot more fun and interesting, which is going to be the assessment and the work that we are doing now and hope to extend into the remainder of 2003, 2004 and beyond, which will really start to put us in a position to be able to quantify the code, get uncertainties that we can use later to propagate in full scale analyses, and use these results to improve and develop new models, which are going to make the code more accurate.

Just by way of introduction, getting the code consolidated I think turned out to be more of a daunting task that what had been anticipated five or six years ago, whenever that started. In order to preserve all of the assessments that had been done previously by RELAP, TRAC-B and TRAC-P, we really run into quite a large number of assessments.

I don't have an accurate count on them, but there is a very broad range that has to cover a large range of conditions not only for the currently operating plants but assessments that had been done in support of the advance plants, like AP600, AP1000, and the ESBWR.

Most of our work over the last two years has not been directed at trying to find out what are the major problems in the models, why is the code behaving as it does, but rather trying to demonstrate that TRACE has the basic equivalency to TRAC-P and TRAC-B, or in the case of mainly the small break analyses, that TRACE has the equivalency to the RELAP code.

MR. WALLIS: Can I ask you about that?

MR. BAJOREK: Sure.

MR. WALLIS: TRAC-P and RELAP don't always agree. In fact, they probably never agree exactly. So, does TRACE have to decide whether it's emulating

TRAC or RELAP and then make a comparison?

MR. BAJOREK: There are only a few cases where you wind up with a RELAP, and TRAC, and then a TRACE comparison where you're really deciding. I think the overall majority of these cases, you're comparing either TRACE to one of these or TRACE to RELAP.

MR. WALLIS: That's what I mean. So, TRACE has to decide whether it's going to be emulating RELAP. Does it behave differently when it emulates RELAP than when it emulates TRAC?

MR. BAJOREK: No, not really. It's really a different test of the models.

MR. WALLIS: Okay, so it's itself, and then you say it either has to be equivalent to RELAP or to TRAC, but compatible?

MR. BAJOREK: They need to be compatible. We need to be able to --

MR. WALLIS: But TRAC-P isn't necessarily compatible with RELAP always.

MR. BAJOREK: No, no, no.

MR. WALLIS: But if TRAC is equivalent to one or the other, it's okay?

MR. BAJOREK: Maybe I don't understand your question.

MR. WALLIS: Okay. TRACE is A and TRAC-B is P, and we'll have to see. It equals B or it equals C, but it can't equal both of them.

MR. BAJOREK: Only in the cases where B and C are equal.

MR. WALLIS: Yes, but they don't.

MR. BAJOREK: No, no.

MR. WALLIS: So, what are you really doing?

MR. STAUDENMEIER: Excuse me, can I interject a bit? It's not saying whether they're equal or not. It's looking at calculation results compared to assessment data and deciding whether it does as well or better than the other code in comparing to that data.

MR. WALLIS: I thought it was supposed to be exactly the same.

MR. STAUDENMEIER: It's not strictly emulating RELAP.

MR. WALLIS: So it's a compromise between the two?

MR. STAUDENMEIER: It's not a -- it's looking at results that the code gives in deciding whether they're as good or better or worse than the other codes.

MR. WALLIS: So it's an independent code. I thought originally it was going to incorporate these codes and then you could sort of make it behave like RELAP if you wanted it to.

MR. STAUDENMEIER: It can run -- well, it doesn't run all RELAP models yet, but it will run RELAP models, but it's using the TRAC models and correlations package in it.

MR. WALLIS: Okay, so it's never really equivalent to RELAP?

MR. STAUDENMEIER: No, we haven't put in the RELAP correlations package to make it --

MR. WALLIS: So what you mean here is that the TRACE results look like the RELAP results. You don't mean that TRACE itself is equivalent to RELAP in all it's parts?

MR. STAUDENMEIER: Yes, in most --

MR. BAJOREK: It should have the functionality.

MR. WALLIS: Oh, it has the functionality, but it's not the same thing.

MR. BAJOREK: But you will not get the same results because we're using model packages from TRAC-P and TRAC-B.

MR. WALLIS: So it's a separate code

essentially?

MR. BAJOREK: Yes.

MR. WALLIS: Okay.

MR. RANSOM: Well, there are some differences in actually the mapping, and I'm wondering, if I generate a brand new model in TRACE, what does it follow? You know, which one of these three options would it follow?

MR. BAJOREK: Right now, it's primarily TRAC-P.

MR. RANSOM: So you would use the TRAC-P topology --

MR. BAJOREK: The models and correlations --

MR. RANSOM: -- in generating that model.

MR. BAJOREK: -- would be used to try to model simulations and processes that had been traditionally done with RELAP. I think the way I'd like to think about it is TRAC-P had traditionally been used for large break scenarios in PWR's, large and small breaks in BWR's where you need the jet pumps. In BWR, you need components. Those have been incorporated into TRACE.

TRAC has the capability of modeling small break processes. It doesn't do it in the same way as

RELAP has done. I think the code that has most commonly been used for PWR small break analyses. Our goal is to show that we can model and approximate the transients with TRACE about as well as TRAC-P could do the large break, TRAC-B could do the BWR unique cases, and RELAP could do the small break cases. At that point, we'll have the functionality in TRACE and will be able to begin to improve the models so we get better accuracy.

MR. WALLIS: RELAP has the added mass in it, and TRAC does not. So, if you had a transient where added mass was important, it wouldn't be equivalent to RELAP anymore, would it?

MR. BAJOREK: I suppose not, no.

MR. WALLIS: I think I understand. Eventually you compare with data, and if TRACE does a better job on the data than either RELAP or TRAC-P or TRAC-B, then it's really good.

MR. BAJOREK: And that's where we want to get. That's what we want to move towards. We want to try to get the basic functionality there and then focus our efforts on improving the models and getting the right models into TRACE so that eventually the code of choice is going to be a TRACE as opposed to any one of these three.

MR. MAHAFFY: This is John Mahaffy. Let me interject something here from a historical perspective. The word equivalent has never been used in this project, okay? He just used it, but you know, he's a newcomer.

Again, Joe Staudenmeier got it right in terms of the assessment. The goal in terms of data has been we are going to do as well or better, and that's why we're looking for metrics, you know, as RELAP or TRAC-P or TRAC-B on any given assessment where it's appropriate.

In terms of the kind of questions Vic was asking, in modeling, we are capturing all modeling capabilities. When you think in terms of like component modeling and whatnot, that all of these predecessors had, and to broad the statement, if you model something in TRACE native mode right now, it's not simply TRAC-P input.

The only exception I can think of that at the moment, and this is going to be corrected within the next couple of months, is the gravity thing we went through yesterday. If I put together a native mode ASCII input deck in TRACE, it's going to lean towards the TRAC-P side right now rather than RELAP5. If you wanted the RELAP5 bends, which I would want, and I'm going to fix it, you would input a RELAP5 deck into the SNAP, and you'd get it. It's just how the input decks get interpreted.

MR. WALLIS: So, I'll ask another question. This means then that eventually, a RELAP capability will disappear unless RELAP is maintained. It is not as if TRACE can emulate RELAP. TRACE is itself.

MR. MAHAFFY: TRACE is itself, but there is a commitment that if you have an input model that's been created based on your understanding of RELAP5 or built by somebody who's a RELAP5 expert, it will do what it's supposed to do in TRACE.

MR. WALLIS: But it won't give exactly the same answer as RELAP5 would.

MR. MAHAFFY: No, and it won't get exactly the same answer as TRAC-P or TRAC-B. It is its own beast, and it will be more its own beast as things go on from a physical constitiate model standpoint, and this is what you're going to hear more about today.

MR. FORD: I'm sorry. For a relative newcomer in this, I was, from your presentation, I was getting the impression that you just had this architecture where you dumped in, you plugged in, TRAC-B or P or RELAP. You just plugged it in to this gizmo and you get the same results as TRAC-B, and you just a consolidator. That's not true. You in fact modified the physics of the code?

MR. MAHAFFY: Not much so far, but that's a process whose rate of change is increasing rapidly, and Joe Kelly will speak to that.

MR. FORD: Okay, but you have changed the physics of the course, of the model?

MR. KELLY: In general, the physical correlations are the same as in TRAC-P, PF1 Mod 2, okay, with the exception of specialized boiling water reactor components like the jet model, where we basically took the entire model straight from TRAC-B and imported it in.

The idea is that volumes and junctions and so on are more or less the same, but treated a little bit differently. More or less the same in the various versions of TRAC and RELAP5. We only want to have one constitiative package in this code. We don't want to have users going okay, this pipe is going to be a RELAP5 model. This is going to be -- you know, who knows what your answer would be. It would be a nightmare.

We want to have one set of constitiative models, and be the very best constitiative models we can possibly find within the code. We know the ones currently in the code are not very good. I'm going to really show some bad problems in my next few presentations.

The idea of the assessment here, it's to show the equivalency, in Steve's terms, but it's more than that, is to find out where it is an equivalent. I mean, we know that some of the physical models in TRAC are going to be deficient, and where they don't perform as well as the models in RELAP, the assessment will highlight. Then we know, okay, that's where we have to go and spend our resources to make the code better.

MR. FORD: Thank you.

MR. KELLY: You're welcome.

MR. BAJOREK: Okay, so what I'd like to do this morning is to show some of the summary of some of the work that we have done for the code consolidation and then show some of the results where we're starting to move ahead and assess individual packages within the code, individual packages of models and correlation, show some of those results and give you an idea of the type of assessments that we have planned for 2003, 2004, and a little bit beyond. I don't think it's worth at this point trying to scope things out much beyond 2004 or 2005, but I wanted to let you see what some of the near term work is we have planned.

This shows a list of experiments that we've completed in 2003 where the mission of the analysts for each of these cases was to take an existing input on TRAC-P, B, or in some cases RELAP didn't show up on this one -- TRAC-P or TRAC-B, develop a TRACE model for that same facility, modify the input so that it runs with TRACE, compare it to its base or constituent code, and to do a comparison to experimental data.

The only exception to that are a couple of cases, a couple of plant calculations where we'd like to try to do a code to code comparison, and we don't have experimental data for that particular transient.

What I'd like to do is to go through, show some sample results for a large break assessment, a couple of small break type assessments, to show what the analyst was looking for, how they were characterizing the transient, and move on to some of the model involved in those.

MR. WALLIS: So what's the highlight?

MR. BAJOREK: Those are the ones I'm going to show later on in the presentation.

MR. WALLIS: Okay, okay, so you have done all the other ones as well?

MR. BAJOREK: All of these are complete, but we could take any one of these and probably spend a good hour, hour and a-half on them.

MR. WALLIS: This looks like the list we saw some time ago. Is it?

MR. BAJOREK: You probably saw this about a year ago as assessments in 2002 that we were planning for 2003.

MR. WALLIS: It was what you were planning to do then?

MR. BAJOREK: Yes.

MR. WALLIS: Okay.

MR. BANERJEE: Did you do some RELAP ones as well, RELAP input origin?

MR. BAJOREK: There are, but they haven't shown up on this. The two problems that have held us back a little bit on the code consolidation, one has been the ability of SNAP to take RELAP decks and convert those over into TRACE. So, our ability to be able to model and simulate some of the more complex small break tests, things like ROSA and BETHSY and semiscale, has been impeded because we've been waiting for SNAP to mature enough so we don't have to have an analyst go spend nine months just to regenerate an input deck. So, we purposely held off on those.

We've also held off on a number of the forced and integral forced reflood test simulations because we've been waiting for the interim reflood model to be completed before we go on. We know what's in TRAC-P right now is insufficient. So, most of these cases don't rely on either developing or converting a RELAP input deck or on the reflood model.

The one thing I do want to point out is where we have done comparisons of TRACE, or previously known as TRAC-M to TRAC-P or B and RELAP, was last year when we looked at Frigg and a number of the level swell experiments. I think we talked about these a year or maybe two years ago, and we showed you a series of calculations, a series of simulations, this being on the level swell tests in the Oak Ridge bundle, where we concluded from that TRAC-M, or TRACE, was doing about as good a job as RELAP or TRAC-B relative to each other and the data.

We identified some problems in the TRACE interfacial drag package, that I think as Joe mentioned yesterday, I think we can resolve by going to the BETHSY on interfacial drag model. That would reduce the void -- this is showing void fraction versus elevation for this particular test. That would reduce this and give us better agreement between the level swell that you would calculate from the two phase level and the collapse level. The collapse level is too low in the TRACE model.

MR. WALLIS: So why did TRAC-M get a different answer than TRAC-B? You put in some different constitutive equations?

MR. BAJOREK: There's a different constitutive package in each one of these.

MR. WALLIS: So you're not really showing equivalents here. I don't quote know what you're showing. Are you just showing that TRAC-M at that time was sort of worse than RELAP.

MR. BAJOREK: Yes, it is.

MR. WALLIS: All right.

MR. BAJOREK: I mean, it's a different constitutive package.

MR. WALLIS: It seems to have got off TRAC at about a certain position, you know. It's gone maybe to the left and then it doesn't come back.

MR. BAJOREK: But our conclusion at this point is if you're going to try to correct any of these to match the data which is the open triangles in here, we're going to concentrate our efforts on TRACE, TRAC-M in this case. We're not going to go back and try to correct RELAP, which for this test was actually looking like it's doing a worse job in the experimental data.

MR. WALLIS: It depends what you look at, yes.

MR. BAJOREK: In ten to 15 of these tests, yes, you'll find RELAP does a better job on some of them. TRACE does a better job on some of them. TRAC-B wins out on some of these. Our conclusion is, for lack of a better term, they're approximately equal, okay? It can model this test. We get about the same results.

If we had defined metrics in terms of level swell, we would get about the same number, even though we haven't calculated that, but now is the time to put this to rest. We'll move ahead. We'll put in the BETHSY on interfacial drag model, and then the next time we do these simulations, we're only going to be looking at TRACE versus this data, and using a metric to try to show how much better we can end up.

MR. WALLIS: One of the problems, maybe at the beginning, so the initiation of flashing or whatever is going on here.

MR. BANERJEE: Is that due to subcooled

boiling, or what's the problem down below axial location?

MR. BAJOREK: This test had very little subcooling at the beginning. It think this is close to a saturated test.

MR. BANERJEE: But why is almost all the predictions are not showing any void until about .8 meters, where as in actuality --

MR. WALLIS: No, in actuality --

MR. BANERJEE: -- there's a significant void by that time.

MR. WALLIS: I don't think so because there's no data until you get to one.

MR. BAJOREK: Yes. Boiling begins somewhere in here. We may have had a DP cell down in here where you've got a zero.

MR. BANERJEE: There's a sharp change in void there.

MR. BAJOREK: Well, if it's subcooled boiling, you get a few bubbles there, and then it will -- there's some subcooling at the bottom.

MR. RANSOM: Is this a vertical system?

MR. BAJOREK: Yes. It's a rod bundle. This is full hot rod bundle, I think something like 60 or 70 rods.

MR. WALLIS: So maybe it starts on one rod before the others. Look, we don't really need to go into this particular one, I think.

MR. KELLY: Actually, it's real simple. Actually, some of it is just simply the experimental boundary conditions. You have very, very low flow rates here. You're talking, you know, a centimeter a second type of velocities. So, the uncertainty in that determines where you reach the saturation line and become two-phase. I mean, I can move about quite a bit just to the uncertainties and --

MR. WALLIS: Because of the uncertainties, they might well cover everything here.

MR. BANERJEE: Yes, so all the codes have the same input models, so they start boiling at the same place. Obviously for this case, the reported experimental values were not quite right, but a very, very small difference in the inlet flow rate, moves where the saturation line is, and that's what you're seeing here. Once you get away from the incipience of boiling, then the flow quality is about right and you're not too far off.

MR. WALLIS: What's the difference between the TRAC-B model and the TRACE interfacial drag model?

MR. KELLY: Okay. Joe Kelly again?

MR. BAJOREK: Go ahead.

MR. KELLY: TRAC-B takes a drip flex model and converts it into an interfacial drag model. TRAC-P, what it uses is a bubbly slug thing where it's size of bubbles and sizes of slugs and ramps between the two and then also puts a profile slug factor on that. So, it tries to be more fundamental, but actually it's a worse model. So, that's why TRAC-B for these kinds of tests will work a lot better.

MR. BANERJEE: The TRACE model here has the TRAC-P model in there?

MR. BAJOREK: Yes. That's correct. It does now. It won't always.

MR. WALLIS: It's going to all have Kelly models eventually.

MR. RANSOM: One question. You mentioned the SNAP not able to convert the RELAP5 decks. That didn't come out yesterday, I didn't think. What are the problems there?

MR. BAJOREK: At the time we had started this work several months ago, it could not.

MR. RANSOM: Now it can?

MR. BAJOREK: Now it can. Now it's getting to the point where it can take either all or most of the RELAP decks and convert those over to

TRACE. Missing on this are things like ROSA, BETHSY, semiscale, some of the more conventional small break application where you would see RELAP doing a credible job right now, and we know we need to get TRACE to do a good job, but it's been waiting for our ability to economically convert those input decks, is why we've had to hold off on those.

MR. RANSOM: That's what I was asking. The status is now you can do that?

MR. BAJOREK: Now we can to that, yes.

MR. STAUDENMEIER: This is Joe Staudenmeier. It was on one of my later slides yesterday that I had to zip through, so I probably didn't get a chance to say it, but we're at the point where we're working on typical PWR base model, actually 1200, which is a typical PWR base model. We have it converting and running all the way through if you change some temperature inputs in the feedwater and the steam generator.

We have a condensation problem that we haven't determined if it's an input mapping problem or an internal code problem yet, but if you make the water temperature hot enough, it will run all the way through. It seems to be converting geometry and control systems and heat structures and models simpler than that, it can convert and run without any problem.

MR. BAJOREK: This is an example of some of the things that we were looking for when these simulations were performed. In each case, when they ran a particular simulation, first of all, the analyst was asked to put everything into an AV script format that I think Chris talked about yesterday, is an automation tool so that we're going to be able to go back, change the code version in the future, rapidly re-run these, and regenerate a number of the figures of merit.

Now, we don't have time to go through all the package of some 50 to 75 different figures that make various comparisons to the data.

MR. WALLIS: Well, the first figure here could probably be predicted by one or two node models, and it's just a system with a hole in it. So, you expect that to work out pretty well.

MR. BAJOREK: Blowdown.

MR. WALLIS: Blowdown, it's blowdown of a system with a hole in it.

MR. BAJOREK: If your break flow is right.

MR. WALLIS: So, all the codes ought to do pretty well if you've got the break flow right.

MR. BAJOREK: They ought to, if they're

break models.

MR. WALLIS: Right.

MR. BAJOREK: Yes. In this case, we compared TRACE to TRAC-P. This shows a cladding temperature versus time. The blowdown peak here, experimental data showed a rewet. Of course, the question was whether that was the exterior amount of thermocouples or the rod itself. In comparison to TRACE, which is shown in the green, and TRAC-P in the red, we looked at those and concluded that TRACE was doing about the same job as TRAC-P.

Now, there are differences in the input models that had been put together. There were some small differences there. There have been changes in the constituent package for TRACE that would make it different from an earlier version of TRAC-B, but our conclusion in taking a look at this simulation is we were getting about the right results with TRACE and was time to move on and focus our attention from that point on on getting TRACE to better match the data.

MR. WALLIS: Is there a physical reason why the big difference between both the codes and data?

MR. BANERJEE: It may not be real.

MR. WALLIS: That's what my question

really was. An explanation as to why the data is not what it is?

MR. BAJOREK: Yes. In LOFT, the thermocouples were not inside the rods as they are in many electrically heated facilities. They're mounted outside of the rod itself. The tip, okay, down at a particular elevation, could potentially have been struck with a droplet and could have cooled prematurely compared to the heated rod surrounding it.

So, what we might be seeing is the thermocouple having rewet and not able to rapidly heat up again because it has been quenched and could continually be struck by droplets in the flow, whereas the rod surrounding or nearby may not have quenched and have been at a higher temperature. I think that's been an arguable point on LOFT since the tests have run.

MR. RANSOM: It has been, but I thought it was pretty much agreed there was a topdown reflood, early rewet that occurred.

MR. BAJOREK: Okay.

MR. RANSOM: I don't remember how the codes predicted that or how well.

MR. BANERJEE: Well, unfortunately in the beginning, they tried to fudge it to fit the data, but then it was found the data was incorrect, so they had to go back and un-fudge it.

MR. RANSOM: That's with the external thermocouple you mean?

MR. BANERJEE: Right.

MR. RANSOM: Yes.

MR. FORD: Is there any way that the model could --

MR. BAJOREK: Model the external thermocouple?

MR. FORD: Exactly.

MR. KELLY: You have to model the thermocouple if you did that.

MR. FORD: If the model is good, extensive enough, then it should be able to tell you what to have been the conditions to give you that observation.

MR. RANSOM: I don't think anybody knows what the effect of the thermocouple was on the film, you know, and whether it would rewet or not.

MR. BANERJEE: There is a significant difference between TRAC-B and TRACE then that you're getting a much earlier rewet with the green line than with the red.

MR. RANSOM: The final quench you mean?

MR. BANERJEE: Yes, and it's quenching

from a higher temperature. What's causing that?

MR. BAJOREK: This quench here versus that?

MR. BANERJEE: Yes.

MR. BAJOREK: It may be in the minimum zone boiling models, but I really don't know.

MR. WALLIS: Maybe Joe Kelly is going to explain it a little later.

MR. BANERJEE: The models are the same, aren't they?

MR. BAJOREK: They're very close, but they're not identical.

MR. RANSOM: Is that the correlation that's causing that?

MR. WALLIS: Well, Landry wants to talk.

MR. BAJOREK: I think they are different in TRACE and TRAC-B.

MR. WALLIS: Okay, would you yield the floor to Ralph?

MR. BAJOREK: It looks like TRACE is too high. We could always ask one of the experimenters from Roth what happened.

MR. LANDRY: This is Ralph Landry from NRR staff. At that point, I was in RES and was managing the LOFT project. The rewet that occurred in the large break loca experiments, L22 through L26 in LOFT, was partially attributed to a film thin effect of the thermocouples that were surface mounted for those experiments. That thin effect was estimated to be about 20 degrees Kelvin. A later study said maybe it was a little bit more, but we were at that point taking the effect to be about 20K.

The energy balances that were performed on the fuels did demonstrate that yes, indeed, there was an early quench. The quench was real and did extract the energy from the fuel.

What we were seeing was a major difference between the design of an electrically heated fuel rod versus a nuclear rod which had a true gap between the fuel pellet and the cladding which did not exist in the electrical rods. The electrical rods tried to simulate the gap but did not have a true gap and were thermally linking the cladding with the fuel much more tightly than is true for a nuclear rod.

The later experiments that were done under the OECD project, the large break loca experiments under LOFT, used an embedded thermocouple in the wall of the cladding. Those experiments would be much more accurate for comparison, but those did show an early rewet also, but not to the magnitude of the experiments that had the surface mounted thermocouples.

It would be much better to use those OECD NEA project large break locas for comparison purposes. The thermocouples were embedded in the wall by machining a small groove in the outer surface of the cladding. The thermocouples were laser welded into place and then ground smooth.

The early experiments were forced, and I think Vic remembers those days quite well, to get the codes to match up with the data. We were seeing RELAP5 in those days giving very different results.

We saw a dramatic comparison with a code work that was done at Los Alamos National Laboratory using TRAC that miraculously overlaid the quench perfectly. Unfortunately, they were using a heat transfer correlation package that was giving the right quench but for totally wrong thermal hydraulic reasons. That was what Vic was referring to as having been backed out at a later date, because they were getting the right result for the wrong reason, and that simply didn't work.

MR. KRESS: Steve?

MR. BAJOREK: Yes.

MR. KRESS: Let me ask you a hypothetical question about this kind of curve, not specifically that curve, but presuming you had great faith in the data and a small uncertainty in the data, and your code had a prediction somewhat like this, which was significantly off, and if I wanted to use data to express some level of uncertainty in my code, how would I do that with this kind of transient curve where sometimes it's right on the data and sometimes it's off and sometimes it's under and sometimes over? How do I use that to determine an uncertainty?

MR. BAJOREK: Okay, I think what you need to do, and I've got a slide coming up that I think addresses that.

MR. KRESS: Okay.

MR. BAJOREK: But I think what you need to do is you need to look at the physical processes that are going on in each one of these periods. DNB, blowdown cooling, Tmin or lack thereof. Let's say this is the rod. Tmin out here. Heat transfer, coefficient --

MR. KRESS: So you could tie the uncertainty to different time frames maybe when different phenomena were occurring?

MR. BAJOREK: From different processes which are dominating why this curve looks the way it does, okay, and I think when you go back and you want to try to get a bias and uncertainty, you need to get biases and uncertainties in those processes that you can go back and use to say something about the models in the code.

Okay, I need a let's say an uncertainty in not reflood PCT here but in the heat transfer coefficient for the steam cooling dispersed droplet heat transfer that's going on. An uncertainty in Tmin of a quench temperature that appears to be different in these two so that I can go into the code and say even though I know I've got a model package that has flaws and it doesn't capture all of the right physics, there's a way that I might temporarily be able to adjust it, fudge it towards the right value, something that will make it right in the data, and then propagate that in a PWR or a BWR at full scale to see what its effect is for transients that may go a couple of hundred seconds as opposed to what's going on here.

One thing that we are keenly aware of is that we do not want to just focus our attention on blowdown peaks, PCT's or let's say a reflood PCT, as the sole parameter of merit to these. So, I think from these, I would look at the processes, the blowdown cooling, the reflood heat transfer minimum film boiling temperature, to go other parameters on this that might tell me how much mass is in the vessel as a way of getting something that will tell me about the interfacial drag that goes on in the core itself.

MR. KRESS: So if I had the appropriate uncertainties associated with my various parameters and models in the code built into it and the code was calculated in the uncertainty as it went along, when those models come into play, you might distribute higher uncertainty during that period, and it would automatically kick it out, and you'd get a transient uncertainty that is distributed.

MR. BAJOREK: That's right because we still have the problem that the scale on this and many other of these experiments is not the same as it is in the full scale prototype.

MR. KRESS: So, the uncertainty, it wouldn't be one sigma for uncertainty. There would be a lot of them, depending on what you're dealing with.

MR. BAJOREK: It's a distribution.

MR. KRESS: It's a distribution.

MR. BAJOREK: We may have some models that do a very good job at the small bias, and because of the experimental database, the uncertainty might be small. We're going do better than the scatter in the experimental data, but there may be other situations where the model is ad hoc, or it may have a very large bias, but the experimental data has such a large uncertainty that you may need to incorporate that in your full scale prototype predictions.

MR. BANERJEE: I have really a different question. Why is there a difference between the red and the green lines? I mean, they're supposed to be based on the same, at least at the time when you ran that simulation, based on the same physical models, right? That seems more puzzling to me. Is there something different in the numerics, or what's giving that difference? You are starting with the same --

MR. BAJOREK: To get the constituent package in TRACE, it's not necessarily identical to all of the models that were there in TRAC-P. There was a selection process that preserved most of these but not all of those.

MR. BANERJEE: Which ones were not, because you're showing some difference, right?

MR. STAUDENMEIER: Let me interject. It's Joe Staudenmeier. I mean, most of the constitutive packages are identical. There may have been some small bug fixes between the two that didn't make it from one to the other, but I think most of those were carried over.

One of the big differences is collocating heat structures in TRACE. It used to be heat structure nodes were on fluid boundaries. Now a heat structure note is in the middle of a fluid cell, so there was a difference in the conduction model and the fuel rods that could be attributed to that level of difference, I think. I mean, there could be other things. There could be fixes -- I mean, we put fixes in the break flow model and various other bug fixes in TRACE that didn't make it back into TRAC-P that could be attributed to that level of difference.

MR. MAHAFFY: John Mahaffy. Let me add to that. You shouldn't underestimate the bug fixes, particularly in a situation where you've got quenching behavior. My experience with this is that, I mean, if you really wanted to understand this, you'd want to take each of these decks and introduce some small perturbations here or there and understand how the system responded to small perturbations.

Some of the perturbations that we've introduced with bug fixes aren't all that small. So, I mean, it's one of the reasons why I was telling you earlier, you know, these are not identical physical models. You can read the manuals and they look like identical physical models, but what's in there? There are differences.

In a case like this, this kind of difference in results should not be surprising to you.

MR. WALLIS: Is there a user effect? Many code experts talk about user effects, or do you have some choice in one of your codes, if that makes a difference to the answer? Is there any user effect with TRAC-P or TRACE?

MR. BANERJEE: They're the same decks, right?

MR. BAJOREK: They should be the same decks, although in some of these simulations, they did have to make some changes to the nodalization to get one to look more like the other. That was more the case when we had some of the RELAP models.

MR. WALLIS: So there could be user effect there?

MR. BAJOREK: Yes.

MR. BANERJEE: But what are you demonstrating with the slide, that same, similar?

MR. BAJOREK: Similar.

MR. BANERJEE: Similar enough?

MR. BAJOREK: These are similar, yes.

MR. BANERJEE: And how do you sort of

quantify the similar enough here? One is rewetting ten seconds earlier than the other, which is significant, and the peak is higher, and it's rewetting from a higher temperature. So, what is the similarity here?

MR. BAJOREK: It's essentially a subjective opinion on looking at the various codes by TRAC-P and TRACE in this case. We have not applied a metric to this, but it's a way of doing the assessments and telling the development team whether there is a code error or a bug that is preventing TRACE from running this, or it is deviating substantially due to some model that may not have been converted correctly.

MR. STAUDENMEIER: One thing about that quench, too, is the quench in LOFT is totally different than a PWR reactor quench because the short core in LOFT, that quench happens when the accumulators empty out and the gas pushes the surge of water in the core. In a regular PWR, it would decrease the temperature for a little bit, but then it would recover, but with the short core in LOFT, it just quenches the whole core all at once. So, that's not typical of a PWR.

Essentially, that quench time is based on when the accumulator empties out.

MR. BAJOREK: I don't agree with that, Joe.

MR. WALLIS: Well, maybe we should move on. We've got a lot more comparisons to look at.

MR. BANERJEE: Well, what this shows at the end of the day is that you're sort of qualitatively similar, but it actually puts some emphasis on these small changes having a fairly large effect.

MR. BAJOREK: I think as John pointed out, don't underestimate the bug fixes. When we were doing development with COBRA track for Westinghouse, we'd periodically find errors in some of the correlations, and those would have substantial effects on both the assessments. In some cases, they would have a large effect on the PWR calculations.

In some cases they wouldn't, or it would be vice versa. It really depended on the bug fix itself, and I don't think you can really generalize that other than that you should expect some difference between a code version with and without the bug fix.

Another case where we were able to start to see whether TRACE can handle a small break transient was in the case of LOFT L3-7, which is a one-inch cold leg break. In this case, there were TRAC-P decks available, so we didn't have to depend on SNAP. Several months ago, converted this into the TRACE format, simulated --

MR. WALLIS: Excuse me. TRACE actually shows that the pressure increases over part of the transient as a whole, and the pressure increases?

MR. BAJOREK: Yes, right over there.

MR. WALLIS: It looks a little suspicious, that whole wiggling around there looks -- that cliff where it goes down and then comes back looks very strange, simply a depressurization through a hole.

MR. BAJOREK: It shouldn't hang on this until you clear a vent path for the break.

MR. WALLIS: The pressure shouldn't rise, should it?

MR. BAJOREK: No, unless there's a problem with your steam generator heat transfer. If your heat transfer in your steam generator is insufficient, the system will, and the code will repressurize in order to give you the delta T to get the heat out.

MR. WALLIS: See, is that the pressure of a secondary or something which is there at that level or just the steam generator pressure?

MR. BAJOREK: Steam generator secondary

pressure is probably down here.

MR. WALLIS: Down there.

MR. BAJOREK: Would I would look for in order to try to correct this is to take a look at the steam generator return.

MR. WALLIS: So it hangs around, the secondary pressure, you'd think, for awhile, right?

MR. KRESS: That little dip around 2000 looks strange.

MR. WALLIS: That's right.

MR. KRESS: There's something wrong with it. I would say there's something wrong with that there.

MR. RANSOM: In this comparison, this is a TRAC-P deck converted to TRACE, and that's the TRACE result. The RELAP5 is just basically a RELAP5 calculation. So, the models are two different models.

MR. BAJOREK: Two different models.

MR. BANERJEE: So when you ran this and you saw that dip, does somebody go in and try to understand anything which looks sort of weird and figure it out?

MR. BAJOREK: At this point, no. We were under the gun to try to get the consolidation moving ahead just to do the basic comparisons, and we made a conscious decision at that point to try to get the comparisons to try to show that there were in general about the same accuracy as the data. Then in the next phase, start to compare what would be the red curve back to the black to really understand why there are deltas between the predicted and the measured and what is causing some of these individual --

MR. WALLIS: There's a funny sort of hump, too. I mean, after 2000, between 2000 and 225 or something, something odd happens.

MR. BAJOREK: Yes.

MR. RANSOM: It would be interesting to see the break flow predicted by the two. I imagine there is some --

MR. WALLIS: Maybe it changes the break model.

MR. RANSOM: -- clues there, right.

MR. BANERJEE: I'm sure that there is a -- if you look at all the tests, you will see some phenomena which are occurring which may be arising from the code. How is that process of examining these sort of results and feeding back that knowledge into fixing things that are going to occur? Is there a systematized way to examine these?

MR. BAJOREK: We have an error correction reporting system, that when we do find problems with the code, these go into the reporting system, and then as we can get to those various problems, we'll isolate and look at a problem like this if that has been reported as a problem.

Right now, because of where we're at in the code development, things have stopped the code, or make the code run excessively slow, are getting more of the attention, okay, rather than trying to find out what are the individual nuances in some of these.

MR. WALLIS: But your attempt was to show that TRACE is equivalent to RELAP? I mean, it looks as if TRACE is doing something new, which is inexplicable over part of the transient which RELAP did not do. Therefore, it's not really equivalent. It's introduced some new thing, and we don't know what it is.

MR. BANERJEE: It's noted anyway and kept in some file, because we're not going to sit and look at all these, and there are thousands of these curves, right? Whenever this is generated and something looks out of sync or an analyst doesn't understand why it is, it should be put into a file of some sort saying is this weird behavior in this figure which I haven't figured it out, but we want to go back and take a look at it at some point and try to understand.

MR. CARUSO: Isn't this why you're developing this ACAP system, figure of merit, to be able to automate these sort of assessments and determine if something like this occurs, whether it's significant?

MR. STAUDENMEIER: That's correct, and I bet if I ran this run again with the code fixes that I put in recently, it would give better results than that.

MR. WALLIS: How much are you betting?

MR. STAUDENMEIER: At the next break, I was going to go up and start up the run and try to get results before the afternoon.

MR. WALLIS: Go ahead.

MR. STAUDENMEIER: So, I'll show them to you and whether they're worse or better at the end of the day if you want.

MR. WALLIS: I'd like that.

MR. KRESS: Tell us what fixes you made, and then we'll put a bet down. I was kidding.

MR. BAJOREK: Joe, when was the release of --

MR. RANSOM: Who is the guy who will make the changes to say improve the situation?

MR. STAUDENMEIER: I mean, the bug fixes I made had nothing to do with this run, but every run I've made with these bug fixes in, it's an interfacial drag model. It's improved everything I've ran so far, and a better way to improve this.

MR. WALLIS: So would you do that? I think that would be a wonderful test. You do that today, and we'll see the results.

MR. STAUDENMEIER: Okay.

MR. BANERJEE: But in the end, there has to be some sort of traceability where this is noted as being a problem. Then when there's a fix, the problem goes away. You know, I think without that, we're just doing very qualitative stuff here.

MR. BAJOREK: It's also, and I want to try to get to this because I'm going to go through some UPTF calculations. It's also a bit dangerous to focus your intention on a single transient or a single run. You start focusing on how good this one might look or what the error or problem might be on that specific transient.

We feel that is of most value right now is to get things set up so that we can do lots of calculations, look at these en masse, in general, and see is this happening in all of our transients? Is this something that's just happening in LOFT, or do we see this in all of our small break transients to give us a means of saying oh, you have a serious problem either in the break flow model, the steam generator heat transfer, and then focus your attention on that.

MR. WALLIS: I think we have to move on. We're going to see a lot of these, and we're going to have the same questions again. This is slide number three I think here.

MR. FORD: The trouble is, it's fascinating stuff.

MR. WALLIS: Well, we can stay until midnight I suppose, too.

MR. BAJOREK: Okay, let me go through the next few of these because I think we're going to wind up with the same types of comments on why is the red curve different from the blue and it's opposite from the data. What I'm going to point out is that we have run a wide variety of transients.

In some cases, we looked at these and subjectively concluded that TRACE is doing about as good a job as its predecessor code. The last one was a separate effects for in surge, out surge. We've looked at the radiation model for BWR components. In this case, TRACE --

MR. WALLIS: The data are right on top of the --

MR. BAJOREK: It's doing a pretty good job.

MR. WALLIS: Well, that's good. Now, that's a nice thing to see.

MR. BAJOREK: I thought you were going to catch this.

MR. WALLIS: How did you not manage to catch that since you fudged everything else?

MR. BANERJEE: What happened to TRAC-B there?

MR. BAJOREK: It was attributed to a difference in the natural convection heat transfer coefficient. In TRACE, it was a bit lower, quite a bit lower than usual.

MR. WALLIS: That's predicted from a correlation, natural convection correlation? Predicted from a correlation?

MR. BAJOREK: Probably, yes.

MR. BANERJEE: How did TRACE get one-fifth the heat transfer coefficient?

MR. BAJOREK: That I don't know.

MR. WALLIS: Now, that is interesting.

MR. BAJOREK: Okay. There are differences in some of the models. There are other cases where TRACE is substantially different from the data or what its predecessor code. These have been turned in, okay, to the model development team.

This one, the suspicion was that there was a problem in the 3-B level tracking model. In addition, there were also problems that were indicated in the transition boiling. They also may be related to the latest bug fix that was found in the annular mist, where some of the nodes, part of the core was stuck, and it was inordinately low heat transfer coefficient, even though physical conditions says it should be quite a bit larger.

MR. WALLIS: It was funny, that one. That was really a big difference.

MR. BAJOREK: Yes, and this is one that we basically --

MR. WALLIS: Generally you'd expect your modifications to TRAC-B to be improvements.

MR. STAUDENMEIER: Actually, that one I looked at a little bit, and I think that's due to some CCFL problems. You're not getting water penetration into the bundle.

MR. WALLIS: TRAC-B does.

MR. STAUDENMEIER: That's right.

MR. WALLIS: And you have the same CCFL --

MR. STAUDENMEIER: No, we don't. The models are different. The interfacial drag models, and I have to look at see about the CCFL.

MR. BANERJEE: Well, there's a big difference between TRACE and TRAC-B at that stage, and TRAC-B I presume, was just the flux model, right? I mean, that's what works in a vertical broad bundle.

MR. MAHAFFY: John Mahaffy here. Let me put this into context and hopefully let it run along a little more smoothly. When you look at these results, what you want to be thinking about are two things. This is a baseline, and more importantly, it disappeared in the noise a little bit.

He's setting this up as an automated process so that all the work that he went through to get these results, the next time around, he punches a button, and they all come out again. Joe Kelly is systematically going through all these physical models. You come back here a year from now, and he'll tell you a completely different story, I hope.

MR. BANERJEE: Can you revisit exactly these ones?

MR. MAHAFFY: You can ask for whatever you want there, but again, this is your baseline. If anybody is going to give you improvement in physical models in one of these codes, Joe's the guy. You want to look for what the changes are over time because they are going to happen.

MR. WALLIS: I think we understand that. I think we understand that any code is going to have trouble because the physics are not very well modeled somewhere, any code.

MR. MAHAFFY: These are a little worse than most.

MR. WALLIS: The point is that eventually, when we want to write or you want to write a letter to I presume to the Commission, the public's going to see, showing that with all this investment of time and money, you have a code which is better than the one before. Otherwise, why did we do it? So, eventually, we want to reach that point. That would be a point we would like to reach not too far in the future.

MR. BAJOREK: And that's what hopefully we're setting ourselves up for because we're at the point now that when we look at all of the simulations en masse, we feel that the code consolidation part of the effort is over. For the most part, we see TRACE doing a comparable job to its predecessor code. There are clearly some exceptions, and even in the cases where it looks like it's doing a comparable job, we see problems. That's what we want to start to focus our attention one.

Every single one of these cases that had been run have been put into the AV script format. So as we do get bug fixes, we're going to be able to go back and repeat all of these. Six months or a year from now as we improve the reflood model, we improve condensation, approve interfacial drag, we hope to be able to repeat most or all of these simulations to find some parameters and metrics in order to track how much better it's getting. So, this is really, I think, as Dr. Mahaffy pointed out, this is a baseline. This is a bit of a starting point.

MR. BANERJEE: So in your protocol, this is the control? This is more or less what everything else is going to get compared to?

MR. BAJOREK: Yes.

MR. BANERJEE: Okay, so periodically then in your protocol, you repeat these and you will have some measures, hopefully not too statistical with those things, but eyes pretty good, and then you come eventually to some point where all of these things will improve. Then you will come to us and say wow, now we've got a code, right?

MR. BAJOREK: We're hoping you say that.

MR. BANERJEE: Okay.

MR. RANSOM: Well, one thing about the assessment effort, you know, in the past you've had people like Brookhaven who were an independent assessment. You know, they were not the code developer and so there was a bit of an antagonistic relationship which was beneficial, actually. You got a little more objective view because you can pick cases and prove about any point you want because of these differences and plus or minus.

So, in a way, the assessment process needs to have some independent objective way, I guess, of giving it an across the border assessment. I guess I haven't seen that yet in your plans. You know, the developers are always going to choose cases that tend to prove the point they want to prove, and it's just --

MR. BAJOREK: No, I think I'm going to show you where that hasn't been the case, and what we intend to do with the assessment matrix and the treatment is going to change. Let me show you that.

MR. WALLIS: Approximately equivalent is a pretty vague term, though.

MR. BAJOREK: Yes, it is. In the next

phase, we need to begin to establish these figures of merit, these parameters, and I think what engineers want, give me a number. Give me some type of a parameter where I can get some type of a numerical measure on how much better your code is getting with time.

MR. BANERJEE: Well, it means to me that the uncertainty bands between the two -- well, say the prediction and the experiments, have at least some overlap. There really something is wrong, right, but if the experiment, your band is there --

MR. KRESS: Yes, I think in general, we ought to think in terms of uncertainty bands as your figure of merit somehow.

MR. BAJOREK: Okay. Just to wrap up the code consolidation, we feel that the code consolidation part is complete at this point. We've identified problems and issues. There's been some situation with robustness that have made the code run slow, not giving us the results we want.

We've seen problems in the level tracking. There's been several bugs in there that have been fixed along the way. The reflood model, we know needs to be improved, which is why we're going to the interim reflood model.

At this point, we'll start to focus on better quantification of the models and improving the models within the code.

MR. WALLIS: Does that mean that TRACE is not ready for use for regulatory purposes?

MR. KRESS: Not ready for prime time.

MR. BAJOREK: I guess that --

MR. KRESS: So example, for AP1000 and ESBWR?

MR. BAJOREK: I think it can do those cases provided you have done the assessment that's very important to those cases. I would not trust TRACE until we do the assessment against APEX AP1000, perhaps some of the APEX AP600 tests, okay? These are small break processes, and we've seen that there's problems in the L3-7 simulation, and we're still moving ahead with some of the other small break cases. So, I don't think we can trust it at this point.

In its behalf, I would add that we've taken the RELAP model, converted that to TRACE. We've rerun the simulation, and have gotten results that look much like the RELAP calculation. RELAP as well, we would have to do some additional assessments in order to look at level swell and entrainment and some of those things that we really don't trust in any of the codes at this point.

Some of the shortcomings of assessments that have been done in the past, one, it really hasn't been efficient use of the available input decks. In some of the cases, ECC bypass for example, you go to the trouble of setting up a model of a Creari or a UPTF. spend months getting one of these decks together, and you might simulate one test.

Well, there's been lots of very good tests run in these facilities, and we haven't always exploited this additional experimental data to look at how does subcooling, how does pressure, how do other flow conditions affect your transient.

In general, and I think you saw that, you know, quite a bit over those last comparisons for the code consolidation. In general, why a code looks good or bad or excellent or whatever type of subjective term you put on that, is really in the eye of the beholder. The idea of assigning or developing a bias and uncertainty to a particular transient or to a model package has usually not been done. We want to try to start getting into that.

So, as we take --

MR. WALLIS: Do you know what the vendors do when they do this 59 runs using statistical stuff?

They put in uncertainty and all the parameters. They know how to put it in, so that is the way that things are going in the use of vendor codes. It ought to be the way to go with your code.

MR. BAJOREK: Well, that's pretty much where we are headed. I think that we have a bit of a tougher job to do because we need to get this code to look at a much broader situation.

When we did the code for TRAC development for Westinghouse, we only had to look at PWR's, and we focused on three and four loop PWR's. By the time it took us to freeze the code where we thought it was doing a good job, to getting something with all of the bugs out so that we and the staff were satisfied with the assessments, that took another three or four years. Now, that's a very small subset of plants. We're trying to do this four a three, four loop PWR's and BWR's and any other variations.

MR. WALLIS: Your model improvement you talk about here is all in the constitiative of equations. It's not in the T's and momentum equations and all that stuff, is it? Why don't you put some effort into that?

We know the representation of multi-junction nodes is very poor so far, but we don't have any measures of its uncertainty. It would seem impossible to put it in then. You've got some model. Put some coefficients in it or something and try to evaluate their range against some data.

Say yes, we've got a model for a T, which is pretty crude, and does the momentum go this way or that way. Well, it's got to have some range. We've got a coefficient which we can compare with data, and then we can use that in our uncertainty analysis, not just that the constitiate level of correlations, but more back of the fundamental bits that go into the balances in the code.

MR. BAJOREK: I think that's a good idea. How would I use that in a full scale application, though, where I have lots of T's?

MR. WALLIS: Well, if you have a momentum equation, which you know, you can put in some distribution coefficients or something for the averaging and say that, you know, you know there are certain situations where it flows around the bend and the liquids are all thrown to one side and so on, where the averaging is not going to be very good across the section. Maybe you're off by a factor of two.

Okay, well, is the data that shows what

the coefficient should be to get a better momentum balance? Can you put that in your uncertainty studies? You know, think about that.

MR. BAJOREK: Okay.

MR. WALLIS: You know, it's not just that the correlation level, but you need to look at the uncertainty.

MR. RANSOM: I think along those lines what you'd like to get to is once you have put those parameters in, then you'll do like 59 calculations and take the temperature traces and what the bound of that is, and you've got 95/95 certainty in terms of you have bounded.

MR. WALLIS: Then you can answer the critics then. You can say that we've made this momentum model and these are the uncertainties in it. We actually put those uncertainties in the code, and we show that for this application, it matters or doesn't matter.

MR. BAJOREK: Okay. Something to think about.

Over the course of 2003, we're starting to get into the point where we're going to start assessing the code in order to try to get some of these biases, uncertainties, parameters, to help to guide us in future model development. I would claim at this point we've done work in UPTF looking at ECC bypass.

A year ago, we had done some work for taking a look at the break flow model, trying to characterize its accuracy, looking at ECC bypass. Joe Kelly is going to spend the rest of the morning talking about work on the reflood model. We start looking at those processes which are highly ranked in most parts.

What we're doing is we're trying to go after those first because we think those are the ones that may have the largest uncertainty in BWP and BWR application. We've done some work on those. We've also been making use of some of the RBHT in order to address level swell and heat up at low pressures, as may be important for AB1000 application.

We're just getting to the point where we're getting TRACE to start doing some of what I would consider a traditional small break assessments that would normally have been left to RELAP.

An example on how we're changing the assessment in the approach. Let me use ECC bypass as an example. If I go back to the developmental assessment manual, you'll find one case in there to look at ECC bypass, which is important because it really sets the stage for reflood and the PCT's and the PWR that determine as you get closer to the 2200 limit. This is really your major period for energy removal from the core.

Right now in the developmental assessment, you'll find a single case in there. UPTF test 6, run 133. We're not going to depend on that single case, and I'll show you why you don't want to. We're expanding that to look at the other cases in test 6, which is a relatively simple thing to do once you have these scripts set up, and it's just a matter of changing the input deck and gathering the experimental data from which to do the comparison.

MR. WALLIS: We have had some concern with the way that NRR lets the vendors sometimes do one assessment rather than a whole lot of assessments. If they really wanted to show that their code is good, they should do a whole patch of assessments.

MR. BAJOREK: Traditionally what the vendors have done is this set right here, and partly because of that concern, these tests look at uniform injection around the downcomer. That's fine for a lot of cases, but it's not for all plants because there are some cases, then you look at their single failure, they have the asymmetric injection around the downcomer.

That can behave significantly different than this type of an injection pattern. That's seen here. AP600. AP1000 has direct vessel injection, okay. You need to look at a case like this, test 21 where the conditions might be quite a bit different.

But, you look at the assessment for the code, they're relying primarily on test 6. So, what we've done is we've taken the model for UPTF. We've tried to use a nodalization that eventually will be preserved, in this case a PWR, preserving the axial noding and the radial noding. In this case, we've picked one where we have eight sectors, so each of the hot legs and cold legs can be isolated into a separate region.

Just by way of reference, UPTF was run by injecting steam into the central region where the coil would -- the steam would go down through the lower plenum up the downcomer, and out through a broken loop, sweeping liquid that might be injected through any of the three cold legs.

Okay, our approach now is to, let's say now, we've missed part of it. This is UPTF test 133. This was the previous developmental assessment. It shows the core steam flow. Subcooling, when it gets up to its maximum pressure, is about 36 degrees. Think of this as a test where the water is coming in essentially saturated, most of it.

Figures of merit that we've defined for this, okay, the ones of most importance, upper plenum pressure, lower plenum water level. We want to see when, and what's the net delivery to the lower plenum.

Is the water going to the lower plenum out the break, or is it collecting in the cold leg, which is possibly in a one dimensional pipe which represents those. So, we define figures of merit. We look at a lot of other things, but these are the ones which we feel are the most important. We made comparisons then to the upper plenum pressure, to the data.

The data are the X's. The upper plenum pressure is the blue curve in this case. It does a reasonable job for this run. This tells us something about the break, where the water is collecting, and the other one, which is of most importance, is the lower plenum water level.

MR. WALLIS: Which curve is which here in all of these?

MR. BAJOREK: Yes. One code, now, okay? We're only looking at TRACE. This top one shows --

MR. WALLIS: More than just one data and one curve?

MR. BAJOREK: In this one, there are pressures at two locations.

MR. WALLIS: There's a TRAC and a TRACE, isn't there? It's hard for me to tell.

MR. BAJOREK: They're both TRACE.

MR. WALLIS: Both TRACE.

MR. BAJOREK: They're both TRACE.

MR. WALLIS: Just TRAC-M. TRAC-M is a trace then?

MR. BAJOREK: TRAC-M is TRACE.

MR. WALLIS: Okay.

MR. BAJOREK: Upper plenum pressure, the blue curve with the circles. E, experimental data are the X's.

MR. WALLIS: So what's the red curve?

MR. BAJOREK: The red curve is the pressure in the downcomer.

MR. WALLIS: Oh, it's not the same place?

MR. BAJOREK: So you want to compare the blue curve to the X's, not the red one.

MR. WALLIS: Okay.

MR. BAJOREK: Okay. This just shows the boundary conditions, the core steam flow is this.

This was the loop flows with this. That's just simply the boundary condition, so there's no comparison there. There's no comparison here either, but this is the total break flow.

This is the steam flow going out the break, so the difference is the liquid that is left out the break. This blue one, this is the net delivery when that flow rate to the lower plenum.

MR. WALLIS: This is all just data?

MR. BAJOREK: This is coming out of the code. There's no data here. There's no data here, but this gives us an indication of what the code -- but the most importance in terms of comparisons to the data, the pressure gives us an indication of the condensation rates.

The most important one, the net delivery to the lower plenum, the data, or the X's, comes out and tops out with the --

MR. WALLIS: That's the cumulative amount of water delivered?

MR. BAJOREK: Yes. The code here in the black, those are the most important. We take that transient. We also evaluate it to get the lower plenum filling rate, basically this blue curve, okay? We get this -- we also have information from the experiment to tell us what that rate was, and it's the slope of the level there, and the condensation efficiency.

Now, life would be just great if we just stayed with UPTF tests, run 133. It's a great job. There's not sense looking at any other cases. This is the result that you would get --

MR. WALLIS: Something's been tuned here.

MR. BAJOREK: Well, either tuned or you're very lucky, but because we are now able to run a lot more cases, we go through the same thing for others, the same figures of merit, the same comparisons, breaking them down. This is the calculated delivery to the lower plenum versus the measured delivery to the lower plenum, okay?

MR. WALLIS: The rate of flow at some time or other?

MR. BAJOREK: Yes, it's the net, the average rate over an evaluation period. There's only a certain period of time where it would dumped, that you would want to make that same time in there.

MR. RANSOM: So it wasn't related to the previous graph. Isn't that the one where you're showing the delivery as a function of time?

MR. BAJOREK: Yes.

MR. RANSOM: In your previous slide, which did not agree all that well, and yet the --

MR. BAJOREK: Well, wait a second.

MR. RANSOM: You're saying the integrated value as well?

MR. BAJOREK: What you're looking through -- well, basically you're getting this blue curve by looking at the slope of the black one. This is what's collecting, the rate at which it's coming in is by this squiggle. There's not data on here.

MR. RANSOM: Right. The bottom one what, is the integrated value?

MR. BAJOREK: The bottom is the collapsed water level in the lower plenum. It's sitting there at zero for awhile. All of a sudden, water starts to dump. It fills up and reached a --

MR. RANSOM: And the squares, though, are the data, right?

MR. BAJOREK: The X's are the data.

MR. RANSOM: And you're showing substantial disagreement, but yet on the other slide, you're showing exact.

MR. WALLIS: I don't see how you can get exact 1000 because the blue curve doesn't give exact 1000. The blue curve average is less than 1000. The third figure down, the bright blue curve, who's going to say that's exactly 1000? This is 1000 here, right? It's doesn't look to me as if it's exactly 1000. That blue curve, the third one down.

MR. BAJOREK: It looks like it should be a little bit under a thousand. I'm going between the two vertical lines.

MR. WALLIS: Average between the two vertical lines, less than a thousand.

MR. BAJOREK: It should be less than a thousand.

MR. RANSOM: Maybe I'm missing something. Is this just the measured rate at the end of the graph?

MR. BAJOREK: It's throughout the evaluation period.

MR. RANSOM: Throughout the entire evaluation period?

MR. BAJOREK: No, we focused on the times when the NPR associates went through and did evaluations and EPTF tests, and they defined some of the evaluation periods that was used in a lot of the 2D3D.

MR. RANSOM: So what is the period that this corresponds to?

MR. BAJOREK: Between these, approximately these two, these two lines. It doesn't make any -- you don't really care about what goes on very early in time because your steam flow is ramping up, okay, and you're starting to inject over in this period. Nothing is getting to the lower plenum. Then there's a period over which you're starting to fill the lower plenum, and then after which it's just basically full.

Okay, so we're focusing on that, when does it start. Once it starts, does the code throw more liquid to the lower plenum than what the data was showing, or substantially less?

I'll go back and check the numbers because that does look a little bit higher than what I would get out of this blue curve. It may be the way it's plotted here, the way the squares shifted up, but when we go through this evaluation that has been used in the past for UPTF, it would tell us that test 133 comes out pretty good.

Now, we're not going to just stay with 133 because we realize there are a lot of other situations. Steam flow could be different. Pressure could be different. Injection patterns could be different. So, we've gone through now, and we've done a series of tests.

Test 6, the traditional vendor calculations here, okay, and do that evaluation for all of them. In general, not too bad. We get more problems, however, when we start to take a look at test 7, asymmetric high patterns.

Now the code is having a bit more difficult time, and we started to look at this, wondering why are we starting to have differences? These aren't too bad, but there's a few cases, these in particular, where the data was showing delivery to the lower plenum, and the code wasn't doing anything near as well.

MR. WALLIS: Was the code a one dimensional code? It doesn't model asymmetry?

MR. BAJOREK: Well, there's cross flow within the downcomer. It's like a 2-D representation.

MR. WALLIS: It's a 2-D representation of the downcomer, so you would catch some asymmetry?

MR. BAJOREK: Yes.

MR. WALLIS: If water were all pouring down one side and the steam going up the other?

MR. BAJOREK: Yes.

MR. FORD: Steve, if you just go back to the previous one.

MR. BAJOREK: Sure.

MR. FORD: There's obviously a distribution of the ratio of the observed to calculated values from that database. If you did the calculations for one of the TRAC models, would you get the same distribution, or would it be offset from one? The distribution of the ratio calculated to observe, would it be offset from one?

MR. BAJOREK: I would suspect that TRAC would do something similar, okay, but the original developmental basis didn't look at any other cases. To my knowledge, I don't think anyone has used any of the codes to take a look at test 7. At least I haven't seen it.

MR. FORD: Because that would be a useful metric for determining whether your TRACE model has improved over the others. That is, what is the mean value of the observed to calculated value, and the variance in that distribution. If you've squashed up the variance and moved it to one, then you're doing great.

MR. BAJOREK: Yes, if we get everything on that line, we're good. Now, what we're getting to now is coming up with that metric. We can take these and get some type of an average bias and uncertainty for this distribution. Of course, we can identify which cases are doing worse, and we want to focus why. We want to correct that, but from this distribution for the baseline model now, we can assign a metric to this, as bias in water delivery and an uncertainty about that.

I wish we had that for TRAC-P or for some of the other codes, but no one has run those, and since this is what we're going to use for future development, I don't think it's worth instituting another project to look at a code that we aren't going to be using anymore.

MR. FORD: Obviously I've never done correlations for thermal hydraulics problems. I have done it for others. The interesting thing is to look at the uncertainty of your measured values. In two of your previous cases, you showed that the measured values is a huge uncertainty, which actually swamps out any of the uncertainty in your models.

MR. BAJOREK: Yes.

MR. FORD: I don't know whether that would be planned.

MR. BAJOREK: I haven't done it to this, but in the past, I remember we had done that for reflood heat transfer. At some point, you realize you can't make the code any better than the scatter in the data.

MR. FORD: Dead right.

MR. BAJOREK: So at some point, when the bounds of this, if I put arab bands on either side of this, and they start to approach the experimental data, my job is done until I get better experimental data where I can run a better test.

So, we started to focus on these two in particular. We weren't getting anywhere near the delivery, and we were realizing, well, these were at slightly different pressures. We were seeing more subcooling going on in these. Well, that's pretty important now for something like test 21 where I'm injecting directly to the downcomer. We were seeing some additional variation, okay. A couple of tests were delivering a lot more and a couple more where we aren't getting anything.

MR. FORD: That one way over in the left-hand side there, that one, is that experimental uncertainty, or you moved that all the way over?

MR. BAJOREK: No, this one probably should be replaced with a question mark at this point, because as we looked --

MR. FORD: Because of the model or the data?

MR. BAJOREK: I think it's something in the model. This is one where I think we really want to go back and look at the input. This one is just behaving so strangely in comparison so something else.

MR. FORD: Down there, too.

MR. BAJOREK: Well, this one --

MR. WALLIS: Down there, yes.

MR. BAJOREK: In this one, this points out a code deficiency. Once we went through these and we got to test 5, which is the same as test 6, uniform injection but very high subcooling. Now it confirmed the deficiency that we were suspecting when we looked at the condensation efficiency of these. I wish I had that prepared because when we went through all of these, rather than getting a condensation efficiency on the order of .8, which is typical for a lot of those, we were getting condensation efficiencies on the order of .95 to one.

We were underpredicting the pressures in many of these, and grossly missing it over here. We delve into this further to find that the root cause of this is the way that TRACE is behaving and generating the interfacial area at the junction between the injection point and the downcomer. It's immediately taking all of the liquid in these cases when it's subcooled, breaking it into a fairly fine mist of droplets, artificially enhancing the condensation. What this is tending to do is it's trying to increasing the vapor flow to the region, and that's sweeping everything out.

So, we look at this for saturated conditions, and we would conclude at this point the ACC bypass model, probably doing a reasonable job. We'll cast that in terms of a metric, in terms of condensation efficiency and lower plenum delivery. That will be a baseline number so that as we make changes, revisions to interfacial area, drag, whatnot, as it affects the downcomer, will be able to track what it does.

MR. WALLIS: This is a very difficult problem. I remember Creari had a whole lot of probes in the downcomer, and they measured the flow pattern, and it jumps all over the place.

MR. BAJOREK: Yes. Even at UTPF, you see a chugging. There is a chugging, and there's a preferential delivery on the opposite side of the downcomer, but you should be getting, and the code should be predicting more bypass consistent with the subcooling.

So, we've noted this as a deficiency.

Future efforts are now going to be correcting this. I think the important thing to point out of this is because the code has been consolidated and we've developed some of these tools that allow us to do additional cases, we're now in a better situation where we can run enough cases to get a bias and uncertainty, and we can run enough cases over a wider range of conditions where we can identify problems that you would not have seen in the prior developmental assessment.

I'm not going to go into this too much. How am I doing on time?

MR. WALLIS: I wonder if when you do all of this, you're doing to learn that some of the vendor codes maybe need to be compared with more data, and the NRC has accepted comparisons with one test.

MR. FORD: I must admit, I'm astounded the way that the GE's and the Westinghouses in this little apex, to get away with just one test. It's unbelievable.

MR. BAJOREK: For AP600, we did also do AP-21, the direct vessel injection. I'm not aware of anyone doing the test 7.

MR. WALLIS: This is good, though. It means that you're being thorough enough to challenge some of the comparisons that we've made before, and you're learning.

MR. BAJOREK: Yes, and we've picked out cases in the matrix. We haven't picked out the cases which are fairly kind. The test 7 is a difficult one because of the asymmetry. Nodalization and a number of models come into play. So, we are picking cases which are truly going to challenge the code and the modeling.

Likewise, reflood separate effects cases. This is in 2003 and 2004. Now that we have the interim reflood model, we want to really start to do a lot more assessment here.

MR. WALLIS: Is this what Joe Kelly is going to tell us about?

MR. BAJOREK: Joe is going to talk about the models. He's going to show you some results, current and with the interim reflood model from 31504, which is a one-inch per second -- one inch per second will be greater?

MR. KELLY: Yes.

MR. BAJOREK: One inch per second case. 31701, Which is 6.1 inch per second case. Both these are run at 40 psi. They've been used traditionally for TRAC-M and a lot of codes developmental assessment. TRAC-M has also used this one.

Well, in a similar fashion, we don't want to rely on tests at just two reflood rates and one pressure because we know there is a large pressure effect, especially when we need to analyze PWR's where the containment pressure might be 20 psi.

So, we're expanding the matrix to include these. We'll keep these. These will hang around, but we'll look at reflood rates, which are lower than one inch a second, and give us peak cladding temperatures greater than 2200 degrees and so is a few thermocouples over 2200. Different pressures, variable reflood rates, and we won't just focus on one particular facility.

MR. WALLIS: Are you going to look at all of the evidence and again a selected set?

MR. BAJOREK: Selected -- I'm sorry, selected set of evidence as to?

MR. WALLIS: Well, previously you looked at three FLECHT-SEASET's. Now you're looking at whatever it is here, a 14 or something. How many tests were there? If there were 100, why not all of them? I don't know, are you looking at all of the tests?

MR. BAJOREK: Well, you wouldn't want to

do all of them.

MR. WALLIS: Why not?

MR. BAJOREK: Because more than two rods burned out in the FLECHT-SEASET.

MR. WALLIS: Okay, if there was something wrong with the test, you can disregard it, but again, that's okay, but are you essentially looking at all of the valid tests?

MR. BAJOREK: I think we have most of the valid tests.

MR. WALLIS: Okay.

MR. BAJOREK: We certainly have the ones that will give us a way to examine reflood rate, pressure, inlet subcooling, and as we take those similar situations to other facilities, we'll see the effect of hour shape, okay, which changes the overall hydraulics in the bundle, pitch to diameter ratio here in the FLECHT 98 rod bundle. We will be using the RBHT. We're getting that data now. We have a model set up.

We haven't selected which tests or how many, but we would expect to put somewhere between a half a dozen --

MR. WALLIS: RBHT is what's going on at Penn States, is that it?

MR. BAJOREK: Yes, that's the Penn State bundle. We'll be getting a number of those -- we're getting that data right now into the format that we can use, but rather than relying on one, two, or three tests, hopefully within a year, year and a-half, we've got to collect data for all of these.

We're looking at the upwards of 25 to 30 different reflood tests from which we'll do similar evaluations to get bias and uncertainties in transfer coefficients, droplet size, steam temperatures, whatever parameters we can glean out of the data.

MR. WALLIS: Now, for regulatory purposes, is this because reflood is the process which determined the peak clad temperature, which is a regulatory measure, and there's a likelihood that PWR's might ask for say power upgrades or something, which would challenge this peak clad temperature, and therefore, the Agency needs more certainty about what that peak clad temperature is going to be? Am I just rambling, or am I talking sense here?

MR. BAJOREK: Well, I think you're making sense because as plants are changing today, we're seeing two things coming up over the horizon. One are power upgrades, trying to get as much reflood out of the core.

MR. WALLIS: Reflood is the key process.

MR. BAJOREK: Right now in most PWR's, it's your reflood and peak cladding temperature that is the lumening factor.

MR. WALLIS: So it's a regulatory need on which you're hanging all this work?

MR. BAJOREK: Yes. It may increase if we start to go to risk informed regulation. One possibility is with performance based fuel, that limit for peak cladding temperature may increase to 2300. It may change to something else. A temperature in some type of an oxidation criteria.

I won't go in, but they're looking at a number of different possibilities here, but it may translate into the core being upgraded, operating so that in a hypothetical accident, it's there at a higher temperature for a longer period of time. Uncertainties in your heat transfer coefficients now are going to be magnified.

We saw some of that in upgradings that we did awhile back when best estimate was first applied. The transients became sufficiently long, and boiling in the downcomer started to become a concern. Transients became longer, okay?

We'd expect to see other types of changes to the transient, perhaps making them longer so uncertainties propagated over a longer period of time have more of an impact on peak cladding temperature or clad oxidation or whatever regulatory limit that is eventually set as the fuel becomes performance based and perhaps as the break size is also redefined.

So, in terms of assessment that we --

MR. WALLIS: What you're saying is compatible with what someone from NRR would say, in terms of the need?

MR. BAJOREK: I've been warned about speaking for NRR.

MR. WALLIS: I mean, it would be good if both sides -- I mean, you must be talking to them, and presumably what you're saying takes into account input from those guys. Yes?

MR. ROSENTHAL: Rosenthal Research, NRR and RES are jointly participating in the efforts to risk inform 5046. There's a working group of NRR and RES people. NRR is putting forward documents that show a rulemaking -- I don't want to get out a head of what's in the concurrence -- rulemaking related stuff.

MR. WALLIS: All right.

MR. ROSENTHAL: I think that we're charged with the technical basis so that in providing information to NRR. So, for example, in the RES branch operating plant, the implication of what would happen if you changed the break size to let's say eight inches, changed the -- allow a power outbreak based on an eight-inch break in the current regulatory criteria, what would then happen to beyond eight-inch breaks, and would that be okay, and what might we be seeing. That kind of work is in our operating plant.

So, I think we're reasonably well integrated, but the code running to support this rulemaking effort will be done in our branch.

MR. BAJOREK: Yes.

MR. WALLIS: Thank you. We're going to see this. In our other activities, we're going to see results of this work?

MR. BAJOREK: Yes.

MR. WALLIS: Thank you.

MR. BAJOREK: I think in terms of the focus of this work in comparison to what NRR is doing right now, I think the focus right now is on the advance plants. There's so much work and so much need to evaluate what's going on there, that some of this isn't quite int he forefront.

MR. WALLIS: Is that really so? I mean, it seems to me that power upgrades for PWR's are really going to happen, or maybe really going to happen. Advance plants may or may not really happen. So, I'd be more concerned about it proving a power upgrade for PWR's that are there now, if there was too much uncertainty about peak clad temperature and about what might happen years from now when a more advanced reactor is built.

MR. BAJOREK: I think completing this is going to help give us a tool by which we might be able to do audit calculations. I don't think we're there yet, but with the interim reflood model, assessment that we're planning here in the near term basis that I'd mentioned on the previous overhead, we've also initiated work to look at CCTF and SCTF.

In some cases, these are run in a separate effects type of mode. In other cases, we have gravity reflood in the case of CCTF and at least one of the SCTF. That's not on here.

As we started to do in the UPTF example, we want to try to characterize the accuracy of the code in terms of a bias and uncertainty on those parameters which are of most interest, not peak cladding temperature or necessarily quench time, but things like the heat transfer coefficients, the carry-over fraction that we get in these reflood tests. The void fraction or mass distribution that we see in the bundles. Steam temperatures, other parameters that we can relate to models which are actively used in the code for that.

Now that we have SNAP up to speed, we're also starting on the small break integral tests. We have people working on ROSA, modeling SB-CL-05, and would anticipate including a number of additional cases where I think this is like an equivalent of about a four-inch cold leg break, and we would be looking at various break sizes and eventually expanding this to look at other parameters that were found to have large effects in the ROSA facility.

BETHSY, it's unfortunate there isn't more experimental data readily available to that because it was a well instrumented facility. It's I believe the only or one of two which has a full integral facility layout, keeping all of the loops, all three of the loops rather than lumping them compared to other tests, or modeling ISP-27, which was a small break in which they had shut off the high head injection system in order to get a peak cladding temperature.

We're starting some of the semiscale tests. We're also using the APEX data that we're just getting for the AP1000 type tests. We're setting up a model, and we'll be running at least first the double ended DVI line cases. We've also got a couple of cold leg small breaks that we're putting in that.

We have run, and we set up the AV scripts to do THETIS as well as the Oak Ridge case, which I showed on the overhead earlier so that as we start to make changes to the interfacial drag model in the core, we're going to be able to rerun those, and we anticipate using THETIS and some of the RBHT interfacial drag tests to help us answer the question that we've been getting out of AP1000, is TRACE and RELAP. We're using RELAP for this assessment as well, over predicting the amount of level swell if we were to get uncovery in a hypothetical accident in AP1000.

MR. RANSOM: Maybe I missed something, but how is the RBHT test series going to provide interfacial drag?

MR. BAJOREK: This was a series -- we had three passes in which those tests were being run. In 2001 and 2002, they ran a series of traditional transient reflood tests. Heated the bundle up, reflooded from the bottoms and various flooding rates, different pressures. So, we have a set of data that looks -- it's comparable to FLECHT and some of that, those types of facilities.

Earlier this year, in order to get some better information on rod bundle interfacial drag, we ran a series of tests in which the bundle was essentially flooded. It was run at different powers, different pressures, at relatively low powers, so that the exit void fraction was on the order of .4 or .5. Joe, do you remember?

MR. KELLY: Maybe a little higher.

MR. BAJOREK: Maybe a little bit higher.

MR. KELLY: But they're basically level swell tests.

MR. BAJOREK: Yes.

MR. RANSOM: But the interfacial drag is obtained by just inference, I guess? You try to simulate it?

MR. BAJOREK: Yes.

MR. RANSOM: See what interfacial drag?

MR. WALLIS: Try to predict the void fraction, presumably.

MR. BAJOREK: What?

MR. WALLIS: You try to predict the void fraction?

MR. BAJOREK: Right, yes. What's nice on this compared to many of the other tests, is we've got a very detailed pattern of DP cells. Three inches in the central part of the bundle, and the test matrix was adjusted to try to get a variation in void fraction to maximize during where those DP cells had their maximum sensitivity and smallest span and getting some unique data that's going to help us look at interfacial drag. This is going to help Joe come up with a better reflood model in the future.

They're also very useful because the lower pressure. The Oak Ridge data and most of the THETIS data was run at high pressures. So, there's been a bit of a crying need for low pressure interfacial drag, and we're very interested in simulating some of these tests. So, we sort of move this up.

We're also doing this, these simulations with RELAP so that at some future meeting when we're asked well, even though RELAP has a low collapse level, is it really flossed up to the top of the core, we're going to have a better -- we're going to be better able to answer that.

MR. BANERJEE: So RELAP has just an interfacial drag without any sort of drip flux model at all in it?

MR. BAJOREK: No, I think it has a drift flux model in it.

MR. RANSOM: That's my understanding.

It's drift flux based.

MR. BANERJEE: There's a simple analytical solution to this level swell, using a drift flux model.

MR. WALLIS: Steve, I think we're a little bit behind in time. I suggest this is an important slide. You talk about this one. Then you jump to the conclusions.

MR. BAJOREK: Let me do that.

MR. WALLIS: Because we're just going to look at curves otherwise.

MR. BAJOREK: Right, but what I wanted to spend just a few minutes talking about is where we're going in the long run with all of this. We're set up now. We've automated a lot of this. We have a baseline code.

As I mentioned in the examples for UPTF and with the reflood, we're going to get now to the point where for various model packages and various models in the code, what would determine a bias and an uncertainty. Now, we haven't exactly decided what parameters those were going to be.

I mentioned a few of those, but as we're going through future assessments and model development, we'll define what those parameters are.

We'll do the assessments, be it for reflood or level swell, condensation, break model, come up with a bias, and use those to come up with a multiplier, a term, something that can be varied within the code so that we can sample these in full scale simulations of the PWR or BWR and the results of that in terms of what we'll call the regulatory parameters of interest, PCT or ECR.

We'll be able to go from biases in certain models to how they are varied within the code to a statistical distribution at full scale, okay, to determine --

MR. WALLIS: Like some of the vendors are doing?

MR. BAJOREK: Similar to Westinghouse took a response surface technique. Framatome has done something very similar to this. The details of how you combine these and what cases you run, whether it's 59 or 114 and how you are -- that's something that we are going to address.

It is in the future because our concern right now is getting the code accurate, quantified, and then in the position so perhaps a year or two from now, then we can start talking about well, does this really correct this bias to get this right, and then when you propagate its effect at full scale, what does it do out here. Then you have I think a much stronger basis then for saying well, this particular model doesn't really affect your situation, or it has a large effect, depending on what your transient.

MR. FORD: Steve, I'm convinced you've got TRACE now in a working mode, and you've got all this data that's been created. Why is it going to take you two years to go through this sort of evaluation process?

MR. BAJOREK: Well, I would think it would take about a year to go through and develop all of these and perhaps another year to really --

MR. FORD: Is it not just a question of plugging in the inputs of your model and comparing the output with your data, or am I oversimplifying?

MR. KELLY: It's a little bit more complicated, like when he talks about looking at heat transfer coefficients, you have to window it over the transient to figure out when you're in that regime and do the comparison. Otherwise, it becomes meaningless.

Just as an example, when the vendors do this kind of thing as part of their, say, large break best estimates, they're talking about 20 to 30 staff years to do this work. Of course, we don't have anything like those kind of resources.

MR. BAJOREK: I'm just going back to the days when we developed the best estimate methodology at Westinghouse, to go from a code that we thought was frozen and pretty good to a time when we got some begrudging smiles from the staff, and in an SER, it took on the order of about six or seven years.

I think I mentioned that what we're trying to do with TRACE is bigger than that because we have more plants that we have to do it. I guess it could sort of happen in two years if we got to the point where we're (inaudible).

I guess we go on. As we get these biases, we'll know what models are being impacted. That's going to guide us in our model development and as we go along, if we need to know what's the effect of disburse flow film boiling heat transfer coefficient on a particular application, we should be able to get that. We'll be able to get individual components.

To wrap it up into a nice, statistical methodology that we're convinced is the right thing for the staff and is independent from what the vendors have produced, that's going to take a little bit.

MR. WALLIS: If the rationale is very straightforward. You take the data, you make some comparisons with something. You calculate bias and sigma. That can be automated. You don't have to have someone looking at all these curves and all that. So, that really ought to proceed pretty quickly, one would think. Someone's got to manage it and check that things are happening, but once you know what you're going to do, you show the procedure for doing it, it shouldn't require all that time.

MR. FORD: I thought the whole idea of this TRACE development was it was relatively simple. Everything was in one box, if you like, and maybe I'm oversimplifying what I thought it was.

MR. BAJOREK: Well, we would love to speed it up, but I think it's a matter of resources. I think in terms of the development team, speaking for everybody, this is the fun part here. It's getting to this and finding out well, why doesn't this particular model work correctly and fixing it. That's the fun engineering.

I think we'd love to spend more time on that, but we do have repeating priorities.

MR. WALLIS: Well, the fun is in the answer, not in the process. The real fun or the real achievement or the real bang for everything is getting the answer, not just in doing the work.

MR. BAJOREK: You won't believe this until you're convinced of this. I think in terms of competing our priorities, we've got AP1000, ESPWR, ACR --

MR. WALLIS: We know these are coming, so we're very impatient waiting for this stuff.

MR. BANERJEE: This will be the first code that really can be used with AP1000, will be taking into account the low pressure, reflood level swell, all this sort of stuff. There's no other code that really does that at the moment, does it?

TRAC-P doesn't do that, and RELAP doesn't quite get the right --

MR. BAJOREK: Nobody's going through this for any small break applications, be it AP1000 or even a conventional plant. This idea or this statistical distribution has only been done for large break applications at this point.

MR. BANERJEE: But you're doing it also for small break.

MR. BAJOREK: We need to get the small break processes into this as well.

MR. BANERJEE: And the ADS and all this sort of stuff? If it's going to be applicable to AP1000, it must have that, right?

MR. BAJOREK: Yes.

MR. BANERJEE: Does this preclude that ADS phase, ADS-4?

MR. BAJOREK: I think that's in there. I'm just thinking in terms of priorities, getting models that affect all plants versus models and uncertainty for one particular unit that isn't operating yet. You're right. I mean, uncertainty is in the ADS performance and CMT. All those would have to be incorporated into that, and I think in a statistical methodology, okay, you would have to incorporate all of these plus any of those unique features.

You know, the performance of the ADS or other components would have to be, those uncertainties would have to be incorporated in this, as would other uncertainties associated with the plant. Has your power shape changed? What's the water temperature at any particular time? What's the burn-up?

MR. BANERJEE: The priority is existing plants with the focus on upgrades? What?

MR. BAJOREK: Yes. I would say that that's --

MR. BANERJEE: I mean, I'm just trying to find a rationale for how you're going to organize this.

MR. ROSENTHAL: Steve, if I could, I think the Office's priority is to risk inform 5046.

MR. WALLIS: What's that?

MR. ROSENTHAL: Of the ECCS. That will include LOCA, break size redefinition, and all the ramifications thereof, and that comes direct from mission interests and guidance.

In order to do that, I think that we recognized that maybe in prior decades, thermal-hydraulics, this kind of work was large break LOCA-centric, and we're trying to get more balance in considering small break stuff as opposed to large break. So, that would be the priority, what kind of work do you need to do to risk inform 5046.

Then power outbreaks, although let me remind you that power outbreaks have mostly been -- the big power outbreaks are boiling water reactors, and the little stuff is the PWR's, while PWR's tend to be LOCA limited, and large boilers are not LOCA limited. Then the new plants, in part, were using (inaudible), and that's a reality.

MR. BANERJEE: But this will work for boiling water reactors, too.

MR. BAJOREK: Yes. Okay, just to

summarized and to conclude, we feel at this point consolidating is behind us, and now is the time where we're going to start focusing on model development, accuracy improvement, quantification of how well the code is doing so that at a future time, we can start using this as a part of a statistical methodology for conventional plants and other type, and newer plants.

As we've been going through these assessments, again I just want to mention again that we're setting these up so that they're automated so that much of the real grunt work in doing these work, generating these figures, it's going to be more automated. We're still going to have to go back and look at these very carefully, but we think we're in a position now where we can look at a broader number of assessments, and the benefit of that is going to be able to make code improvements faster and hopefully come up with models which are more accurate.

MR. WALLIS: Thank you very much. Are there other questions from the members of the subcommittee?

MR. RANSOM: Could you remind me what ECR stands for

MR. BAJOREK: Equivalent cladding reacted.

MR. RANSOM: Reaction?

MR. BAJOREK: Reacted, clad oxidation.

MR. BANERJEE: Do you have a list or a table of the tests that you are going to be using as the basis for these assessments?

MR. BAJOREK: This is the one that I can get copies of this.

MR. BANERJEE: Is it in here?

MR. BAJOREK: No, it's not in there, but --

MR. BANERJEE: That would be helpful.

MR. BAJOREK: I can get you a copy. This is more or less the tests that we've either run or are running or plan to. It's just a working copy that I --

MR. BANERJEE: That would be good, if you could supply that.

MR. WALLIS: Are there any more questions or requests?

I'd like to thank you, Steve, for being very informative and for having a good interaction with the subcommittee.

MR. WALLIS: Thank you.

MR. WALLIS: As always, we could always spend more time.

MR. BAJOREK: Well, one thing, too, what

we would like to get out of this subcommittee is where would you like to focus attention in future meetings. I mean, there's an awful lot of stuff in any of these simulations. Joe is going to start with the reflood model. We'll go into that, but in the future, if you want to see us go into a condensation model, more into the bypass problem or you know, take a particular transient and tear it apart, we can do that.

MR. WALLIS: We are planning, I think, a series of three meetings, isn't it? Isn't this the first one?

MR. RANSOM: At least three.

MR. WALLIS: At least three meetings, and we can set up the agenda for the next meeting. I think it's going to involve going more deeply into certain aspects of TRACE. I hope we don't have to get some rehashing with some of the faults knew about in the other codes, which is still there.

Anyway, we are going to set up several meetings.

MR. WALLIS: Okay.

MR. BANERJEE: Just one thing, Steve. ROTH was pointing out that the OECD data on LOFT is more reliable. So, perhaps you should look at that as well as --

MR. BAJOREK: We've got a few of the OECD --

MR. BANERJEE: You have access to that, of course, don't you?

MR. BAJOREK: Yes, we have some of that.

MR. BANERJEE: Okay.

MR. BAJOREK: That was -- was it LB-1? That's in there. L2-5, L2-6 are in there. Those are good because they're higher temperature. I think LB-1 might have been the highest temperature one. That's in the test matrix.

A lot of driving the code consolidation is what was already out there and what they had used already for TRAC-P or TRAC-B. So, we kind of had to stay with that, but now that we have the baseline, now that sort of frees us to start looking at tests which are more interesting.

MR. BANERJEE: But also could be more accurate.

MR. BAJOREK: Well, that's true. I mean, rather than focusing on let's just semiscale, for example, they will make more use out of ROSA, which was a test that was run later on, has other instrumentation, has tests which are, you know, unique and give you information that you didn't get from those prior assessments.

MR. BANERJEE: Also, the series of I don't know how many LOFT runs were done, but one where, you know, the pump effects, the pump rundown was very important in LOFT, which is why you got this rewet.

MR. BAJOREK: Yes.

MR. BANERJEE: That was artificially cut off, so that you don't get this early drop. You might consider those, too.

MR. BAJOREK: I think we do have -- there's a pumps on or off. I think it might be L2-5 and 6. I can't remember the numbers, but those are in the matrix. I don't know if those were OECD tests or not.

MR. BANERJEE: That's why I want to look at the matrix and take a look and see where it is.

MR. BAJOREK: Let me take a look.

MR. BANERJEE: Okay.

MR. WALLIS: Okay. I think this is a very good time to take a break. We're running a little behind. I'm not sure if Joe Kelly is going to be able to go any faster, so we may be here a little after 5:00.

Anyway, we'll take a break now, from 10:30 to 10:45.

(Whereupon, the foregoing matter went off the record at 10:32 a.m. and went back on the record at 10:48 a.m.)

MR. WALLIS: Let's come back into session. We'll be hearing from Joe Kelly.

MR. KELLY: I'll be talking about the interim reflood model development for TRACE code. My colleague in this is Weidong Wang.

I'm going to divide the presentation basically into three parts with a brief introduction. I'm going to address two questions. First, why do we need a new reflood model? That's what TRAC was supposed to always be able to do to begin with. Second, why is it called an interim model?

To give an example of some preliminary results, what this is is the interim model is developed. It's running. I'm going to show some of these results and compare them to results with the RELAP code. For two cases, a low plating red case and a high plating red case. It's like one inch a second, six inches a second.

Then what I'm going to do is show you an example of how I developed the model for one particular heat transfer regime, and that's the inverted annular film boiling. I'm not going to describe all the input models because we'd be in a two-day meeting, just with that alone. Its reflood touches many different regimes and many different models within each regime.

MR. WALLIS: I wondered if you'd rehearsed your presentation of 43 slides and checked that it's only going to take 50 percent of the time. That's the rules, you know?

MR. KELLY: Right, so please feel free to cut me off if need be.

This is an example of an assessment case I did with TRAC-P actually several years ago, clad temperature versus time. There are three data curves, and a predicted TRAC temperature, which is obviously nowhere close. The reason it's nowhere close is there were very large oscillations in the calculations. Basically vapor explosions, they were throwing all of the liquid out of the rod bundle and FLECT-SEASET the upper plenum acts as a steam separator. So, once the LIFT was thrown up to the upper plenum, it's gone.

An example of those oscillations, this is vapor temperature versus time. The blue curves are measured basic temperatures, and you see the TRAC results, which is totally out here.

MR. WALLIS: Well, these temperatures represent something way beyond what's permissible, too.

MR. KELLY: Right. That calculation, there is no way you can use that model. That's why we needed something new. Now the question is why am I following this new model and interim model.

The reason is we needed something that would be reasonably accurate, but we needed it quickly, and that's that we can go ahead and do the large break LOCA assessment cases that Steve showed in the previous presentation, and hopefully be able to do some realistic auditing calculations to get to the housing.

Because we needed it quickly, we couldn't wait for the implementation of the droplet field which John Mahaffy is working on now, and likewise from analysis of the data from the NRC experiment at Penn State.

Consequently, we do plan to take the Penn State data, take the work that John's doing in the droplet field, and come up with a true best estimate reflood model, and that work is planned for the 05-06 time frame.

We'll be taking advantage of the droplet field and the RBHT data. We'll be implementing a grid spacer model. You saw some of the test data. You saw how important the grid spacers were, but also, and this is one of the deficiencies, and one of the things that makes it very hard to do, we'll need to put in some kind of subgrid resolution scheme and fluid solution.

What I'm not going to talk about here is the way we model the heat structure, and what I'm talking about is we call it a fine mesh rezoning model. It's been presented here before, but it's basically an adaptive grid scheme, and it's applied to the fuel rods in order to resolve the axial profiles at temperature and heat flux.

So, typically our hydrocells are in the order of a foot. We get down to heat transfer cells that are less than a millimeter because the entire transition blowing region is only about two centimeters long, and that's where all the big heat splash is.

MR. WALLIS: You've seen the results, though, from the RBHT?

MR. KELLY: Yes.

MR. WALLIS: And in developing this interim model, you can't ignore them. You're not going to develop an interim model which is incompatible with what's observed there because that's making trouble down the road.

MR. KELLY: That's true, but we don't yet have the RBHT data and electronic data.

MR. WALLIS: Yes, but you have seen the kind of results they're getting.

MR. KELLY: Right.

MR. WALLIS: You cannot ignore them when you're doing this interim model.

MR. KELLY: That's true, and I haven't. I won't talk about that today, but there is one -- I actually tailored some of the tests there to look at something that I thought was an uncertainty, but I wasn't able to have a reduced version of that data and use it in helping you do the model, which is unfortunate, but that's a timing thing.

MR. WALLIS: That's what's troubled us all along, is to do the experiment. The experiment has got to feed in as soon as possible in the models, not to wait for four or five years.

MR. KELLY: Well, I started this work more than a year ago, so it is a timing thing.

Okay, we have a model that works now, and I'm going to do some preliminary assessment on it.

It's the second part of the presentation.

FLECHT-SEASET forced flooding rate tests. FLECHT-SEASET is 161 rod bundle. It's full height, top cosign power profile, and we looked at two tests. They are both 40 psi and the flooding rate is one inch a second and six inches a second. Both of these cases are pretty high with subcool, not just in the traditional type subcoolings, which are more like water you get out of an accumulator rather than the water you get out of a lower plenum in Tsat.

Now, what is the --

MR. WALLIS: Do you ever get that kind of subcooling in the real world?

MR. KELLY: Only if the initial accumulator discharge. If you look at all of the FLECHT-SEASET cases which were run back in the 70's and 80's, most of them have these high inlet subcoolings, and that's what most of the code assessment has been against.

Now, I picked these cases because they've been used before. What we are going to do, as many of the subcooling cases as there are in FLECHT-SEASET, and one of the deficiencies we addressed in the RBHT program was we ran a number of cases with the low end on the subcooling, basically always paired. We got some dramatically different behavior.

MR. BANERJEE: What about the effect of the lower plenum itself? I mean, these tests didn't have anything, right?

MR. KELLY: Not at all. This is separate effects that help me with development for one specific model, reflood model in the core.

MR. BANERJEE: Right, now what did the SCFT and I don't remember --

MR. KELLY: CCTF was cylindrical core test facility.

MR. BANERJEE: Right.

MR. KELLY: SCTF is slab core test facility.

MR. BANERJEE: Right.

MR. KELLY: They both are reflood, so they don't do the blowdown in the ECCS bypass phase.

MR. BANERJEE: Right.

MR. KELLY: They both have approximately 2000 heater rods, so instead of something yeah big, you're talking about a pretty sizeable vessel here. So, you now have the possibility of two to three dimensional effects going on inside. Cylindrical core is exactly what it says. Cylindrical, the slab core, you take the same eight rod bundles, and you put them in a plane.

The idea is it's supposed to be a 2D slice from the centerline of the reactor core through the downcomer.

MR. BANERJEE: They have the downcomer and the plenum?

MR. KELLY: That's correct.

MR. KRESS: These are, both were full length, weren't they?

MR. KELLY: Yes, they're both full height.

MR. KRESS: They're both full height.

MR. BANERJEE: Now, those tests, were there effects which would be different, like due to the gravity effects that were oscillations?

MR. KELLY: They're core inlet oscillates.

MR. BANERJEE: Yes, oscillates.

MR. KELLY: And that can completely disrupt your model if it's sensitive to that. That's why we absolutely have to assess the model against those tests.

MR. BANERJEE: So you're developing a model using, the logic is use this to develop a model, but we know that there's going to be oscillations that bend that because that's what real life is.

MR. KELLY: Right.

MR. BANERJEE: And that these models must be robust then to oscillations.

MR. KELLY: And we'll find out.

MR. BANERJEE: Yes, because that affects carry-over and all sorts of things.

MR. KELLY: Oh, yes. So, I'm going to do these calculations twice, or with both TRAC and or TRACE and you'll have five. In TRACE, the input model has a 1D vessel, because it's only a 161 rod model. Twelve axial nodes in the heated length. I picked the length of those cells to match the DP cells so that I could do comparisons between the amount of water in a TRACE cell and what was measured in a DP cell. That gives you one put, or 30 centimeters.

Two heat structures. One heat structure model the actual electric heater rods, one for the bundle housing. The one thing I did that you should always ask is what is your graphics edit interval. We do these plots with the squiggly lines. How often are you pulling points from the code calculation in plotting?

So, I'm matching that with the test data. This is a two Kilohertz symbol.

MR. WALLIS: Otherwise you don't know what things that --

MR. KELLY: You don't know.

MR. WALLIS: Right.

MR. KRESS: Now, the grids, and A grids, are they following the middle of these 12 axial nodes?

MR. KELLY: In the interim model, there is no specific grid spacer model other than the pressure loss coefficient.

MR. KRESS: Okay.

MR. KELLY: I don't do drop shatterings.

MR. WALLIS: But we know that that can be important.

MR. KELLY: And I've developed those models before. I developed ones that are in COBRA TF or COBRA TRAC, but that was back in 1984 because the code has a droplet field, is one of the reasons that we're implementing the droplet field with TRACE.

MR. KRESS: Yes, okay.

MR. KELLY: RELAP5, the input model, is identical to the TRACE model, so that we could do an apples to apples comparison. They don't have a vessel, so we're using a pipe. The reason we used the vessel in TRACE is that's what the reflood model is implemented, and the first doesn't yet work for the 1-D component, so the pipes are heated --

MR. KRESS: Now, when you say pipe, does

that mean the heat is coming from the walls of the pipe?

MR. KELLY: No, it means that a pipe is just an axial stack of volumes and attributes it says on the pipes. I used pipe models, and you can attach heat structures to it in any way.

MR. KRESS: In any way you want to.

MR. KELLY: So in effect, there's a rod in the middle, you know, and a porosity factor, if you will, and then another heat slab to model one more housing, both connecting to the same volume.

MR. WALLIS: Why isn't the vessel the same as the pipe? A 1-D vessel looks to me like a pipe.

MR. KELLY: It is, but a 1-D -- in TRACE, there are parallel codes for the 3-D component and 1-D components, and so the solution and minimum equations for the 3-D vessels done one place, 1-D stuff somewhere else. So what I did is I used the 3-D vessel model. I just only discretized it in the axial direction. So, there are no radial rings and no aspect of the sectors.

We're taking it a 3-D component and making it a 1-D.

MR. WALLIS: So why is it different from a pipe?

MR. KELLY: It isn't, but --

MR. WALLIS: Well, you made a big deal of it being different.

MR. RANSOM: Do the both use the same constitiative package?

MR. KELLY: Well, remember I was preparing TRACE and RELAP5.

MR. RANSOM: No, I'm talking about in TRACE.

MR. KELLY: No.

MR. RANSOM: TRACE you have a vessel and you have a pipe, probably.

MR. KELLY: Right. In the current version of TRACE, there are differences between some of the models using the vessel and some 1-D components. You know, you shouldn't have different constitiative packages unless there's a good reason for it. There are specific components where you should have models developed for that component.

But what we're going to eventually do is consolidate it into one constitiate package that would be applied both to vessels, or 1-D components, but within that constitiative package, there will be branches out for different types of components where you expect the phenomena to be different.

I mean, obviously, you're talking about interfacial drag. Interfacial drag in a rod model is not the same as interfacial drag in the downcomer, which is not the same as interfacial drag in the preferred model.

MR. WALLIS: The one dimensional balances are all the same for the pipe and the one dimensional vessel. It's just that the constitiative equations are different somehow?

MR. KELLY: Most of the equations would be the same. Some of them are different. The thing here is this has all the overhead of being able to calculate radial and azimuthal minimum equations stuff. It's not being used, but it's there. So, I'm going to show you come computational statistics on run time stuff, and that's going to be impacted because this is a vessel.

This is quench front versus time, quench front elevation versus time for the case 31504. I'm only showing the TRACE result because there is not a plotting variable in RELAP5 for the quench front position.

Obviously, the blue diamonds are the data. The black curve is the bottom of the quench front coming up. The orange curve is the top quench front, which is not moving. It's just sitting at the top, which is about what happened in the test. We do need some improvements here.

A good prediction up through the core mid-plane, then they underpredict the quenching rate in the top half of the report. You can see that in our slide. Now, this is clad temperature. This is time. It's a 78 inch elevation, which was actually the peak temperature elevation.

The blue curves are all of the thermocouples in the center of the bundle that I plotted and were reasonably valid. The black curve is TRACE. The orange curve is RELAP5. Both codes turn over and miss the peak temperature. RELAP does a little bit better job here. Both quench late with TRACE doing a little bit better.

Moving up to 90 inches, you see exactly the same kind of behavior. The flow codes underpredict peak temperature. The flow codes quench late, where TRACE does slightly better.

This is vapor temperature at 78 inches versus time. Moving on, there are two different steam flows at that elevation, and those would be only instruments at that elevation.

When the steam temperature drops down to

Tsat, that doesn't mean the steam is really in Tsat. What it means is enough droplets hit these probes that the probes quench, and that's why, you know, it's both codes continue to show super heated vapor until that elevation quenches.

MR. WALLIS: This is the same run that was so bad before?

MR. KELLY: Yes.

MR. WALLIS: And you have fixed the code up in some way you haven't told us?

MR. KELLY: Right, and I'm going to give you an example of one of those regimes.

MR. WALLIS: You're going to tell us how you fixed it, or is it a secret?

MR. KELLY: Yes. I'm going to go all the way through inverted annular, and then I'll come back and talk about other regimes. Like I said, that would be a two-day meeting if I were to go through all of the models.

MR. WALLIS: No, it's okay, but you've just given us the bottom line now and now you're going to tell us how you got there?

MR. KELLY: Exactly. I'll give you a good glimpse of the process we went through.

So, this is TRACE and this is RELAP5.

TRACE is too high. RELAP5 is too low. Even though we both underpredicted the quad temperature, and that's because the heat transfer models.

There's a lot more noise in a RELAP calculation, and that's because that's one of the things I went after in the TRACE development, was to minimize unphysical oscillations, oscillations that shouldn't be there.

This is void fraction versus time at the four to five-foot elevation. So, what I'm looking at is the amount of water between one DP cell which is over a one-foot span. That blue line is the data. What you really see, up in here is in dispersed flow. The DP cell is not going to give you anything except, you know, frictional pressure drop. You're not going to see the amount of water.

If you look in the TRACE calculation, the void fraction here is pretty consistently about .995. There's very little water from a volume fraction standpoint, but there's a lot of water from a quality standpoint. The quality might well be 30 to 50 percent.

So, there's a lot of entrained droplets, but you don't have much in volume fraction. You can't do any comparisons here.

What's happening in this part where the void fraction in the data dies, it's just simply the quench front is going through that DP exam. This is one of the tests where you almost have a dissident continuity at the quench front. You have a two phase mixture, you know, bubbly slug plant flow below the quench front, and you have dispersed droplet flow above it.

So, what's happening is the quench front comes through and you basically go from something near one down to about 40 percent.

Both codes have similar behaviors. They drop as the quench front goes through. So, what you're seeing in here is not so much how accurate the interfacial drag package is, just where was the quench front relative to that DP cell? That's unfortunate, but it's one of the things that makes coming up with figures of merit difficult.

A couple more things to notice, the TRACE curve is remarkably smooth. If you've ever looked at calculated void fractions in any of these codes, the RELAP5 is a little bit noisier with some jumps, but both codes come to about the same answer.

This is about when the elevation quenched, is in here, and what you're seeing is the effect of decay heat on the void fraction coming out. You notice they both overpredict. The reason they overpredict is this is the first cell at which you get a two-phase mixture, and actually about half of that DP span is subcooled water at this point in time. In the other half is two-phased mixture.

Well, reality can do that, and you get a void fraction of about 12 percent. The code thinks it's all two-phase mixture and in effect uses your J sub G to do a void fraction, and you're getting a void fraction of around 25. So, that's why that looks that way. If I use smaller nodes, the answer would change.

MR. WALLIS: So you should run smaller nodes and show it does.

MR. KELLY: Yes, and you'll see that one of my things had a slide on work that needs to be done, and one of those is doing conversion studies, both on voiding size and noding size.

Moving on up, you'll notice the codes quench later, so we can see that fall on to be dispersed two phase later. Again, interestingly enough, even though the codes have drifting interfacial drag packages, it becomes almost exactly the same answer. In this case, we matched the data very well.

Now, in doing this interim reflood model development, my philosophy was to only change the post-CHF reflood models. What I wanted to do was take everything from before pre-CHF, you know, normal two-phased flow stuff, use the extant models in TRAC. That's what I tried to do. It turned out I couldn't do that.

There were some oscillations in the interface flow package which destroys this calculation. I had to go in and replace the bubbly slug model, the bubbly slug interfacial drag, and that's when I chose the Bestion model based upon some work, some assessment work that had been done earlier. It works very well, and it's a relatively simple drift flux correlation whereas the one in UF-5 is the EPRI model, which is very complicated, which seem to give about the same answer, at least for these conditions.

MR. BANERJEE: Does the model in RELAP5 work as well as the Bestion model, or is there some problem?

MR. KELLY: It'd say it's -- well, from my experience when I was working with it in '96 or so for the AP600, I'd say it has some problems because there is a lot of switching in it, and so that it will tend to be noisier, but if you forced the condition to say be for an upflow, it will give the answers of equivalent accuracy.

MR. BANERJEE: But the Bestion model doesn't have this problem?

MR. KELLY: It's a very simple one-line correlation. The EPRI package goes on for a couple of pages.

MR. BANERJEE: Right, right, okay.

MR. RANSOM: Well, isn't this when you damp the drift flux model, it's complicated by wall friction enters into it and you really have to worry about the partitioning or the difference, you know, how wall friction and interface drag both interact. I assume you've done the same thing here.

MR. KELLY: And there is a huge thing in RELAP5 to try to take you to make, if you will, the wall drag, the void fraction neutral, and I think that's wrong. You shouldn't do that, but what you should do is develop your interfacial drag package with the wall drag model that you're going to use so it's a consistent behavior. It turns out --

MR. RANSOM: Is that what you've done here?

MR. KELLY: No.

MR. RANSOM: What have you done for that?

MR. KELLY: Well, I selected a model. I didn't develop one. Now, what's actually in my condensation talk, we'll talk about wall drag and its partitioning some, because it's wrong. It shouldn't be done. In these cases, all of the wall drag should go in the liquid. The liquid is what's in contact with the wall.

It turns out for these conditions, it's a no never mind because your velocities are so low that the wall friction is basically negligible, and your void fraction prediction is governed almost entirely between the buoyancy balance and interfacial drag.

Now, where that would not necessarily be the case is maybe a boiling water reactor operating condition where you have very large flow rates, and then your wall drag becomes appreciable. We're going to have to check that through assessment. It's hard to remember everything we've done, but we did do assessments with the Frigg test which are at boiling water reactor conditions, and the Bestion correlation did perform acceptably.

But you're right, we're going to have to check how the wall drag is done, and if the time comes that we decide we need to develop an interfacial drag model, then we're going to do it with a consistent wall drag model so that they are together and work together.

MR. RANSOM: In the TRACE results, there is a wall drag model.

MR. KELLY: Yes.

MR. RANSOM: And somehow you're also then backing out an interfacial drag from a drift flux model, Bestion model, right?

MR. KELLY: Yes.

MR. BANERJEE: Have you written this up somewhere, a page or two?

MR. KELLY: Not the interfacial drag stuff.

MR. BANERJEE: How do you get from the Bestion model to the interfacial drag?

MR. KELLY: No, I haven't, but I will.

MR. BANERJEE: Yes, that would be useful to have so we understand the assumptions you've made. Precisely as Dick says, you must have assumed something about the wall drag at that point.

MR. KELLY: Actually, what I did was follow what they did with the CATHARE codes where the Bestion model was developed.

MR. BANERJEE: Right.

MR. KELLY: And they basically ignored the wall drag.

MR. BANERJEE: Okay.

MR. KELLY: And said, you know --

MR. BANERJEE: So that's an assumption.

MR. KELLY: Yes.

MR. BANERJEE: And then --

MR. KELLY: It works great for these conditions.

MR. BANERJEE: Right.

MR. KELLY: And I think it still works okay for BWR conditions, but I haven't checked it, but I will.

MR. BANERJEE: But you will write up something so we know how you went through this procedure?

MR. KELLY: Yes, and that will be part of the revisions to the theory end. The way the theory manual is going to be done, like if I look at the physical model stuff in that now, it would be a huge job to rewrite it all just so I can move the TRAC E's and make it clearer. I don't see the point in that because I think most of the models over the next few years will probably be replaced.

What we're going to try to do is replace them as a result of a rational selection process, and as we replace them all, then we will be rewriting that section of the manual with a new model to try to do a better job of it.

MR. BANERJEE: Does CATHARE have an explicit expression for the interfacial drag based on this drip flux correlation?

MR. KELLY: They now have a more complicated drop flux model, but yes.

MR. BANERJEE: They had an explicit saying whatever the interfacial, this is the formal fit?

MR. KELLY: Basically you say okay, this is, for a certain void fraction, this is what the buoyancy force should be, and then that's recorded one-half FA narobes D relevant squared. Then you go in and plug in the drip flux model for the void fraction, and you come up with what the interfacial drag coefficient ought to be.

MR. STAUDENMEIER: I was going to say, actually in the development of the TRAC-BWR models and correlations, there is a derivation of how you go from drift flux to interfacial drag in steady state conditions. I mean, essentially you're declaring its equivalency in steady state conditions, and shows the transformation on how to take a drift flux correlation and turn it into an interfacial drag correlation. There's a NUREG on that, and we can get you a copy of the NUREG if you're interested in a derivation of how to do that.

MR. KELLY: That's right, thanks.

MR. RANSOM: Is that from TRAC-B you said?

MR. STAUDENMEIER: Yes.

MR. RANSOM: I'd like to have a copy.

MR. BANERJEE: Well, originally the paper, I remember a paper on this written by Rohatki way back, before it sort of entered into fact, but I've forgotten all of the details. I want to look at it again.

MR. KELLY: Moving on to the high flooding rate case, which is 31701, and we're talking about six-inch per second reflood rate. This is case that would be dominated by the input annular regime, whereas the previous case was dominated by (inaudible).

A quench run versus time for TRACE. The bottom quench run does quite well, a little slow in here, but still within the spread of the data. The top quench run doesn't move for awhile. In total, the very top of the rod cooled enough for liquid film to be deposited. Then we have the top one coming down.

MR. KRESS: Is that a counter flow limit?

MR. KELLY: No, it has to do with the way the model transitions, and it's something that needs to be worked on in order to do this job correctly, and especially in order to do it for when we start on RELAP.

MR. KRESS: What does TRACE do when the top front and bottom front meet? It looked like a little strange dip in there.

MR. KELLY: There is.

MR. KRESS: Yes, I didn't understand that.

MR. KELLY: Okay. The quench front model, if you will, is an adaptive grid technique. So, what you've done is you take the heater rod, you look at both axial profile wherever the temperature profile exceeds certain criteria, and it remeshes. So you get these very small nodes where the quench fronts are.

MR. KRESS: I see.

MR. KELLY: So there's no actual quench front model. What these numbers are, this is a plotting variable, and what it does is it starts at the bottom of the rod and says okay, I'm cold, so I'm below the quench front. It goes up until it sees a transition from one node that has nuclear boiling to the next node that has transition and says ah, the quench front is between those two. That's what I'm plotting.

The other one, it searches from the top down. When the whole bundle is quenched, there is no quench front. So, the search kind of fails, and I'm going to have to make sure that's a little more robust.

MR. WALLIS: It seems to be really disbursed on the top. You've got three to the left and two to the right.

MR. KELLY: Yes, you've got 161 rod bundle, and it becomes somewhat chaotic. You know, at the same elevation, some quench, some don't. Sometimes you can say ah, and the data you see here, I threw out the rods near the housing. We're only looking at the rods more in the center part of the bundle. But even then, you can sometimes go and look, okay, this thermocouple is on a rod facing a guide tube.

MR. WALLIS: That makes sense. This is a CCFL limit, isn't it? It's steam coming out the top or something?

MR. KELLY: Actually, not really, because we have water coming up from the bottom here.

MR. BANERJEE: Thermocouple limitations.

MR. KELLY: And sometimes it, you know, you wet the rod and you form a liquid film. It just kind of hangs. It may go up or down, but it doesn't, you know, it's -- you'll never model this exactly because some rods quench, some don't, and it's just hard to say. You can look at the data and say ah, I know why that one quenched. It's near a guide tube, but then you look at one that's nowhere near a guide tube, and it's surrounded by hot rods that didn't quench.

MR. RANSOM: Do the ones that quenched early, do they heat back up again at all? Do you have any way to tell that?

MR. KELLY: Some might. Not in this test because this is a six-inch per second reflood case, and it's got just so much water going up through the bundle that once you quench one of these, it's quenched.

MR. SIEBER: It stays.

MR. KELLY: In a low flooding rate case, you could have that, and we talked about what Professor Banerjee said. In a real case, we have an oscillating inlet flow. Then you could definitely have a quench line receding and preceding.

MR. RANSOM: Also in the changes you have made, you started out saying you were only going to change the post-CHF regime, but you've now said you also changed the interface drag.

MR. KELLY: Right.

MR. RANSOM: Have you changed the heat transfer correlations that are used in the different regimes?

MR. KELLY: Post-CHF, everything was changed.

MR. RANSOM: Okay.

MR. KELLY: Wall heat transfer, interfacial heat transfer, interfacial drag, everything. It's an entire package in its own little module.

MR. RANSOM: For post-CHF?

MR. KELLY: For post-CHF. That's all the regimes. Everything was changed. Now, in pre-CHF, first off, you have CHF. I changed that as well. I went to using the AECL, and I decided to go ahead and do a consistent package because -- we're talking about wall heat transfer now. I couldn't make things match up between pre and post, so for the wall heat transfer, I went ahead and did a consistent package. So, I changed to nuclear boiling, and I changed forced convection both to single phase vapor and single phase (inaudible). When that case, we actually implement correlations specifically for rod bundles instead of just using an old standby.

In pre-CHF phase, for the fluid solution, and what I'm talking about is interfacial drag and heat transfer, I implemented an interfacial drag coefficient for a bubbly slug regime for rod bundles, and I had to do that because the model it was in, TRACE, the low pressure was causing such large oscillations that I couldn't have the -- I couldn't calculate the low flooding rate cases.

I made a small change to the super heater liquid interfacial heat transfer, and the one at TRACE you can't really dignify with the name of a model. It's something like 10 to the 7th for ramp to 10 to the 8th over one degree, you know, something like that.

MR. RANSOM: It has a subcooled boiling model, though, I assume.

MR. KELLY: It's super heated, but --

MR. RANSOM: Right.

MR. KELLY: And I put in a more typically based model because that was giving me some very large oscillations as well. You get a little bit of super heat, and the liquid went down, but that's pretty much it. I tried to leave those models alone.

MR. RANSOM: So, it's pretty much a revised heat transfer package, I guess then, right?

MR. KELLY: Yes.

MR. RANSOM: Are the other assessors using the same package yet?

MR. KELLY: They will be soon.

MR. RANSOM: Not yet, okay.

MR. KELLY: We're just now making it available to them.

This is cladding temperature versus time. Again, the blue curves are the data. The black curve is TRACE, and the orange curve is RELAP5. In this case, they both do an excellent job of predicting the behavior, and they both quench about the same time but just a little bit late.

This is void fraction versus time, and again, the blue curve is data. Now, in this case, some of the blue curve actually indicates real two-phase conditions that would be inverted annually. What I mean is you have a significant amount of water in the DP cell to give you a void fraction reading before the quench front gets here.

The quench front gets here right about in here. So, this has to do with the quench front going through the DP cell, and the rest of it is actually real water in the cell.

You see the RELAP5 calculations. Noisy, but the results of efficiency in the TRACE calculations are much smoother except for this big blip.

MR. WALLIS: What's that original dip at the beginning there in TRACE?

MR. KELLY: That's when the first node quenches. What happens, the bottom of these bundles is very cold because they're basically started at Tsat and you have a cosign power shape. So, the bottom two, 3T to this is sitting there, not much of a Tsat, just you know, it's basically in transition boiling, and it's just waiting for a little bit of water to get there.

One of the problems is when you bring water into that first cell, how you discriminate where the water actually is, and that's -- when I talked about having a subgrid resolution scheme for the fluid solution, now there's a model end, -- not a model. There's a scheme for interpolating void fractions and vapor temperatures computed by the hydrocells onto those small mesh nodes.

MR. WALLIS: So you must be taking that

water as it first comes in and squirting it up to the --

MR. KELLY: Exactly.

MR. WALLIS: Four or five foot level?

MR. KELLY: All the way through the bundle. Once you throw it up, push it up, you have enough vapor generated that you can just carry it on the rest of the way. You notice RELAP has the same kind of behavior. It's just a problem, and it's one of the things we need to address.

MR. WALLIS: Is this seen in the tests at Penn State?

MR. KELLY: Well, tests don't have nodes, okay?

MR. WALLIS: But they visualized the flow and so on.

MR. KELLY: There are quite often, when you first start one of these tests, you do get some violent boiling and throw water up through the bundle, but that's not -- this isn't reality. This is because of the node being in there.

MR. RANSOM: The problem is that an entire node changes its heat transfer regime, right, and so more mass.

MR. KELLY: Well, actually not heat

transfer regime because we have the fine mesh nodes, but when water comes into that cell and you do an interpolation of the void fraction profile, you artificially say there's water where there shouldn't be. So, some of those fine mesh nodes that are in the transfer heat boiling regime get high heat fluxes. So, the vapor generation rate for that cell goes high.

MR. RANSOM: Right.

MR. KELLY: Really, the velocity at the top of this cell, it ends up determining the void fraction of the cell. So that's what was happening there.

MR. WALLIS: So you instantly get enough steam velocity to carry liquid all the way through?

MR. KELLY: Well, to move it up a couple cells, but move it up a cell, then there's more vapor generation in that cell, and just as you go up, the vapor velocity keeps increasing, and the vapor would carry it out.

MR. WALLIS: It doesn't evaporate at all then?

MR. KELLY: Not as much as you'd think. One thing, it's fairly cold water, and even when it's a droplet, they go through the bundles so quickly, not as much evaporates as you would expect.

The one deficiency, well, other than this of course, is this, and this has to do with the quench time entering the cell. This is inverted annular film boiling while the void fracture decreases here.

Now the quench time has cracked the cell boundary into this one-foot section, and the vapor generation rate went up a little bit and comes up with a disproportionately large increase in the void fracture. This is one of the things that I have remaining to work on.

It turns out that, you know, it's a non-linear behavior, and if I were to plot the data generation rate, you know, it would be coming along actually, and a very small little blip will give you that kind of change in void fraction.

MR. RANSOM: Could you say a little bit about what you've done to achieve smoothness?

MR. KELLY: One thing I've done, you'll see some of this when I talk the model development, is I try not to have unphysical transitions, and I try to make one regime evolve naturally.

MR. RANSOM: Does that mean you spread them out or something, or put a delay factor?

MR. KELLY: No, I try not to use ramps whenever possible, but it's hard because I don't want to get into a two-day discussion.

When you go from inverted annular and you want to break up that inverted annular and go to the next regime downstream, what most codes have done in the past is try to look at some break-up criteria.

What I did say is okay, you are going to make me two criteria. You have to be able to break that liquid column up, but then whatever your liquid fragments are, you have to have enough vapor velocity to carry them out. If your vapor velocity isn't enough to carry them out, they would fall down, and the column would reform.

It turns out the carry-over criteria is a more stringent one, so I made the break-up, the change from one regime to the other at the point where the liquid could be carried up. What I did for that regime, quite often what a code will do, they may think they know something about disbursed flow, and they may have some half-way decent ad hoc model for inverted annular, and between the two they'll do a ramp based on void traction.

I didn't do that. I came up with a new regime in between which is kind of like a simplified fluidized bed model. So, it's large liquid fragments, and if you will, something like a fluidized bed. I made it so that the interfacial drag coefficient is the same at the point where it breaks up.

MR. RANSOM: So, this is something you do at each junction, I guess then?

MR. KELLY: But it was, by doing the physical models, so that when I developed the one for interfacial direct for inverted annular, I made it so that the interfacial friction naturally transitioned into the next regime instead of having something that is an order of magnitude apart and putting a ramp between them. I made them naturally evolve.

MR. RANSOM: Providing the physical basis for that link between the two. Interesting to see how those work in things like level swell where, you know, it's more depressurization rather than heat transfer from the walls type of thing. I assume the assessment process will eventually get in.

MR. KELLY: You'll find lots of deficiencies going on. That's right, I haven't gotten to the interesting part of this.

This is computer time, computational statistics, comparing TRACE and RELAP5 on the two different tests. Transient time, you know, it's 700 seconds for the low flooding rate case, 200 for the high. This is what is the maximum time step that we used for inputs. I made them the same between the codes. I used a smaller number here, and that's just because things happen fast through here.

This is the number of time steps the code takes, 14,000 for TRACE, 42,000 for RELAP5. This is because the SETS numerical method allows you to violate the time limit. We're running this at about a time limit of three, where there is a time limit conditions around five.

In a high flooding rate case, it's not quite so dramatic, but still it's more than 1-1/2 times the number of time steps.

Total CPU time, it turns around the other way. RELAP5, 174, TRACE, 276. So, it's less than a factor of two that the TRACE is slower.

MR. WALLIS: That's not really common. That's what really counts. If you're spending more time in the time step, you haven't gained anything if you cut the time steps. There's no real gain in cutting the time steps if you take from the grind time.

MR. KELLY: If you didn't cut time steps, this would be three times, worse, okay?

MR. WALLIS: Yes. The SETS method presumably is taking longer to grind than the RELAP approach.

MR. KELLY: It's not so much SETS, though. It's everything else. Also, the difference between this being a pipe and this being a vessel, is a lot of overhead in the vessel. You're leading into my next slide.

MR. WALLIS: Let's go there, then.

MR. KELLY: Yes, well, the grind time, and this is what -- the amount of time it takes to do one cell and one time step, okay, and that's where you see the fact of a little bit greater than four. So, RELAP5, the structure is about a fact of four faster than a TRACE vessel.

I knew comparing a vessel to a pipe wasn't quite fair, so I had Weidon do a series of cases to do pipe to pipe comparisons. What I did is basically level swell tests. Instead of an artificial, I took the conditions in the low flooding rate case after the entire bundle had quenched. So, it's just simple boiling two-phase swell. We've got a transient to a steady state two-phase condition.

Again, the same kind of thing. You do four calculations here now. RELAP5 with the 1-D pipe, which took about 6300 time steps, which is very similar to a TRACE pipe, using the Semi. We used the

SETS method. You're dropping on the time steps to 4700, and for some reason with the SETS and a 1-D vessel, it's about 4200. I haven't understood that.

RELAP5 is still the fastest, but you compare RELAP5 with 15 seconds to SETS 1-D pipe and 23, that's about 50 percent, and actually, that's not too bad. It's about what we'd expect. The grind time is a factor of two.

MR. STAUDENMEIER: If I can say a little bit. If you'll remember from my slide yesterday, the TRACE time per time step is about 1.7 times what the F77 time for time step was, and once we tracked down that unaccounted for difference and speed that up, then I think we're back in the same time range as RELAP5 for 1-D components.

MR. KELLY: So, a summary of this second part of the presentation, code accuracy. The overall accuracy of the interim reflood model is slightly better than RELAP5. The TRACE calculated results are much smoother, but we still need some improvements in accuracy for the low flooding rate test. I need to work on peak clad temperature because I undercut those significantly, and the quenching rate for the upper part of the bundle.

For the high flooding rate tests, the

accuracy was okay, except for that void fraction bump associated with the quench front entering the cell.

MR. WALLIS: This is conclusions just from two different runs. Now, if you had compared with other ones, something else might have happened.

MR. KELLY: Right. I at least know I've got to work on these.

Computational efficiency, TRACE can use larger, therefore fewer time steps, but its grind time is higher than that of RELAP. If you compare the pipe versus the vessel, it's about a factor of four. So, we need to put some effort on making TRACE faster.

So, this is what I have left to do. We have to apply the interim reflood model to 1-D components. Right now it only works with the vessel module, and that has to do with data transfers. So, I have to get it to work for the pipe, and after we get it for the pipe, we can work for BWR channel.

We need to improve the models for top-down quench, and I haven't even begun to look at blowdown rewet yet, and that's something that's important for LOFT.

Of course, the accuracy improvements that I noted on the previous slide.

Assessment, we need to greatly expand the matrix. Looking at two cases alone is not sufficient, and actually, the two cases I picked are the ones that were done as part of the RELAP5 assessment.

I won't go over this. This is basically what Steve said earlier, but I do have to develop metrics so I can do quantitative comparisons and just kind of say ah, the black curve is a little bit higher or closer to the data than the orange curve, and I need to do conversion studies, both on time step and noding size.

Of course, the ever present documentation.

Now, the fun part of the presentation. We're going to talk about one of the models in detail and how I developed it. So, we're going to talk about inverted annular film boiling. This is a picture from an early FLECHT-SEASET or maybe even a FLECHT document. What it shows is the various regimes as you go from the bottom of the rods --

MR. WALLIS: Is this what you think happens, or is there evidence that this is what happens?

MR. KELLY: There's evidence that this is what happens.

MR. WALLIS: There aren't any spaces in here.

MR. KELLY: No, depending upon the flooding rate.

MR. WALLIS: If these things hit the spacers all the time as they go through, all this stuff does.

MR. KELLY: Right, and if you're in a low flooding rate case, that can have a drastic effect on the vapor's superheat just downstream of the grid.

The transition boiling region is indicated here. That's where most of the vapor generation occurs, where the quench line is. What you're looking at is a region that's maybe one, two centimeters long. It's very small, and that's where most of the vapor is being generated.

Just above that is what's labeled here a film boiling region, and that's what's generally turned inverted annular. That's what I'm going to speak about today.

Then there's this chaotic regime where that liquid column breaks up into liquid slugs.

MR. WALLIS: But you can't do it annular because the tubes are tubular so that it's a -- it's not an annulus.

MR. KELLY: Right, but that's, you know --

MR. WALLIS: It's what people have called it.

MR. KELLY: Yes, ever since Bromley, you know, back in 1950 something.

So, that's the terminology we're going to use, and this right here is the regime we're going to look at. For the moment, we're going to ignore all the others.

The first thing you want to do if you're going to develop a model is try to educate yourself a little. So, you look at data. You also look at a lot of other models people have done, but you'd better look at data.

The one I picked is a fun, low quality film boiling experiment. It's a tube with a hot patch, and the reason for the hot patch is you can freeze the quench front here so that you're able to have the entire tube and film boiling, so you can have steady state film boiling at low flow rates without going to temperatures that, you know, would destroy your experiment.

The reason I picked this, in addition to the ten-wall thermocouples, it has a gamma densitometer and measures void fraction at five axial elevations. So now I'll be able to have heat transfer measurements as well as void fraction, whereas a lot of the tests, all you get are the heat transfer measurements, and you're left to guess at what the fluid condition was.

The one bad thing about this test is the atmospheric pressure. I would like to have rod bundle tests, but this is very hard to do with a rod bundle.

MR. WALLIS: Doesn't Mr. Fung have a theory, too?

MR. KELLY: I think the answer is yes, but I don't remember, but it wasn't developed within -- to work within a two fluid framework.

MR. WALLIS: Okay.

MR. KELLY: That's always one of the problems whenever you grab something out of the literature.

MR. RANSOM: Whose experiment is this?

MR. KELLY: I think it was done at AECL. it was funded by the NRC, and the person's name was Fung, F-U-N-G, This isn't a NUREG.

Well, when you take this test data, what does it look like? So what I'm plotting is --

MR. WALLIS: We've seen this before I think.

MR. KELLY: Yes, you've seen some of this before.

MR. WALLIS: All right.

MR. KELLY: This is heat transfer coefficient versus void fraction. I'm showing all of the test data, not one test. I'm showing every measurement where there's a measured void fraction and thermocouple for all six different mass fluxes. The only thing I'm leaving out is data where the equilibrium quality is greater than zero. So, I'm only looking at subcooled data, and that's because that's where you expect to find inverted, what I'll call inverted annular conditions.

MR. WALLIS: So by void fraction, you mean some measure of the thickness of the film, essentially?

MR. KELLY: In this case, it's a gamma densitometer, so in effect it's measuring how much water is there.

MR. WALLIS: Yes, the annular flow. It gives you a measure of the --

MR. KELLY: The film.

MR. WALLIS: Heat transfer resistance in the film, presumably?

MR. KELLY: Exactly. We're getting there. But what you see is this almost exponential decrease to nearly constant value. Very strong void fraction dependence, because like you said, the film thickness.

MR. WALLIS: What is the value of the void fraction of zero? Single phase liquid, void fraction of zero? What is the value at the top then on the axis?

MR. KELLY: Oh, you mean the closest?

MR. WALLIS: No, on the axis. No, it would be right on the axis. You'd have a single phase liquid. I just want to know how high that is.

MR. KELLY: Oh, if there's no vapor film --

MR. WALLIS: Is it way off the graph, or is it --

MR. KELLY: Yes, it would be --

MR. WALLIS: Or does it magnitude off, or is it just off? So, it's an extrapolated, too?

MR. KELLY: Yes, it does extrapolate.

MR. WALLIS: You don't have it.

MR. KELLY: There's no data for films that small.

MR. WALLIS: But you can predict it from theory?

MR. KELLY: Right, and actually that's in the --

MR. WALLIS: It depends on G?

MR. KELLY: That's in the model, and you actually will see that coming up.

MR. WALLIS: Okay.

MR. KELLY: The other thing to notice is a factor of five in the mass flux here, and you might be able to fine some mass flux dependence here, but you're not going to find much. It's pretty much within the scatter of the data, and that's contrary to a whole lot of models that are out in the literature.

The other thing I want you to note is this is subcooled, but the void fraction is greater than 70 percent, and that's very different than what you see in normal bubbly two-phased flows. You might get void fractions up around 40 percent in subcooled boiling, not 70 percent. I mean, that's most of the tube is vapor, and the liquid is subcooled.

Did I skip one? Yes, this is the slide I'm looking for.

Now what I've done is include the data for which the equilibrium quality was a positive. So, this is all of the data points now, and instead of plotting it versus void fraction, I plotted it versus equilibrium quality. What you see is in a negative quality region, which is where I expect to have inverted annular, I do. I have this very little mass flux effect. All of the data points, you know, they're scattered, but they have the same behavior.

Shortly after the quality becomes positive, there's a change in the behavior, and a mass flux dependence sets in.

MR. WALLIS: There's a real trend with mass flux, too.

MR. KELLY: Right. This would be where you're breaking down into a highly dispersed flow with a lot of vapor superheat. You notice as you increase quality, you should be increasing your vapor velocity and you'd expect to increase your convected heat transfer coefficient.

That doesn't really happen with the lowest case because of the superheat, but as you go to the higher mass flux cases, that's exactly what is happening. The vapors relatively close to Tsat so as you increase the quality, you increase the vapor mass flux, you increase the conductive heat transfer, and you see that trend.

So, what this says is if you look at the subcooled part, what that's primarily a function of is the liquid subcooling because that's all the equilibrium quality is when it's negative.

If it's primarily a function of the liquid subcooling, then what is controlling the heat transfer process? It's the interfacial heat transfer inside this liquid core. It's from the saturated interface to the subcooled liquid in this inverted core.

MR. WALLIS: It doesn't know what G is because it's -- the wall is away from it by vapor.

MR. KELLY: Yes, and the vapor film has to adjust so if the heat transfer through the vapor film matches what the liquid core can assume, can take up. That's exactly where I'm headed.

MR. WALLIS: Because the water isn't touching the wall, the fact that it's going faster is not so significant for it.

MR. KELLY: Right, and the --

MR. WALLIS: Unless I didn't know what G was. That's what I meant.

MR. KELLY: Right. Unless that liquid mass flux affected the interfacial.

MR. WALLIS: Yes, somehow.

MR. KELLY: And a lot of people used Dittus-Boeter for this. We're going to get to that later.

So, if we're going to do this for a two fluid model, and that's what we're faced with --

MR. WALLIS: Well, most of the velocity

grade is presumably in the film. The core is going along without much velocity variation across it.

MR. KELLY: Right.

MR. WALLIS: You'd expect the velocity differences in the core to scale the next thing or whatever.

MR. KELLY: Yes, and in the core, the typical velocity in the order of centimeters a second. In the film, meters per second. There's about a two order of magnitude difference between the two.

A little cartoon with a wall, superheated vapor film, subcooled liquid core. The primary heat transfer mode from the wall is wall to the vapor film, but of course thence from the vapor film to the saturated interface where some of that energy generates some vapor which produces this vapor film, but most of it goes into the subcooled liquid core.

So, what models do we need in order to simulate this in a two-fluid code, right? You need water vapor heat transfer, vapor to the interface, liquid to interface, interfacial. There's also a contribution of the water liquid radiation. You need interfacial drag between this vapor film and the liquid core. Then I already alluded to you need some criteria for the regime transition.

The little note down here says I've got this idealized drawing. It doesn't look like this at all. Even in two, at very, very highly subcooled conditions, you may get void fractions that are like 10 percent, in which case you will have a very, very nice, smooth, flat film, and you can even see this in some of the FLECHT-SEASET reflood tests when you look through the windows,, but most of the time, once you get very much vapor at all, there are waves on this, and they tend to be very large, disruptive waves.

Not only do you have the waves, you have this entire column moving around. There are some neutron radiographs taken in England by Castigan and Wade, I believe, and it's really neat. I mean, you actually see this thing moving around inside a tube. Then other visualizations you can see the wave on this.

So, the actually fundamentals of this are incredibly complicated.

MR. WALLIS: Well, heat transfer coefficient is defined in terms of the wall temperature minus the bulk liquid temperature, or bulk temperature minus saturation?

MR. KELLY: Sorry, I didn't explain that. In everything I've shown up to date, when I compared the heat transfer coefficient, that's based upon saturation temperature.

MR. WALLIS: So, it's based on the vapor being the resistance?

MR. KELLY: Well, it's just that Tsat is what you know.

MR. WALLIS: That's the difference, though. The difference is of course the vapor, the TW minus Tsat?

MR. KELLY: Right, and that's just traditionally, that's the way they define heat transfer coefficients for film boiling, because Tsat is a number you know. You don't know the vapor temperature, and you don't know the liquid temperature.

MR. WALLIS: Unless you have some way of calculating it or something.

MR. KELLY: Right.

MR. WALLIS: If it's subcooled, it makes a difference. You ought to bring it into account.

MR. KELLY: Right, but from the experimental standpoint, he doesn't know it, so he uses Tsat.

This is your same heat transfer stuff that I showed before, reference to Tsat, and I should have said that, versus void fractions. The same data you showed before.

What I'm doing now is saying this is what the wall heat flux would be, and if you take, you assume it's a laminar smooth film, and this is exactly where you were headed before, the heat transfer coefficient is nothing more than the vapor funnel connectivity divided by the film thickness, which I'm using delta for. That's this black curve.

You'll notice that very small values of the void fraction does a very good job. The theory, if you will, of a smooth, laminar film, works. As you go to the higher void fractions, it breaks down completely and underpredicts significantly.

MR. WALLIS: It looks like a lower limit, though, which is a useful thing.

MR. KELLY: And one of the first things people did in trying to fix up Bromley type models was to say this vapor film could go turbulent. So, if by now make this a turbulent force convection with the characteristic length being the film thickness, I do this.

So, to prove it, I cut the difference in about half, but I'm still undercutting the data, but that's assuming, you know, smooth parallel plates.

Like I just said, this core is not smooth. Once you get past about here, you have something that's very chaotic, lots of waves, very agitated structure. So, that's not surprising that turbulent would undercut it as well.

So, what are we going to do to develop a model? Okay, well, this is a Nusselt number definition I'm going to use. It's no magic. Two times the film thickness. It's just, you know, the hydraulic diameter for parallel plates, if you will.

What I'd like to do is correlate this heat transfer coefficient as a function of vapor Reynolds number, and what I'm talking about now is the heat transfer coefficient from the wall through the vapor to the saturated interface.

I know it's a function of the vapor Reynolds number, but I had no idea what the Reynolds number is.

MR. WALLIS: Because the vapor is going very much faster than the liquid?

MR. KELLY: Right.

MR. WALLIS: But you don't know how much -- do you know how much heat transfer -- you're down to the split between subcooling and vaporing?

MR. KELLY: Exactly. So there's no way to know what your vapor velocity is because you don't know how much of the heat was absorbed into the liquid cord.

So what do you do? Here's where I borrow an idea come up with by CATHARE, the work at PSI, and we got this through the camp program as a NUREG-IA. What we said is we don't know the vapor Reynolds number. Let's instead use the film thickness and correlate it based on that, and through an analytical solution, you know, for laminar, you can show the relationship between the film thickness and the Reynolds number.

MR. WALLIS: Just like fudging the friction factor and annular flow as a function of film thickness.

MR. KELLY: Exactly, exactly. So, I'm going to use the nondimensional film thickness where this is a, you know, viscous gravitational link scale which if in the condensation world, that would be called a Nusselt link scale, and we use that to correlate the Fung data. This is it. It's very simple.

The Nusselt number is equal to two, is a smooth laminar film. So this added part is due to the wave enhancement.

MR. WALLIS: This .1098 is --

MR. KELLY: It should have been .11. I saw that last night, and I went oh, you idiot. Why did you do that?

MR. WALLIS: That's what you got from correlating data. You didn't get that from CATHARE?

MR. KELLY: No, I did the curve fit, and I put the four digits there, and that's wrong. I apologize. That's wrong.

MR. WALLIS: Well, I wasn't criticizing that. I was just saying you got it rather than CATHARE, but it's okay.

MR. KELLY: No, I did that, but I mean, it's silly.

So, this is the result with you do that. Nusselt number versus non-dimensional film thickness, and it does a better job that I ever thought it would, to be such a simple model.

MR. WALLIS: Well, you could have done it versus alpha presumably. You could have said two plus something times alpha.

MR. KELLY: And I'll show you exactly why I don't --

MR. WALLIS: Because you and the row don't change all that much, do they?

MR. KELLY: They don't.

MR. WALLIS: Delta is a measure of alpha.

MR. KELLY: That's true, they don't, unless you go to high pressures.

MR. WALLIS: Right, so that's the test. You have to go to a different pressure.

MR. KELLY: And at Winfrith, they did a series, a very similar kind of experiment, not as much data, however. So, what I've got here again is the heat transfer coefficient versus void fraction. I'm sorry you can't read this very well, but at five different pressures. You notice at five bar, there's a number of data points, and you get about the same kind of behavior we saw with Fung.

A couple of points, at 20 bar, 40 bar, and one at 70 bar. If I just correlated it with respect to alpha, I wouldn't be able to do this, but having the nondimensional film thickness in here which has basically the vapor density, the delta row term, that's what gives you this, and it does -- I mean, it's not perfect by any means. That point is probably in a different regime.

MR. WALLIS: Well, dimensionless delta is gravity versus viscosity.

MR. KELLY: Right, and the delta row in

that is what gives you a lot of the pressure scaling.

MR. BANERJEE: But if it's turbulent, why would viscosity enter that? Or is that the wall effect? You know, George Galligher developed a mechanistic model for this many years ago where he partitioned the -- he actually took the amount of heat that went into the liquid film, dissolution of the liquid region. What was wrong with his model?

MR. KELLY: Trying to put it into TRACE. If we're talking about the same thing, maybe I'm even thinking of one of your papers, the one where he --

MR. BANERJEE: I never developed a model.

MR. KELLY: No, this one, it was a model for inverted annular, but you had a two pressure solution and looked at the ways.

MR. BANERJEE: It was a two fluid model.

MR. KELLY: Yes, but it's --

MR. BANERJEE: With the surface tension because he wanted to take into account that if you have different surface tension, say you did liquid nitrogen boiling or liquid water, but it just fell out of the two fluid model.

We didn't make a correlation. We just did it.

MR. KELLY: Right.

MR. BANERJEE: It was a pure two fluid model, and the instability of the interface was predicted simply based on the two-fluid model which gave you the wave lengths.

MR. KELLY: Right.

MR. BANERJEE: But George actually took this, and he made a mechanistic model.

MR. KELLY: I looked at, you know, more than 100 papers easily trying to find a model that I thought would fit well, and I compared a lot of them to this data, and none of them came out as well as this, because I didn't start out saying I'm going to develop a model. I started out saying I'm going to select a model, but this is how I ended up.

MR. BANERJEE: For example, in this model, there's no surface tension dependence. Now, clearly, even with water, as the pressure changes, you're going to get an effect because surface tension has an important effect on the waves.

MR. KELLY: On the waves, that's right.

MR. BANERJEE: As does the density difference.

MR. KELLY: Yes.

MR. BANERJEE: So, I mean, I wonder if this model will work over a wide range of parameters or not because it's not very mechanistic.

MR. KELLY: No, but I do have one point here at 70 bar.

MR. BANERJEE: Why is G in there? You'd think if it was an experiment in space, you'd still have inverted annular.

MR. KELLY: Yes, but this, the vapor film is buoyancy driven. You know, the force balance between the vapor film and the liquid.

MR. WALLIS: Buoyancy driven, okay.

MR. BANERJEE: You know, you get the same thing in a horizontal pipe where G acts 90 degrees to this.

MR. KELLY: Right, but now it's the film flowing up underneath the liquid pool, and so you do have the gravity.

MR. WALLIS: Okay, so now you're going to tell us how to predict delta. Maybe you need to do that.

MR. KELLY: Well, even before we get to that, we now have a model for wall heat transfer.

MR. WALLIS: You don't know what delta is except from the experiment. You don't have void fraction.

MR. KELLY: Right, but before we get

there, we've talked about wall heat transfer through the wall through the vapor film.

MR. WALLIS: This is K over delta times delta. The second is a constant, the second term is a constant. Delta is cancelled, the second term. Delta over delta.

MR. KELLY: That's basically true.

MR. BANERJEE: So it's just a density correction.

MR. KELLY: Yes. I didn't realize that, but you're exactly right. See, that's why we need peer review.

We have this wall correlation, but it's across the vapor film. In the code, that encompasses two heat transfer models, wall to vapor and vapor to interface. So how on earth am I going to get that? I don't know how hot the vapor is. I know it's somewhere between Tsat and Twall. I'm just going to partition it equally.

Say that the vapor is exactly halfway between the temperatures, and set the resistance to heat transfer the same between the wall to the vapor and the vapor to the interface. I know it's not right, but it gives me the same -- you know, I'm saying these two heat fluxes are equal, and then I'm just setting these two coefficients.

MR. WALLIS: Well, you could do a parametric thing where you couch this into different ways and so I sensed that the answer was to it.

MR. KELLY: Right, and you know, I would just be causing myself more trouble, and this is --

MR. BANERJEE: In the two-fluid model, doesn't this all sort of get calculated? You have a temperature --

MR. KELLY: The vapor temperature does.

MR. BANERJEE: Yes.

MR. KELLY: But I need these interfacial heat -- well, this is a wall heat transfer coefficient, and this is an interfacial. That's what I'm saying, what do I use for the interfacial?

MR. BANERJEE: But that would depend on I guess whatever you calculate as the interfacial roughness, right?

MR. KELLY: Right, and what I'm saying is I don't have enough knowledge to do that. I know the total resistance to heat transfer across the film, and I'm just splitting it into two equal resistances.

MR. WALLIS: How do you know the total resistance?

MR. KELLY: That's this model. Turn this

heat transfer coefficient into resistance.

MR. WALLIS: Delta, okay. So, it's all going to be tied together in the end?

MR. KELLY: Yes. Well, what I've done is put together a model where if --

MR. WALLIS: An interim model?

MR. KELLY: Yes, where if we have the right film thickness, and therefore the right void fraction, I get the right heat transfer. The previous incarnation of the ACRS highly criticized TRAC-P F1 Mod2 in doing CSA use study for quite often doing a pretty good job on the heat transfer, but being out to lunch on the void fraction. They're saying you can't have the heat transfer right and the void fraction wrong.

So here, if we have the right void fraction, that's an if, we'll get the right heat transfer.

MR. WALLIS: We are an incarnation?

MR. KELLY: Okay, we reconstituted. Okay, now I developed this model based upon tubes because I had good quality, steady state tube data. It's very hard to do steady state film boiling tests in a rod bundle, especially under these low quality conditions.

So, is it applicable? You got to ask that question. Basically rods and tubes, rod bundles and tubes are very, very different.

So what I did is something I call a frozen quench front approach. So, I take a reflood test and take one point in time and look at an axial snapshot of the conditions downstream of the quench front. So, I'm going to look at the axial profile of those wall heat transfer coefficients, but I want void fractions as well.

We don't have gamma densitometers on these, unfortunately. Instead we have DP cells. So what I'm going to have to do is infer void fractions from the DP cells, interpolate them in the axial direction to the locations of the thermocouples, in order to generate a heat transfer coefficient versus void fraction.

I'm going to use data from two FLECHT-SEASET tests. I reduce these, a six-inch per second test, a three-inch per second test, and three PERICLES tests. This dates from the time when I worked as a member of the CATHARE team in France.

We develop the same model --

MR. WALLIS: Except it's not point three?

MR. KELLY: Right. It went from .11 to .3, and that's really not surprising that it's higher in rod bundles. It's even higher in rod bundles than force convective flow.

This is what it looks like. This is a FLECHT-SEASET run. This is a Pericles run. What I've done is looked at snapshots at four different points in time when the quench front is just downstream of a grid spacer so that I have almost two feet before I hit the next grid spacer and the flow is completely disrupted.

So, I have all of the thermocouples here, and what you'll see is that as you get farther away from the quench front and the void fraction goes up, the film thickens. At some point when this nondimensional film thickness is around 35 to 40, you go through a regime change, and you go from something that looked like inverted annular to something that's, you know, whether it's a distorted slug --

MR. WALLIS: It's a lower heat transfer coefficient, though.

MR. KELLY: Right.

MR. WALLIS: It's got to be worse than annular.

MR. KELLY: Right.

MR. WALLIS: How could that be?

MR. KELLY: Well, because you're breaking up to things like dispersed floor film boiling. You no longer have that liquid smack up close to the wall. It's now dispersed.

MR. WALLIS: I think dispersed will make it better because the droplets would come closer to the wall. The furthest it can be away is when it's concentrated.

MR. BANERJEE: It's inverted annular thing actually, this goes looping around, you know.

MR. KELLY: So that's what's happening here. This is when you do a regime transition.

This was a model I developed for tubes, and that's -- I hate -- I wouldn't call it a correlation. It's more of a co-fit, but you see it worked pretty well for the Pericles test at 30 psi and the FLECHT-SEASET at 40.

MR. WALLIS: Okay.

MR. BANERJEE: What's the dark blue stuff in Pericles?

MR. KELLY: You mean here?

MR. BANERJEE: Yes.

MR. KELLY: Well, if you could see it, you'll notice there's these points down in here, too.

MR. WALLIS: It goes around.

MR. KELLY: So this again is the point

where, you know, there's some kind of flow regime change.

MR. BANERJEE: That's at a lower pressure or something?

MR. KELLY: Yes, this is at 30 psi and this is at 40. So, it's not perfect, believe me, but at least I looked at dry bundle data.

MR. WALLIS: It suppresses the turbulence somehow or something. Okay, you'd invent some concept.

MR. KELLY: So back to my little cartoon. We've now taken care of the wall to vapor heat transfer and the vapor to interface. The next thing, remember what I said the controlling process was, was the interfacial heat transfer from the saturated liquid interface into the subcooled liquid core. That's the controlling process, and that's what we're going to talk about now.

We had a discussion about the definition of

MR. WALLIS: That's how the subcooling exerts an influence.

MR. KELLY: Right. Vic and I had a discussion at one of these meetings about the definition of ad hoc and whether or not it's bad.

This is where you can go towards being an ad hoc model, but I'm going to do my best to base it on data

MR. RANSOM: Ad hoc means special purpose.

MR. KELLY: Exactly. I actually did go back and look that up from the dictionary after our discussion, and you were right. If my Latin weren't so rusty, I'd say it means to this or something.

So, we're going to talk about interfacial heat transfer between the saturated interface and the subcooled liquid core. So first what I want to do is make an observation, and that is that when the liquid is significantly subcooled, most of the wall heat transfer just simply goes across the vapor film and then vents into the liquid core. So, it's primarily increasing the sensible heat of that liquid core.

The thickness of this vapor film is going to be self-regulating, if you will. It's going to adjust itself so that the heat transfer across it matches what is possible for this subcooled liquid core to absorb.

So, you know, leap of faith here. What I'm saying is the wall heat flux, which I've said is equal to this heat transfer coefficient times Twall minus Tsat, is approximately equal to the heat flux going into the subcooled nuclear core.

MR. WALLIS: There's no vaporization occurring at all?

MR. KELLY: I'm saying it's small.

MR. WALLIS: It's small. You could probably estimate it and show it's small. It would be useful.

MR. KELLY: Yes, well, you're going to see in just a second when it isn't small. That's very much supposed to be approximate.

What I'm going to try to do is take this database and infer what this interfacial heat transfer coefficient might be.

MR. WALLIS: So are you saying essentially that the vapor is formed there, but it doesn't really do very much except that the main thing is that there's a space there?

MR. KELLY: Exactly.

MR. WALLIS: The velocity of the vapor doesn't matter very much?

MR. KELLY: Not too much. The waviness of the liquid film does, but the main thing is what happens in the liquid core.

MR. WALLIS: There's nothing in your slide that indicates what the delta is of the vapor film or anything?

MR. KELLY: None whatsoever.

MR. WALLIS: That makes it a little suspect. It works just as well for downflow?

MR. KELLY: Haven't tested that yet, but that's something that needs to be done.

So, what I'm going to do is get an estimate of this, and I have to emphasize estimate a priori. I don't even know the order of magnitude of that, and if you go and look at the literature, people do all kinds of things. You know, from using Dittus-Boeter, saying that well, you know, with this turbulent liquid, and we're just going to pretend the interface is a wall. Well, I know that's not right because the wall --

MR. WALLIS: Well, the turbulence is created at the wall. It is not touching the wall. I don't see how it can be turbulent in the same sense it would be if it filled a pipe.

MR. KELLY: Well, what they're saying is that before you got to the quench front because, you know.

MR. WALLIS: Okay, then it retains the turbulence it had?

MR. KELLY: Right, and so they'll use the Dittus-Boeter model for this.

MR. BANERJEE: Or the alternative is when you have high vaporization with drag on the interface generates.

MR. KELLY: Right, that's another one, but you don't see models really in the literature doing that. The other thing people will do is they'll say it's a transient --

MR. WALLIS: There must be something to stop the liquid slowing down and falling back down again. There must be something pulling it up there.

MR. KELLY: That's the interfacial drag.

MR. WALLIS: That comes from gravity. That's the delta row part.

MR. KELLY: Right.

MR. WALLIS: So at least you know that the vapor holds up, keeps the liquid going.

MR. KELLY: Exactly, but you have to have enough vapor to do that, and that's when we get --

MR. WALLIS: It's where your delta row G comes from maybe.

MR. KELLY: Exactly.

MR. WALLIS: It's a measure of interfacial share which is also a measure of the creation of turbulence and mixing.

MR. BANERJEE: But then if you have down

fluid which changes.

MR. KELLY: Yes, but I mean, I can go in the literature, and people do transient conduction solutions. You know, developing boundary letters and flag plates, all kinds of things.

Like I said, I don't even know the order of magnitude of this thing. So, what I'm going to try to do is figure it out, you know, in a very approximate way.

So, what I'm going to simply say is, based upon this approximation, it's equal to the wall heat transfer over Tsat minus T liquid.

MR. WALLIS: Well, you're going to take where it's going to and get some way of interpolating between the two, essentially.

MR. KELLY: Well, first I'm going to get an estimate of what it is.

MR. WALLIS: Yes, but you're going to get where it's going to and then you've got the other part, and you can add them together.

MR. KELLY: Right. So, again, I took all of the subcool data, and I got what I'll call an inferred Nusselt number for this interfacial heat transfer.

MR. WALLIS: Well, that's the constant

we're adding. This is the constant you want, isn't it? You take the curve which is coming down like this and you add a constant to it. It goes up to --

MR. KELLY: Yes, but this is a very different curve.

MR. WALLIS: Different curve than what we saw before?

MR. KELLY: Yes, this is completely different. That's why I was befuddled for a second. This, I'm now taking the wall heat flux and dividing it by Tsat minus T liquid, where T liquid comes from an energy balance, or basically my equilibrium quality, coming up from the bottom of the test section. I'm turning that into a Nusselt number. So, this is interfacial now, is what I'm trying to do.

The reason for this tail has very little to do with the film thickness because it's turned around. The highly subcooled cases out here, this is when the void fraction is ten percent, is out here. This is where the liquid has almost reached saturation. This is void fraction of 70 percent. So, in a sense, exactly flipped around.

MR. WALLIS: Okay.

MR. KELLY: My approximation that I made is reasonable out here, and the approximation is that almost all of the wall heat flux makes its way into the liquid core with very little vapor generation. As the liquid subcooling goes towards zero, that assumption falls apart.

I mean, obviously when the liquid saturated, all of the wall heat transfer is generating vapor, and none of it is going in. So, what I'm doing is dividing my denominator is going to zero, and that's the main reason you get this exponential shape.

The point of this is that over a pretty wide range where I think my model, that assumption is more or less valid, it almost comes to a constant value where the Nusselt number equals 200.

Now, and that's similar to what Saha and Zuber got for subcooled nuclear boiling in the conduction control regime where they came up with a Nusselt number of 455. Is this absolutely right? No, but is it a whole lot better than guessing something? Yes.

So, this is what I'm going with, and again, there's no noticeable mass flux effect, and within the assumptions, it does a pretty reasonable job.

MR. WALLIS: Well this number 200 means there's a characteristic length which is much shorter than one percent of the actual length.

MR. BANERJEE: It suggests its turbulent.

MR. WALLIS: Where is it coming from if it's not coming from turbulence, and why should it be constant? It should depend on some mechanism of some sort unless it's a surface tension. Anyway, you should proceed. This is very interesting.

You're getting towards the end, I think.

MR. KELLY: Right.

MR. WALLIS: Because we need to finish.

MR. KELLY: So, I'm going to compare it to the Winfrith data so I can look at the effect of different pressures and also it's always better, if you can, to use more than one experiment.

MR. WALLIS: Okay, so you're going to pull all of this together.

MR. KELLY: And that has the same trends and comes to about the same value.

MR. BANERJEE: But the only points there are at the lowest pressure, right?

MR. KELLY: Right, that's true. But this is five bar and Fung was one bar. So, at least that helps a little, but you're exactly right.

Wall to liquid radiation, we can dispense with that. We have no way of knowing what it is, so

I'm just going to use concentric cylinders.

MR. WALLIS: That's probably what it is.

MR. KELLY: And that's as close as I can guess.

MR. WALLIS: Liquid is a pretty good absorber.

MR. KELLY: We've gotten through everything except interfacial drag. I'm not going to talk about the break-up criteria, although I already did some.

How are we going to get the interfacial drag? Well, we have a problem, one of many problems. This is where ad hoc comes from.

MR. WALLIS: Adjust to enough to keep the liquid going.

MR. KELLY: Yes.

MR. BANERJEE: Why don't you just use Graham's?

MR. WALLIS: No, it's inverted.

MR. KELLY: I'm going to talk about that in the condensation presentation. The fact velocities are unknown. The liquid is subcooled, so even in these very simple, steady state experiments, I have no way of knowing what the actual vapor floor rate is. So, I don't know what my relative velocity is.

I know the void fraction, so I can know what the buoyancy force, what is, and consequently what the interfacial drag force was, but since I don't know the relative, I can't get the interfacial drag coefficient.

MR. BANERJEE: Doesn't your two-fluid model give you the velocities at the end?

MR. KELLY: Well, yes, but I'm trying to do this from data, okay?

MR. WALLIS: It seems to me that that's subliquids there, the liquids in the middle. If you don't have enough drag on it, it's going to slump down within the film.

MR. KELLY: Exactly.

MR. WALLIS: So that the drag -- you've got a kind of adjust it in order to keep the liquid going.

MR. KELLY: Exactly, and so what --

MR. WALLIS: Make it enough to keep the liquid going.

MR. KELLY: Well, what is that?

MR. WALLIS: It's the weight of the liquid.

MR. KELLY: That's the force.

MR. WALLIS: Because it's not changing.

It's not accelerating.

MR. KELLY: Right, and that's what I just said, is I know the buoyancy force needed, so that gives me the -- I need the F's of I that I'm going to -- and I don't know -- I know the --

MR. WALLIS: It adjusts itself in order to keep the thing going.

MR. KELLY: I know the force, but I don't know the REL, so I can't get the coefficient.

MR. WALLIS: You don't need the coefficient. You just need the force.

MR. KELLY: In the code, I can't say, you know --

MR. BANERJEE: But you can put Tow W per unit area there.

MR. KELLY: Yes, but if I say Tow W is equal to alpha, one minus alpha --

MR. WALLIS: That's what you need, yes, do that.

MR. KELLY: G delta row, that means no matter what amount of liquid it is, I'm going to have enough force to hold it there.

MR. WALLIS: That's right. That's right.

MR. KELLY: Well --

MR. WALLIS: It adjusts itself. I mean,

it doesn't change -- the alpha doesn't change.

MR. KELLY: Physically, you're speaking truth.

MR. BANERJEE: It sounds like drip flux models.

MR. KELLY: In numerical space, we're talking loads of problems.

MR. WALLIS: Okay, let's move.

MR. BANERJEE: Isn't that how you get your coefficients for the drip flux model? I mean, it's the same thing.

MR. KELLY: Yes, but they're applied to a Vrel. I don't just stick a force in. I use the actual Vrel calculate by the code in computing the force.

MR. WALLIS: The force is what you need to fight the buoyancy of the bubbles.

MR. KELLY: Right, yes. So, I don't have any way of calculating these from the data. So, what am I going to do? Again, I want to make some observations.

So, I'm going to go out and look at some single phase, you know, pressure drop tests and ducts with either grooves or wavy walls. I'm talking about grooves that are orthogonal to the flow so it kind of sort of looks like waves. This is the friction factor follows that for its move parallel plates for Reynolds numbers. Reynolds number is between 200 and 400. It looks like the smooth laminar flow.

When you've got a higher Reynolds numbers, the friction factors approach a constant value, like a fully rough turbulent number where that's a function of the amplitude to the wavelength ratio of, you know, whether the ridges or the waves.

Well, that helps. I know an expected behavior, but it doesn't give me any numbers yet, and then I went and looked at some horizontal stratified flow data from Andritsos and Hanratty. Again, they came up with the interfacial friction factor being a function of the wave amplitude.

Okay, well, I could have guessed that, but of course, I don't have models for the wave amplitude, and nothing that I know would justify trying to put something like that in.

They went further, and they showed that at least for their case, the interfacial friction factor ended up being proportional to the vapor Reynolds number. So, what they're saying is the vapor velocity has something to do with forming these waves, and that takes you to this fully rough turbulent condition.

So, I'm going to propose a model, and this is what I want it to do. I want it to reduce to the smooth parallel plate for low values of the vapor Reynolds number, okay? I want it to be linearly proportional to the vapor Reynolds number at high Reynolds numbers. You know, when I expect it to be in this fully rough turbulent condition where surface waves are what are important.

These next two conditions fall out by me looking at other models. What I want is for the vapor Reynolds number to monotonically increase the film thickness. There are some models that don't meet that condition. I want to avoid unrealistically high vapor Reynolds numbers like you would get if you just assumed it was smooth, parallel plates.

So, this is again, you hate to call it a model. This is what I'm using. It's a maximum of, you know, 24 over the Reynolds number. That's a smooth parallel plate, and the simple function of a non-dimensional film thickness.

MR. WALLIS: Where did that function come from? Comparison with data or something?

MR. KELLY: I'm going to tell you.

MR. WALLIS: The next figure. I was trying to figure out. I'm on the next slide.

MR. KELLY: Yes, well, don't go there yet.

MR. WALLIS: Okay.

MR. KELLY: The .7, the power, came about because that's the power that gives me a linear, the interfacial friction factor increasing linearly with Reynolds number, and I'll explain how that comes about in a second. This coefficient --

MR. WALLIS: Has to do with the one-third power and all that stuff?

MR. KELLY: No. It's much simpler than that. It matched the interfacial drag at the point where I expect this liquid column to break up and go into the next regime. That was my degree of freedom that I chose in order to get me a smooth transition. This is what results when you do that.

MR. WALLIS: Now, these are data points here?

MR. KELLY: Yes and no. Each one of these points is one of Fung's tests.

MR. WALLIS: It is, okay, based on the data, based on a test.

MR. KELLY: Yes, now it should, on the next slide, you'll see an inferred in front of this Reynolds number, because you remember we don't know the vapor Reynolds number. If I did, I could have calculated these things, but now I have a model for interfacial drag.

I can go and solve my two fluid equations given the void fraction that was measured in these tests. So, like you were saying, I know the buoyancy force, so I know the interfacial friction force. I have this model for interfacial friction.

MR. WALLIS: That's the .72 power that's on there?

MR. KELLY: The .72 power on the non-dimensional film thickness makes this be Reynolds number to the one, okay? So, what I did is I back-solved from the momentum equations for a given film thickness, what the vapor velocity would be using my model. So, I solved for these Reynolds numbers and then plotted what the interfacial friction factor was.

MR. WALLIS: That's right. That's why it's such a straight curve, I guess.

MR. KELLY: This is the 24 over RE, which is exactly what you expect. This is where that max kicks in, and it becomes a function of the film thickness. The derivative of this with respect to the Reynolds number gives you a slope of one. I mean, it's linearly in proportion. That's what I'm trying to say.

The same kind of plat, except here I was more truthful, or at least more careful, and said inferred Reynolds number. So, I'm back-calculating that Reynolds number based upon my model and plotting it versus non-dimensional film thickness.

MR. WALLIS: And all this is what went into the code that gave the results that you showed at the very beginning?

MR. KELLY: Yes, this is all that went into that one regime, and that's just one of many regimes in reflood.

MR. WALLIS: This is why it took so long to grind one point?

MR. KELLY: Yes, and it's also why it's, you know, something that hasn't yet truly been solved, even though we've been working in the reactor safety area since the ECCS hearings, '73.

MR. WALLIS: All this is based on your fantasy about what's happening?

MR. KELLY: Right, but in a two-fluid framework.

MR. WALLIS: It's sounds nice, nice story.

MR. KELLY: So, that's what we have, and you know, basically what I'm saying is we're going to do some future model development.

MR. WALLIS: Since this is so perfect, we should stop the Penn State tests because they might disprove it.

MR. KELLY: Well, they deal with some of the other regimes, which are even more uncertain than this, and the regime that comes in between.

MR. WALLIS: Now, this is the one where the quench front is. This is the most important part to get right, isn't it?

MR. KELLY: It's hard to say.

MR. WALLIS: Isn't that why you were doing it?

MR. KELLY: Yes, because it does help drive the velocity of the quench front, and that gives you your vapor source, but if you mess up the dispersed flow film boiling by a lot, then your PCT is directly affected by that one.

MR. WALLIS: Has this been written up in a form that can be peer reviewed?

MR. KELLY: Not quite. Actually, I didn't put this model in the code. Weidong did for me. What I did, I wrote up a fairly brief description of all the models and handed that off to Weidong, and Weidong implemented it for me.

MR. WALLIS: Yes, but it's going to be

written up as a NUREG or something?

MR. KELLY: Yes.

MR. BANERJEE: Well, before that, a draft should come to us, I think.

MR. WALLIS: I don't know. We don't have to do a peer review for everything.

MR. KELLY: I agree, that's exactly what I want to do.

MR. BANERJEE: This is the sort of thing that you could publish, so the best peer review is to send it to Journal of Heat Transfer and see what they say.

MR. KELLY: And I will. It's just, you know, this is one regime out of many, and as you can see, I'm already working on tube condensation, and you know, so we're balancing time here.

But this work is just about at the end, and one of the main things I have left to do is the documentation, and that's where I will try to publish something from this.

MR. BANERJEE: There is a lot of stuff beginning to come out of direct numerical simulation with the formable interfaces. Clearly, of course, these are coming out of JFM. There are a couple of papers in JFM and so on.

There's a group in the AETH 5 as well. We're looking at condensation with George. So, many of the issues, assumptions you are making here will be directly available from DNS. Their actual direct solution of the stuff is not approximations anyway. So, it would be useful to see whether any of this actually falls up because you can't measure these things easily. Certainly you can calculate them in codes.

MR. KELLY: When I look at how chaotic the structure is --

MR. BANERJEE: Well, they're fully turbulent.

MR. KELLY: Yes.

MR. BANERJEE: I mean, just for the regimes where you have interfacial waves, not when they're breaking up. Even that can be done now by DNS, but certainly the regime with interfacial waves are being calculated and being published after peer review in DFM.

MR. KELLY: Yes.

MR. BANERJEE: So we should look at that.

MR. KELLY: Okay.

MR. WALLIS: Is there any further data that you can use to check your model?

MR. KELLY: Well --

MR. WALLIS: Besides what may come out of Penn State? Maybe the Penn State results are the answer.

MR. KELLY: One of the things, when we, you know, decided to do the Penn State test, one of the things I insisted upon was trying to measure the void fraction more accurately, and so we did this with delta P cells every three inches over about three to four feet in the middle of the bundle, so that we can have a little bit better idea of the axial profile of the void fraction.

So, that ought to at least give us heat transfer coefficient versus void fraction in this regime whereas now I'm having to interpolate over one foot delta P cells, which is insufficient. Unfortunately, we didn't have enough money to put a gamma densitometer on that test because that would have been better, and would have helped support it.

MR. WALLIS: I was thinking about that because the DP cell measures pressure drop, and there's a friction. There's a wall of friction term in there. That interface friction is enough to hold up the liquid, which is the hydrostatic term you want to get. Presumably, the wall friction is the same order of magnitude. So, you may have trouble taking void fraction directly from DP cell.

MR. KELLY: Yes, they're not too bad when you're in a highly subcooled, low void fraction regime, and you're in a place where there's no grid spacer.

MR. WALLIS: But now since you have a way of calculating pressure drop with your model.

MR. KELLY: You can compare that.

MR. WALLIS: You can put that with the DP cell.

MR. KELLY: Right.

MR. WALLIS: That will make the day feel even better.

MR. KELLY: Yes. So, RBHT will help, but one of the big -- I mean, the model, there are two models in here that are most uncertain. That's the interfacial drag, which we just went through, which I think has the right behavior, but I wouldn't swear to the magnitude, and the interfacial heat transfer for the saturated interface to the liquid core. That's the one I really pulled out of the air, remember?

That one I would love to have some data for. I am going, when we get the RBHT data in house, I'm going to look for it, and what I mean is I can make the same kind of assumptions, but I don't know what the liquid temperature is because I can't do, you know, the easy integration of the test section.

We have fluid thermocouples hanging down from the grid spacers. If those fluid thermocouples can measure liquid subcooling while it's in film boiling, then I can at least check this estimate of the magnitude of the interfacial heat transfer, but this is an area where I would like to do a smaller separate effects test, and we just have to see where the agency priorities are and whether this is something we can spend more money on or not.

MR. WALLIS: Okay.

MR. KELLY: And believe it or not, because I started 15 minutes late, we got lucky.

MR. WALLIS: I was just going to say, you have done extraordinarily well in terms of my experience with you. You've taken exactly the time allotted.

MR. KELLY: I can always talk more.

MR. WALLIS: In spite of having a lot to say. That's very good.

MR. KELLY: Thank you.

MR. WALLIS: And you're going to do the same thing this afternoon. You're going to be on time, right?

MR. KELLY: Yes.

MR. WALLIS: Good. So it's now time to take a break unless my colleagues have a great desire to say something.

We'll take a break until 1:30. Thank you.

(Whereupon, the foregoing matter went off the record at 12:31 p.m. and went back on the record at 1:32 p.m.)

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

(1:32 p.m.)

MR. WALLIS: We'll come back in session, and we'll hear the next presentation by Joe Kelly. Looking forward to it, as always.

MR. KELLY: And I'll be speaking about the tube condensation model in TRACE, and my coworker on this is Birol Aktas of ISL.

I'm going to split it into, again, three parts. The first part is going to be fairly brief. There's going to be one slide showing you what the diagram of the ESBWR is. There's a reason for this, is the need for tube condensation is to model the intube condensation for the isolation condenser and the passive containment cooling systems of the ESBWR.

I'm going to try to briefly explain what's in TRACE now, and these are the legacy models from TRAC/PF1-Mod2. Then before you do any model development, you always have to answer your question, do you need to. So, the first thing I'm going to do is what I'll call an investigatory assessment where I try to determine if the current models are good enough.

Of course, if they were, you wouldn't see this other topic, and that's where I'm going to describe the model development effort. This is a work in progress. I just started this not long ago, and so you're only going to see a partial description of the following models.

This to some extent is the inverse of what I was talking about in the last presentation.

MR. WALLIS: I was just going to say, you're such an expert in modeling wall friction, interfacial shear, wall fluid heat transfer, interfacial heat transfer. With those four models, you could fit almost anything.

MR. KELLY: Right. You could also not fit just about anything, too. So, you're familiar with the ESBWR design. Just to show you, ICS, again they're basically vertical tubes, heat exchangers, sitting in a pool of water. We're going to be talking about the condensation in the inside of the tubes. The ICS tends to be more at higher pressure and pure steam driven from the reactor pressure vessel, delivering the condensate back to the pressure vessel.

The PCCS system, the same design heat exchanger except for a non-condensable gas vent. It takes its intake from the dry wall in the containment. So here, you're primarily talking about condensation with non-condensables.

Air normally, but in case of a severe accident, you would have a hydrogen steam mix. So, driven by the pressure difference between the dry well and the wet well, you have the noncondensable steam gas mixture driven through the condenser.

The condensate feeds back to the suppression pool -- excuse me, the condensate comes to this little holding tank back to the vessel. It's the old design where it was drained to the GDCS pool. That was an SBWR design.

This line is a vent path for non-condensables. It kind of burps the noncondensables out periodically.

MR. WALLIS: So presumably there's very little pressure drop between the containment and the suppression pool?

MR. KELLY: Right. So now we're going to talk about the current models in the TRACE code. So, when you go to the condensation regime, the effective wall heat flux is the super position at two heat fluxes, one from the wall to the vapor and one from the wall to the liquid. This is just the way it's currently done, and if you're in condensation, it ends up using the sync temperature being Tsat instead of the vapor temperature for what I'll call the wall to vapor component.

MR. WALLIS: Why do the two components if only one component is on the wall?

MR. KELLY: Good question, and in the model development, there will only be one. This is what's in the code now.

MR. WALLIS: So one of these H's is zero, presumably?

MR. KELLY: No.

MR. WALLIS: No?

MR. KELLY: No.

MR. BANERJEE: Except for drop condensation.

MR. KELLY: Right, which this isn't.

MR. WALLIS: I don't get that.

MR. KELLY: This isn't at all. It adds the two. Here's the wall to vapor and here's the wall to liquid. It uses this waiting factor which is a function of quality, and I'll explain this in a second.

You can pretty much forget about the vapor convective term. This is very, very small. The vapor condensation term, this tends to be a large number.

MR. WALLIS: This doesn't make sense to me at all.

MR. KELLY: I'm not going to defend this. This is what's there now.

MR. WALLIS: What? Why is this in there?

MR. KRESS: That's just the way the guy decided to do it because he didn't know how else to do it.

MR. WALLIS: Didn't know how else to do it, okay.

MR. KELLY: This H liquid convective, that will end up being, if you will, a two-phase convected heat transfer coefficient, and so this, if it were right, this is all you would need, but what they use for that model --

MR. WALLIS: It's like the Chen correlation for boiling. So, adding a boiling effect to a convective effect. Here you're adding a condensation effect to a convective effect.

MR. KELLY: Kind of but not really because this is ramping between them.

MR. WALLIS: That's not really what's happening.

MR. KELLY: But it's funny --

MR. WALLIS: This is the interface and the convection is at the wall.

MR. KELLY: I agree. I agree completely.

MR. WALLIS: Okay.

MR. KELLY: But it's funny you should mention the Chen correlation.

MR. WALLIS: Chen is a subscript here.

MR. KELLY: Yes, because that's what's going to be used for this convective heat transfer.

MR. WALLIS: He just borrows anything from anywhere and uses it.

MR. KELLY: And in this waiting factor, the quality here, that Chen, the .71, that's according to the database, that's the highest quality that you're supposed to use that model at. That's why --

MR. WALLIS: The boiling model or the --

MR. KELLY: No, the forced convective part, what I guess they'll call the macro term.

MR. WALLIS: For boiling?

MR. KELLY: Well, it's for forced convection evaporation.

MR. WALLIS: It's evaporation. It's not condensed.

MR. KELLY: Right.

MR. WALLIS: So they've got the same condensing as for boiling, the X Chen?

MR. KELLY: Well, this they used, they say we won't use the correlation at a quality higher than this because that would exceed its database, and that's where this ramp comes from. So, if the quality is one, give it pure condensation to the vapor, and I'll show you the models for that.

If the quality is between one and .71, you ramp between these two, where this is a two-phase conductive term. Below .71, this is all you get. Then it's even funny, because if you look at the definition of quality, it uses a quality that they take a static mixture enthalpy and convert that to a quality. It's now a flow-in quality.

That's actually not a bad idea because a flow quality in these transient to fluid codes can go between zero and one, time step to time step, especially in low flow conditions.

MR. WALLIS: They're extraordinarily different.

MR. KELLY: Yes.

MR. WALLIS: Going much faster than the liquid, and the influx quality, which is a bad use of the term, is nothing like the same as the flow-in quality.

MR. KELLY: Exactly, and here it's being used as a weighting factor between these two.

MR. WALLIS: Why won't these guys show the view?

MR. KELLY: Now, that I don't know. I wasn't at the NRC when this work was done.

But now let's talk about the wall to vapor condensation heat transfer coefficient, the HV subscript conv that I have. This made up of two correlations with a weighting factor. The first, and I'm just using the terminology that's straight from the theory manual, is the good old Nusselt formula for laminate film condensation.

You know, it's an analytical solution for condensation on a plate where the L, the denominator here, is the length of the entire plate. So, this is for laminate film condensation.

They did realize that that was not applicable to turbulent films, and they found an empirical formula which I had trouble tracking down. I now know it's by Grigull, and it's from several, like maybe the fourth edition.

MR. WALLIS: Reliable, right.

MR. KELLY: Of Creef's book. So, it's the same kind of formula except, you know, the L and delta T's in the numerator instead of the denominator. Again, this is the characteristic link scale. It should be the entire --

MR. WALLIS: Is this the condensation drive only by gravity?

MR. KELLY: That's correct. No effective interfacial shear.

MR. BANERJEE: On a vertical plate.

MR. KELLY: On a vertical plate, and we're going to make it even more interesting. This is supposed to be the length of the entire heat transfer surface. We use a node length.

MR. WALLIS: Oh, come on.

MR. KELLY: No, that's true. It's exactly what's done in TRACE, and until --

MR. WALLIS: Everything gets re-established every node?

MR. KELLY: Yes.

MR. WALLIS: This is what's done in TRACE?

MR. KELLY: Yes.

MR. BANERJEE: It's from the surface renewal model.

MR. KELLY: That's not bad, and until 1996 or something, and we started doing AP600 and SPWR, this was what was in RELAP, too.

MR. KRESS: It changes every time we change their node size.

MR. KELLY: Yes.

MR. WALLIS: That's okay. You can tune it then.

MR. KELLY: I mean, they try to do some physics in a sense, so they wanted to wrap between a laminar to a turbulent correlation as the heat transfer surface. You don't ever get nodes. To be fully turbulent, this has --

MR. WALLIS: Maybe the H was so big, it really didn't matter too much what it was? H was so big without condensers, it really didn't matter what it was.

MR. KELLY: That's probably true, and you know, it wasn't the mission of the TRACE code when it was a large break LOCA code only to look at this kind of stuff.

MR. WALLIS: Just like ATT.

MR. KELLY: It doesn't completely excuse bad physical models, but you're right. In RELAP now, they have replaced this, but it's a user input. You tell it the length of the surface.

MR. WALLIS: But it's not condensation that you care about in a break, in a typical PWR. You don't get condensation. It's flashing and boiling.

MR. BANERJEE: Well, you get a condensation on the --

MR. KELLY: On the ejected ECC.

MR. BANERJEE: The ECC.

MR. WALLIS: But that's so big, it probably doesn't matter too much.

MR. KELLY: This is wall condensation. You're right. For a large break loca, this isn't --

MR. WALLIS: This is the danger of having a bad model which doesn't make any difference for certain purposes which you accept, and then you inherit it and use it for a reactor which depends upon condensation to work.

MR. KELLY: That's right. Now the other term was the wall convective stuff, and that's where we talked about Chen, which is really this Dittus-Boeter in this flow factor. The flow factor is a function of the Martinelli parameter. What it really is is nothing more than the ratio of the hydraulic diameter of the film thickness.

Okay, so physically this is not a bad model, and it does somehow put interfacial shear in, but only in a round-about way. It takes a maximum of naturally two natural convection correlations, which have a Grashof number, which once again end up using the node sizes of characteristic length.

Now we're going to talk about interfacial heat transfer.

MR. WALLIS: But you can't do that. What's the length in the Nusselt number?

MR. KELLY: Well, if I remember, for the natural convection models, they end up using the node size as well. So, for the turbulent when it cancels and for the laminar one that almost cancels, but I mean --

MR. WALLIS: You have the same dimension in the Nusselt numbers and the Grahof numbers.

MR. KELLY: Right, and that's what they do.

MR. WALLIS: They don't. They have the hydraulic diameter of the Nusselt number, and they have a length for the Grashof number.

MR. KELLY: Ah, you're right. I'm sorry. See, I get these codes mixed up. We just had the L's, so they cancel.

MR. WALLIS: Yes.

MR. KELLY: And I copied this out of the theory manual, so unless I copied it wrong, that's completely wrong.

MR. WALLIS: I seems to be.

MR. KELLY: Yes.

MR. WALLIS: Okay, so you're going to

replace it with something good, right?

MR. KELLY: Well, we're going to try. Now, I'm not going to explain the entire interfacial heat transfer package, for the same reason, that we'd be here, you know, forever, but I am going to show what the code has currently for the non-condensable gas effect.

It uses an empirical model by Skover and Rodivilin, and it's developed for a cross-flow of an air-steam mixture on a liquid jet, okay? This has nothing to do with wall condensation whatsoever.

Here's the formula. What it does, it decrements the normal liquid interface heat transfer coefficient, and there's lots of limits on these things because obviously if you have a liquid mass flux in the denominator, that can go to zero.

So, this does introduce a non-condensible gas effect by putting it on the interfacial heat transfer coefficient between the liquid and the interface, but that only affects the condensation if the condensation is drive by heat transfer from the liquid to the wall. If condensation is driven by the H, the con term, what I'll call the wall of the vapor condensation in this model --

MR. WALLIS: There isn't any. There isn't any direct condensation.

MR. KELLY: Well, there shouldn't be, but there is in the TRACE model.

MR. WALLIS: I see, okay.

MR. KELLY: Then in that case, --

MR. WALLIS: There's nothing real with this. This is so bizarre.

MR. KELLY: See, if I set the bar low enough, I can make it over it.

MR. WALLIS: This bar is buried.

MR. BANERJEE: Maybe we should look at the whole heat transfer packet.

MR. KELLY: Yes.

MR. WALLIS: Okay.

MR. BANERJEE: In TRACE.

MR. KELLY: Well, we're going to get there. You know, eventually, and that's why I don't want to spend my time rewriting the entire constitiative models in the theory manual, when I have the expectation the bulk of them will. We're just going to try to change them in an ordered way.

I think this model is overly complicated and unphysical. I found, when I'm looking at the assessment results, I found it was difficult to even be able to tell which model was being used.

MR. WALLIS: You sound like the ACRS reviewing some of the models we've seen.

MR. KELLY: Well, I should be. I should be here. My responsibility has changed in the group, and my responsibility is for the physical models, and I'd better be skeptical or they're going to be bad.

The analytical models, you know, applied inappropriately by using the link scale of the node size. There's no effective interfacial shear in the condensation heat transfer. That's plain wrong. The non-condensible gas effect, it's a model that would be questionable to condensation at best, and it's only applied to the wall to liquid part where I had the water vapor part, does not affect it at all.

MR. WALLIS: What about the wall to liquid part? There's no gas in the liquid. I mean, the wall to liquid has nothing to do with the vapor.

MR. KELLY: We'll have to talk about this some other time, about how they --

MR. BANERJEE: The vapor to liquid.

MR. KELLY: It's a mass transfer model.

MR. WALLIS: It's being killed about ten times over now, so maybe we should move on to something that's alive.

MR. KELLY: This is the assessment matrix

I used, okay. I wanted to look at laminar film condensation. I took the pure steam tests that were done at UCV by Kunz. That was the last series of tests.

For turbulent film, I just looked at one test from the NASA series. There's a lot of them to do, and for non-condensable gas tests and tests done at MIT by Siddique. What I have are the pressure -- you'll notice I did a nice little parametric on pressure from one to five bar.

The gas Reynolds numbers at the inlet, the Film Reynolds number is at the outlet. So, this just shows you what the conditions are.

MR. WALLIS: Dorse or something had a lot of data on condensation in tubes.

MR. KELLY: Well, I only put one here because that's all I need to show how bad the model is.

This is the noncondensable gas mass fraction, and I'm going to be using a lot of that data in selecting a revised model.

This is for laminar film condensation. The calculated heat transfer coefficient versus measured. Again, this would be perfect agreement. A few points are not bad. A few of the all pressure ones look reasonable.

Most of them are greatly underpredicted, and the trend worsens as the pressure gets higher. I was actually surprised any of these were anywhere close.

Now we're going to look at one of these tests, the one in three bar, I have heat flux versus position and heat transfer coefficient versus position. This is the data. You know, it's a nice, almost linear decrease in heat flux as you go down the tube.

TRACE underpredicts it but also has a rather strange behavior. This is almost pure vapor condensation so it's using like the Nusselt formula. This is where it's switching between two. This is the ramp down.

MR. WALLIS: You start off from the same point when it's all vapor.

MR. KELLY: Pretty much, yes. And there, must accept the characteristics of the scale being the node size. That wouldn't be so bad.

This is the ramp down to two-phase conduction, and this is the pure two-phase conduction.

MR. BANERJEE: Well, it's conservative.

MR. KELLY: It won't be when we get to

non-condensable gases.

The same test. What I'm plotting now, by phase velocity versus axial position. The blue curve is the vapor velocity, and it does decrease as you condense the vapor. Guess what? Here's the liquid film. This is at five meters a second.

MR. WALLIS: It's going faster? This is a TRACE prediction?

MR. KELLY: Yes.

MR. WALLIS: Gravity is pulling it.

MR. KELLY: Gravity is pulling it, and the wall is not. This results from the partitioning of the wall friction factor. Now, what people try to do -- well, actually in TRACE it's not as sophisticated as what they try to do in RELAP. Basically almost no wall drag here because the liquid just falls. It's wrong, and this is the result of it. This is the film thickness, and I think that's supposed to be microns versus -- let me think.

MR. WALLIS: It's millimeters.

MR. KELLY: No, this is millimeters, that's right.

MR. WALLIS: Is there any measure of the film thickness?

MR. KELLY: No.

MR. BANERJEE: Is the Nusselts solution?

MR. KELLY: Not in any of these, exactly. I plotted the Nusselt solution, which for this test should slightly overpredict because this doesn't have interfacial shear. So, laminate film with no interfacial shear.

MR. WALLIS: How about the interfacial shear due to mass transfer? By enough condensation rate --

MR. KELLY: Oh, you're very right. So, it would be thinner than this, but it's up here. It's not down where TRACE is calculating it.

MR. BANERJEE: Yes, but they overestimated the velocity.

MR. KELLY: Exactly, because there's no wall drag. So now we're going to talk about --

MR. WALLIS: Isn't this being contact with vapor or something?

MR. KELLY: Yes. The way TRACE does it, it calculates a friction factor and applies that friction factor to each phase using the phasic momentum flux. So, for the liquid, instead of one-half row V squared, it's one-half one minus alpha row V squared. It's that one minus alpha that kills the wall drag because with these thin films, the void fraction is, you know, .999, you know.

This is turbulent film condensation. It's one of the tests of Goodykoontz. Heat transfer coefficient versus axial position. You get a decrease as the vapor condenses and you have less interfacial shear, and the film thickens.

You don't see that in TRACE. TRACE gives an almost constant value, and that's a factor of seven.

MR. WALLIS: You shouldn't call it TRACE. You should call it TRAC or something.

MR. KELLY: Well, I was trying not to do that, but yes. But if you took the code today and ran this test, that's what you'd get.

Well, blowing this up so I can look at the TRACE result, you have the heat transfer coefficient that has this little funny dip in it. Plot the node length. Where this heat transfer coefficient went down, it's because the node size went up.

MR. WALLIS: Why is the node size varying so much?

MR. KELLY: Because I select, rather than a uniform node size, I picked a node size to match thermocouple locations. So, I could do, you know, easy comparisons to the data.

MR. WALLIS: So if you choose equal nodes, you can have a reasonable curve then?

MR. BANERJEE: No, you would be flat.

MR. KELLY: Yes. I wouldn't have seen this.

MR. WALLIS: Right.

MR. KELLY: Okay, now let's go to the non-condensable gas effect. Calculated heat flux versus measured heat flux, and the full test -- now let's look at just one, this run 24. So this would be near the two, the first measurement station.

MR. WALLIS: Which data set is this?

MR. KELLY: It's MIT.

MR. WALLIS: MIT.

MR. KELLY: Actually, the use of Goodykoontz is probably better data, and I will be using that primarily in the assessment, but I'll have some of these tests as well.

MR. WALLIS: Is this Berkeley, you mean, UCB?

MR. KELLY: Yes, because they went through four different graduate students, learning as they went along and making the experiment better every time, whereas this was the first graduate student doing this at MIT.

So, if we look at this one test, it's here. The second point here, third, fourth, all overpredicting the condensation rate, and then you underpredict.

MR. WALLIS: By a lot.

MR. KELLY: But you underpredict because you've condensed all the steam, okay? That's the one thing that's kind of nice about, you know, condensation with non-condensables. Once you get rid of the steam, you can't do more than that.

This isn't very good. I was surprised it was as close as it is, given the models that we saw, that don't make much sense, but it is non-conservative, and it's not very good. Those errors are unacceptable.

This just shows, you know, an axial profile of the heat transfer coefficient. Here's the data during a nice linear decrease, and here's a TRACE calculation. In this particular case, this node was the vapor condensation formula, a Nusselt. This was natural convection to liquid, and this was a two-phase forced convection. So, not very good.

I pretty much said everything on this. I don't think I need to repeat it.

MR. WALLIS: I'm not quite -- I would

think that the effects of vapor shield were much bigger at the beginnings, and this wouldn't be very good at the very beginning of the pipe.

MR. KELLY: In this case, you know, there's not any real film to drag that.

MR. WALLIS: I think if you were using a real two-fluid model, you wouldn't use missile at the beginning because the interfacial shear is bigger then.

MR. KELLY: That's right.

MR. WALLIS: Thanks.

MR. KELLY: So, I judged the models to be deficient, and we need to develop or at least implement a new model, and that may mean just selecting current models when you can.

The reason is again to do an M2 condensation that's applicable to the ICS and PCCS systems, the approach. It should work within a two-fluid framework. That's very important. When you try to shoehorn something in, you can really come up with things that don't make a lot of sense.

My opinion is the model should take advantage of the quantities that TRACE calculates. If you're going to use Nusselt, which makes sense if you have a laminar film. Well, the code calculates the film thickness. So, if you can calculate the film thickness correctly, a Nusselt heat transfer coefficient is this. The liquid conductivity over the film thickness. You don't need to evaluate this, you know, group of physical properties with, you know, the conductivity cube and a reg scale in it, which you don't know, because the code is integrating the conservation equations down the length of the condenser tube.

Why throw that solution away? Use it, but use it in a sensible way.

The model is going to first be implemented as a specialized package, which will be available to pipes that are labeled or have an attribute car condenser tubes. One of the reasons for doing this is I can put this set of constitiative models is without changing all of the TRACE results in every calculation that's ever done. That gets me out of trouble with Chris.

But, as I'm doing this, what I want to do is look for the models that could be generically applicable, and when those models have been proved to do so, they'll be migrated over to the normal constitiative package.

MR. RANSOM: John, a little bit of defense of the models that they did put in there. A lot of that was envisioned to be drop-wise condensation on horizontal tubes and, you know, things of that type.

MR. KELLY: Not this stuff, though.

MR. RANSOM: No, well, not the application, but the model --

MR. KELLY: The model is the film condensation. There are models for drop-wise. This ain't them.

MR. RANSOM: Well, you know, the only application in the past is primarily the condenser itself. You know, balance it with the components. I'm wondering if that isn't where a lot of that came from.

MR. KELLY: I don't know.

MR. RANSOM: There's no write-up or history of this in the TRAC manuals or theory manuals?

MR. KELLY: They try to explain what the models are, but they don't say why. They do talk about following films, and they don't say a word about condenser tubes. Of course, if it were a condenser tube, your length would be the diameter of the tube.

MR. RANSOM: Sure.

MR. KELLY: Certainly not the node length.

Back to my little cartoon. Just like we

had before, except now I have the liquid against the wall and the vapor out here. In the normal presentation and when we open the heat transfer textbook and you look at a condensation heat flux, it's a condensation coefficient times T wall minus Tsat. That's with the definition of the heat flux being negative when it comes into the wall.

Two fluid model, it's a little different. Now the wall heat flux is the wall to liquid heat transfer coefficient times T liquid minus T wall. I mean, it actually comes from the interface to the liquid, liquid to the wall. If this is the wall heat flux, the vapor generation or in this case condensation rate, is the sum of the two interfacial heat transfers divided by the latent heat, and I'll do a mea culpa for doing HFG when it's really the, you know, the delta H stars that John talked about yesterday.

MR. BANERJEE: Which we still don't understand completely.

MR. KELLY: Right.

MR. WALLIS: Well, this is like what we talked about yesterday. You've got the different temperatures you need for your energy balance than you need for your heat transfer, and you can get some weird things.

MR. KELLY: Right.

MR. BANERJEE: But as you've written it, it's correct here.

MR. KELLY: Right, and I wrote it this way just so we wouldn't go off in a long discussion that I thought would be secondary to what I'm trying to say. We can address that another time. These are just the interfacial heat fluxes.

So, you have the possibility here, if you have a cold wall, saturated interface, you pull heat from the interface to the liquid to the wall, and condense vapor here, the vapor on the other hand can be either subcooled or superheated. If it's subcooled, then the vapor will condense on the interface by itself from this interfacial heat transfer.

If the vapor is superheated, it would be trying to evaporate some of the liquid, and it would be the sum of these two that determines whether it's a condensation process or an evaporation process.

MR. BANERJEE: But that's correct, what you just said.

MR. KELLY: Yes, and that's the mechanistic part of the two-fluid that gives you the possibility to model something, but it's only correct if you have halfway reasonable models for these.

MR. WALLIS: Well, these heat transfer coefficients are affected by the simultaneous mass transfer.

MR. KELLY: Yes.

MR. WALLIS: It's not in here at all.

MR. BANERJEE: Yes, well, that would show up in whatever correlation they write for the heat transfer. We'll have to wait and see that.

MR. KELLY: Yes, this one.

MR. BANERJEE: Yes.

MR. KELLY: And likewise when you get to the effective non-condensables and you turn this into a mass transfer process here. Then the suction of the effect of the condensation of the liquid film affects the mass transfer rate.

MR. WALLIS: We know that if you have a cold enough film, you can get condensation at mach one.

MR. KELLY: Unfortunately, I'm not doing condensation with liquid metals, because I'm always getting the question about the accommodation coefficient.

MR. WALLIS: Right.

MR. KELLY: We don't have that in the code.

MR. WALLIS: You don't, no.

MR. KELLY: No. One other further thing I want to say is that this interface temperature, the assumption in the two-fluid code, at least all the two-fluid codes I'm acquainted with, is that this is saturation at the bulk vapor partial pressure, which is fine if there are no non-condensables.

MR. WALLIS: That's right.

MR. KELLY: You know, it's just the total pressure. If there are non-condensables, you know there's a distribution of non-condensables as you go towards the interface, and the interface is actually at a lower temperature because the partial pressure is lower there.

I had to think, did I say that right? But that's not the way the numerics in the code works. The code is always going to assume that this interface that drives these interfacial heat transfers, is at the saturation at the bulk partial pressure.

MR. WALLIS: Are you always going to assume that?

MR. KELLY: Unless we make drastic surgery to the code.

MR. WALLIS: We did assume that, but you'll have to investigate whether you can cobble up things to represent it this way.

MR. KELLY: And that's what you do. That's why, for the moment, let's pretend we don't have this, okay? We've only got wall condensation. All we have to worry about is this one.

Now, if you have non-condensables, that weight is going to be decreased by the mass transfer that goes on here. That's going to be the limiting rate process.

So, what you end up doing is coming up with what the rate limiting to the mass transfer is and giving it a modifier that you stick here. You increase the heat transfer resistance between this interface and the liquid.

MR. WALLIS: So rather than dropping TI, you change the H?

MR. KELLY: Right, because that works within the current numerical framework. You could switch the code to spread solves for this, but that's rather major surgery to the code.

MR. BANERJEE: Even more difficult than that because you'd have to actually calculate the local concentration.

MR. KELLY: Yes.

MR. BANERJEE: That's really difficult.

MR. WALLIS: Well, you'd probably use some kind of film theory or something.

MR. RANSOM: You're getting counter-current diffusion there as well.

MR. WALLIS: The noncondensibles to the bulk and the vapor has to diffuse through.

MR. KELLY: Right, There's a series of papers on the interface temperature model by Ghiassian at George Tech, if you're interested, but that would be fairly major surgery to the code. I can't do that within this timeframe.

So, if we're going to develop a model for film condensation, what do we -- we need to apply both pure steam and non-condensable steam gas mixtures for both following and sheared films. So, these are the models that are needed, and they're the same ones that we talked about before except now we have the addition of a non-condensable gas effect.

So, let's talk about a relatively easy one first, that's film thickness. What you need in order to be able to calculate the film thickness is if it's a falling film, all you need is wall friction. We're assuming the code can do gravity right.

MR. WALLIS: There's no pressure gradient in the masses?

MR. KELLY: Well, you end up, you know, the pressure gradient, if it's a falling film, we're talking about a wall and heat volume. So, it's just wall drag.

MR. WALLIS: For interfacial shear here?

MR. KELLY: Yes, here it's just wall drag. If it's a sheared film, then you have the pressure gradient, and also there is the balance between the wall and interface and buoyancy terms.

MR. BANERJEE: This is just a Nusselt solution.

MR. KELLY: Right. I'm just doing this because I'm going to use it in a second. So this is actually from a two-fluid momentum equation, and I'm just reducing it down, using the thin film assumption, and getting the solution that you're very familiar in getting the Nusselt result.

MR. WALLIS: Well, we know that when the film is thin, the interfacial shear dominates gravity. So, you're going to have to stop this somewhere down the pipe. It's certainly not valid with the top.

MR. KELLY: Right, but at the moment, all I'm talking about is a falling film. I'm going to make the leap to sheared film first, but I want to solve an easy problem first, get that solution, and start adding a complexity.

Okay, the reason I showed that is because of this. I put together a film thickness database, okay? So it's got a non-dimensional film thickness where a non-dimensional buys the Nusselt link scale which was on a previous slide, versus film Reynolds number, and a corrected data from a variety of sources. Then you have this pretty characteristic behavior. It's turbulent out here and laminar down here.

MR. RANSOM: Is the length scale in the Reynolds number the length of the tube?

MR. BANERJEE: No, it must be the thickness.

MR. RANSOM: The diameter.

MR. KELLY: Actually, it's --

MR. BANERJEE: It has to be the thickness.

MR. KELLY: Yes, it's four times gamma over new. Where gamma is the condensate for film floor rate divided by the weighted perimeter.

MR. WALLIS: Well, yes.

MR. KELLY: Okay, and if you use that --

MR. WALLIS: This is classical stuff.

MR. KELLY: Yes, if you use that definition, then the film Reynolds number becomes the same as the liquid Reynolds number, which is the liquid mass flux times the hydraulic diameter over viscosity.

MR. WALLIS: Hewitt and Wallis, whoever they were, '63, show exactly the same curve with exactly the same theory.

MR. KELLY: And now what I do is take those wall drip, film thickness measurements, and using what I talked, the previous derivation, convert it to a wall friction factor versus Reynolds number, okay?

MR. WALLIS: You can do that, too.

MR. KELLY: That's easy enough. It's just straight algebra.

MR. WALLIS: Who is that line there?

MR. KELLY: Oh, the ones --

MR. BANERJEE: These are all wavy.

MR. KELLY: That's some data by Chen from his thesis, and I haven't tracked down why those points are so far off, but I'll note it.

So, this is the model I'm going to propose. When it's a smooth laminar film, 24 over the Reynolds number, okay, it's easy enough. Flat plate stuff. I know that this is going to slightly overpredict the film thickness because it doesn't take account of ripples.

I know that the ripples affect the heat transfer because they effectively thin the film. So, when I go to the heat transfer model, I'm going to do something to cover ripples.

For turbulent, a simple, explicit approximation to Colebrook-White. This term doesn't really matter very much, but I'm using this because I'm hoping to eventually replace the wall drag model and track with it. You know, generically. What we have here now is not as good as this. This is a better approximation that what's in the code presently, and use a power combination.

MR. WALLIS: Well, how big is interfacial shear in the real device here compared with these? Are you going to show us that, because this whole thing is all very well if you just have a pretty quiescent vapor.

MR. KELLY: But we're starting somewhere. We're going to add where there's a complexity as we go, okay?

The answer is if you look at the UCF Kunz, the pure steam condensation ones, the interfacial shear from the standpoint of actually shear, is relatively small.

MR. WALLIS: What I mean is an ESBW, what sort of steam velocities do you get, and there's the interfacial shear report, because that's your application.

MR. KELLY: It's for the passive containment cooling system, which is with non-condensables where the condensation rates are not so high. There the interfacial drag is relatively small. okay? It's not negligible, but it's relatively small, and a lot of it would be the mass transfer part.

MR. WALLIS: I think it's something like Goodykoontz. Goodykoontz have very high velocities. I think his interfacial drag may have been dominant.

MR. KELLY: Definitely, and that's where in the isolation condenser system, which is pure steam and higher pressure, higher flows, that's where you might move more towards that regime, but now I'm also trying to put something in that won't -- where you won't fall off the end of the earth, if you go just a little bit outside of the bounds.

So, this is the wall friction factor versus film Reynolds number. The laminar model, the turbulent, and the power wall combination, and it looks pretty good. But so far, I've done all of this on a spread sheet and pen and paper. What about the code?

So Bill implemented it for me and tested it for me, and so this is a non-condensable film thickness versus film Reynolds number again. The blue curve is the TRACE we have today, and you see it greatly underpredicts the film thickness, and that's because of the wall drag partitioning.

Somehow it goes through some kind of laminar turbulent transition and wanders around, and heaven knows where it is. The black line is when Birol implemented the correlations I had on the previous slide, and there are actually a couple of glitches that we have to look at. He told me just a little while before my presentation that oh, he found a mistake, and the results get better, but I didn't have time to change the slide.

MR. WALLIS: In the laminar region, you're not going to present a kink like that.

MR. KELLY: No, there's an implementation, something wrong, and we'll figure that out. These are pretty much hot off the press. This is what I'm working on, you know, as we speak.

Now we're going to talk about sheared

films. What I want to do is somehow select an interfacial friction model applicable to co-current downflow. I had one database that I could easily get to cover that, and that was by Andreussi and Zanelli.

Now, what's nice about their test, they measured the film thickness. They measured the axial pressure gradient, so they were able to back out the interfacial shear. They also measured the entrainment fraction. So, I know which tests have entrainment and which don't.

Then they reduced the data to give values of the interfacial friction coefficient. So, I didn't even have to do that. I could just take their data and plot it, and that's what I did, and I compared it to these various models.

I started with Wallis and Wright, but it's a good place to start because it's simple. It's not simple minded. It's simple. Simple is a virtue, believe me, but it's also the model that's currently in TRACE for the annual mist flow regime.

MR. WALLIS: I know there's a motion models in these codes, very crude things developed in the 60's, and haven't evolved since.

MR. KELLY: Well, and the Bromley correlation is the 50's.

MR. WALLIS: Yes. It's always puzzled me why they haven't evolved to something better.

MR. KELLY: Well, we're trying. What I'll call modified Wallis because a lot of people will say oh, you should take the friction factor in front of it and actually use it as a function of the Reynolds number. A model by Henstock and Hanratty, Professor Hanratty from the University of Illinois. Bharathan, which is developed for countercurrent swell. I didn't expect it to be very good, but you know, I may end up using this for countercurrent flow some day, so I wanted to check it here.

Professor Hanratty again, with a graduate student named Asali, two models, one with entrainment and one without. The most recent one is by Jayanti and Hewitt. Again two models, one for ripple waves and one for disturbance waves.

What do they look like? Well, this is Wallis, and it's amazingly good, actually. This is not bad.

MR. WALLIS: For the ultra-simple model.

MR. KELLY: This is interfacial friction predicted versus measured, and you'll notice the blue --

MR. WALLIS: But you have to get the

entrainment right. That's right.

MR. KELLY: The blue, actually there's no fancy things here with entrainment. It just uses the film thickness, and it works quite well.

For the data without entrainment, which actually is closer to my condensing, it overpredicts.

MR. WALLIS: But it's a smooth film.

MR. KELLY: Yes.

MR. WALLIS: It has to be a rough thing.

MR. KELLY: This is the best comparison. It's the model by Asali and Hanratty with entrainment. They gave two correlations, as far with entrainment, did a better fit to the data. It's not too surprising it does well because this data is in its database. So, it should fit this.

There's even another trick which I'll tell you in a minute, but this looks pretty good, and this is the model I'm going to use.

MR. BANERJEE: What happens if you take their own data out?

MR. KELLY: I don't know because, you know, it's hard to get this kind of data. There's not that much, and if you correlate all of it, you know.

This was the Jayanti and Hewitt, disturbance wave model. Very small scatter, which I liked, and again, this is a simplistic model in the sense that it's a turbulent wall friction coefficient with a roughness parameter in it, and the roughness factor they used it five times the film thickness. So, I like how simple it is, but it does underpredict significantly.

Then I tabulated the results from all eight different models, and these are very relative error. It's on average value, the maximum and the RMS, and you can see this model --

MR. WALLIS: This is in what units? Relative error?

MR. KELLY: Yes, so it's delta F over F.

MR. WALLIS: Okay.

MR. KELLY: So this is actually quite good, and that's what I'm going to go with, but again, this doesn't look very complicated, fairly simple. It's the ratio of interfacial to a smooth tube friction factor. The function of a gas Reynolds number and a non-dimensional film thickness. I apologize. I'm using M here for the film thickness, which you get from the chemical industry where delta comes from the heat transfer people.

This is the way it's nondimensionalized.

MR. BANERJEE: By the wall interface?

MR. KELLY: Now, interfacial, that's the trick that helps make it so good.

MR. WALLIS: Because it speaks for itself.

MR. KELLY: Yes, you have the answer on both sides of it. I actually used the measured values of this when I evaluated the correlation. So, no wonder it fit the data, you know, or it should have.

Well, that gives me a problem because now this is implicit. So what I did is substitute for this --

MR. WALLIS: Now, wait a minute. FI over F is squared, so it's correlating against itself.

MR. KELLY: Right.

MR. WALLIS: X equals X.

MR. KELLY: Exactly.

MR. WALLIS: Well, Hanratty ought to know better than that.

MR. BANERJEE: He probably knew.

MR. KELLY: It certainly helps when you have the answer on the right-hand side.

MR. WALLIS: Oh, yes, that's right.

MR. BANERJEE: So what happens when you solve it for --

MR. KELLY: Well, that's what you're going to see. That's the next two slides. So, that's exactly what I did. I substituted for this, turned it into a quadratic equation for the square root of FI and solve it, and I bet I've gotten the quadratic formula, right? Where this is the definition of the two coefficients.

MR. WALLIS: Well, why is it quadratic? Isn't that square root --

MR. BANERJEE: Square root of F5 --

MR. WALLIS: It is the square root, you're right.

MR. KELLY: Yes.

MR. WALLIS: That's right, it's quadratic.

MR. KRESS: It's assuming A is equal to one.

MR. KELLY: And so then I did the data comparison with the explicit formulation where I don't --

MR. WALLIS: That's no better than that Wallis correlation, is it, for the blue one.

MR. KELLY: Actually, the RMS error is still better than any of the other correlations.

MR. WALLIS: Okay, all right.

MR. KELLY: Because I did check that.

MR. WALLIS: To the untrained eye, they look equivalent.

MR. KELLY: Well, that's why we have to have quantitative figures.

Okay, again, so far I did all that on the spread sheet. Now let's stick it into TRACE and see what happens. So, this is the calculated film thickness and this is the one in microns, versus the measured film thickness.

This is what you get with TRACE today, grossly underestimating the film thickness. For two reasons, for overpredicting interfacial drag or underpredicting wall friction because of the partitioning.

With the models that I've given Bill, this what he got. It works very good for these points. We underpredict the film thickness here, and what he told me was that those points go up when he makes a correction.

MR. BANERJEE: What correction.

MR. KELLY: I don't know. He did the implementation with TRACE, and he called me just before my presentation and said I made an error. It gets better.

MR. WALLIS: Let's go back to the sheared films here. You didn't give us an equation. Are these sheared films with condensation and gravity or just pure sheared?

MR. KELLY: This is with gravity, but air water.

MR. WALLIS: Air water and a vertical pipe?

MR. KELLY: Right.

MR. WALLIS: No condensation?

MR. KELLY: Co-current downflow.

MR. BANERJEE: What happens if there's an angle?

MR. KELLY: And there is an entrance and exit of those tubes.

MR. BANERJEE: Well, just because, you know, this isn't a very scientific thing. The waves are caught implicitly. So, I mean, the wave structure completely changes with angle. So, since it's not mechanistic, you expect it to change with angle.

MR. KELLY: Well, and that's why you should have empirical models for all those kind of things, and we don't. Like when we were talking about the effect of the virtual mass term, and I mean, I know that TRAC will not do two-phase flow through a nozzle correctly because we don't have that term.

MR. BANERJEE: Right.

MR. KELLY: But on the other hand, do any of you believe that interfacial drag correlations developed for vertical pipes work as you accelerate a two-phase flow through a nozzle? Of course they don't, and that will swamp anything having to do with the added mass. Now, whether the added mass term gives you numerical stability and a more reasonable answer, that's another question.

As far as accuracy goes, what happens to the structure of the two-phase interface as you go through that nozzle is a lot more dramatic than anything else. You're right, we don't have models for interfacial drag as you go around corners.

MR. BANERJEE: Right.

MR. KELLY: I'm just trying to get them right in a straight pipe because they're not right for that yet, which is a little bit humbling. I mean, this is 2003, and this isn't really difficult stuff. I just make it look difficult sometimes.

So back to my cartoon, the things that were grayed out were the wall friction interfacial shear. We've selected models for those, and now we're going to talk about wall heat transfer, and we're going to talk really about condensation heat transfer. So, what it's going to involve is both the wall to liquid and then the liquid to interface part.

So, we're going to talk about the total heat transfer and how it gets split between those two.

MR. BANERJEE: You know, the friction factor has to be a function of the fluid number in some way.

MR. KELLY: Oh, certainly if it's incline, yes.

MR. BANERJEE: Yes, so I mean, if you look at the way these things, it's never just a function of the Reynolds number, in this case. It's worrying that they don't come out that way. I'll have to think about it.

MR. KELLY: Okay. We're going to talk about film condensation now. I'm going to start out talking about falling films. We're going to start out simple and add complexity.

I assembled a database and the flaw in the database is that it only includes condensation heat transfer coefficients that are averaged over the entire surface because that's how the experiments are done. You can't, you know, control your power that you're pulling out of a specific area.

So, almost all of the data I could find for condensation is this way. The data are presented in terms of non-dimensional Nusselt numbers, which is very simple. It's the heat transfer coefficient averaged over the surface times the Nusselt link scale over the connectivity. And this just shows you, if you do the Nusselt solution in these terms, the local value is 1.1 over the Reynolds number to the one-third power. The surface average value is 1.47 over that.

The data are going to show an enhancement to this through a course of the ripples. So, this is some of the water data. This is some of the freon. They're slightly different panel numbers. The non-dimensional Nusselt number averaged over the surface versus the Reynolds number, and as expected, it's 15 to 20 percent low.

You have a laminar region and a turbulent region, but the data in the turbulent region again are averaged over the whole plate, so they cooled part of the plate and maybe most of the plate, being in a laminar flow, and that's part of the reason for this broad minimum here.

MR. WALLIS: So the data are both there because the shear has an effect? Is that what it is?

MR. KELLY: Oh, no, these are falling films.

MR. WALLIS: There are no shear at all?

MR. KELLY: No shear at all. It's

waviness.

MR. BANERJEE: Well, it's turbulenced.

MR. WALLIS: Waviness.

MR. KELLY: Well, if it's very, very low, it's the little ripples. They decrease the film thickness, and they may induce, you know, velocity normal.

MR. BANERJEE: Submixing.

MR. KELLY: Submixing, right. Here's Nusselt, one of the other well known ones is by Kutateladze, so it shows some enhancement over Nusselt, and there's another one by Nozhat, and we'll just show these on that data.

MR. WALLIS: How does it show enhancement over Nusselt?

MR. KELLY: Well, if you divide this by Nusselt, what you'll end up getting is an enhancement factor that's a function of the Reynolds number, and in this case, it's --

MR. WALLIS: Can you get your Reynolds number small enough?

MR. KELLY: Yes, it's a Reynolds number to the .07.

Here we are. This is Nusselt, Katateladze --

MR. WALLIS: This is almost beginning to look like materials data here.

MR. KELLY: Yes, you can draw -- I mean, that's the long scale, too, you know. So, for now --

MR. SIEBER: There's a pattern to it.

MR. KELLY: This is the one I'm going to pick, the one by Nozhat.

MR. WALLIS: And why is that?

MR. KELLY: Well, Kutateladze is a little high, Nusselt is low. This one's in between. When you average it with something in regard to turbulence --

MR. WALLIS: I don't like the way that the data scatters so much, though.

MR. KELLY: Well, if I can raise it to a level of importance high enough -- no, actually, I already have the UCB data, and that's prototypic tube size and stuff. So, I don't need to do a separate effects test for wall film condensation.

This is what you find out there.

MR. WALLIS: I would want to know if the UCB data are near Nozhat or whether they're near one of the other extremes of these data.

MR. KELLY: We won't get that far in this presentation, but you'll see it.

MR. WALLIS: I'll see it one day?

MR. KELLY: Yes.

MR. BANERJEE: But they have shear, right?

MR. KELLY: Yes. The shear is not very large, and actually most of it comes out through the mass transfer effect. I've started reducing some of that data now. I just haven't finished it.

MR. BANERJEE: Because you have non-condensables.

MR. KELLY: Well, they did 42 lines with pure steam, which is nice.

MR. WALLIS: The interfacial shear due to mass transfer, just the momentum transfer then?

MR. KELLY: That's much larger than the actual -- normally call interfacial shear.

MR. BANERJEE: And what you call gamma and to UG roughly?

MR. KELLY: Right. That's exactly what's in the code, and that probably overpredicts it a little. The interface velocity should be less than that, but how much less is hard to say. We run into the same thing we did with the H primes.

MR. WALLIS: It films the boundary there anyway, and that's the way it increases the facial shear.

MR. KELLY: Yes.

MR. RANSOM: When you say UCB data, you're referring to Schrock's data, I guess, what he did for GE?

MR. KELLY: The fourth series of tests, the ones done by Kunz.

MR. WALLIS: I think the effect is about half, is the mass transfer and the friction and the mixing and the turbulence. It's about half, from just adding it simply.

MR. KELLY: Yes. You see papers that say .6, l9.

MR. WALLIS: Whatever, yes.

MR. KELLY: The code uses all of it. If you take the momentum out of the vapor phase, you'd better put it somewhere.

MR. BANERJEE: There is a set of data which is not extreme, which is horizontal. Do you know, George Bankoff did a lot of experiments on horizontal.

MR. KELLY: And I'm going to look at those, too.

MR. BANERJEE: Right.

MR. KELLY: Especially for the interfacial part.

MR. BANERJEE: You'll find that the models that work there are purely turbulent centered. They have to be because it's all shear driven with the mass transfer.

MR. KELLY: Right, and when we get to the interfacial part, that's one of the ones I'm going to look at. I haven't yet, but I'm going to.

MR. WALLIS: I think Bankoff is a very extensive piece of work, isn't it?

MR. BANERJEE: Yes, very.

MR. KELLY: And I have the NUREGS.

MR. WALLIS: It's in the NUREG. Isn't it a fairly fat NUREG?

MR. KELLY: Several NUREGS.

MR. WALLIS: Oh, several NUREGS, right.

MR. KELLY: Okay, I selected a laminar film mode, but it was for the condensation rate. So, just like we talked about before, this is now the heat transfer coefficient across the film. What I really need is the heat transfer coefficient between the wall and the film, an interfacial one between the film and between the liquid and the interface.

Well, how am I going to do that? You can do a straightforward energy balance, saying that the condensation heat flux has to equal the wall to liquid, which in turn has to equal to the interfacial one. We're talking about just straight shot through those resistances.

MR. WALLIS: There's no subcooling of the liquid film or anything like that that comes into the energy balance?

MR. KELLY: It does, but --

MR. WALLIS: There's a correction.

MR. SIEBER: Not there.

MR. KELLY: Yes. Now what I'm saying is I know this one. How am I going to split it between these two? If you do this, and I guess I shouldn't have called it an energy balance just because what you're saying. Then you can do a resistance kind of thing, and this is what you come up with.

MR. BANERJEE: What is that now?

MR. KELLY: I'm basically taking the two resistances. I'm solving for HOI --

MR. WALLIS: You're solving the temperatures from those two equations?

MR. KELLY: Exactly, and solving for HOI in terms of the wall to liquid and the condensation heat transfer coefficient.

MR. WALLIS: You mean one over H?

MR. KELLY: Yes, one over H minus one over H.

MR. WALLIS: Yes.

MR. KELLY: And now I happen to -- this one, wall to liquid, I don't have any local condensation data, but I do have a lot of the local heating data. Okay, now we're talking about just wall to liquid. So, the only difference is the direction of the directional flow of the heat, and that's kind of a second order effect.

MR. WALLIS: What you've done here is really evaluating resistances in series.

MR. KELLY: Right, exactly. So, what I'm going to say is I have this heating data. Let's use it to help me pick a model that is wall to liquid. In a laminar one, if you go to the loca model, it's 1.88.

MR. BANERJEE: I guess the way it's coming out is because of the way you define HC. It's Tsat minus TW.

MR. KELLY: Exactly.

MR. BANERJEE: The TG minus TW, you'd get a different thing.

MR. KELLY: Yes.

MR. BANERJEE: Okay.

MR. KELLY: Now, this is if you look at

film heating data. It's a lot of Wilke's data and some by Ueda and Tanaka. I want to look at the laminar part, and it fits that beautifully, okay? Of course, it was developed from that data, but it was basically an analytical solution. So, I could do that, and what I'm talking about is taking this and taking the nose-out model for that and coming up with this.

One problem. The problem is those two models intersect, and I'm subtracting them in the denominator. My denominator goes to zero, which would imply my interfacial heat transfer coefficient goes to infinity, and I don't want to go there, okay? So, back up and try again.

If I had a smooth laminar film, this is something I can solve. I can take the parabolic velocity profile, the linear temperature profile, and this is what I get for the bulk liquid temperatures. Five-eighths time Tsat, 3/8 times the wall. It's closer to the interface temperature because that's where most of the liquid is because the liquid is moving slower next to the wall.

If you then convert that into these Nusselt numbers, you get the wall to liquid being 8/5 times, the condensation in the interfacial, 89/3 times the condensation.

Okay, that's a fair enough way to split it out, and that's exactly what I'm going to do. So, again, it's adding up the resistance of things.

MR. BANERJEE: But this is just a laminar force.

MR. KELLY: Right, but that's all I'm talking about right now, is a laminar falling film.

MR. WALLIS: Does this get rid of your infinity?

MR. KELLY: Yes. I'm going to split them this way rather than what I did on the other plot.

MR. WALLIS: So you always end up with something which is finite?

MR. KELLY: Exactly. We did the easy problem. We did laminar falling films, okay?

MR. WALLIS: Yes, but when you do this thing, does it still give as good a correlation as you showed in the previous slide?

MR. KELLY: Yes. The correlation was for what we use in Nozhat, which is for the condensation rate, or the heat transfer across the film. How I apportion it, that total heat resistance to heat transfer, between wall to liquid and liquid to interface? The only thing that effects is the condensate temperature. It just moves where that temperature is between T wall and Tsat, and I picked it to be where it would be for the laminar solution, you know, and being ignorant of where it should actually be. That's what I chose as being reasonable.

MR. WALLIS: Well, no condensables, because they affect one of these coefficients and not the other, presumably.

MR. KELLY: Right. I talked about non-condensables briefly on my last slide.

MR. WALLIS: You haven't done that, though.

MR. KELLY: We're not going to get into it today. Okay, that will be next time because remember, this is work I'm doing now, as we speak. So, you're seeing where I am, not what I've done.

MR. WALLIS: This is a homework assignment, and it's due next week.

MR. KELLY: Well, that's what my boss keeps telling me.

Okay, we did a laminar falling film. That's the easy problem. Let's make it a little bit more difficult and go to turbulent falling film.

The first problem is the database. I simply don't have turbulent falling film data except ones that have been met, averaged over an entire surface.

MR. WALLIS: That's why I've seen curves like the one on your slide 48, many, many, many times in the literature.

MR. KELLY: Slide 48?

MR. WALLIS: It's a standard thing, the Nusselt number versus Reynolds number.

MR. KELLY: Yes.

MR. WALLIS: Condensation.

MR. KELLY: Yes, there's no --

MR. WALLIS: It's in all the textbooks.

MR. KELLY: I'm not making a big advance to the science here. What I'm doing is trying to take something that's known and put it inside a two-fluid card and get it to work in a rational way, not developing models from scratch really. I'm trying to select models that I can implement.

So, I don't have local turbulent falling film data. I only have stuff that's averaged, and then it is polluted, if you will, by so much of the plate being in laminar.

So, what I'm going to try to do is take a turbulent heating data in order to select a correlation to work with, because I have that data.

I'm going to look at interfacial heat transfer models, and the one due to Bankoff is one of the ones I'm going to look at.

When you add these two together, you then can predict the condensation rate, but I don't have a database to really compare it to. So what I'm going to do is take what I'll say is a well established turbulent falling film condensation correlation and use that as my discriminator, and the one I'm going to pick, or at least I think I'm going to pick is by Labuntsov. But I just said, I'm going to use a well established model, and now I'm just going to show you there's no such thing.

Here's a laundry list for condensation correlations in time, okay, from 1933 to '87. They are all the non-dimensional Nusselt number in terms of Reynolds and Fandall. Notice that Reynolds dependence goes from .2 --

MR. WALLIS: Don't you have anything done by Germans, or just probably thorough?

MR. KELLY: By who?

MR. WALLIS: Don't you have Grober, Erk, and Grigull, or someone who's done a really thorough job and investigate everything under the sun, and it works? That's what it looks like, as if these are all just looking at partial data. Actually, there's somebody who's done a really thorough job and pull it all together. It's amazing to me. It's such a simple problem, has so many authors. Then when you plot them, they're so different.

MR. KELLY: Right, and you're right. Part of it is because they look at different pieces, instead of comprehensive. Now, if we talk about film heating, Wilke has a correlation that stands the entire range, but there are two things here. One is the Reynolds number dependence varying so much, and my expectation. Now, these are non-dimensional, so we can actually multiply it by the film thickness. You're going to end up changing this Reynolds number dependence.

You're basically going to multiply by about .58 to .6. So, you expect something a little bit greater than .2.

MR. WALLIS: Yes, but if you look at the next slide, you've got these plotted?

MR. KELLY: They're everywhere.

MR. WALLIS: Well, there's an error factor of three between the correlations?

MR. KELLY: Yes.

MR. WALLIS: It's a very, very simple

problem.

MR. KELLY: I agree.

MR. WALLIS: It's not such a simple problem, unfortunately.

MR. KELLY: Actually, one of my favorite papers is one I read just recently. It's by Palen, and it's entitled, "What We Still Don't Know About Condensation." I enjoyed it enormously.

MR. WALLIS: But, you know, you've got a scatter here which is 50 percent or something.

MR. KELLY: Yes.

MR. WALLIS: And yet --

MR. KELLY: And go pick up a textbook. One will tell you to use this. One will tell you to use this. Another one will tell you to use this.

MR. WALLIS: In a two-phase flow, you'd expect things to be worse. This is a single phase, isn't it?

MR. KELLY: This is relatively simple, right.

MR. WALLIS: Well, it's a free surface.

MR. KELLY: And that's where some of this comes in.

MR. WALLIS: So if the lab is vibrating to shaking, it makes a difference, doesn't it, because you get waves.

MR. BANERJEE: Well, to begin with, surface tension has to enter. It's called waves, right.

MR. KELLY: And that's exactly where we're going. You don't -- let me move on to the number dependence, and I'll talk about just what Professor Banerjee mentioned. The number dependence, well Colburn doesn't have it, but he looked at a fairly small range of panel numbers. This is empirical. This is to the one-third, .4, one-half, .65, okay, and that's a pretty large variation.

What you expect from cooling data, like you did that kind of stuff, you expect about .4. So, film heating is well correlated by .34. He expects something of that order.

If he had mass transfer problems, and you're talking about gas absorption into liquid film, the Schmidt number dependence normally comes out to be one-half. Where on earth do these things come from, these .65?

Well, and if you look at the data that they correlated, you need that in order to fit it. I just read a paper. It's by Al Husseini, Tuzla, and Chen, and they put together a model actually for evaporation of liquid films. It's one where you model the boundary layers, and in your integrated crossing to end up with a very complicated looking heat transfer correlation. Wall boundary layer effect and core turbulence, and then a free surface effect.

What they said is the reason these panel number dependents are all over the map is because they're not modeling the right thing. It isn't a Prandtl number dependant that's separating the data. Instead, it's a wave effect. What you're doing, you know, some of these are comparing evaporation of water to evaporation of oils, which are very thick and viscous. What you really need to do is use a Kapitsa number.

You know, panel number for the thermal stuff, a Kapitsa number for the wave effects, and that's not what's being done.

MR. WALLIS: This is a liquid methyl data in there, too?

MR. KELLY: No, not in this. So, that's what I'm going to look at when I go to an interfacial heat transfer part.

It turns out, if you look at heating data where you don't worry about the film surface, you end up with a Prandtl number dependence about .34, which is about what you expect, and you don't have this wide scatter.

Like you said, this shows where the correlations are, and these are all, you know, --

MR. WALLIS: Well, if it depends on waviness, it's going to depend on the length of the performance.

MR. KELLY: That's true as well.

MR. WALLIS: Because if waves are developing.

MR. KELLY: Now, these are all models that people recommend you use, and they're all over the map. So, for the moment, in trying to pick one to use as a benchmark to guide my development, I'm going to pick the one that's kind of in the middle, the one by Labuntsov.

I could probably pick Soliman just as well, and it's kind of in the middle. I've got, you know, Prandtl number one and Prandtl number two here. It's not a wide variation, that that kind of brackets the water applications I'm looking at. You know, two is basically saturated water, one atmosphere, and one covers saturated water, a whole range.

So what I said I was going to do is take the condensation correlation, use that as a benchmark, use heating data to select a wall to liquid heat transfer correlation, go to the literature, find some models for interfacial heat transfer, add those two together, and then compare them to condensation data.

So, what I'm doing now is trying to pick a model for a convection from the wall to the liquid film, and we use heating data to do that. This is an example of Bay's data. The Wilke correlation has four piece-wise pieces, and it's kind of a standard in all of this. It's pretty accurate, and that's the broken blue line.

Gnielinski is one of the more recent, more modern ones. It's much more accurate than Dittus-Boeter, and in fact, it's what I use in the reflood model for --

MR. WALLIS: It's forced convection? I mean, we're talking here about a falling film.

MR. KELLY: Right.

MR. BANERJEE: But the wall.

MR. KELLY: Yes, and what I'm using now is a characteristic of something like full scale as a film thickness. When you do that, what we talked about, like when we were talking about Chen, about the ratio and the hydraulic diameter of the film thickness.

MR. WALLIS: Labuntsov looks just like the close convection.

MR. BANERJEE: Yes, except the battles to the half.

MR. KELLY: Right, exactly. So, Gnielinski does a pretty good job. It's not quite as accurate, but I think the behavior is actually better, and I actually looked at a lot more data than this, and again, remember I said I want to when I can pick models that I want to migrate over to the normal constitiate models. So, Gnielinski fits right in with that. So, as long as I know the film thickness, I can use that and do just as well as a model developed for heating.

MR. WALLIS: It seems to me that the regulator has a problem here, that you develop all this stuff and you choose Labuntsov and Gnielinski, and some vendor is going to come along and say we're using Colburn and Nickelgruber or somebody, and what do they do?

MR. KELLY: They'd better. When you see the next few viewgraphs, I'm going to say, and the assessment will be. Remember what I said the other day? Never believe one of these codes for a new application, unless you sat down, and whether you want to call it a PERT or whatever, that you figured out what the important phenomena are, when they're important, and what range of parameters import and over.

MR. WALLIS: Which will cost in experiments which are something like full scale. Perhaps we're getting there.

MR. BANERJEE: In this case, there are.

MR. KELLY: Exactly.

MR. WALLIS: The giraffes and pandas and the whole menagerie.

MR. KELLY: Well, and panthers.

MR. BANERJEE: And panthers.

MR. KELLY: Which was done in Piacensa. It's actually the real, full-scale thing. Now, you don't have the detailed measurements from it necessary for model resolvement, but you can assess your model against it.

MR. WALLIS: It will be interesting to see how all these animals fit on your --

MR. KELLY: We'll get there. This is a --

MR. WALLIS: Not today, though.

MR. KELLY: Actually, I did that once a long time ago, but I need to, you know, this is deja vu here. When I first joined the agency in '93, my very first ACRS presentation was on this stuff.

MR. WALLIS: You've come full circle.

MR. KELLY: So I dusted it all off.

MR. WALLIS: Has there been any progress since then?

MR. KELLY: Just a little. I've gotten a little bit smarter, and a little more cynical.

MR. WALLIS: You got smarter, but is the result any better?

MR. KELLY: We'll see. So, at any rate, this is Nusselt, non-dimensional versus film Reynolds number. The orange line is the Labuntsov correlation. That's for condensation.

The blue line is Gnielinski, which is just from the wall to the liquid. When you then add an interfacial correlation to it, and the one I added here is by Al Husseini, Tuzla, and Chen. That's from Lehigh University.

That produces the black curve, which amazingly enough comes somewhere close to this, that actually surprised me. So, when it's fully turbulent, it's pretty close.

When you go towards the laminar transition, it nets down, actually as it should. So, I'm not too unhappy with this.

MR. WALLIS: This is just a turbulent falling film.

MR. KELLY: Yes, it's a simple problem.

MR. WALLIS: This is step number one.

MR. KELLY: It's a simple -- well, step number two. Laminar was the first step.

MR. WALLIS: Okay.

MR. KELLY: Well, actually, step number one was getting the film thickness right. So, this is just the very first one I looked at. I'm going to look at others and then try to pick one, but this isn't bad, considering what I've done to get there.

Then of course it's going to have to be assessed. Before we do the assessment, because the assessment that I'm going to look at is all sheared films. That's the condensation data I have that makes some sense, particularly the UCB test done by Kunz, which at lower vapor Reynolds numbers, and in a NASA Goodykoontz data.

I have some others by Ueda and Blangetti and Schlunder, where they actually measured the film thickness and the pressure drop and tried to back out where the interfacial friction was. I can actually do these guys on a spread sheet where these I'm going to have to end up doing on a card.

Okay, so where are we? When you look at a sheared film, the first observation you make is that the heat transfer is hot. It goes way up, and the reason, what seems to me and to others, is that the main reason it gets a lot higher is the film just simply gets thinner. If you thin the film down to next to nothing, your heat transfer rate goes way up.

So, what I'm proposing to do --

MR. BANERJEE: But you also enhanced your turbulence. You get turbulence at the interface and at the wall model.

MR. KELLY: Yes.

MR. BANERJEE: Whereas previously, you only guarded the walls.

MR. KELLY: And you were damping it at the interface. Yes, that's true, but as a first shot, what we're going to do is use heat transfer models developed from the following film data, and then translated to a sheared film by using the calculated film thickness.

MR. WALLIS: I think that Collier and Hewitt and people way back in '69 or something. Did a lot of experiments with annular flow, and the heat transfer as well. They found they had approached something like this, but they had to fudge it by a factor of about two to get the missile number right.

MR. KELLY: And that's what we're going to find out.

MR. WALLIS: A long study of that.

MR. KELLY: Yes, you know, and typically the approach is to go off and use the Martinelli parameter into some kind of multiplier. I don't want to do that because I'm already calculating the film thickness. What I remember is something like a two-phase heat transfer coefficient ratio is like one over one minus alpha to the .8. So, we're going to see where this takes us, okay?

So, this is the approach I'm going to use.

MR. WALLIS: How many years do you have?

MR. KELLY: I've only got a couple more months. This is the assessment I'm going to do, so I've got to get busy and pump some stuff to Birol, and he's got to get it in the code and test it. Remember, I'm coming to the end of the presentation, and this is the work I'm doing now. So, you're getting what I'm planning on doing.

MR. WALLIS: That's not very far with the real problem.

MR. KELLY: That's true. On the other hand, we've corrected the wall drag models and the interfacial models.

MR. WALLIS: You're going to show us the next slide.

MR. KELLY: Yes. So, this is an example of the approach. This is the data from the Ueda, and it's the Nusselt number, non-dimensional Nusselt number versus film Reynolds number. What they did is they were able to back out or infer the level of interfacial friction. Actually, these are non-dimensional values.

So as you go from a value of 10 to 40 to 70 to 120, on up to 300, of course the films got thinner, and the heat transfer rates got higher.

Yet instead of plotting it as a non-dimensional Nusselt number, we use a Nusselt number we're a little more familiar with, which is a heat transfer coefficient times the film thickness over the conductivity. It's unfortunate that I used an asterisk for times here, because it looks like it's the non-dimensional.

So, if you use the standard definition of the Nusselt number --

MR. BANERJEE: Whose film thickness is that? From them or --

MR. KELLY: Measured.

MR. BANERJEE: Measured.

MR. WALLIS: So the other Nusselt number was in terms of this dimensionless length?

MR. KELLY: Right, because that's how you find condensation data. The rate scale used is the Nusselt one, the viscous gravitational one.

MR. WALLIS: Right.

MR. KELLY: So here I'm using the actual measured film thickness.

MR. WALLIS: Because you've got a sheared flow rather than the gravity flow.

MR. KELLY: Right. Now, what that does is it collapses the data down rather remarkably, given this scatter you've seen in condensation data. Now not only do they have condensation data. They have film thickness measurements, which are not the most accurate thing in the world either.

Then what I did on this plot was I went ahead and plotted Gnielinski, and the Gnielinski plus the ATC interfacial. So, this is what I would have expected the condensation heat transfer to be, and you'll notice this data overpredicts it. For some reason, that line happens to go right smack dab through the middle of it, which it shouldn't, because I was saying there's no interface resistance.

MR. BANERJEE: Right. The lower, of course, all your heat transfer is coming from the shear at the wall, right? It's without shear.

MR. WALLIS: I'm puzzled here.

MR. BANERJEE: The dotted line is without shear.

MR. KELLY: No, in both of these, I'm using the measured -- well, I'm just using the film Reynolds number.

MR. BANERJEE: Right.

MR. KELLY: Okay.

MR. BANERJEE: But the other one is without shear.

MR. KELLY: Right.

MR. BANERJEE: I don't know what the black line is. Where does that come from?

MR. KELLY: Okay, the black line is the heat transfer coefficient, or Nusselt number, from the wall to the liquid. This is wall to liquid plus the interfacial effect.

MR. BANERJEE: Because you sheared the interface.

MR. KELLY: I've removed the shear by plotting it this way.

MR. BANERJEE: Yes, but the correlation

that was developed, the one you showed us before, was for turbulent falling liquid without shear.

MR. KELLY: That's correct.

MR. BANERJEE: So that's what that -- and you can plot that against the film Reynolds number.

MR. KELLY: Right.

MR. BANERJEE: Okay, and that's what you've done there?

MR. KELLY: That's what I've done.

MR. BANERJEE: Now, when you put shear, of course, you bring additional turbulence in addition to tending the film.

MR. KELLY: Okay.

MR. BANERJEE: So, you've got the real film thickness down there, but that has to move up.

MR. KELLY: Well, that's a good point. Thank you. I appreciate that. I hadn't thought of that. In fact, I just put these curves on here last night, which is why it's not in your handout, but yes, I think you're right. The caveat to that is there's some other data by Blangetti and Schlunder where they do this same kind of thing. I didn't put the plot up here, but if these points are about 35 percent higher than theirs.

MR. BANERJEE: Could be many things.

MR. KELLY: And that's why I got --

MR. BANERJEE: Did they measure the film thickness as well?

MR. KELLY: I don't remember.

MR. BANERJEE: Because this is a measured thickness.

MR. KELLY: Right.

MR. BANERJEE: So you have to check that.

MR. KELLY: Right, but I'm going to keep your comment in mind because one of the next things I'm going to do is start comparing this to the UCB data with pure steam condensation, and then also the NASA Goodykoontz and see where this comes, and also try other models like the Bankoff one and see what I get.

MR. BANERJEE: Because opposite, you'll find with the Bankoff model, it's all driven by interfacial shear. The wall shear is not very important.

MR. KELLY: No.

MR. WALLIS: What's the message with this ATC thing? You add the ATC and the correlation gets worse?

MR. KELLY: For this set of data, yes. I probably shouldn't have even shown it, but you know, I put it on --

MR. BANERJEE: Why don't you go back to your previous slide where you showed these -- yes, that one.

MR. KELLY: So for this, it underpredicted, but for the shear driven one, it underpredicted, and you may be exactly right, that this model, because it doesn't take account of interfacial shear, overpredicts the resistance to heat transfer at the interface.

MR. WALLIS: Right.

MR. BANERJEE: Now, what is that other blue line coming from? That's just a forced convection heat transfer, is it?

MR. KELLY: Which I'm using for the wall to the liquid. In this line is the resistance from this plus the resistance to the interface. If I applied these as resistances, you would see them adding up to this, and that would make more sense, yes.

MR. WALLIS: Parallel.

MR. BANERJEE: Well, in series.

MR. KELLY: Series, right. So, this is just the approach I'm going to try to follow, and we're going to see where it leads next time, which may be a couple of months from now.

The next to the last slide, noncondensable gas effect, which is actually one of the main things for all of this, and if you go to the final report of the UCB project, they go through a lot of models, and in the end, they look at a fusion layer model and what they call a mechanistic approach using a mass transfer conductance model. They show that for their own data, that is more accurate than the empirical models they develop from their data.

So, this is the approach I'm going to try to follow and implement, and then this is the assessment cases. Now, I'm going to do a number of all of these and see how good it is.

So in summary, I looked at the original TRACE models. They do a poor job. They overpredict condensation with noncondensables present. They underpredicted for pure steam. So, I started the development of a constitiative package to be applicable for ICS and PCCS condenser tubes.

We've made improvements to the wall drag and interfacial friction. I've started looking at condensation and laminar falling films. I've chosen them all for that. I'm looking at turbulent films now, and I'm going to be looking at sheared films and then use a mass transfer approach for the noncondensable gas, and then do a relatively large amount of assessment and then have quantitative metrics.

These are nice as far as quantitative metrics.

MR. WALLIS: Are you going to look at Diraf and Pendar and all that sort of thing, too, and panthers?

MR. KELLY: In the development of the model, I'll use the simple, separate effects test, and if you will, the validation model. That's when we start expanding it out to the larger, more interval facilities.

MR. WALLIS: You've given us a couple of examples here where the codes were not doing a very good job. You started to try to figure out how to improve.

MR. KELLY: Right.

MR. WALLIS: And you've made some steps forward, but you've got some way to go.

MR. KELLY: That's true.

MR. WALLIS: So one has to wonder how many other parts of these codes are in the same state.

MR. KELLY: Yes.

MR. WALLIS: Now, is it that the rest of the codes are in fine shape and these are just some odd things that weren't done too well because they didn't matter at the time, or do we have to look at a lot of parts of the code as well?

MR. KELLY: My intent is to look at all the other parts of the code. I mean, like if we're talking about small break loca, for example, which is a nontraditional application of TRACE. Now, unfortunately, it doesn't even apparently do large break loca very well. That's why I had to do the reflood stuff.

There's a lot of things important in small break loca that TRAC had never been assessed against. Loop seal clearing, reflux condensation. You know, all these things have to be looked at, and we're going to do them one at a time.

In most cases, we'll be doing comparisons from TRACE versus RELAP5, and if the models in RELAP5 are significantly better, we'll just port the model over, but talking about small break loca, one of the things you know is important is the level swell. We already know from our assessment that the interfacial drag model in TRACE was not adequate, and we've made the decision to go ahead and replace it.

MR. WALLIS: So that's another example where you had to look at fundamentals and you had to make a significant change in the interfacial drag model.

MR. KELLY: Yes.

MR. WALLIS: It was a factor of --

MR. KELLY: Oh, interfacial drag coefficient, you know, that can be an order of magnitude real easy.

MR. WALLIS: Or major different from what was assumed before.

MR. RANSOM: Did I hear that stratified flow models have not been put into TRACE yet?

MR. KELLY: No, there is a horizontal stratification criteria. How good it is --

MR. RANSOM: I'm thinking of the counter current flow modeling.

MR. KELLY: Yes, I haven't looked at the interfacial drag and countercurrent flow in a horizontal pipe yet, so who knows. I don't know.

MR. BANERJEE: I think he means even the terms and the equations which are missing.

MR. KELLY: Oh, no, no, no. That is here. Yes, the gravitational head due to a void fraction profile, that's there, so water does run more level --

seek its own level in a pipe.

MR. RANSOM: Okay.

MR. KELLY: Yes, if it didn't have that, you know, that's there, but I mean, these codes have hundreds of constitiative models in them. Some good and some not so good. They're terrible. You have to address the application, and I'm talking about TRACE here, but it's true of RELAP5. It's true of the vendor codes. It's true of any code, and you have to assess it for your application, and you have to do a very good job, and whoever is in charge of that code better be just as inquisitive as you guys are. You'd better ask the tough questions.

MR. WALLIS: Of course if you'd done this for 40 or 50 years, and you seem to be rediscovering things that we did a long time ago. I'm trying to figure out why the steps haven't been taken before. Conceivably it's because the regulatory framework is that an applicant gets some young engineer out of college and says put together some models for our code. He or she puts together whatever they can to make something work that seems sort of reasonable, and if it gives good enough results for the regulatory --

MR. KELLY: You move on.

MR. WALLIS: -- argument that they want to make about large break loca or something, they get it approved by the NRC and they don't want ever to look at it again. So no one has any incentive to do anything about it.

MR. KELLY: I won't speak to the vendors in here but I can speak about myself and my own experience because most of my career was spent in a national lab, and most of that as a contractor to the NRC. What happens is the development deadlines tend to be aggressive because you want results now. You don't want them a couple of years from now.

Contractors are relatively expensive. NRC research budgets, these days, are not that large, and so you have to do a very lot with very little staff power, and if I were a contractor right now and someone came to me and said we want you to put a tube condensation model to handle noncondensables into TRACE, chances are they would give me one or two staff months of effort, because that's already big bucks. Two staff months is 50, $60,000, okay? If you're going to give me two months to do this work, and that includes putting it in the code, documenting it, changing the stuff in the manual, how much time does that leave for intellectual curiosity? Not much.

You're going to go to the first textbook or first journal paper you can find, and you're going to see a correlation that someone has referenced and recommend it, and you're going to try to find some way to shoehorn it into that code and get your work done so you meet your milestone. I mean, you're going to do your work as best you can, but you do not have the luxury to sit there and read a couple hundred technical papers and educate yourself on one of these topics. There simply is no time, and time is money.

MR. WALLIS: Then there's some manager saying that's good enough, and trying to convince the agency that that's good enough.

MR. KELLY: Now, I'm speaking from the NRC perspective. I was an NRC contractor for most of my career.

MR. WALLIS: You speak from that side, but I think it's very true of industry as well. The pressure to produce something now is probably even greater.

MR. KELLY: There it's real money. It's bottom line.

MR. WALLIS: I look at some of the people who I know produce some of the work, and they really didn't know very much, and you can see that they put together things based on what they knew, which is a good start, but the thing is they prostelize at the start. No one goes back and says that was a pretty poor model. It's not really good enough for this thing. Let's do something about it. It doesn't seem to be an incentive to do that.

MR. KELLY: Well, that's because --

MR. WALLIS: I'm talking about the vendors now.

MR. RANSOM: Yes, but hasn't a lot of that been driven by the appendix K where we're simply to be super conservative, and as long as you're under whatever the NRC with audit and come up with, why that's good enough.

MR. KELLY: But even when you move into best estimate space, and I mean people try. Most people out there are honest and hard working and want to do a good job, but there is no time. Right now, I am in a very nice position with the Agency. My management has given me the job to look at these models, and they're giving me time to spend to go assemble these databases, go check models out, try to make some rational decisions instead of just grabbing the first thing I can find and sticking it in the code.

I'm very, very appreciative of that, and

I'm learning a lot, and that's what makes my job fun right now.

MR. WALLIS: It looks as if there's going to be a pay-off because you're getting results which are significantly different from what was predicted before, and will influence decisions made about something like the ESBWR probably. So, it's not that it's just interesting work. I mean, it has a real pay-off for the Agency.

MR. KELLY: Right.

MR. WALLIS: Which may not have been appreciate before. You know, that letting you do this would have a pay-off for the Agency.

MR. RANSOM: I think the one thing that seems still kind of disturbing is, and I'm not pointing at anybody in particular, but you know, after six years of being at this, you still won't have an ability to use it in the NRC licensing sense. The question that comes up to me is how much longer will it be before you actually achieve that goal.

MR. KELLY: And I would say that depends upon the application. No, I agree with you completely. The first few years of the project were taken up by things like trying to modernize the architecture and trying to bring in TRAC-B models, things like the heater component, the Chan component with the radiation model, the jet pumps, et cetera. That was the first few years of the project.

Then where we really got bogged down was when we went to be able to do RELAP5 input decks. As you very well know, the philosophy behind the interconnectivity of the components and the type components are different in the codes. In trying to do that --

MR. RANSOM: Was that not recognized at the outset?

MR. KELLY: None of us realized it was going to be as hard a job as it was. I mean, it's much, much harder than I'm saying. When you try to map one of these components, even a pipe or a valve, they don't quite go one to one. Now, put a control system on top of this, and you're going to map the control system as well. That control system expects it to be this junction in this pipe. That doesn't exist after you've mapped it.

So, you have to map it, redo the control system. I mean, it gets very complicated.

MR. RANSOM: The thing that bothers me is the NRC should have realized this because actually, most of that framework goes back to the 60's. It started out at General Electric and you know, through the INELE, and so there's an awful lot of history behind that, and probably no real good reason to change that view of the world.

MR. KELLY: When we first started this, the idea of the consolidated code, one of the metrics was not going to be able to re-use the RELAP input decks.

MR. RANSOM: Oh, really?

MR. KELLY: Not six years ago. That came along after one or two years, and we realized that we wanted to keep the investment in input models. That raises --

MR. CARUSO: I'm going to disagree.

MR. STAUDENMEIER: No, the original intent wasn't to map everything directly. The original intent was map what you could easily, which would be 1D components, pipes, and things like that. Then SNAP would give the user a message on things it couldn't map and tell it you have to do this on your own, and this is how we think you should do it. We can't do it -- I can't automate it for you, but then it was turned into -- that would get you probably 90 percent of the stuff, 90 to 95 percent, but when it was turned into 100 percent type of thing, that's where the work just multiplied by a lot.

MR. CARUSO: The message was that the existing decks should not have to be redone. You should not have to pay a zillion bucks to recreate all those data decks, and if it required a little bit of tweaking, that was all right, but there were, just before the decision was made to consolidate the code, there was a bad example of going from one version of RELAP to another version of RELAP that meant that some decks that had just been delivered by a national laboratory were not able to run in the new version of the code, and a substantial amount of money had to be spent to get those decks run on the next version of the code.

MR. KELLY: Yes, I remember that.

MR. CARUSO: That caused some unpleasantness.

MR. KELLY: And I don't recall it quite the same way because I recall more of a discussion about, you know, new code versus re-using one of our codes, and if we're going to re-use one of the codes, which one. It turned out that TRAC at that time, there was a project by the Office of Naval Reactors to modernize the architecture of the 1D components in TRAC.

MR. WALLIS: I remember that.

MR. KELLY: And that, together with what we believe was a more modular structure in TRAC, helped drive -- and the 3D vessel -- helped drive the decision that way. That turns out to have really been a difficult problem to be able to re-sue RELAP5 input decks.

MR. RANSOM: I imagine, yes.

MR. KELLY: And with lots of fitful starts along the way. You hear us talk about things like the TPR file. That didn't just be perfect in this first inclination. A whole lot has been done there. We have not advanced the state of the art as far as our computational capabilities. I mean, zero, okay, except for bug fixes.

But what we now have --

MR. RANSOM: What's your estimate? How long will it take to produce this, put this code into the licensing arena?

MR. KELLY: Well, if we're talking about large break LOCA, okay, that may be within the next year. See how the assessment goes.

Small break LOCA, maybe another year after that. As you saw with Steve's presentation about his plans for the assessment of the code, this code will end up being better assessed than any of the vendor codes, but as we start down that path, we're going to identify deficiencies, and we're going to have to rectify them.

The one thing in our favor now is the code really is much, much more modern and easier to go in and make changes than it's ever been in the past. Compared to the TRAC code we started with, it's more than an order of magnitude. There are no pointers. There's no test on bits. All that archaic stuff is gone.

It's relatively straightforward, easy to read Fortran, and you can go in and do your work. We have the automated testing tools. Much, much better quality assurance than we've ever had before, and so yes, we haven't made the answers better yet, but we've gotten ourselves in a position where we can.

MR. RANSOM: Well, I think one difficulty that may be faced too, and this is somewhat new to me, too, but licensing is now moving towards its first conformed regulations, which means a slightly different way in which codes are going to be used. I think that will still be very important, don't you?

You know, good physical models will be important in that framework, too, because that will tend to reduce the amount of uncertainty that's involved in any one of these calculations.

I think that research is going to have to you know, bring these codes into that arena, too, which is a new thing.

MR. KELLY: And Steve just walked in the door, and that certainly is -- he worked as part of the best estimate team at Westinghouse, and has a lot of experience in that area, and that's where he's pushing us.

MR. WALLIS: I'm just going to call a break, not because Steve walked in the door. We'll take a break until 3:30. Thank you very much, Joe.

(Whereupon, the foregoing matter went off the record at 3:15 p.m. and went back on the record at 3:34 p.m.)

MR. KROTIUK: I'm Bill Krotiuk, and I work in research. At Joe's request, Joe Staudenmeier's request, I'm sort of presenting some of the applications that I have done, specifically using TRACE, and this pertains to the development of load inside the steam generator, and following a rupture of a main steam line or a feedwater line.

The specific guidelines were to use TRACE for this analysis, but one of the things that I found it to do is because of the questions about the applicability and everything, I wanted to run some sensitivity studies with the code and also to do some comparisons with appropriate tests that would be representative of the type of phenomena that I would be seeing inside the steam generator.

So, what I ended up doing was that I wanted to use specific test comparisons to look at test data that would test the code regarding its ability to follow the acoustic wave transmission through the depressurization process, and also to assess pool swell effects that could occur inside the steam generator following the rupture.

The codes that I specifically looked at were Edwards, the very simple comparison, a LOFT Semiscale blowdown test. Then the more specific tests that were more complicated were the GE vessel blowdown and the Westinghouse MB-2 testing.

MR. WALLIS: All of these tests measured pressure wave propagation?

MR. KROTIUK: Okay, these first two did, Edwards and the LOFT Semiscale did. The GE vessel and the MB-2 were more attuned to the pool swell phenomena, and specifically, the MB-2 actually was a

Westinghouse steam generator with two support plates and the pressure measurements requested, and tube support plates.

MR. WALLIS: And will the pressure wave propagation is very early after the break, presumably.

MR. KROTIUK: Right.

MR. WALLIS: And the pool swell is something that happens later?

MR. KROTIUK: Later on.

MR. WALLIS: Later on, because it's different times altogether?

MR. KROTIUK: Yes, there's different time scales on that, and well, I'll show you that it turns out one is dominant over the other.

MR. WALLIS: Okay. Does he have to start again?

MR. KROTIUK: Okay. This is just a slide just summarizing the Edwards pipe blowdown problem, and what I did to try to look at sensitivity, I did divide the problem into different number of nodings, and also did look at the two numerical schemes, the sets and the nonsets type of situation.

I have, you know, numerous comparisons, and I just chose two points here. With regard to the pipe blowdown, these were two positions near the coldest end of the pipe, and this is for pressure and this is for basically void fraction. One finding that I did in doing this sensitivity study was that the NOSETS equals zero, which means using the SETS option actually provided results that were closer to test data. Then this was just some indication of the type of node size that I needed to follow that acoustic wave.

MR. WALLIS: Now, the acoustic wave is over in a very short time.

MR. KROTIUK: That's correct. Yes, this problem is within the second acoustic wave is over.

MR. WALLIS: Isn't it much shorter than that? Four meters long, and you open the end of it, and the wave rushes from one end to the other and bounces off?

MR. KROTIUK: Right.

MR. BANERJEE: It's the first wave --

MR. KROTIUK: But don't forget, there are reflections back and forth, so --

MR. WALLIS: But isn't the sort of millisecond time range at the beginning?

MR. KROTIUK: Yes, you get a lot of time right here, yes.

MR. WALLIS: All right.

MR. KROTIUK: But you are getting reflections.

MR. WALLIS: Are you interested for loads? Are you interested in --

MR. RANSOM: Between that very early phase range. The long terms of the weight propagation. Are you interested in the loads on the steam generator?

MR. WALLIS: Are you interested in that very short part with millisecond time scale, or are you interested in --

MR. KROTIUK: No, I'm interested more in the one second time scale.

MR. WALLIS: You are?

MR. KROTIUK: Right.

MR. WALLIS: Isn't it the wave that hits the steam generator that you're concerned about?

MR. KROTIUK: No, when you get the pipe break on say the steam line, which turns out to be the worst case, you get a depressurization rate of traveling back and instead depressurization wave that's going back that gives you the forces on the two support pieces.

MR. WALLIS: That's over way in the --

MR. KROTIUK: Don't' forget, the time scale on this is very small. I mean, yes, you're right, and I do have another problem which would --

MR. RANSOM: Comparing an eight meter pipe with one that, I don't know, is probably 40 or 50 meters long.

MR. KROTIUK: Agreed.

MR. RANSOM: While the time scale cannot be compared directly as far as what's important.

MR. KROTIUK: Right, and this is the first problem. I'm just trying to get some sensitivity of the ability of the code to predict this, and then I'll go to the next one.

MR. WALLIS: My concern was that NOSETS, whatever it is, that this solution from the code predicts what happens after a few milliseconds fairly well, but it doesn't predict the acoustic wave propagation, does it? Or does it?

MR. KROTIUK: It does, and the next case I show will show you specifically acoustic wave.

MR. WALLIS: Okay, thank you.

MR. BANERJEE: Can I ask you a question?

MR. KROTIUK: Yes.

MR. BANERJEE: In let's say something like the feedwater line break.

MR. KROTIUK: Right.

MR. BANERJEE: It's subcooled water like

the Edwards situation here. In the Edwards experiment, you don't show it, but I remember, since I know Tony Edwards. There was an incredibly large pressure undershoot. It almost went down to somewhere between around 1.5 on your scale, before the pressure came back up. That happened in the first few milliseconds.

MR. RANSOM: That's at the closed end.

MR. BANERJEE: This is at the closed end, he said.

MR. KROTIUK: Well, this is at the closed end, but that is --

MR. RANSOM: Oh, that is the closed end?

MR. KROTIUK: Yes, that is the closed end. There is, and I'm just looking at, and as I say, I didn't make copies of all of the test points. There is an undershoot at position 7, which is --

MR. BANERJEE: There was an undershoot at a number of points.

MR. KROTIUK: Yes, there is an undershoot, yes.

MR. BANERJEE: Where is that undershoot?

MR. KROTIUK: It doesn't show up on that particular set of data, but it does show up on some of the other ones.

MR. BANERJEE: Did you predict that undershoot?

MR. KROTIUK: Let me just look very quickly. I don't remember --

MR. WALLIS: It's just the reflection of a decompression wave, isn't it?

MR. BANERJEE: No, it's the subcooled wave that goes through --

MR. KROTIUK: No, it does --

MR. BANERJEE: For bubbles nucleate, and it takes a certain time to nucleate the pressure drops.

MR. KROTIUK: Right. Now, the code is not predicting that undershoot.

MR. WALLIS: What you would get with a reflected wave in just pure water.

MR. BANERJEE: Right, but a feedwater line break is that, right? I mean, it's water.

MR. RANSOM: If you didn't get vaporization, you'd double down, actually. So, it becomes very low, but vaporization actually keeps it from getting --

MR. BANERJEE: Yes, but the feedwater line is water.

MR. RANSOM: Sure, sure.

MR. BANERJEE: And it will hit that. There will be an undershoot before you start vaporization.

MR. RANSOM: Absolutely.

MR. WALLIS: It's a bit like a water hammer where the reflected wave wants to go down to pressures which are subatmospheric, but it creates vapor, and it doesn't go down, sub-atmospheric.

MR. KROTIUK: It's almost like a vapor formation followed by a collapse, something of that nature.

MR. BANERJEE: So, what happens? Do you feel that the wave that comes from the rarefaction wave in its reflection for the feedwater line break is not important?

MR. KROTIUK: It turns out the feedwater line is not the design case. So, it's not important in terms of the analysis that I performed. The steam line break turns out to be most severe.

But just to elaborate on what you said, I've done other work with TRACE, and that the problem you're alluding to, I have noticed in doing some of the other test comparisons, and that problem still has to be addressed. It has to do with, as you're saying, the flashing of subcooled liquid as the pressure drops and things of that nature.

There are some situations that that -- well, there are problems with the code that it doesn't exactly handle that situation.

MR. BANERJEE: Well, the reason I'm saying that is that I'm aware of a situation where in Sweden, we had to consider the feedwater line breakers being very important, and look at the pressure undershoot. So, I'm surprised you're able to do that without considering it.

MR. WALLIS: Does this pressure undershoot actually yank the end of the pipe? I mean, it pulls on it?

MR. KROTIUK: For the Edwards problem?

MR. WALLIS: Yes.

MR. KROTIUK: Yes, it will, over a short time frame, but whether the pipe responds at all is another thing. It's a dynamic response. I mean, you know.

MR. WALLIS: It's over so quickly that nothing much happens?

MR. KROTIUK: That's what I've, you know, working over the years with dynamic stress analysis codes, lots of times it doesn't. Of course, you have a dynamic amplification factor that many times you apply to static analyses to approach that type of calculation, and lots of times it's very small. It's close to one, basically.

MR. RANSOM: In fact, it's very difficult for these kind of codes to predict that because they're driven by heat transfer to interfacial area, and when you're in a pure fluid, you have no interfacial area. So, you have to use some kind of seating in order to get the process started.

That's been a issue with TRAC because they used to use seating that remained all the time, and so you'd see waves propagating at more nearly the ATM speed, you know, rather than a pure liquid speed. This came up in the Savannah River water, and I don't know whether that's been retained in the latest versions of TRAC. It was called like dirty water or spongy water. That was the word that was used.

MR. BANERJEE: There are two separate waves that go. One is the subcooled wave.

MR. RANSOM: Right, that's going through the liquid.

MR. BANERJEE: And the two-phase wave, which is the --

MR. RANSOM: Acoustic sound of the water, 5,000 feet a second.

MR. BANERJEE: Right.

MR. RANSOM: Meters per second.

MR. BANERJEE: I mean, that's like water hammer.

MR. RANSOM: Sure.

MR. BANERJEE: So I mean, when you break this, you get something like water hammer.

MR. RANSOM: Oh, absolutely.

MR. KROTIUK: It is. I'm calling it acoustic phenomena really, and if you have your water, it's going to be a water hammer.

MR. RANSOM: But the interesting thing is that if you have spongy water, the wave propagates at about less than 500 meters per second, you know. You know, 500 compared to 1000. So, it arrives, and in fact, if you look at those two curves, it looks like it maybe takes twice as long for the wave to arrive as it does in the data, and I don't know whether that's true or not. You'd have to blow up that region to find out.

MR. BANERJEE: So are you telling us that basically you're considering here the steam line break where the phenomena is not likely to be important but for the feedwater line break, it's likely to be important, and that will be talked about later, or what?

MR. KROTIUK: The steamwater line breaks and feedwater line breaks, but when you -- you'll see when I back calculate the loadings on the tube support plates, that the loadings that come from a steam line break are substantially higher than anything that would be developed by a feedwater line break. So, it's not of an immediate concern, even if you have this deficiency.

MR. BANERJEE: Why is that? Is that the back of the envelope calculation, or what?

MR. KROTIUK: That I will address specifically.

MR. BANERJEE: You will address it?

MR. KROTIUK: Yes, if you just --

MR. BANERJEE: The raptures and so on are not aggravated by these things? I mean, is the whole plate that's moving?

MR. KROTIUK: The whole, there's a force -- yes, there's a force that builds on the entire tube support plate.

MR. BANERJEE: Due to the imbalance of --

MR. KROTIUK: Due to the imbalance of the pressure across the plate.

MR. BANERJEE: And what happens to -- is there no possibility of raptures of the steam generator tubes?

MR. KROTIUK: That was the purpose for doing this because ultimately what I did is that after developing these forces on the tube support plates, the loadings was transmitted to basically people doing stress analysis who would look at the stress on the tube support plate and transmitted stresses to tubes, and they would make assumptions about, you know, was there a tube that maybe was possibly ruptured or ready slightly, or whatever, and would that aggravate the situation.

So, that's done -- the effects of that is done on the stress analysis portion. My main task was to develop the loadings for the stress analysts.

MR. SIEBER: There was a situation where a couple of licensees rolled the tubes into the tube support plate that locked in there.

MR. KROTIUK: Yes.

MR. SIEBER: And that becomes a erious problem when a steam line breaks because as the tube support plate acts as a membrane, it puts tremendous tensile stress on the tube because it's locked there. Ordinarily there's enough clearance so that the tube support plate can go up and down, more or less free of the tube except for the chemical crud that's sitting in there.

MR. KROTIUK: But again, I won't be addressing that part of this. I'm mainly concerned with just the forces developed, you know.

MR. SIEBER: Did you consider that the membrane may have a couple of nodes in it?

MR. KROTIUK: Excuse me?

MR. SIEBER: The tube support plate? You know, some calculations I have seen shows that --

MR. KROTIUK: No, because I'm not doing any of the stress analyses. I'm not doing that for part of this. This is just addressing the development of the thermal hydraulic forces on --

MR. SIEBER: This is just to get the forces.

MR. KROTIUK: This is just to get the forces, correct.

MR. SIEBER: All right.

MR. BANERJEE: But without the pressure undershoot.

MR. KROTIUK: Right, without the pressure undershoot that would exist.

MR. BANERJEE: In reality.

MR. KROTIUK: In the feedwater. I think this following one is kind of a nice problem that I like to use as a real example of more of what you would call almost like a water hammer type of an effect because this is a test that I had found a number of years ago of the semiscale blowdown test, and it's basically a tank that has a rupture starting on it. This is the initial conditions. Let me just show you the figure.

What it is is that we have a rupture at this location, and unfortunately there were only two points that the data was taken. One was near the rupture at this location and one at the end over here. This was initially filled with liquid, as I said, and then ruptured at this location. You could follow the transmission of the wave back and forth, and you could actually see the initial wave with a reflection, and so on.

I've chosen, I've done a number of comparisons.

MR. WALLIS: So now we're looking at milliseconds?

MR. KROTIUK: No, we're looking at very short time frames.

MR. WALLIS: Right.

MR. KROTIUK: And what I also did is that a number of years ago, I'd written characteristics codes to look at this specifically. It was modified to include the two-phase effects. So, I have that data plotted here, and let's see, that's in blue, compared to the predictions from TRACE and then the solid line is the test data.

MR. WALLIS: Now, is there any two-phase flow going on here, or it's all single phase water?

MR. KROTIUK: Let me just remember quickly. With this, it's towards the -- I think there was --

MR. WALLIS: It's all single phase water, isn't it?

MR. KROTIUK: There was primarily single phase, but I believe there was some vaporization towards the end of the problem. Let me just see if I can find that. Oh, here we go.

Yes, towards the end of the problem, and this was at what location? Again, I don't have everything, but towards the end of the problem, I was coming up with void fractions, and at this particular location that I see here is maybe between 30 and 40 percent. So, there was some two-phase effects towards the end of the problem.

MR. WALLIS: Well, if you're going to get that higher void fraction, then you must have some flow out of the break to make space for the voids to form in the vessel.

MR. KROTIUK: Yes. This was shown to, you know, yes, you would get some flashing as the liquid is coming out.

MR. BANERJEE: So if it gets pressure undershoot here in the single phase region, why do you say you don't see it in the Edwards case?

MR. KROTIUK: I didn't see it in the Edwards case, and to tell you the truth, I didn't look specifically at it, and maybe I should have looked more closely, but I don't know why at this point. This did predict this initial undershoot.

MR. RANSOM: Those are not necessarily undershoot at that point.

MR. KROTIUK: Well, it's a depressurization wave coming back.

MR. RANSOM: You mean undershoot compared to the saturation or --

MR. KROTIUK: The undershoot according to the depressurization.

MR. BANERJEE: Well, one is going through the subcooled liquid initially. Is this subcooled?

MR. KROTIUK: This is subcooled liquidation.

MR. BANERJEE: What happens is that, and this is well understood now, is it depressurizes things to below saturation, in fact, and bubbles start to nucleate, and as they grow, you start to get a two-phase wave. In order to catch that, of course, we have to have some way to look at bubble growth and nucleation as we were saying.

MR. RANSOM: Right.

MR. BANERJEE: I mean, many people have done work on this.

MR. RANSOM: What you might want to consider here is he's depressurizing from a small break into a rather large vessel, so the wave is already made smaller, tenuated, I guess to a certain extent as it spreads out and passes down the vessel. I would guess also you have all of the surface area, you know, the vessel too, which is creating nucleation. The combination of that, I don't think you really see much undershoot in this kind of experiment.

MR. BANERJEE: It's less than in the Edwards one.

MR. RANSOM: The Edwards was a whole pipe.

MR. KROTIUK: It was just a straight pipe, yes.

MR. BANERJEE: Where the undershoot was much larger than this.

MR. RANSOM: Right.

MR. BANERJEE: But he's also doing the calculation with a method of characteristics, presumably initially for single phase flow, right?

MR. KROTIUK: No, I tell you, that code that I used for that was initially a single phase code, but I had modified it myself and came up with a two-phase MOC code.

MR. BANERJEE: So how did you do that without an undershoot and so on? I mean, did you have a bubble nucleation model?

MR. KROTIUK: Yes, I did.

MR. BANERJEE: Okay, so where does the two phase flow start in this time frame?

MR. KROTIUK: Probably something of the order around this location here.

MR. BANERJEE: Okay, so you're still sustaining an undershoot in the single phase liquid.

MR. KROTIUK: Yes.

MR. BANERJEE: I'm just confused that you don't see it in there with the experiment, but you see it here. You know, we need to rationalize this.

MR. KROTIUK: Yes, I think that that's a valid concern, and it might be my fault for not just looking closely at it enough either.

MR. BANERJEE: But also, to get this undershoot, you need a bubble nucleation model. It just doesn't happen spontaneously. So, how do you get the void forming? Did you have a nucleation model of sorts, or does TRACE have one?

MR. KROTIUK: TRACE does approach that, and to some degree, but I don't know the details on that. That's what I was saying, that other problems that I have show that this model does not, you know, just some work needs to be done on that model.

MR. STAUDENMEIER: The TRACE model is undershooting the break flow, and that's how it computes. It has an undershoot model in computing its subcooled break flow. Then the flashing model in TRACE, which is, as Joe Kelly said before, isn't really physically based on anything as far as he could tell, is I guess the nucleation model for both cells as they get superheated. But in the break flow itself, the subcooled break flow model, does model that undershoot that you're talking about. I think it's a Lienhard-Jones or something like that.

MR. BANERJEE: So, that's the model which is used?

MR. KROTIUK: Yes.

MR. FORD: Could you say something about the scaling on this? I notice you say semiscale here, and yet it's nothing like the scale of a steam generator.

MR. KROTIUK: No, I know, but this is the LOFT -- it's called the LOFT semiscale facility, and this was a --

MR. RANSOM: Okay, well, it's just a tank.

MR. KROTIUK: It's just a tank.

MR. FORD: Okay, well --

MR. KROTIUK: It's not scaled --

MR. FORD: What uncertainties do I have in going from this test, which you've very successfully predicted, to a much larger problem? I'm not a thermal hydraulicist, so lead me through the thought process.

MR. KROTIUK: Well, my thought process on this was that I wanted to look at some applications that are more acoustically dominated and then look at some test data that had longer time frames so that I could see the effects longer out in time after the acoustical portion.

When you're talking about pool swell phenomena with the liquid on the bottom of the steam generator vaporizing. So, it was done in two stages, you know, looking at two different facts separately.

MR. FORD: Okay.

MR. KROTIUK: Okay. This was some testing using the GE vessel blowdown tests. Initially, this one had tanks, and this shows the nodalization that used in TRACE, and this was another tank with the nodalization. This gives you dimensions and pressures and temperatures.

What I've chosen to do is to just show -- this was done actually by ISL, this work. I just chose two points here to show the void fraction at different elevations within the vessel for one of the tests, and showing the test data versus the TRACE predictions.

MR. BANERJEE: What TRACE?

MR. KROTIUK: At the time I did this, it was called TRAC-M, and so the labeling is TRAC right now, but it was just an earlier version of TRACE.

MR. SIEBER: Before it was an engine.

MR. KROTIUK: Before it was an engine, right.

One interesting thing about this one, when

Birol did the analysis, he did this analysis with a level tracking model and without the level tracking model. Basically the level tracking model did produce some better results than without the level tracking model included.

Basically the comparisons, though, were pretty decent, with or without the level tracking model for this particular problem. It wasn't a major concern. Then this is just two points that I chose to show.

MR. BANERJEE: You're not really interested in the long time scales here. You're looking at the very rapid --

MR. KROTIUK: Well, relatively rapid. I mean, you know, --

MR. KROTIUK: This goes out to 200 seconds, but that's, you know, definitely shorter time scales.

MR. RANSOM: Which test are you showing here, the 1004-3?

MR. KROTIUK: It's the 1004-3, correct.

MR. WALLIS: This is different. The break is in the steam region.

MR. KROTIUK: The break is in the steam region, correct. It's more likely steam line break.

MR. WALLIS: And the time scale we're talking about here is very different from the milliseconds we talked about.

MR. KROTIUK: Right. We're looking at different things.

MR. WALLIS: Right.

MR. RANSOM: The most interesting part of this occurs in the early time when the level actually swells up, and even some goes out the break, as a matter of fact. So, there's a void distribution going up the vessel, which you can't see by just looking at two points. Apparently the bottom is -- this is near the bottom, I guess, at about .25. That's the bubbly regime.

MR. WALLIS: This is like Sanjoy's chemical plant where you open a vent and the level swell hit the vent.

MR. KROTIUK: Right.

MR. RANSOM: And a significant amount of water does go out the break during that, and then the later collapses down, and you just get steam flowing out, which this shows steam flowing out beyond 50 seconds.

MR. KROTIUK: Yes. This test was probably the most, the closest application because it is a steam generator model test. Basically there were two tests that I looked at. Again, this was done by ISL, this particular work. The model was, the RELAP5 model already existed, so it was translated to TRACE, and we looked at these two tests. It's a scaled steam generator.

The break is occurring on the top of the steam generator and basically we are looking at the swell effects through the steam generator. The actual geometries are shown a little bit more in detail here. I just wanted to point out a couple of things, is that we do have some support plates in the steam generator at various locations with pressure sensors along the central area.

These, among other comparisons that were made, I just chose two points across two and three, so this is a delta P measurement across two to three, with the data in green and the predictions in black. Then we have six to seven here, which is basically across the tubes sort of on the top. Again, the comparisons between test data and the predictions.

MR. RANSOM: What is happening on the primary side in this test?

MR. KROTIUK: The primary side, since it was a test, it was simply a flow rate. I just think the flow rate was -- let me just make sure if I wrote it down, or I didn't write it down. The flow rate was just maintained at the conditions.

MR. RANSOM: Yes.

MR. KROTIUK: There was a heated flow coming in here.

MR. FORD: Again, calibrate me. I heard early on Graham saying that pressure, changes in pressure was a relatively easy thing to predict, and you're seeing here that the system response is reasonably well done, and yet I look at those two curves, and they're fairly far apart in certain parts. Am I misreading Graham's promise to me that it's an easy thing to do?

MR. KROTIUK: Well, remember one thing on this, is that we are not -- I didn't plot up absolute pressures here. I'm plotting up pressure differentials now, and that's a little bit different than saying matching pressures.

MR. RANSOM: I'm wondering what causes that pressure differential and the change. I mean, in the real situation, I guess flow across a tube bank, and I don't know that much of that is in the codes, though.

MR. SIEBER: No.

MR. RANSOM: You know, in terms of the obstruction or the drag, and especially to come down and go back up. I'm not sure what --

MR. KROTIUK: Well, there's a certain amount, and I'll elaborate on this a little bit more, but I actually did a hand calculation following an acoustic wave, starting at the steam generator and coming back down. The time frame that you're looking at is actually, the time frame for the travel of acoustic wave length, it does match up.

MR. RANSOM: What, going down the downcomer?

MR. KROTIUK: In other words, like say I would have a just for argument's sake, let's just go through this one here. Say a break at this location, you get a depressurization wave that would occur at the break and then travel down the steam generator.

MR. RANSOM: How tall is that?

MR. KROTIUK: What were the dimensions on this? I don't remember that.

MR. RANSOM: Twenty meters maybe.

MR. KRESS: Seven meters.

MR. RANSOM: Seven meters.

MR. BANERJEE: Presumably initiates at time zero, right?

MR. KROTIUK: Well, that tested an initiate times zero. They sanctioned this --

MR. BANERJEE: What is the initiate?

MR. KROTIUK: Sixty.

MR. BANERJEE: Right, okay.

MR. KROTIUK: Sixty.

MR. BANERJEE: That's times zero?

MR. KROTIUK: That's the time zero, right.

MR. RANSOM: When do you break it?

MR. KROTIUK: That's when the break occurs.

MR. CARUSO: How do you model the acoustic wave propagation through the steam separator and to the dryer?

MR. SIEBER: You probably don't.

MR. KROTIUK: It's the way that you're using say a controlled volume approach, is that it's basically resistances, and if you notice on the test, for instance, on the semiscale test, the damping, in other words, the damping of the pressure wave is actually over damped, and the test data is -- and the predictions when compared to the test data. So, there is a way that the code is using the shear basically, the friction within the solution of the conservation equations that is coming up with drops in pressure.

MR. BANERJEE: But the acoustic wave is traveling at the speed of sound.

MR. KROTIUK: The speed of sound, or either steam, or if it's two-phased, it would, you know --

MR. BANERJEE: Let's talk about the steam rate now.

MR. SIEBER: Right.

MR. BANERJEE: In the steam line, and as it's traveling, the fluid is also accelerating, right?

MR. KROTIUK: Yes.

MR. BANERJEE: So U minus A, which is the speed of propagation against the flow.

MR. KROTIUK: Okay.

MR. BANERJEE: The flow reaches the sound speed, it chokes.

MR. KROTIUK: And that's when you get the pressure wave traveling back.

MR. BANERJEE: Traveling back.

MR. KROTIUK: Right.

MR. BANERJEE: So, how do you calculate this by hand because in a way, the whole flow is accelerating, right, so there's inertia that you have to take into account against that.

MR. KROTIUK: Could I answer that in about four slides?

MR. BANERJEE: Oh, okay.

MR. KROTIUK: In about four slides, I actually have that.

MR. RANSOM: One thing that I think is important here is if you like, let's say this is seven meters long and, you know, even on small speed, would be 100 meters per second.

MR. BANERJEE: 300 Meters per second.

MR. RANSOM: So, anyway, that's only 700ths of a second, the wave reaches the bottom. Most of what you're seeing is a two-phased response, you know, and probably due to things other than just acoustic propagation.

MR. KROTIUK: For this problem, yes. For the other problems, for the semiscale, that's different.

MR. RANSOM: Yes, that's different, but for this one, you know, you've got 100 second time scale there.

MR. KROTIUK: Absolutely.

MR. RANSOM: The transient is over, and most of the acoustic part is over in the first second.

MR. KROTIUK: Right, but again, like I said, I was looking at two different phenomena. I was looking at the acoustic part and I was looking at the pool slope part.

MR. RANSOM: Right. I think most of your changes in pressure are due flow that's induced within the tube bundle.

MR. FORD: So, I'm being a bit thick here. I'm trying to understand what this data is telling me. On the secondary side of this model generator, you've got a pressure of 1,101 psi absolute.

MR. KROTIUK: Yes.

MR. FORD: And those graphs you're showing there are telling me that the pressure changes by one psi?

MR. KROTIUK: Across the tube support plates, yes. Well, let me backtrack on that just a little bit. These are measurements of absolute static pressure. So, in actuality, the force on a tube support plate is not the static pressure difference, but you have to look at the pressures that are due to the flow phenomena through the plate itself.

So, this is just comparing, looking at the static delta P, when are you actually calculating the supports on the tube support plates, you have to do -- you have to use the information that is calculated by the code with the velocities, the static pressures and all, and calculate the actual force on the tube support plate.

MR. RANSOM: So the void fraction probably enters into it.

MR. KROTIUK: The void fraction does enter into it.

MR. FORD: The pressure is what you measure.

MR. KROTIUK: The pressure is what I'm measuring, yes.

MR. WALLIS: Regardless of the cause.

MR. KROTIUK: Right, but for instance, I'll show you for this particular --

MR. WALLIS: Not much of a pressure.

MR. FORD: Why does it go to zero at the end?

MR. KROTIUK: It's --

MR. SIEBER: The amount of water.

MR. WALLIS: The amount of water? So, everything becomes the same pressure?

MR. SIEBER: Basically your flow is coming down low enough that you're depleting your mass.

MR. CARUSO: What's the maximum delta P across all of those tube sheets?

MR. KROTIUK: Okay, well that's -- I'll

show you. Well, no, there isn't.

MR. BANERJEE: Well, it seems to be .7 or something like that.

MR. FORD: Point 7 or one.

MR. KROTIUK: Hold on, let me make sure I put up the right one. Okay, here. For this one right here, which is 2-3, so that's the bottom part here. If I do the calculation to convert the void fraction velocities, densities, everything to an actual force on the tube support plate, okay, just come up here. It actually looks like this.

MR. WALLIS: Pretty small pressure differences.

MR. KROTIUK: Yes, they're pretty --

MR. SIEBER: Well, it's lower in the generator. If you were right on the tube sheet, the velocity would always be zero.

MR. KROTIUK: Correct.

MR. SIEBER: The higher you go in the steam generator, the higher the velocities get, and the higher the DP's get.

MR. KROTIUK: So, that plate is actually outlined there, when you back calculate that.

MR. FORD: So is the conclusion that we are coming to right now, is that there's not much of a pressure across, a bending pressure across that plate?

MR. KROTIUK: That's for this particular plate, right. Now I will try to look at an actual steam generator design with conditions.

MR. SIEBER: That bottom plate --

MR. WALLIS: Well, this pressure drop looks as though it's the pressure drop between PO-2 and PO-3, which are pressure taps, which are actually -- what are these units, in centimeters and inches? They're actually two feet apart or something, which is one size. Is this hydrostatic head?

MR. KROTIUK: The hydrostatic head does enter into it, yes.

MR. WALLIS: That's what it looks like. It just looks like hydrostatic head. It's all that's happening. So, it's a very mild --

MR. KROTIUK: But this is not hydrostatic head here. This is the actual force across the plate, because this is a calculation, and I'm sorry. I could give you a copy of this. I just didn't include it in the presentation, but this is the actual calculation of that force on that plate itself.

MR. SIEBER: One would expect the bottom tube support plate for the shock wave to be a higher force than the flow.

MR. WALLIS: This is nothing to do with shock waves. This is way, way down.

MR. KROTIUK: This is long because this is out from the 30-second time frame.

MR. SIEBER: The flow is from the break. The shock wave there probably would dominate, whereas at the top, it would be reversed.

MR. KROTIUK: No, there's a shock wave effect on the -- if you wanted to talk about the pressurization rate.

MR. SIEBER: I'm talking about relative forces.

MR. KROTIUK: And actually, I kind of disagree with that. I think you're going to have, from what I've seen, you actually have a bigger force on the top than you do on the bottom, and you might actually get a force reverse on the bottom, possibly.

MR. SIEBER: Okay. I think that's what I said.

MR. KROTIUK: Oh, okay. Is that what you were saying?

MR. FORD: Now, I assume that these differential pressures across the tubing will depend on the design of the holes, et cetera?

MR. KROTIUK: It depends upon the design of the tube support plates. That's a criteria, yes. That is a --

MR. FORD: Is it by much? I mean, it's not much of an argument to change, but does it change it much? If you had a quatrefoil type hole.

MR. SIEBER: It makes a big difference.

MR. FORD: A big difference, I would imagine.

MR. KROTIUK: I didn't look at other drawings.

MR. SIEBER: The drilled support plates don't have much of a flow area. There are some extra holes in there where they aren't there.

MR. KROTIUK: Specifically for -- yes.

MR. SIEBER: But the flow area is pretty small. The quatrefoil, you open it up by you probably increase the flow area, available flow area by a factor of ten.

MR. WALLIS: There must be some flow through these plates.

MR. KROTIUK: Yes, there are flow through the plates, yes. There are actually holes that are drilled through the plates.

MR. SIEBER: Yes, they're there to avoid

the normal differential pressure you would get during the operation.

MR. KROTIUK: That's correct.

MR. SIEBER: But the flow through the holes where the tubes are is generally either non-existent or very low. It's not high enough to typically clean the product.

MR. WALLIS: It flows through somewhere else then. There must be other holes.

MR. SIEBER: Well, you can go around the outside. They can go through the drill holes.

MR. KROTIUK: Through the flow holes. There are flow holes in the plates.

MR. WALLIS: Isn't this pressure differential simply the pressure drop through the flow holes?

MR. KROTIUK: Correct.

MR. WALLIS: That's all it is.

MR. KROTIUK: That's correct. That's it.

MR. SIEBER: That's right.

MR. KROTIUK: But it's not the static pressure. That's what I was just trying to -- it's a pressure drop through the flow holes, which includes any gravitational effect, any frictional effect, and the acceleration effects.

MR. WALLIS: Right, but it seems such a small value. Is it a problem?

MR. FORD: That's why I'm asking the question. And you're going to get to the real answer any minute.

MR. KROTIUK: Okay.

MR. SIEBER: Part of the design is so that you can accommodate this boiler, you know, because that's what it does during normal operation, and the outsides of them are not closed in.

MR. FORD: I think the original concern was if you have a means to, you might break the whole tube sheet. It's going to buckle from -- just because there's always that chance.

MR. SIEBER: That's different. The differential pressure on the tube sheet is 10,000 pounds, and when you remove the secondary pressure, you have --

MR. FORD: Across the tube itself.

MR. SIEBER: No, the tube sheet. If you look at the channel head in this sketch here, it's 2000 pounds in the channel head, and once you have a steam line rupture, the secondary side goes to zero.

MR. KROTIUK: Yes.

MR. SIEBER: So that will push the tube

sheet up.

MR. WALLIS: You're talking about a different tube sheet, aren't you? Talking about the bottom of the whole thing?

MR. SIEBER: Yes.

MR. KROTIUK: Yes.

MR. WALLIS: But he's talking about the separators.

MR. SIEBER: No, he said the word tube sheet.

MR. RANSOM: He means tube support plates.

MR. KROTIUK: What I'm looking at is the tube support plates, but you're right regarding the tube sheets.

MR. SIEBER: Peter said tube sheets.

MR. KROTIUK: Tube sheets is different.

MR. SIEBER: Tube support plates are different.

MR. FORD: Tube sheet is a massive thing.

MR. SIEBER: But it bends.

MR. FORD: Yes.

MR. SIEBER: It's not designed to take the full RCS pressure.

MR. WALLIS: Okay, but in the steam line break, it does, doesn't it?

MR. SIEBER: It does, and it will distort but not fail.

MR. BANERJEE: And the tubes will not rupture, either.

MR. SIEBER: That's correct, because there's, you know, it's a factor of three margin on the tube.

MR. BANERJEE: Even if they are slightly deteriorated.

MR. SIEBER: When they had the steam line break at Turkey Point, the tube sheet was bent. I mean, it was bowed up, and bowed up enough to break the weld on the divider.

MR. WALLIS: I don't know what GSI 188 is, so I don't know what question is being asked. That's probably one of my problems.

MR. KROTIUK: Well, I think, as I said, the important aspect that I'm looking at is simply the development of the time dependent loadings on the tube support plates within the steam generator.

MR. WALLIS: Okay.

MR. KROTIUK: Okay, to take this comparison a little bit further, I took an analysis that was done on a Westinghouse Model 51 steam generated that Westinghouse had done using TRANFLO and

RELAP5. I'm basically going to repeat that analysis with TRACE, and looking at conditions of hot standby, 100 percent power, and then two steam line break sizes and one feedwater line break size.

This represents a full guillotine break and a full guillotine, and this is deep flow restricted break.

I have looked at some generated forces, not only against the tube support plates, but of course primary tube where they bend and also of course, the cylinder around the -- in the steam generator itself. I'll only present tube support plate values.

MR. SIEBER: Right.

MR. KROTIUK: This is the actual steam generator, and we have tube support plates. Let's see, one, two, three, four, five, six, and then seven.

MR. SIEBER: There are supposed to be seven. That's a Model 51.

MR. KROTIUK: That's a Model 51.

MR. SIEBER: Okay.

MR. KROTIUK: And this shows the nodalization of the secondary site itself, and this is showing the nodalization on the primary side with the heat transfer nodes, representing the tubing itself.

MR. SIEBER: Right.

MR. RANSOM: Jack, are those baffle plates? Do they extend all the way across the two banks, or do they cause cross-flow?

MR. SIEBER: No, they go all the way across, but not to the wrapper. In the outside of the wrapper is the downcomer. So, there is flow space between the inside of the wrapper and the outside of the tube support plates. Plus, there's also holes drilled in there, their slots.

MR. SIEBER: In fact, from what I understand, they just really fit. In terms of support plates themselves, they only fit on little ledges, and maybe tack welded or something of that nature.

MR. SIEBER: Okay, it's not a strong --

MR. RANSOM: They're not baffles. They cause cross flow.

MR. WALLIS: They're to vent flow induced oscillations? Is that what that --

MR. SIEBER: Pardon?

MR. WALLIS: Did they have to prevent flow induced oscillations at the tubes?

MR. SIEBER: Yes.

MR. KROTIUK: Okay, let me show you a comparison. What Westinghouse had found in their analysis is that the full double ended -- I'm sorry, the guillotine steam line break produced the largest forces, and this day, the conclusion was based on also the TRANFLO or the RELAP5 analysis.

What I'm presenting here is a comparison of what Westinghouse presented using TRANFLO calculated using RELAP5 and then the TRACE calculations. This is for the guillotine steam line break, and then on the bottom here, I also presented it for the limited flow steam line break. These are comparative. This one is a slightly different scenario.

MR. WALLIS: Are these loadings at some particular time in the transient?

MR. KROTIUK: These are the peak loadings on the tube support plates. Now, what you could see here is that that top plate can get a fairly significant loading on it.

MR. SIEBER: Yes.

MR. KROTIUK: And this is, again, the worst case, and you're talking about 9 psi across that plate.

MR. RANSOM: What does Westinghouse believe? I mean, TRANSFLO is quite a bit lower.

MR. KROTIUK: Yes, I can't trust what

TRANFLO is doing.

MR. RANSOM: That's the Westinghouse code?

MR. KROTIUK: That is a Westinghouse code.

MR. FORD: Now, when you say you don't trust it, what was your basis --

MR. KROTIUK: I don't know much about it. The results are so different from either RELAP5 or TRACE.

MR. RANSOM: Westinghouse believes.

MR. KROTIUK: Well, in the report that I have from Westinghouse, they had originally reported the TRANFLO results, and then subsequently did the RELAP5 analysis because it is implied -- it didn't say directly, but it was implied that they themselves were questioning what was coming out of the code, out of TRANFLO.

Subsequently, they instructed their, you know, their plants to anyone who was doing an analysis, and I have a copy of all of the reports. All of the subsequent reports were done using RELAP5, subsequent analyses.

MR. RANSOM: Well, the agreement between TRACE and RELAP5 would kind of indicate that something may be wrong with the other one.

MR. BANERJEE: Well, you don't know,

because TRACE and RELAP5 are similar.

MR. SIEBER: They're actually the same, are they not?

MR. STAUDENMEIER: The thing about TRANFLO, this is Joe Staudenmeier. I was in NRR when they first submitted a calculation like this with TRANFLO, and it had no code assessment at all, and they refused to assess it, so we refused to approve it for the application.

MR. SIEBER: There you go. Is that what gave rise to the flow restrictors in the outlets, this calculation, or was that before that?

MR. STAUDENMEIER: I don't know what gave rise to the flow restrictors, but it wasn't this calculation that gave rise to them, I don't think.

MR. SIEBER: All right.

MR. FORD: But the reason, quite apart from pride, we're saying that TRACE and RELAP, which are the same, is the correct answer rather than TRANFLO, is because of the agreement you see in this model test, this one here?

MR. KROTIUK: Yes.

MR. FORD: But you haven't seen or done equivalent analyses of this test for TRANFLO, because you don't have access?

MR. KROTIUK: Don't have access to the code, no.

MR. WALLIS: TRACE and RELAP are supposed to be equivalent, and some of these numbers are quite different. 3.2 psi isn't 1.16 psi, and 2 psi isn't 1.15 psi. I'm not quite sure what to conclude from this.

MR. STAUDENMEIER: The one thing that hasn't been discussed is these are real sensitive. When the steam generator starts flashing, it has to decide where the stagnation point is, and then from that point, flow will go up and then flow will go down. So, part of the bundle flow will be going up. The other part flow will be going down, and up the downcomer.

MR. WALLIS: That's negative pressure.

MR. STAUDENMEIER: That's right.

MR. KROTIUK: That's right.

MR. STAUDENMEIER: In that the answers, the peak load is real sensitive to where that point is where the flow splits. If you want to do a true boundary calculation, you could block off the downcomer, make all the flow go up through the tube sheet, and that would give you a peak load.

MR. KROTIUK: I'll address that. I have

a slide that actually talks about that.

MR. SIEBER: That's why you get negative numbers in these lower nodes.

MR. KROTIUK: And I'll address that in a little bit more.

Just to sort of complete the picture here in terms of the TRACE calculations, this table simply just presents the range of breaks that were looked at, the steam line breaks and the feedwater line breaks at the hot standby and in 100 percent power conditions. So, the conclusion that was originally given, that this was the most severe case, was borne out by what TRACE said.

MR. SIEBER: That's a double ended break there.

MR. KROTIUK: Double ended steam line break, right, at hot steam line.

MR. WALLIS: Which is what you might expect, that the biggest hole gives the biggest load?

MR. KROTIUK: Yes.

MR. SIEBER: No, well, you would expect these results from that kind of a break, but you would not expect that kind of a break as the primary initiating event. More likely have a break in a bolted joint like a water safety valve was bolted on or something like that, as opposed to the pipe itself.

MR. KROTIUK: For that most severe break, and what I previously listed on the tables were these maximum values. To show you what the actual transients looked like, and this is the time frame of two seconds. You can see the highest loading was on the top tube support plate, and you can see the slight negative loading on the lowest tube support plate.

MR. WALLIS: Are these support plates just rest on some sort of a --

MR. KROTIUK: Just a ledge.

MR. WALLIS: They may bounce on the ledge?

MR. SIEBER: No, there is threaded stay rods that separate them. So, they're, you know, they don't all fall through the bottom.

MR. WALLIS: They might pop the stay rods?

MR. SIEBER: Pardon?

MR. WALLIS: If this load is big enough, you might break the stay rods? Is that it?

MR. KROTIUK: Well, that's --

MR. SIEBER: I would doubt it. I think it's designed to be sturdy enough so that that doesn't happen under steam line break. The more likely thing would be that the tube support plate would act as a membrane and get into some oscillatory node where it pinches the tube and pulls the tube.

MR. KROTIUK: That's a primary concern, but again, that's what the stress analysts would be looking at because they would be looking at the movement of the plate.

MR. SIEBER: Right.

MR. KROTIUK: Now, as a further check on this, what I did, there was a conservative bounding calculation, and this is now we're going to be talking about the transmission and reflection of acoustic waves.

To show that this phenomena really was an acoustic wave phenomena, what I did is that I used Moody's approach for just calculating blowdown from a basically a tank of liquid, and from that approach, you could calculate a value for the depressurization wave upstream of a break, and then doing -- this is a tedious, hand calculation where I'm actually looking at relative flow areas between the pipe into the steam generator into flow restrictions in the steam generator and so on, to follow how the depressurization wave would be transmitted or reflected on these various objects.

Then coming finally to the tube support plate, knowing the value of that pressure at the top of the tube support plate, and assuming at an instant of time that the pressure underneath has not changed, and then coming up with a result in delta P across that tube support plate, and then following it down to the seven-tube support plates.

The one caveat of this is that I didn't follow what you were talking about, the transmission of a wave back down the annular area. Now, I did that for only one point because that started getting really very tedious to follow by hand.

MR. SIEBER: Yes. That's probably a secondary effect anyway, is it not?

MR. KROTIUK: Well, I'll show you. For that one value, I'll show you the comparison.

MR. SIEBER: All right.

MR. KROTIUK: Now, this is the comparison of the Moody calculations, initial conditions, and then for the guillotine steam line break and for the restricted area steam line break, and I just put here the TRACE calculated results at standby 100 percent power conditions, and just the various comparisons. For instance, the maximum break flow rates, and the discharge pressure, which is really the initial depressurization pressure.

Then for the limited break, which has a

now FL over D factor in it in terms of the Moody methodology, and then comparison of the flow rates and the pressures at discharge.

Now then using this data, I did a hand calculation for the forces on the tube support plates, and for the two cases that I looked at, the guillotine steam line break and the limited area steam line break, these were the comparisons, and you could see, I just again listed for the hot standby and 100 percent power condition, the comparison of the calculated pressures across the tube support plates.

Basically at least they are somewhat in agreement. Now, one of the things that I was concerned about is again this bottom plate and the fact that I didn't calculate for the depressurization coming down the annulus area. So, I did that calculation to adjust for that, for the annular feedwater area, and came up with this reduction in pressure using the Moody approach. So, it comes out from 3 psi to 1.6 psi across that bottom plate, just --

MR. SIEBER: This is pushing.

MR. KROTIUK: No, this is pushing up because it comes -- this is a --

MR. SIEBER: The other one is a vacuum.

so, it's pulling.

MR. KROTIUK: This is actually pushing up right here. This one is actually pushing down.

MR. SIEBER: Right.

MR. WALLIS: This is acoustic wave in pure water?

MR. KROTIUK: This is --

MR. WALLIS: It goes through in the tenuators. It goes through the holes and --

MR. KROTIUK: It's through steam and water.

MR. WALLIS: Steam and water?

MR. KROTIUK: Right, because the top of the steam generator will have steam in it.

MR. WALLIS: Okay, so this is basically propagation through with the two-phase mixture which is there when the break occurs?

MR. KROTIUK: Correct, and again, I came up with an appropriate sound speed to use for that.

MR. WALLIS: Okay.

MR. KROTIUK: That application. Just to indicate again, this is the again, just showing the comparison for that hot standby condition with the full break, the comparisons between TRANFLO, RELAP, TRACE, and the Moody acoustic calculations.

MR. SIEBER: Now, the Moody and whatever other one you pick are additive, right?

MR. KROTIUK: What do you mean?

MR. SIEBER: You would add the two together? You have one due to flow and one due to shock.

MR. KROTIUK: No, this is the --

MR. SIEBER: That's the differential, or is it --

MR. KROTIUK: This is the differential pressure across tube support plates.

MR. SIEBER: It's the absolute value including both effects?

MR. WALLIS: This is shock. This one's due to shock.

MR. KROTIUK: This is the absolute value. This is due to the --

MR. WALLIS: This is the shock one.

MR. KROTIUK: This is the one due to the travel of the acoustic wave.

MR. WALLIS: The shock, right.

MR. KROTIUK: Right.

MR. SIEBER: Right.

MR. WALLIS: And the flow is the different problem altogether.

MR. KROTIUK: Yes, but you know --

MR. SIEBER: It occurs at a different time, too.

MR. KROTIUK: Yes, but there is a relationship, you know, because as the wave is traveling --

MR. WALLIS: This is the flow.

MR. KROTIUK: Yes, this is the flow, right. So, you could calculate a force across the tube support plate by, you know, two different methods. One is looking at the pressures at the exact top and bottom of the tube support plate or by calculating the pressure drop.

MR. WALLIS: The CV and the delta P and that sort of stuff.

MR. KROTIUK: Right.

MR. SIEBER: Okay.

MR. KROTIUK: And this was for that smaller break size, the steam line break size again, comparing again, calculations with TRACE, RELAP and TRANFLO.

MR. SIEBER: Now that's about the size of a safety valve flange, right, that 1.4? The big one, an agents valve?

MR. KROTIUK: Yes, I don't remember

specifically.

MR. WALLIS: Now, all these wave propagation methods assume that the metal surfaces are not compliant? Then you load the support plate with these loads and see what they do. In reality, the support may be compliant, and that helps to attenuate the weight.

MR. KROTIUK: There will be --

MR. WALLIS: This is in the model.

MR. KROTIUK: There will be some effects of that, and I have read reports about codes that do the fluids, acoustic and the structure. In other words, it does it simultaneously.

MR. WALLIS: It doesn't gain you much, does it?

MR. KROTIUK: It depends upon the problem. I've seen --

MR. WALLIS: In this case, do you think?

MR. KROTIUK: No, in this case, no, but there are problems that have seen that effect being important.

So, this is the last viewgraph that I have, and basically I'm just trying to, again, show what I was trying to do, is generate the forces on the internal forces in the steam generator due to steam line break or feedwater line break. Use TRACE to do the calculations. Seem to predict results that were consistent with both the RELAP5 calculations and with the conservative hand calculations.

It does appear that the largest forces on the steam generator were due to the acoustic effects, not to the long term full swell effects.

MR. FORD: But even so, they're very low for the tube support rates.

MR. KROTIUK: Yes, well, you're talking about 9 psi. Do you consider that low?

MR. WALLIS: It depends how well it's secured.

MR. KROTIUK: Yes.

MR. SIEBER: It depends on how big the tube support plate is. Nine pounds over a big area is a lot.

MR. STAUDENMEIER: Actually, the original problem, too, was that there were these cracks hidden underneath where the tube went through the tube support plate, and it was a calculation of if the tube support plate moved far enough to expose the crack, and then it would open up.

MR. SIEBER: This sounds like the work that was done when the DPO and steam generators was --

MR. WALLIS: But if the support plate is going to move enough to do that, then you've got to put it into the analysis, because its compliance is going to affect everything.

MR. KROTIUK: Well, it depends how much you calculate.

MR. STAUDENMEIER: It's going to move about a quarter inch or something like that.

MR. KROTIUK: Yes. You calculate it's going to move. I mean, I happen to know, you know, the stress analysis has been done, and I happen to know that movement was not --

MR. WALLIS: Not very much?

MR. KROTIUK: Not very much, no.

MR. WALLIS: Okay.

MR. KROTIUK: In fact, they sort of came up with the conclusion that it wasn't really a problem.

MR. WALLIS: So there's nothing in the new calculation that would make us think it is a problem. Was that sufficiently different from the old one?

MR. STAUDENMEIER: I guess the other assumption in it is that the tube would stick at its maximum deflection by some crud build-up because the maximum DP isn't right at the steam line break. It's actually -- that's not too big there, and they would evaluate this break-up in criteria at the maximum DP, which as 2500, which was after the steam generator had totally blown down and they had refilled the system water solid because of some other failure, and it would go up to the primary safety valve limit, essentially, and that was the DP they used, and they used the peak port plate deflection that happened in the first second or so, and assumed that it locked there.

MR. FORD: And I assume that some tests would have been done at Argonne or wherever, to see whether the deflection in the plate at the hole, assuming that you had a circumferential vector site crack in that crevice where that didn't just shear off the tube.

MR. STAUDENMEIER: I don't know anything about that.

MR. KROTIUK: That's not in our area.

MR. SIEBER: Not for the DPO. I remember the calculation because they had graphics of the motion of the tube to port plate.

MR. CARUSO: I believe that's what we're going to hear about in February.

MR. WALLIS: So this GSI has not been

resolved?

MR. KROTIUK: That's correct. February is planned for a person to present.

MR. SIEBER: It was very interesting. Thank you.

MR. KROTIUK: But as I said,, the main purpose of this was to show an actual application.

MR. SIEBER: Yes, well, I think we had seen it before from the other end where we were looking at the gross effects as opposed to how it was calculated. This fills in a lot of the blanks.

MR. KROTIUK: Okay, good.

MR. WALLIS: Okay, are we through for today? Thank you very much. This is just for your interest.

I don't think we're writing a letter. This is part of our investigation of this code. Our intent was to have a meeting today, and have another meeting and another meeting, and three or four or whatever, and really to make sure that this code is coming along, to see if we could add value in any way to what you're doing.

If it were appropriate, to say at this time that things are good or bad, or you need to change direction or anything. We might want to think of writing on that. I think the intent was so we would not have a full committee and write a letter unless the subcommittee thought it was really necessary.

We will make a report to the full committee on what we have seen here.

MR. BANERJEE: When is the letter due?

MR. WALLIS: I think the immediate concern we have is we have to write a research report where we evaluate the research that's been going on, and this would be input to the research report, which is being written now. That's where there would be an influence.

MR. SIEBER: Our comments are due tomorrow. I'm not sure I have the draft.

MR. WALLIS: Would my colleagues like to make comments now that would be fed back to the staff, and it would help me, too. I have to make the report to the full committee. Would you like to make some comments now?

MR. RANSOM: Well, I'm encouraged. You know, I think that the effort certainly is better off than I thought it would be, I guess. At the same time, it's still a little disturbing that we've spent six years and still, you know, and probably two to three years more before you can really utilize this thing in a regulatory framework, but that's I think the state it's in, and at this point, there's really no going back. I mean, you have to finish the job.

I think there are a few issues that we can certainly take up in future meetings and maybe add value, as you say, in terms of some of the energy partitioning, some of the momentum treatment. I guess those are the major areas that I see right now.

I think one thing that the NRC ought to think about is independent assessment of the code. It's unusual to have the developers actually doing the assessment, and I guess there is some independent assessment being done by the ISL contractor, which provides a measure of independence, but you'd like to be able to look at the warts as well as the successes.

I know that from a development point of view, just from my own perspective, you always try to show your best, and put your best foot forward.

MR. WALLIS: I thought we saw some warts today, didn't we?

MR. RANSOM: Pardon?

MR. WALLIS: Didn't we see some warts today?

MR. RANSOM: Well, I think we saw a few,

but it kind of makes you wonder if there really are more serious warts somewhere.

MR. SIEBER: We were shown a few.

MR. RANSOM: That's all I had.

MR. FORD: My concern yesterday was that I didn't have any clear idea of what the definition of success was, the quantity to the expectation of whether the TRAC or the TRACE was any better than the existing codes. Steve put that concern to bed largely.

My recommendations on that issue, in terms of the quantification of success, it returns to accuracy, comes from my much aligned colleagues who see materials as scattered all over the earth. I guess they are.

The first thing is that I hope sufficient attention is given to the quantity of data against which the code is being assessed. I still find it curious that we allow licensees to get away with just one set of data.

The other question is the quality of the data, its relevance to the reactor, and the quality of the system definition. That comes directly out of my materials background because I'm sure it applies here.

I think the metric of accuracy should be the mean and the variance of the ratio of the calculated to observe parameter for the PCT or whether the reflood temperature or whatever. You have to assess if that mean is closer to one and the variance is smaller then those calculated for RELAP or TRAC, P or B, and I think the acceptability of criterion should be somewhat like the sigma methodology, that the variance should be small enough where you don't have any risk for the outlines.

MR. KRESS: Well, I thought I saw a lot of progress since the last time we reviewed, and I too am encouraged that they're on the right track. When they get to the point where they've got the architecture right and the SNAP working correctly and the glitches out of the code and the models corrected to where they think we have some good models, then I think you need to start thinking about having a built-in uncertainty capability.

I think I would give that high priority to the code. I don't think that's part of the program right now. I'm not sure.

Then there needs to be some thought given in my mind to how to use the experimental data to develop this uncertainty. Now, I don't think it's as straightforward as you seem to think it would because

--

MR. FORD: Probably not.

MR. KRESS: Yes. I think the uncertainty is the measure of the quality, how good it is, but you don't just -- what you've got is a set of separate effects data. You've got a set of some integral data, and these are transients and different figures of merit, and they go through different time frames, and how to convert that into an uncertainty is not clear to me, but I heard some things from Steve on how we might do that with looking at different phenomena and different time frames, and I was encouraged that he's on the right track with how to develop some sort of a measure of uncertainty.

With respect to that uncertainty and what it means, is I think we need to give some real serious thought on how we choose node sizes. You know, I have never been enamored with this choosing node sizes for the experiments, and making the full scale node size look like that. I've never been happy with that, and that needs to be given more thought on how we do that.

I still think I'll fall back on the old canard that you bury the node size until it doesn't make anymore difference in the answer, but then you can't have a node size that's tuned to the experiment if you do that. So, I think some more thought needs to be given along those lines.

With respect to some of the models, we still are basically in 1D space, and there are places where I'm sure multi-dimensions are important. For example, in the momentum uses at T's and expansions and places, and where I think a CFD calculation has a compliment to the TRACE would be useful, but I'm not sure how it's to be done. I think there's a need to have a CFD code as one of the plug-in modules. I don't know if it's possible or not, but that's something that can be thought about.

I don't give much value in having the difference between the TRACE and say the TRAC-P and the TRAC-B, just as long as they're qualitatively similar. I think this qualitative assessment is good enough. What I want to see eventually is to have an acceptable uncertainty with respect to real data, and with respect to that. I don't know what the definition of acceptable uncertainty is.

I guess I also don't put much value in comparing the run time to the previous codes. So long as you get a run time that's good enough to use with the computers as we now have, I don't think it has to be faster or better than the run time in the alcoves.

It just has to be run time that's fast enough that you can efficiently use it without wasting all your time.

I guess a frivolous comment is, I don't think TRACE is a very good name for this code. I wish you would have come up with a different one, but that's just a personal thing. Somebody is going to think it's for looking at TRACE contaminants or something.

Anyway, that's my feelings right now.

MR. WALLIS: Thank you.

MR. KRESS: I'm encouraged. I think they're on the right track. I like the way they're going, and I think they're doing good work.

As far as peer review and the developers maybe not having as good and skeptical, I didn't see that. I thought Joe Kelly was real skeptical, and had the right viewpoint, and I'm not so sure peer review is needed except for appearance sakes.

Okay, that's all I have.

MR. SIEBER: Okay. I was taken in the beginning by Dr. Wallis's remark that said something like we have reg guides and regulations that describe what you have to do to these codes to assure their quality. What popped in my mind was Reg Guide 1.168, which really applies to process and protection computers and not analytical tools.

On the other hand, the description of what you're doing as far as what I consider to be independent review follows along the lines of what we've been doing for years under Appendix B for calculations and codes of records, and I think that's a good idea. I would encourage re-reading Appendix B and following it because it to me presents the minimum safeguards for code integrity and accuracy.

I was also sort of struck by how one determines that the code is functional, and it's typically by comparing test data, but the test data comes from prototypes or facilities that are sort of look-alikes of parts of power plants, and you can perhaps model what goes on in the prototype, but you have two sources of errors that come in. One of them is how good did you build the prototype and how well does it mimic the actual plant.

The other one is how well did you analytically model the prototype with the presumption that if you do that, that you adequately model the plant. So, I was looking for opportunities to pick on folks for a failure to do that, but I didn't find that. In fact, Joe Kelly's explanation and zeal for improving the various modules within the code, I felt, was very encouraging.

What was discouraging is that that came from RELAP and TRAC-P or TRAC-B, which has been around since the 1970's. So, these errors -- I wouldn't call them errors, but they're modeling looseness has been in existence and used as codes of record to establish compliance with the regulations for about that length of time, and they still exist that way. It would appear that TRACE will end up being the best code out there if the staff continues to pursue its efforts in this area. To me, I think that's a great thing, and so I would encourage that.

As far as criticizing for anything other than the amount of time and resources that are directed to this, it doesn't seem to be a crash project. On the other hand, people are working on it all the time. I would just like to see it move a little bit faster.

So, I think overall, I'm very encouraged by what I've heard over the last two days, and plus I also understand a little bit more about how the individual modeling works and what goes into it and what databases lie behind it. So, to me, that was very helpful.

That's it.

MR. BANERJEE: Well, I look at this from two different viewpoints. One is that the staff are doing work within a certain set of constraints, and those constraints were set about six or seven years ago when this part was embarked on.

Now, the wisdom of that choice at that time is not one that I can debate right now because I don't think that's worth talking about. It's done. Having said that, though, what struck me is that I sat in some of these advanced code review group meetings back in the 70's when Novak Zuber and Stan Favik used to organize them. If I look at the field equations and the structure and things today, and many of the correlations, before Joe Kelly got his hands on them, nothing much has changed, actually.

So, we are now looking 30 years or 25 years down the road, and frankly, I feel pretty disappointed that the state of the art is the same today as it was then. We are making some advances, and notably I like the interface. It's nice. It allows people to use the code more easily I think in the future. We nodalize more easily.

I like the fact that now the code will be usable on different computers, including the Linux clusters, which no longer will allow people to make the excuse that they can't finally nodalize because now they will be able to finally nodalize and make runs in reasonable length of time.

So, from that point of view, the excuse that we have to only use 100 nodes or 200 or 300 or whatever the number is, no longer will exist because you'll have enormous computing power readily, and you can nodalize as finely as needed.

So, I think that that is an advance, and I like the fact that the architecture of the code is being made in such a way that it will be transparent, that it will be running on borrowed machines on clusters in the future, and therefore, and also written in Fortran 95 now, which will allow some degree of modularity so things can be changed relatively easily.

I also like the fact that there is a big effort being made on the side of improving the physical models, removing let's say, as Vic would say, ad hoc character, and Joe Kelly certainly seems to be treating this with a skeptical eye, which is commendable and is to be encouraged at all costs, I think. However long it takes to put these models on a physically sound basis is very important.

Now, having said that, though, the rest of the world in other fields are moving on to take advantage of the enormous increases in computing power and simulation capability in many fields, and this is really a field where we are still back in the 1D days, which we were 30 years ago. We are not even thinking in a clear way as to how to put 3D components in into locations where they are absolutely needed.

There's no way you can defend a peculiar set of nodes which gives you a right answer which don't sort of follow the equations in any way when you sort of refine the nodes down to what Graham wants, which is mathematical conversions. So, from that point of view, I must say, I'm very disappointed that a greater effort isn't being made in that direction.

So, overall, I think it's a commendable effort given the constraints that they have started with, and given the new relatively new capability to parallelize and to improve the physical models. On the other hand, from the basic structure of those constraints, you know, I think we need to start to see how to break out of them and put this in a sounder physical basis where possible.

MR. WALLIS: Well, I have said things throughout the presentations, which you can read from the transcript, to respond to some of the points made by my colleagues. I think one of the independent assessments which is very useful is for you to publish this stuff. If Joe publishes his work in the technical literature, then it gets, as it were, endorsed by the technical community, and too much of this stuff is hidden in proprietary methods of vendors and so on, and is not exposed to this sort of review.

When the ACRS gets to see it, that's about the only outside group that ever gets to see it. So, if you can publish this stuff, so much the better.

In the matter of measures of success, yes, meaning variance and uncertainties, two of my colleagues point that out. You really need to find a way to use the code and the data to evaluate these measures of quality and uncertainty, and that's the way you're going to have to do it in order to use risk informed methods.

This ties up I think with -- I think I disagree with Dr. Kress about the need for these codes to run faster. Unless you can get a lot of runs done quickly, you cannot sort of explore the space of, and I meant to say in a Monte Carlo sense, that you like to perhaps do for risk informed regulation.

MR. KRESS: Well, I got the impression they were running fast.

MR. WALLIS: Oh, I'm not sure that they are.

MR. KRESS: But I may be wrong.

MR. WALLIS: I'm not sure that they are. Yes, something has to be done, I think, about these sort of basic building blocks which are not really appropriately modeled by 1D approximation or guesswork, which is very often not well explained or justified in the documentation. So, I think you need to do something about these uncertainties in building blocks like the equation for funny looking parts of the system, which are certainly not like straight pipes, and all these equations, as I see here, are straight pipe equations. They can't apply, not just to plan, but even to bend some things like that. There are all sorts of places where the basic building block, particularly momentum equation, has uncertainties in it.

There's got to be a way to put fudge factor or something in there and assess their effect on the answer.

I like Sanjoy's point that this is useable on different computers, particularly the most modern sets of computers, because we had that problem before. The codes seemed to be restricted to running on old fashioned platforms.

It's too bad the state of the art hasn't advanced over 30 years, and I'm very glad to see Joe Kelly trying to make his advance. He needs to stick with it. I just wonder how much he can do because it isn't trivial. I mean, a lot of people worked on building up the state of the art. To improve it is going to take some doing. So, you have to put your effort where it can really pay off, and stick with it. I just hope that the ACRS can do something to keep the support of management consistent so you can complete this work.

So, I'm impressed by the amount of work you still need to do, and I'm also frustrated by the fact that we don't have this code so we can say this is it, and this is a great success story. Now let's go out and use it.

I think you need to show some successes in solving topical problems as well as this long term effort. If you could show that TRACE has really been able to do something with this problem that is a concern now or next year or two to the agency, hopefully in a better way than could be done before. Then you win points and then you can keep your effort going and justify it better.

I'm impressed by what I see as a good morale of this group, and what seems to be good support from the management. There have been ups and downs I think over the years in that respect.

So, I still hope that the finished product comes out before I leave this committee.

MR. SIEBER: Forget it.

MR. KRESS: I'm really skeptical about your fudge factor use to evaluate the 3D effects, or the uncertainties to the 1D models. I think that would end up being a real mish-mash of --

MR. WALLIS: Well, it might be a mish-mash, but if we did, could say have CFD models or something better for these things, we could see that there is an error of maybe 50 percent in evaluating momentum flux or something. That would give us a fudge factor we could put in there.

MR. KRESS: Yes, I would just as soon see that you have a good model being plugged in.

MR. WALLIS: Well, that may be too much to do.

MR. KRESS: It may be. It may be.

MR. WALLIS: Maybe a bridge between the CFD, but you cannot really ignore the fact that these --

MR. KRESS: Then it wouldn't be such a flexion of geometry and flow rate.

MR. WALLIS: But you ignore it now. You make some assumption and that's it.

MR. KRESS: Yes, I know it would be better.

MR. WALLIS: There's no reality check at all at the moment.

MR. BANERJEE: Well, the fudge factor is the nodalization right now.

MR. WALLIS: Well, the other thing to do is simply to put, as I've suggested, put a factor of two on the momentum flux on all nodes and see if it makes a difference. If it makes no difference, then we don't need to worry about it.

Okay, now we're going to see you some more, so I think that when we dig into the details, you may get some more value added at that level. At the moment, I'm pleased with what I see.

MR. CARUSO: Could I ask you all to think about what you want to hear next time?

MR. WALLIS: We could share it with you. We'll send you an e-mail.

MR. CARUSO: That's why I'm just -- there is attached to the status report that I gave to you before this meeting a long term plan. It's got three meetings scheduled. This is the first one, and then it's got suggested topics for the next meeting and the third meeting. I'd like you to look at that and tell me if those are what you want to hear or if you want to add something or move things around.

Give me some ideas, and Joe was very good to work with on this, and thank God he put together quite a good presentation today that gave us both an overview of the code and specifics about some of the new things they're doing. If you want to get into the details and spend two days with Joe Kelly standing up there talking about heat transfer, we can arrange that.

MR. SIEBER: Put him on video and send it to us.

MR. WALLIS: Well, I have noticed that he tends to produce results when he has a deadline.

MR. CARUSO: What I'm saying is look at that list, okay, and give me ideas, and we can work it out. We would look, we have a meeting in January to talk about ESBWR. There is a subcommittee meeting scheduled for the 2nd, 3rd, and 4th of February to talk about this GSI-188. I'm going to ask you to come back probably the next week to talk about AP1000.

MR. WALLIS: That's a lot of meetings.

MR. CARUSO: I understand that, but the AP1000 effort --

MR. BANERJEE: The next week.

MR. SIEBER: I think I might get an apartment here.

MR. CARUSO: The next week, the second week in February, the 10th and 11th. Put these days down on your calendar and let me know what you think about the 10th and 11th.

MR. BANERJEE: And when is the second meeting of this, the 2nd and 3rd?

MR. CARUSO: There's a meeting the 2nd, 3rd, and 4th. I'm not sure I'm going to ask you to that because that's going to be GSI-188, and that's going to be mostly -- yes, that's mostly going to be Peter on that one. I think it's going to be mostly materials issues.

MR. BANERJEE: But when is the second meeting of this TRACE? Will it be together with the AP1000?

MR. CARUSO: That's what I'm not sure about. It depends on whether we can do AP1000 in maybe a day or if AP1000 is not settled at that point and we have to go two days.

MR. WALLIS: We don't yet know what the staff position is.

MR. CARUSO: We don't know yet what that position is, so I can't plan for a second meeting to talk about the code.

MR. KRESS: You're talking about AP1000. You're talking about TRACE applications in AP1000?

MR. CARUSO: No, no, no. The AP1000 issue.

MR. KRESS: You're talking about the certification issues?

MR. CARUSO: Yes.

MR. KRESS: Thermal hydraulic certification issues?

MR. CARUSO: Yes, that's correct.

MR. WALLIS: I would like to see when we discuss this ESBWR and AP1000, that we have actually TRACE calculations we can look at to help guide us in deciding about the issues.

MR. CARUSO: I will mention that to the staff for the ESBWR meeting, and I'll see what they can provide.

MR. WALLIS: All right.

MR. RANSOM: That's in January?

MR. CARUSO: January 14 and 15.

MR. WALLIS: That's not something that we'd ask these folks to do. We'd ask NRR to do it.

MR. CARUSO: NRR has got the responsibility.

MR. WALLIS: So if TRACE is to be valuable, it has to be used by the customer.

MR. CARUSO: Yes.

MR. WALLIS: To answer current questions.

MR. CARUSO: Yes.

MR. WALLIS: So let's see if we can make that happen. I'll encourage it.

MR. RANSOM: Is NRR presenting on the AP1000 also?

MR. CARUSO: In February, yes. They have to come in with Westinghouse and tell us where they have finally ended up, because the last time we met, it wasn't clear.

MR. BANERJEE: So those dates are not set and therefore we cannot set the TRACE meeting dates?

MR. KRESS: January 14 and 15 was some application to ESBWR.

MR. CARUSO: Yes, ESBWR, SCR, that's right.

MR. KRESS: SCR. So, that's what's you're talking about.

MR. CARUSO: That's in January. Then in February, you have this meeting on this GSI-188. That's a joint meeting with the materials subcommittee. Then I want to hold another meeting the next week, and I've got the 10th and the 11th of February blocked out to talk about the AP1000.

That meeting can be very short if Westinghouse and the staff are in alignment. If they're not in alignment, I might not even hold the meeting because I don't want to hear them, that they don't agree.

MR. WALLIS: So if they don't agree, we can hear about TRACE then?

MR. CARUSO: Maybe we can hear about TRACE, but if not, then we may try to hear about TRACE at that time. We may wait until March.

MR. WALLIS: I hope it doesn't take too long for you to prepare for meetings because you know, this is work you are doing. You're right on top of it. So we say we'd like to hear these other things in February. It's not going to be a great struggle for you to get ready for it.

MR. KELLY: It depends on like my reflood presentation. That's work I've been doing, so say a couple of days. If you wanted someone to say expound the equation and that would be all new stuff, that can take a long time.

MR. WALLIS: I think just somebody may get up and say it's the same derivation that you find in all the other codes and has the same --

MR. KELLY: I mean, if I were to take this question seriously and really try to explore to get you a good answer.

MR. WALLIS: I think that would -- might require some research.

MR. KELLY: Right.

MR. BANERJEE: Well, in one of the topics, it's detailed discussions of the two fluid model for 1D and 3D. So, I assume we'll look into the equations, the model, the requirements for closure relationships within the 1D and the 3D context to revisit this. I don't know what is meant there, but I assume that's one of the topics.

MR. WALLIS: Okay, so we will work on this calendar, all right?

I'm ready to close the meeting. Okay, we'll close the meeting then. Thank you very much, everybody, including our transcriber.

(Whereupon, the above-referenced meeting was adjourned at 5:15 p.m.)