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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS (ACRS)

SUBCOMMITTEE ON THERMAL-HYDRAULIC PHENOMENON

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TUESDAY,

FEBRUARY 15, 2005

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The meeting was convened in Room T-2B3 of Two White Flint North, 11545 Rockville Pike, Rockville, Maryland, at 8:30 a.m., Dr. Victor H. Ransom, Chairman, presiding.

MEMBERS PRESENT:

VICTOR H. RANSOM	Chairman
GRAHAM B. WALLIS	Vice Chairman
RICHARD DENNING	ACRS Member
F. PETER FORD	ACRS Member
THOMAS S. KRESS	ACRS Member
JOHN D. SIEBER	ACRS Member

ACRS STAFF PRESENT:

RALPH CARUSO	Designated Fed. Official
CHRIS MURRAY	RES
RALPH LANDRY	NRR/DSSA/SRXB
WILLIAM BURTON	RES/DSARE/SMSAB

SHAWN MARSHALL	RES/DSARE/SMSAB
JOSEPH STAUDENMEIER	RES/DSARE/SMSAB
JOE KELLY	RES/DSARE/SMSAB
JIM HAN	RES/DSARE/ARREB
ALEXANDER VELASQUEZ	RES/DSARE/SMSAB
WILLIAM KROTIUK	RES/DSARE/ARREB
DONALD CARLSON	RES/DSARE/ARREB
CHESTER GINGRICH	RET/DSARE/SMSAB
AMY HOLL	NRR/DRIP/RLEP-B

ALSO PRESENT:

CLAUDIO DELFINO	ISL, INC.
TOM DOWNAR	PURDUE UNIVERSITY
JOHN MAHAFFY	PENN STATE UNIVERSITY
NORMAN YEE	SELF

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

8:31 a.m.

CHAIRMAN RANSOM: The meeting will now come to order. This is the meeting of the Advisory Committee on Reactor Safeguards Subcommittee on Thermal-Hydraulics Phenomenon. I am Victor Ransom, Vice Chairman of the Subcommittee. Thank you. Subcommittee Members in attendance are Tom Kress, Graham Wallis, Jack Sieber and Peter Ford. The purpose of this meeting is to review the continuing development of the TRACE Thermal-Hydraulic Computer Code.

The Subcommittee will hear presentations by and hold discussions with representatives and the NRC Staff and our contractors regarding these matters. The Subcommittee will gather information, analyze relevant issues and facts and formulate proposed positions and actions as appropriate for deliberation by the full Committee. Ralph Carusso is the designated federal official for this meeting.

The rules for participation in today's meeting have been announced as part of the notice of this meeting previously published in the Federal Register on January 31, 2005. A transcript of the meeting is being kept and will be made available as stated in the Federal Register Notice.

It is requested that speakers first identify themselves and speak with sufficient clarity and volume so that they can be readily heard. I have not received any requests from members of the public to make oral statements or written comments. We appreciate the cooperation of NRC research in volunteering today's agenda and sharing with us the status and some of the details of the TRACE Code Development Project.

The ACRS has supported the objectives of this project from its initiation and we feel that it is very important to the mission of the NRC. That said, we are concerned about the length of time it is taking for this project to make a significant contribution to safety and licensing issues. During the past three years, we have encountered several instances in which application of verified and unified NRC Thermal-Hydraulic Analysis capability could have resulted in a more straightforward resolution of safety issues.

In particular, we are interested in hearing about what technical challenges are being encountered and the prospects for a timely resolution of these. Plans or prospects were overcoming some of the traditional problems of two phase codes like, for example, stability, diffusion, numerical anomalies, etcetera. The third plan is for incorporating uncertainty information associated with basic formulations and constituent of models. And finally, development of techniques for performing probabilistic calculations for making risk informed licensing and safety decisions.

We realize that not all of these subjects will be covered today and we will look forward to future interaction to learn more about the ultimate potential of this project. We will now proceed with the meeting, and I call on Mr. Staudenmeier of the NRC Staff to begin.

MR. STAUDENMEIER: I just want to turn it over for a minute or so to my section chief, Butch Burton. He is new to our organization and has took over as section chief back in August.

MR. BURTON: Thanks, Joe. Good morning, as Joe mentioned, my name is Butch Burton and I currently serve as the Chief of the Code Development Section in the Office of Research. My branch chief, Pat Baranowsky, as well as my division director, Farouk Eltawila, could not attend today and they do send their regrets. I do want to thank you for giving us the opportunity to share with you the work that we have been doing with regard to the TRACE Thermal-Hydraulic Code.

Mr. Chairman, you already identified both the purpose and the objective of the meeting. I just wanted to give a little bit of history, which I'm sure you're already familiar with, as we begin. As you know, historically, the Agency had four primary Thermal-Hydraulic Codes, including RELAP, TRAC-P, TRAC-B and RAMONA, which provided analyses of both small and large break LOCAs for BWRs and PWRs.

Maintaining these codes required separate software and multiple knowledge bases. In addition, these older codes did not take advantage of the considerable advances in computer technology, because maintaining separate codes was neither efficient nor effective, the Agency decided in the mid '90s to consolidate the capabilities of these codes into one code called the TRAC RELAP Advanced Computational Engine or TRACE.

By consolidating these codes and developing a common graphical user interface, considerable efficiencies could be attained. The staff believes that by consolidating the four codes into one all purpose code and incorporating into the consolidated code advanced numerics, neutronics and thermal-hydraulic models as well as improved user interfaces, the TRACE Code could serve as the primary Thermal-Hydraulic Code that the Agency can use to more effectively and efficiently audit vendor and licensee analyses of new and existing designs, establish and revise regulatory requirements, study operating events and otherwise develop and support technically sound safety decisions.

Today you will have the opportunity to hear from the Agency staff and contractors who have the lead roles in developing TRACE. You will hear about the latest work being done in the areas of advanced numerical methods, neutronics, graphical user interfaces, code verification and quality assurance and model development. I hope you will find the presentations informative and now I'll turn it over to Joe.

VICE CHAIR WALLIS: Well, we're going to hear about the latest work being done. We're going to hear about use of the code for practical purposes.

MR. BURTON: Absolutely. One of the things that we will talk about is how we have been using the code in the current applications.

VICE CHAIR WALLIS: They are supposed to

make the Agency more effective and efficient. Is it doing that?

MR. BURTON: Well, as you'll see from the presentations, some of the applications of the code, you'll see that we are trying to use the code to make safety decisions on a real time basis. You will see that.

VICE CHAIR WALLIS: Okay. Good. Thank you.

MR. BURTON: Yes.

MR. STAUDENMEIER: Okay. I just want to give a quick few minute overview of what is coming up for the day. For meeting objectives, we wanted to provide an overview and current status of our code development effort. Another big thing we want to do today is address issues raised in the anonymous letter that was sent to the ACRS and forwarded to us, so this is our public response to the anonymous letter, since our written response was kept as sensitive Agency material apparently or wasn't allowed to be released to the public, and provide information on new and future physical model and numerical methods development in TRACE.

We have new management organization in the part of research where we work. Farouk Eltawila is still our division director. As Butch said, Pat Baranowsky is our new branch chief. He took over from Jack Rosenthal back in November and Butch Burton is now our new section chief for the Code Development Section.

VICE CHAIR WALLIS: So what's more important is who is doing the work? I mean, who is underneath these people?

MR. STAUDENMEIER: That's true, I guess.

VICE CHAIR WALLIS: How many people do you have?

MR. STAUDENMEIER: It's shaped like a pyramid, you know, the big support base at the bottom of this diagram gives --

VICE CHAIR WALLIS: So there are people actually below this level here?

MR. STAUDENMEIER: Yes.

VICE CHAIR WALLIS: How many are there? How many are there down there?

MR. STAUDENMEIER: Well, let's see, we have five people in the section that spend at least part of their time on TRACE and SNAP, and at ISL we have a contractor who works most of the time on TRACE in terms of co-ed support, so we have one essentially full time body at ISL. Out at Purdue we have our

PARCS Development Effort going on at Purdue, that level is, in terms of dollars, less than an FTE at a contractor.

VICE CHAIR WALLIS: One graduate student or something like that?

MR. STAUDENMEIER: Yes, graduate students under Professor Tom Downar. At Penn State we have John Mahaffy doing base numerical development. Also at ISL, we have some assessment going on. We have code assessment going on and also some ACR-700 specific development at ISL, which if ACR-700, if we continue on over a year, we will have spent about three FTEs on --

VICE CHAIR WALLIS: So the only full-time person is the ISL person? The other people are all part-time and get diverted onto other things?

MR. STAUDENMEIER: Yes, essentially. I mean, we essentially have one base level person at ISL and the rest are added on for specific projects.

CHAIRMAN RANSOM: Are there any other personnel at Penn State helping John?

MR. STAUDENMEIER: He has students occasionally that work with him. At one time we had another staff member at ARL at Penn State working part-time on a project, but in terms of professional staff, actually, we added another professional staff person at ARL recently that is going to start doing some work, at least part-time.

MR. FORD: Just to follow on, you said essentially there are three full-time employees overall working on this contract and in-house. Does that signal a fact that you are pretty well close to completion?

MR. STAUDENMEIER: Well, it depends on what you mean by completion. I mean, we can use the code for some things now. I mean, there is like things like ACR-700, if you want to use it for ACR-700, there is quite a lot a bit of work to do to the code to use it for ACR-700. ESBWR, we're wrapping up some development for ESBWR. We need to do some assessment on that. There is more than three. I would say if you count full-time equivalence, it's maybe on the order of five if you add up all the part-times.

MR. FORD: But of those five full-time employees, how many are actually doing, other than managerial sort of contract oversight type of activities?

MR. STAUDENMEIER: I'm talking about real code development.

MR. FORD: Oh, okay.

MR. STAUDENMEIER: Not the -- like myself, I'm nominally doing code development. My job is to do code development, but over the past year, it has probably been a third of my time doing code development.

MR. FORD: Okay.

MR. STAUDENMEIER: Probably and the other two-thirds doing miscellaneous things. Someone like Joe Kelly has spent most of his time doing code development over the past year.

MR. FORD: Okay.

MR. STAUDENMEIER: And it depends on the person you are talking to and what year it is on how much of their time has been spent on code development.

CHAIRMAN RANSOM: Could you comment on the effectiveness of this sort of splintered operation where you've got several organizations involved? And I know personally from my past experience that it was a lot easier when you had a cohesive group that was working on something, and so I'm wondering how effective is this?

MR. STAUDENMEIER: I don't think we have problems with different organizations working on it. What runs into problems is when we have like over the past year we have had a lot of different developing going on for different projects and sometimes you hit a choke point and if a lot of people want to get updates into the code at the same time just because we've become limited by Chris Murray's time, who isn't always spent full-time on code development, you know, this past year we have had to spend a lot of -- he spent a lot of time on computer security.

They took our Linux cluster off the main network because of security reasons and we had to develop a security plan to get it back on and a lot of his time was taking up during that.

VICE CHAIR WALLIS: You mean TRACE is something that might be used for evil purposes?

MR. STAUDENMEIER: Well, I think it's more or less us corrupting their network, I think. So with TRACE being a virus that will spread out and take over the rest of the NRC network.

CHAIRMAN RANSOM: Well, is NRC the code architect? The one who keeps the code?

MR. STAUDENMEIER: NRC maintains the code. Chris Murray, who will speak later, is in charge of code configuration control. He is in charge of putting all the updates in the code and running through the testing and putting out a code version.

Like if you were to look now at our holding bin of things waiting to go in the code, there is probably about 10 to 15 items. And it always sits there at about that 10 to 15 items level and we prioritize as to what is holding up code development the most or holding up a release that we're planning on getting out on his priority in getting in different options.

VICE CHAIR WALLIS: Do you keep track of the number of hours that TRACE is actually used for regulatory purposes?

MR. STAUDENMEIER: Keep track? I don't know if anybody really keeps track of how much it is used for regulatory purposes. I mean, NRR, essentially, has used it for ESBWR. I don't know what else they have used it for, what applications they have used it for other than that. In-house, we have used it for steam generator blowdown loads. We're going to be using it for 50.46 brake size redefinition. We're using it in terms of ACR-700, it hasn't been used for any licensing calculations yet, but calculations performed with it have supported the ACR-700 review to date as to helping people ask questions and things like that.

Actually, maybe it might be better to postpone some of these questions to my later presentation, the overview of NRC code development. I just meant this is a short introduction right now and to go over what is going to follow for the rest of the day. Joe Kelly is going to be up next talking about the new TRACE Condensation Models that he developed for ESBWR analysis. He will give an overview of the overall TRACE Code Development effort.

Professor Tom Downar is going to talk about PARCS status and development. Professor John Mahaffy will talk about issues related to the anonymous letter and other numeric issues associated with TRACE, numerics development issues. Chris Murray will talk about the TRACE QA and configuration control and issues related in that related to the anonymous letter. And to end the day, Chester Gingrich will talk about the status of SNAP.

VICE CHAIR WALLIS: Well, is TRACE available to the public?

MR. STAUDENMEIER: Yes, TRACE is.

VICE CHAIR WALLIS: A university could get it and use it if it wanted to?

MR. STAUDENMEIER: Yes, a university can get it and use it. Actually, John Mahaffy uses it to teach an analysis, systems analysis class at Penn State.

VICE CHAIR WALLIS: How much of success might be that you had a 100 clients out there actually using this?

MR. STAUDENMEIER: Yes, we do have some people. There are some camp members who have started using TRACE, so it's starting to get adopted. One barrier to adoption is training. We haven't had TRACE training yet, but we are having that in March. So that will help bring more people in to using TRACE.

VICE CHAIR WALLIS: A good code doesn't need too much training.

MR. STAUDENMEIER: Well, a lot of people have asked for it. Okay.

CHAIRMAN RANSOM: One question I would like to raise. You know, you mentioned you are going to address these anonymous letters, which is fine. But I'm wondering wouldn't it be better if you actually went out and got some peer review yourself, you know, some experts in the field who would do a more detailed review and then give feedback? You know, the anonymous things have the problem that generally the details are not dug into. Maybe we will hear a little more today about what you make of that. But I personally feel that if you subjected the code to peer review by some experts who spend enough time on it, you know, that they could give an informed judgment about the methods that are being used. That would provide you with a lot more ammunition, I think, that you're on the right track so to speak.

MR. STAUDENMEIER: Yes, actually, we would like to have peer review for the code, I think, before we put it out for, I guess, what you would call the official public release, which we think will take place in about two years from now, with assessment and complete documentation. I think that once we settle on all the features that are going in there and get a set of base documentation, I think that's the time to get peer review to go along with the release.

Right now, I think things are still in development and moving to really have peer review, because we don't have finalized documentation yet and we need good final documentation, I think, to get good peer review.

CHAIRMAN RANSOM: One of the problems with waiting until you're -- that late is if they do come up with something that would be helpful, then it becomes much more difficult to incorporate that into your development.

MR. STAUDENMEIER: Yes, I guess, that's one way to look at it. I don't think that there would be any serious things that would be found in a peer review. I think the weakest thing now is our models and correlations have changed quite a bit or are changing and haven't undergone assessment and shaking out of errors yet. So that's probably the weakest point of the code right now, I think.

In terms of numerics, if we're getting the wrong answers, then every other code out there is getting the wrong answers, because we all get similar answers on similar problems. So I'm not real worried about some big hidden trap in our numerical methods that we're for some reason getting answers that are totally out of whack and mispredicting reactor safety. But peer review is something that we want.

Right now, we really are a resource limited development effort and that's something that will have to be budgeted for for research. Everybody likes peer review. Nobody likes to budget for peer review, but that's something that we will have to budget for in the future and get it in. I will propose it and we'll see what happens when management reviews the budget before it goes out and sees if they will budget for peer review.

VICE CHAIR WALLIS: If you look at commercial codes in say CFD, they were all very similar, all very similar in purpose and origin. But the ones that are successful are the ones that offer a lot of customer support. They listen to their clients. They have good user manuals. They have good training. And the code is then easy to use. And it seems to me you guys are developing something you like, you may be missing, so the key to success of this whole thing, which is to produce something that users are going to like.

MR. BURTON: Can I speak to that?

VICE CHAIR WALLIS: Yes.

MR. BURTON: As mentioned, I just joined the development team relatively recently. And I think one of the benefits of that is to be able to look at the whole process sort of with fresh eyes. One of the things that I have noted is that there is a certain lack for resource limiting reasons as well as some others, that there is a lot of room to improve in the development process.

Chris is going to talk when he comes up to talk about the development lifecycle for the code and I will tell you that there are challenges in terms of being sensitive to the end user of the product. So what I hope to do over the next few months is to develop a transparent process and procedure to start with doing accurate needs assessment with regard to the end users of the product, what needs to be done in terms of assessing whether those functions and features that are being requested are, in fact, in place and to do the necessary model development, code testing and all the other things that need to be done to ensure that we actually reach that end state.

To begin with the end in mind and in effect and to make that process more transparent. So those are the things that we hope to do in the near future.

MR. STAUDENMEIER: Yes, I guess one other comment is I would say the limiting thing in learning to use TRACE isn't the learning curve in actually functionally using TRACE and doing calculations, it's much harder to understand the underlying two phase flow and physics in the calculations and understanding what the calculations mean, than it is to use the code. We have had people come over to use the code on rotations and also people out in John Mahaffy's classes. He teaches at Penn State.

People will pick up the code and start running it within a few days or a week and can functionally do things that, you know, you want them to do without a great deal of difficulty.

Understanding the results is another matter though. You need a lot of training, years of training and experience to understand the results that are coming out.

VICE CHAIR WALLIS: It is portable to many platforms, is it?

MR. STAUDENMEIER: Yes, it is a portable code and I'll talk about that in my next presentation.

MR. MAHAFFY: Okay. This is John Mahaffy. I would like to make one comment on ease of use. As he indicated, I've used this in a power plant simulation class. It's just a basis to get some simulations that they then learn how to think about sensibly and college seniors, they really have had no problems producing results from the code. You know, taking a geometry, implementing it and looking at some answers.

And to NRC's credit, they have even had the developers of the front end come to my class to watch the students do what they do to even try to refine the intuitive features of the front end. It has been a pretty good experience for students who really have had no experience whatsoever with the simulation environments before.

VICE CHAIR WALLIS: Thank you. That's

really informative.

MR. KELLY: Sometimes it seems like the transparencies were easier.

VICE CHAIR WALLIS: Just don't let it go into hibernation mode. We did that last week and we couldn't get it back.

MR. KELLY: And I guess I need the microphone, because I'm going to talk on my feet. It's one way to lose it. I was originally scheduled to go later this afternoon, probably in case I went overtime as I normally do, but because of personal reasons I need to leave early today, and so people were nice enough to reschedule me first thing in the morning. So I'll try not to go too far behind.

MR. KRESS: You don't have any trouble over there, but we'll see that you get going.

MR. KELLY: Okay. What I'm going to be talking about today is condensation with non-condensibles and we're doing this for the PCCS component at ESBWR. And as you may remember, I talked to you about this about a year ago and that's when I did most of this development work. But then the model sat on the shelf and languished for a while, not being put into the code basically because of contractor unavailability to do the work. But we have started in

TRACE recently and so this is my first opportunity to come back and show it to you.

Okay. I'm going to give the presentation based between three parts, an introduction, then I'll give a detailed model description and some assessment results, some cases that I have run to show that it was put into TRACE correctly. In the introduction, I'll go over the background and status, the modeling approach and then I'm going to show you the model accuracy. For the model description, it's actually a package of constitutive models that you need in order to do this right within the context of the two-fluid code.

So background and status. Well, pretty obviously we have the application or the pre-ap for the ESBWR design. One of the most important components is the Passive Containment Cooling System and that relies on condensation in the presence of non-condensible gases. So when this first came up as a priority for a TRACE application, the first thing I was tasked to do was look at the models that were extent in the code. Did they make any sense? Were they any good?

So I actually did a model review, if you will, a peer review being done by me and then some model assessment. I ran a bunch of pure steam cases as well as steam-air cases just to see if the current model was good enough. If it was, nothing was needed. But that wasn't the case. There were significant deficiencies, both in the modeling approach. I mean, if you actually were to do a review of the models, which I did, and this I reported to you last time, and then also the predictive capability was pretty lousy. And I have a couple of those slides which you will see.

So that started in new model development. That development has been completed and it has been installed in the TRACE. I have compared the model to a very large condensation database, including pure steam, air-steam and helium-steam mixtures. As I said, it's in TRACE now. I have performed some preliminary assessment cases that I'm going to show you today. And then a more extensive assessment is underway right now at a contractor, also in-house. So we'll be looking at data from both tube tests, primarily those from University of California Berkeley, but also heat exchanger tests.

For example, the full scale PANTHERS facility that was done in Italy a number of years ago and also we have run some PCCS tests in the PUMA facility. So we'll be looking at both of those.

VICE CHAIR WALLIS: Now, these are all vertical tubes?

MR. KELLY: Yes.

VICE CHAIR WALLIS: They are not horizontal tubes?

MR. KELLY: No, it's all vertical, because that's the design of the ESBWR. Now, a lot of the same models would be applicable, but they would have to be adjusted.

So we're talking about the model development effort. Well, the objective is pretty straightforward. We need a model in TRACE for in-tube condensation with non-condensible gases in order to handle both the Isolation Condenser and the Passive Containment Cooling Systems.

So what about the approach? Well, instead of saying should, this ought to say "The model must be compatible with the two-fluid framework." If you look at constitutive models and a lot of other codes and in TRACE, you will see a lot are the old HEM kind of models that were developed, you know, many years ago and they are kind of shoe-horned into a two-fluid code. And a lot of time it doesn't really make a lot of sense. So it must be compatible with the two-fluid framework.

Also, it should take advantage of the quantities that TRACE already calculates through the solution of the conservation equations. Most analytical condensation models go to great lengths to try to get the axial distribution of the condensate flow rate. TRACE, through the solution of the mass and energy equations, already knows that.

So if you have it, you should use it and that's what we're going to do. We're going to take advantage of the axial distribution of the condensate flow rate and the film thickness just as a simple example.

VICE CHAIR WALLIS: This is just straight condensation through the film?

MR. KELLY: Right.

VICE CHAIR WALLIS: Because if it's turbulent film or has waves on it, it's presumably different.

MR. KELLY: Right, exactly. This is just a very simple little example. If you have a laminar-smooth film, the Nusselt no. is nothing more than the width of thermal connectivity divided by the film thickness. If it's a wavy laminar film, you're then going to multiply this by a function of the film

Reynolds no., and then if it's a turbulent film, it's a function of Reynolds and Prandtl.

The other thing I did is I developed the model as a set of specialized constitutive package, which will be applied to pipes, but you have to label the pipes as "condenser tubes." There is a little flag that you can set.

And by doing that, I was able to put these models in the code without changing all of the answers of everything we do today, so that makes the migration, the path, a little bit more quickly, but also once the models have been tested and proved to be generally applicable, then they are going to be migrated over to the normal constitutive package and the special component will go away.

And in fact, some of these models like the improvements to the Wall Drag Model have already been put in the code, you know, as part of a normal constitutive. So I said I was going to show you something about --

VICE CHAIR WALLIS: The user then has a choice of these models in some way?

MR. KELLY: Yes. When you build an input deck, the condenser tubes are nothing more than pipes, and there is a little flag on the pipe where --

because we use pipes for things like accumulators, as well, with their own special set of constitutive models. So it's like a 1 for an accumulator and it's a 5 for these things, so you change one number in the input deck to flag that tube as a condenser tube.

So I wanted to tell you something about the model accuracy, because I developed the model outside of TRACE, basically in a spreadsheet type format, and then eventually put it into TRACE, so I wanted to have a pretty large condensation database. And I divided that into three parts, pure steam condensation, air-steam tests and helium-steam tests.

Now, you will notice this UCB-Kuhn test appears in all three. That's also known as UCB-4 and it's probably, at least in my opinion, it's the best data we have for condensation with non-condensibles. It was the fourth graduate student, if you will, of Professor Schrock. They started with Karen Vierow who is now a professor at Purdue and moved all the way to the graduate student named Kuhn. So it's a series of four experiments and each time they took the lessons learned from the first experiment and applied it to the second.

Also, he did a better job than most students will do in, if you will, filtering his data later, going back and looking at it, seeing what data made sense, what didn't and throwing away the bad points and only reporting the ones that make sense. So this is the best database we have.

The MIT test, Siddique was the first graduate student there and that was MIT's first attempt at these tests, and so it has a lot of the flaws of the first UCB test, but at least it was a test done somewhere else. And then the final one was Hasanein and that was a follow-on to Siddique, but he used the same facility, just changed the experimental procedure a little. So those tests I have less confidence in.

The NASA tests are pretty old. I think they are from the '60s if I remember right, but they are pretty much all we have. If you look in the tube diameter column, you will notice that all of the tests are, basically, at the diameter of the ESBWR condenser tubes with the exception of the NASA test, which is only 7 millimeters, so we're talking a pencil here.

Pressure range, pretty consistent, 1 to 5 atmospheres all the way through with the exception of the NASA test, which actually goes subatmospheric, because they go to complete condensation.

If you look at the gas Reynolds no. and

you look at the Kuhn test, you will notice it's fairly modest. What that means is -- I mean, it's above the expected range for ESBWR, if you bracket it that way, but it's fairly modest, so these films are going to be more falling than they are sheared. There is interface for shear, but it's not much.

In contrast, and this is why I added the NASA test, now this Reynolds no. is pretty large, but if you think about the diameter it's being applied to, you come up with very high velocities here. We're talking almost 100 meters a second. And so these films are very highly sheared, very, very thin.

If you look at the Kuhn test and you look at the film Reynolds no., somewhere down near the bottom of the tube quite often these tubes are beginning to get near to turbulent transition. The NASA tests are the only ones I have that actually have any significant amount of the condenser surface in the turbulent regime, and that's the other reason they are here.

For the air-steam condensation test, it's pretty much the same thing, a fairly modest Reynolds no. for the Kuhn test, but you will notice it's much larger than the tests that were done at MIT. So those films will be more sheared than the ones at MIT. But the bigger difference is in what the Reynolds no. goes down to. The tests at MIT go way sub-turbulent and that means they are going to be in a mixed convection type regime. This has probably got some mixed convection in it, too, but that's a big effect in these other tests.

VICE CHAIR WALLIS: Another effect, which may be present, I think in the NASA data, is the momentum transfer due to the condensation itself.

MR. KELLY: Yes.

VICE CHAIR WALLIS: Affecting the interfacial shear.

MR. KELLY: Yes, and that is modeled in TRACE.

VICE CHAIR WALLIS: Yes. And I think probably in the NASA experiments it shows up the most. Is it because the velocities are higher?

MR. KELLY: And the condensation rates are higher.

VICE CHAIR WALLIS: Are much higher, yes.

MR. KELLY: So this is the results of doing that assessment. What I'm comparing those tests to, the far column is a TRACE Model done in a spreadsheet format not inside of TRACE, and I'm comparing it to basically three other correlations.

It's a little unfair to put Vierow-Schrock on here. This was the first model out of Berkeley.

VICE CHAIR WALLIS: So what are you comparing here, the pressure drop or the heat transfer coefficient?

MR. KELLY: The heat transfer coefficient.

VICE CHAIR WALLIS: Or the amount of condensation?

MR. KELLY: Yes, it's the heat transfer coefficient. Sorry. You run out of room on the slides.

VICE CHAIR WALLIS: Okay. These are percent changes or these are absolute?

MR. KELLY: I was going to get to that. Those are relative.

VICE CHAIR WALLIS: Relative.

MR. KELLY: So the way I --

VICE CHAIR WALLIS: The 2.9 meters off by a factor of 3?

MR. KELLY: Right.

VICE CHAIR WALLIS: Okay. So this is calculation minus data divided by data.

VICE CHAIR WALLIS: Okay.

MR. KELLY: That's the way I always do it. Okay? So this was the first correlation out of

Berkeley and it's totally empirical. It basically takes a falling film heat transfer coefficient and multiplies it by the so-called F1 factor for the effective interfacial drag and the F2 factor for the effective non-condensibles.

And you know, if I had the other slide up and could go back and show you the Reynolds no. for Kuhn versus the Reynolds no., which I don't have, that was done in Vierow's experiments, those Reynolds nos. are a good bit higher, so the F1 factor for interfacial shear is way outside of its data range.

So like I said, it's unfair to put this model here, but I wanted to do it to make the point that if you use an empirical model outside of its range of applicability, all bets are off.

And also, this model or derivatives of it are still being used by people today, so it better be used with caution. The reason for the large over-prediction here is that interfacial friction factor, the F1, which is a function --

VICE CHAIR WALLIS: Most errors are a factor of 3. There must be some points where the error is a factor of 10 or something large, really large.

MR. KELLY: There were some that were

pretty large, but they are all way off. I mean, if the average error is, you know, 260 percent too high, so you're off the charts and it's because the mixture Reynolds no. is too high. So like I said, it's a little unfair to use this correlation outside of its range, but people should be aware of that.

VICE CHAIR WALLIS: I think that these errors are positive. That means the correlation is predicting too much heat transfer. Is that what it is?

MR. KELLY: If the average one is, it means there is a bias towards too much. So what you're really looking at is a bias and an uncertainty, you know, within how well I do or don't do statistics. That's really all I wanted to say.

VICE CHAIR WALLIS: If you go back to the work done in Harwell in the '60s, I think, they did this sort of thing, too. You don't seem to have that in your database. I think they found that using this Nusselt idea as just h is k over Delta didn't work out too well and there was a consistent error. I think it was a factor of about 2. I'm just trying to remember, but they did annular flow, condensation and --

MR. KELLY: Yes. I don't remember. Well, I wasn't able to find the data.

VICE CHAIR WALLIS: Back in the '60s, yes.

MR. KELLY: But I found a reference to it where they said it was about 20 percent too high.

VICE CHAIR WALLIS: It was only 20 percent?

MR. KELLY: If I remember right again. But Kuhn-Schrock-Peterson, I should call it a model and not a correlation, because the first thing you have to do is solve a cubic equation for the film thickness. So it does have some effect of both gravity and interfacial drag in it.

So you solve for the film thickness and then, again, there is an F1 factor, which in this case is a function of the film Reynolds no. to account for film waviness or turbulence effects. Well, actually not turbulence, because the database doesn't go that high. And then an F2 factor, which again is an empirical model for the effect of non-condensible gases.

VICE CHAIR WALLIS: And so the best correlation is, of course, on Kuhn data?

MR. KELLY: That is certainly what you would expect. I mean, I know for condensation data this is pretty remarkable.

VICE CHAIR WALLIS: But there's probably

some punch factors, which were adjusted and things like that.

MR. KELLY: Well, actually, you will see the model, part of the model, later. But, and this is the point I wanted to make, of course I have used one of the correlations from Kuhn, is this is almost as good against the Kuhn data and if you then go to the NASA data, which of course this is inapplicable to because it's just completely outside its range, and then you look at the Shah Model, which is an empirical correlation, which used the NASA data, you know, as part of development, and this again is pretty good especially if you look at how scattered the NASA data is. This isn't much worse.

So the conclusion here for the pure steam condensation is that the model that I have in TRACE is almost as good as empirical correlations on their own database.

MR. FORD: Let me just make sure I understand. None of them are very good.

MR. KELLY: Have you ever looked at condensation data?

MR. FORD: No. But you know, you mentioned that the Vierow-Schrock data is questionable, presumably in terms of quality control of the data itself, but the others you say are pretty good. But you're showing that the TRACE Model is under-predicting by a factor of 5. Am I reading those numbers correctly?

MR. KELLY: Which one?

MR. FORD: Well, the .018 you have there on the top line there.

MR. KELLY: Okay. That's --

MR. FORD: Is that not the same?

MR. KELLY: That's an average error of 1.8 percent.

MR. FORD: Oh.

VICE CHAIR WALLIS: That's very good.

MR. FORD: That's right.

VICE CHAIR WALLIS: Have we done it with crack growth?

MR. KELLY: Okay.

MR. FORD: Okay.

MR. KELLY: So the point here was with pure steam, the model in TRACE is almost as accurate as empirical models against their own database and, obviously, it's better than those models when they go outside their database.

When you look at the air-steam condensation test, again Vierow-Schrock for this one is outside of its range of applicability, so you just have to be careful if it's an empirical model.

VICE CHAIR WALLIS: It would be good if this slide could stand on its own, so you have a title, which said error in heat transfer coefficient or fractional error or something like that.

MR. KELLY: That's true.

VICE CHAIR WALLIS: We knew what we're talking about. And this is the average heat transfer coefficient along the whole pipe?

MR. KELLY: No, these are LOCA values.

VICE CHAIR WALLIS: They are LOCA? Okay.

MR. KELLY: Now, it's LOCA not as in a point sense, because, obviously, with condensation you don't --

VICE CHAIR WALLIS: Did you also measure the LOCA values?

MR. KELLY: Yes. What you do, you do a heat balance on the secondary side where you're measuring the liquid temperature over increments so it's LOCA, but in a sense of, you know, 10 centimeters kind of thing, not a point.

MR. CARUSO: You keep saying that you have to be careful about applying it outside its range of applicability. Well, what if the user becomes creative? Does the code say ah, I'm outside my range of applicability and turn on a big, red light?

MR. KELLY: Well, no.

MR. CARUSO: Or a yellow light warning?

MR. KELLY: No. But when I'm saying that, I'm not saying that about this. I'm saying that about empirical models that are used in other codes. Okay? Now, that's not to say you can't get this one out of its range either, but part of my philosophy in developing it is not to do that.

But you're right and, at some point, probably in a post-processor mode, because you don't want to have 100 if tests, you know, checking every correlation in the code, but that would be a good post-processing tool to check and flag places where the code was outside its range of applicability, but we're not there yet. We have got to get good answers first.

CHAIRMAN RANSOM: Where are these comparisons? Are they at the entrance of the tube or at the exit of the tube or somewhere in between?

MR. KELLY: They are pretty much everywhere, except for where either the experiment or I threw the data point out. For example --

CHAIRMAN RANSOM: So this is a combination of different points within the tube?

MR. KELLY: Yes, yes, every point where there was a measurement unless that measurement just was thrown out by the experimenter or I looked at it and said this doesn't make any sense at all and threw it out. And you'll notice there is a fairly large number of data points. Okay.

For the air-steam test, for example, there's 571 data points from the UCB-Kuhn test that I am comparing to, and the model that they developed and fit to that data has an RMS of about 25 percent, whereas the model I'm going to show you today is only 16 percent. So we're actually better against the Kuhn data than the Kuhn empirical model and that's not bad.

The same thing happens, we're as good for the helium-steam. When you look at the MIT test, first off you notice the uncertainty band has grown. I think that's partially the data quality of the test. But you will notice the TRACE Model also has a bias. It's about 40 percent too low and if I went back to the previous slide, that's because a lot of those tests are below the turbulent transition, and I'm talking about the transition for the gas vapor core. So when you do the mass transfer solution, you're now off in a place where mixed convection would be very important and we don't model that here.

VICE CHAIR WALLIS: How accurate do you need to be for ESBWR assessment?

MR. KELLY: Preparing to hibernate. That's somewhat ominous.

VICE CHAIR WALLIS: Oh, it's hibernating. It did that the other day and it never came back.

MR. KELLY: Okay.

MR. SIEBER: Well, I think you're done.

MR. KELLY: Yes. Sleep is one thing. Hibernating doesn't sound good at all.

MR. SIEBER: Ralph didn't fix it before either.

VICE CHAIR WALLIS: Yes, it sleeps for a whole month now. A question. How accurate do you think you need to be for something like ESBWR assessment?

MR. KELLY: Well, I don't honestly know. When we start doing more of the ESBWR calculations, we'll have a better idea. Now, you know, if the code model that was there before had been pretty good, then we could do those calculations, range the model and get some idea, but they were so bad that that would have been a fruitless exercise.

But that's part of the reason I'm not

carrying development of this model much further. I mean, like I said, the reason that we under-predict the MIT data is primarily because we ignore mixed convection effects.

VICE CHAIR WALLIS: But you probably can't do any better.

MR. KELLY: I could, but it would be a research program.

VICE CHAIR WALLIS: Well, it may be that for ESBWR assessment you can be off by a factor of 2 and it doesn't matter. I just don't know.

UNIDENTIFIED SPEAKER: It's not plugged in.

MR. KELLY: You know, I'm not sure about the factor of 2, but 50 percent, because there is to some extent a self-correcting thing about that with condensation heat transfer and that's why you don't have to get it to the last data point, and so I think this is plenty accurate enough.

VICE CHAIR WALLIS: Now, can we get rid of this hibernating function somehow?

MR. KELLY: Well, we could switch to a Mac.

VICE CHAIR WALLIS: Maybe in the break we can figure it out.

MR. SIEBER: We can plug it into here.

VICE CHAIR WALLIS: It's one of those Microsoft conspiracies.

MR. CARUSO: It's plugged in, but it's not --

VICE CHAIR WALLIS: If you don't do anything for awhile, does it hibernate or is that what it's --

MR. CARUSO: Well, it has to be the battery.

UNIDENTIFIED SPEAKER: It's only hibernating, because it's not plugged in.

MR. KELLY: It's low battery.

VICE CHAIR WALLIS: Well, if it's plugged in it shouldn't have a problem, should it?

MR. CARUSO: It is plugged in, but it appears like it's not charging.

MR. FORD: Do you want to borrow mine, my computer? Bad transformers.

UNIDENTIFIED SPEAKER: It could be, because it's definitely --

MR. SIEBER: Why don't you plug it in here, this outlet over here.

MR. FORD: So you can see what's going on.

MR. CARUSO: I think I got it now.

MR. FORD: Don't you need to plug in the other end?

MR. CARUSO: No, no, it's still showing the battery is low.

MR. FORD: What about the other end there?

MR. KELLY: Do you have power there?

MR. SIEBER: Yes.

MR. KELLY: Okay.

VICE CHAIR WALLIS: Keep from losing your work. I mean, it's really threatening.

MR. KELLY: No, you know what is happening?

UNIDENTIFIED SPEAKER: The power supply.

UNIDENTIFIED SPEAKER: We have a problem with this plug.

UNIDENTIFIED SPEAKER: Keep going, Joe.

CHAIRMAN RANSOM: Joe, if you were to incorporate an uncertainty function, you know, into a model like this and a code, what would you place the uncertainty on?

MR. KELLY: It's a good question, because it's built really of five different constitutive models. There's, you know, wall drag, interfacial shear, those two affect the film thickness, which is part of this model. There is a heat transfer coefficient between the wall and the liquid. There is an interfacial heat transfer coefficient and then there's the mass transfer effect, which brings in the effective non-condensibles.

Where would you put it? I would probably put it on the mass transfer, because that would directly affect the condensation rate and then that would perpetuate through all the others. If you try ranging all of the others individually, you can do that, but it gets difficult and that's always one of the questions about any of these codes when you do an uncertainty analysis.

MR. FORD: It is charging.

UNIDENTIFIED SPEAKER: It's charging now. I think it's charging.

MR. KELLY: Now, if for example you're doing dispersed flow film boiling, is it wall heat transfer to the vapor? Is it drop diameter? Is it interfacial drag? Is it interfacial heat transfer of the drops? You know, that's a very hard question.

VICE CHAIR WALLIS: Really, I would like to measure all those things. Excuse me.

CHAIRMAN RANSOM: Right. As you incorporate this model into the mainstream of the code, is there just going to be like a flow regime parameter or something like that that turns it on, basically, to differentiate from the normal interface drag or the normal wall friction?

MR. KELLY: Well, for the wall drag, we actually went ahead and migrated that over and that is something that I can talk about next time, because I changed the way the two phase wall drag is done in the code.

VICE CHAIR WALLIS: So that's global more or less?

MR. KELLY: So that's global now.

VICE CHAIR WALLIS: Okay.

MR. KELLY: For the interfacial drag, I made that global, but in the sense for co-current downflow, co-current downflow in the annular regime. Okay? I have spent enough on this. And now just to show you what it looks like.

VICE CHAIR WALLIS: What does total mean?

MR. KELLY: I should have just said wall heat transfer, but that's what I mean here.

VICE CHAIR WALLIS: That's LOCA wall heat transfer?

MR. KELLY: Yes. When I saw that slide last night, I went ah, I wish I had time to change it. So it's calculated versus measured. This is for the

Kuhn pure steam test and I have got plus or minus 25 percent bands on here, and you will notice almost all the points are within that, except for a few. And when you look at those few, those are the ones that are at a higher liquid film Reynolds no. where it's starting to get into the turbulent transition region, and the model I have in over-predicts this data for those Reynolds nos.

Now, I'm just overlaying on it the air-steam data and, again, it's very good for almost all the points, but you will notice there are some that are under-predicted. And when you go back and see which ones are under-predicted, it's the ones where the gas core Reynolds no. has gotten low, down into below 10,000 and they are starting to go towards a laminar transition.

So these are ones where a mixed convection effect in the mass transfer is important. And you can put a mixed convection type model in and bring these points up, but when I did that, I made some of the other ones worse, and that's where your comment about how accurate does the model have to be comes in. So I figured it was not worth the extra development effort to go after the mixed convection effect.

MR. KRESS: What strikes me about that

thought, Joe, is I was under the impression that non-condensibles would have a big effect, but it doesn't seem to make much difference.

VICE CHAIR WALLIS: They are in the theory, aren't they?

MR. KELLY: It does have a huge effect, but what you don't necessarily know is how much the LOCA conditions have changed. This is a log scale.

VICE CHAIR WALLIS: Your air-steam theory has the non-condensible effect in it.

MR. KELLY: Right. What he means is --

VICE CHAIR WALLIS: Otherwise, you would never get those points which are measured to be four times what you are predicting.

MR. KELLY: Right. What he is asking is why are the yellow triangles overlaid by some of the orange ones.

VICE CHAIR WALLIS: I thought he was saying the non-condensibles have a big effect on heat transfer.

MR. KELLY: Yes.

VICE CHAIR WALLIS: And you weren't showing it. But I think you are, because it's in your theory.

MR. KELLY: Right. He just was -- I think you were surprised that there wasn't a larger non-condensible effect. But part of that is that this is a log scale.

VICE CHAIR WALLIS: This doesn't show the non-condensible effect. It just shows that your theory compares with the data.

MR. KELLY: Well, what he means is that the absolute values of the measured ones are not --

MR. KRESS: Are not that much different.

MR. KELLY: -- that different.

VICE CHAIR WALLIS: Well, they're much lower at the lower end.

MR. KELLY: Right.

MR. KRESS: Well, yes.

MR. KELLY: And that's the key. But there are some that overlap and I just --

MR. KRESS: Yes, that was my point was that there.

MR. KELLY: The ones that overlap are probably the lower gas concentrations and the higher gas core mixture Reynolds numbers.

MR. KRESS: So it wouldn't make much difference.

MR. KELLY: So that they are not necessarily mass transfer limited. Some of those tests can actually still be filmed, you know, liquid film limited.

VICE CHAIR WALLIS: Presumably, this is all the data, but this is obviously a group that seems to lie right along the line.

MR. KELLY: Yes.

VICE CHAIR WALLIS: And then there are some other ones.

MR. KELLY: Well, and the other ones, this is adding the helium-steam.

VICE CHAIR WALLIS: Yes.

MR. KELLY: That's where the gas core mixture Reynolds numbers got low. And so I'm under-predicting them because I don't consider mixed convection effects. And that's one of the nice things about having a more mechanistic kind of model as opposed to empirical correlation is I can TRACE this, if you will, to a physical phenomenon that I'm not modeling.

VICE CHAIR WALLIS: It may well be for regulatory purposes you want to under-predict the heat transfer coefficient to be conservative, in which case you're doing fine.

MR. KELLY: Well, if I were to make an error, that's where my event would lead me to be, but

I also didn't figure that it made much -- typically when you go to the lower Reynolds numbers, you're down near the end of the condenser. And most of the steam is gone anyway and that's where, you know, you come, you know, well, how accurate does it have to be? To some extent, this is a self-correcting process anyway, as long as you're not, you know, very far off.

VICE CHAIR WALLIS: You may or may not care, I mean, in something like suppression pool, my G likes to assume that all the steam is condensed. They say essentially 100 percent is direct, but they don't want it to pressurize the space. So getting that last little bit of steam right is very important in that scheme. The ESBWR, I forget how important it is to get the last little bit of steam condensed, but if it goes through the condenser, then it goes into another space and can pressurize it.

MR. KELLY: Yes.

VICE CHAIR WALLIS: So the system interaction gets informed.

MR. KELLY: Right. And again, if you pressurize a little, then the pressure difference between the dry well and the wet well goes up. You drive more flow through and you get back to higher Reynolds numbers, etcetera.

VICE CHAIR WALLIS: Right.

MR. KRESS: You've got to apply this to the Passive Containment Cooling System.

MR. KELLY: And also the isolation.

MR. KRESS: You've got one over -- you don't want to over-predict.

MR. KELLY: Right. So the summary to the introduction is a model has been developed. It has been put into TRACE. I took advantage of the things that TRACE actually calculates like the film thickness. I showed you the accuracy.

VICE CHAIR WALLIS: When you say that is applicable, do you mean it could be applied?

MR. KELLY: Right.

VICE CHAIR WALLIS: So you haven't yet compared?

MR. KELLY: I haven't demonstrated it.

VICE CHAIR WALLIS: Other animals that were tested.

MR. KELLY: That's true, but we're going to be doing that and we'll be using it in the PANTHER data which is for --

VICE CHAIR WALLIS: Was the PANDA tested too or not?

MR. KELLY: There are PANDA tests as well and I believe -- see, we now have two branches. There is an Advanced Reactor Branch and they will be assessing the code for the ESBWR applications. And it's my job to provide them a tool. And when they report back deficiencies in that tool, then it is my job to fix them.

VICE CHAIR WALLIS: Now, PANTHERS tests were done for GE presumably.

MR. KELLY: That's correct.

VICE CHAIR WALLIS: And GE has their own model?

MR. KELLY: Yes.

VICE CHAIR WALLIS: Did you do better or worse than their model?

MR. KELLY: I am not sure what the GE Model is now. At any rate, it would be proprietary. Back at the time of the --

VICE CHAIR WALLIS: But you could get access to it, couldn't you?

MR. KELLY: Yes. Back at the time of the ESBWR submittal, they used the Vierow-Schrock correlation with modification in its implementation to limit some of its over-prediction.

VICE CHAIR WALLIS: Yes.

MR. KELLY: I don't know what is in tri-GE now. But, you know, we looked at the accuracy and the accuracy is quite good. When it is not good, it's because of a phenomena that we're not modeling, like mixed convection. Back to my contents. We finished the introduction and, at this point, I will give you a couple of choices. You can accept the model is good enough and I can stop and we can be an hour and a half ahead of schedule, which would be a first or we can jump ahead. I can show you how it works in TRACE and, at that point, stop if you will or I can go ahead and give the entire presentation. I think I know the answer.

I showed some of this the last time, about a year ago, and the parts that I showed before, I'm going to go through very quickly. When you talk about film condensation, the traditional representation, you know, when you are doing your heat transfer coefficients on a piece of paper, it's a heat transfer coefficient times T_{wall} minus T_{sat}, and that's what we're all used to. Now, you go and you calculate using whatever the appropriate model is. But when you stick it into a two-fluid code and you want to make it work as part of the numerical framework, the wall heat flux is really a heat transfer coefficient between the wall and the liquid.

And in the same temperature here is the liquid film temperature, not T_{sat}. So what we're talking about is this heat transfer path between the wall and the liquid. The condensation rate --

VICE CHAIR WALLIS: This say T_l or T_i there?

MR. KELLY: That should be T_l.

VICE CHAIR WALLIS: What is it actually there?

MR. KELLY: It is T_l there. It's just hard to read.

VICE CHAIR WALLIS: It just looked like an i in a way. Okay.

MR. KELLY: Right.

CHAIRMAN RANSOM: That's the average liquid temperature in that cell?

MR. KELLY: That's correct. It should be somewhere between T_{sat} and T_{wall}, and that's about all you know. The condensation rate use some, the two interfacial heat transfer rates, vapor-interface, which is heat to the interface and liquid-interface, which is actually removing heat from the interface and divided by the weight and heat. And those are shown here.

CHAIRMAN RANSOM: The question there would be what if the vapor is super heated somewhat and how is that super heat then taken into account in the mass transfer model?

VICE CHAIR WALLIS: Yes.

MR. KELLY: Well, with the mass transfer model, we'll get to that later. But let's say this is pure steam at the moment. If the steam is super heated, there is an interfacial heat transfer relation. It's basically Dittus-Boelter kind of thing for hot steam to this film. And so in that case, Q_{vi} would be positive, Q_{li} is negative, and it's the sum of those two that becomes the condensation rate.

So it's kind of like you have evaporation and condensation and you sum them and whichever one is the largest wins.

VICE CHAIR WALLIS: Yes.

MR. KELLY: In normal cases and especially for the mass transfer case, the sensible heat transfer from the vapor gas mixture is basically negligible. We're down in a couple percent. And so it's much, much smaller than the uncertainty in the data. And one thing I wanted to make a point of on the slide is the interface temperature here, this T_i, that is, and any two-fluid code I know of, it's assumed to be at the T_{sat} at the bulk vapor partial pressure.

VICE CHAIR WALLIS: Afraid it isn't, because of --

MR. KELLY: That's true.

VICE CHAIR WALLIS: -- mass transfer boundary.

MR. KELLY: Exactly. And so you have to make an adjustment for that in your mass transfer model. In effect, you have to put an additional resistance in this path. So I have this long laundry list of models and we're going to start with wall friction. And the wall friction -- now, I have to be able to do condensation of both pure steam and non-condensible gas mixtures. And it has to work for both falling films and sheared films.

Now, this is why we're talking about wall friction. This is a result of a calculation with the existing TRACE Model, and if you read through the manual, what you will see they won't use the word partitions, but, in effect, that's what it does.

VICE CHAIR WALLIS: It says this drag on the vapor even though it doesn't touch the wall?

MR. KELLY: That's correct.

VICE CHAIR WALLIS: Very strange.

MR. KELLY: Well, that's incorrect. Okay.

VICE CHAIR WALLIS: That's what it does?

MR. KELLY: That's what it does.

VICE CHAIR WALLIS: Why on earth does it do that?

MR. KELLY: Because when the developers were doing that, they were worried about large break LOCA. They were not at all thinking about annular flow or condensation. And so the model they put in was suitable for their application, but that's why you need to review those models when you bring it forward to a new application. You know, there was a no, never mind for large break LOCA. It's important here if you use a model that's based upon the film thickness.

So the first effect of this, and this is the phase velocity versus axial position, this is for a pure steam condensation case at 3 bar. The blue line here is the TRACE calculated vapor velocity.

VICE CHAIR WALLIS: You mean it's going faster than the vapor?

MR. KELLY: Exactly. And that's exactly what -- this is the behavior expected. As the vapor condenses, slows down, the liquid hangs up around 5 meters a second. Now, I don't know about you, but I can be driving at, let's say, in excess of the speed limit in a rain storm and I don't see any liquid films on my windshield moving at 5 meters a second. Maybe when the windshield wipers go over, but that's about the only time.

VICE CHAIR WALLIS: Right.

MR. KELLY: So this is just plain wrong. Okay. The result of this, if the liquid film is moving that fast, it's basically in free-fall. The film thickness that you get is minuscule.

VICE CHAIR WALLIS: Then you get a huge heat transfer coefficients?

MR. KELLY: If that's how you use it. If you use the film thickness in the heat transfer coefficients, that's right.

VICE CHAIR WALLIS: That's a previous TRACE Model or what exists now?

MR. KELLY: Previous.

VICE CHAIR WALLIS: Okay.

MR. KELLY: I don't have a film thickness, you know, a measurement.

VICE CHAIR WALLIS: TRACE came from TRAC and RELAP. Was this wrong because RELAP was wrong or because TRAC was wrong?

MR. KELLY: Both.

VICE CHAIR WALLIS: Both?

MR. KELLY: It's wrong because TRAC is wrong. But when I looked at model on condensation on non-condensibles in RELAP years ago, it was wrong in RELAP as well. We had exactly the same kind of partitioning and with exactly the same kind of miserable result. And it was fixed at one time in RELAP and I think that got into the official code version. Wei Dong and I fixed that.

VICE CHAIR WALLIS: If you look at your right hand figure there, that means that your heat transfer coefficient would be off by an order of magnitude.

MR. KELLY: If you are using the liquid film thickness in the heat transfer coefficient, that's correct. But what TRACE, the old TRACE, did was it used a Nusselt, the laminar analysis thing, where, in effect, it was treating each node as a heat transfer surface, so it didn't use this at all. Now, this was a void fraction, but it paid no attention to it.

MR. FORD: Could I ask a materials question?

MR. KELLY: Certainly.

MR. FORD: Presuming all this wall friction stuff depends on the state of the physical properties of the surface, so if you are highly oxidized, for instance, you're going to have different results. Is that true or is this just a no, never mind?

MR. KELLY: Well, okay. For single phase flow, you're absolutely correct.

MR. FORD: Yes.

MR. KELLY: That you can increase the wall drag on maybe as much as order of magnitude by going from a polished, you know, a drawn tubing, something that looks more like a concrete duct.

MR. FORD: Yes.

MR. KELLY: For annular flow, and these are fairly thin films, the effect of tube roughness has never really been established. At least that's a quote out of, I think, one of Professor Hewitt's papers, was that they had never -- you know, it has never been systematically investigated.

MR. FORD: Experimental wasting of --

MR. KELLY: They had never seen it.

MR. FORD: -- smooth plastic tubing. This is what I was wondering. Are you introducing a constant error in the data, since the model is trying to catch up or what?

MR. KELLY: Well, I think, compared to everything else that is fairly minor.

MR. FORD: Okay.

MR. KELLY: In that the biggest problem with condensation experiments is knowing what the heat transfer rate is.

MR. FORD: Yes.

MR. KELLY: Now, if you're doing a heating experiment, you just turn your Rheostat and you know how much power you're putting into it and you divide it by the length and you've got the heat flux.

MR. FORD: Yes.

MR. KELLY: Then you use your thermocouples to give you the wall temperature and you can get a heat transfer coefficient. That's not the case here.

MR. FORD: Okay.

MR. KELLY: You have to do mass balances. And typically there is like a secondary side which will be single phase liquid and you have to measure the axial change in the liquid temperature and assume that represents, you know, some bulk temperature and use an energy balance and get a heat flux over a region. And so that becomes highly uncertain. And that was one of the big things they kept trying to improve in the Berkeley experiments.

MR. FORD: The reason I'm asking the question is you're applying this to the ESBWR and the

ACR-700 and maybe others in the future, in which we are changing materials for construction.

MR. KELLY: Yes.

MR. FORD: Are you therefore introducing an unknown quantity? What I'm hearing you say is it's a no, never mind.

MR. KELLY: I think it's a no, never mind. And in the turbulent part of the correlation that I use, there is a roughness effect, you know, that was established for single phase flow, whether that is applicable to annular flow or not is unknown.

MR. FORD: Thank you.

MR. KELLY: So the model that I'm putting in is very simple. You know, parallel plate for the laminar, turbulent and then a power-law waiting and you want to know does that make any sense. So the first thing I did was put together a database of basically all the film thickness, falling film thickness data, I can find. You notice it covers almost 4 orders in magnitude and film Reynolds no. going from very laminar, almost smooth films, to highly turbulent films.

And when you take the model that I just showed you and you put it into TRACE, turn off interfacial shear to make it look like a falling film, the borderline is the code calculation. Where did these calculations form?

VICE CHAIR WALLIS: When you get Haaland expression.

MR. KELLY: Yes.

VICE CHAIR WALLIS: If a big Reynolds no., am I being stupid, a big Reynolds no. then that thing in the internal bracket is less than 1, a big Reynolds no.

MR. KELLY: Yes.

VICE CHAIR WALLIS: An absolute zero. The log of a number less than 1 is negative.

MR. KELLY: Yes, and there's a square.

VICE CHAIR WALLIS: So you square it to make it positive?

MR. KELLY: Yes.

VICE CHAIR WALLIS: Gee whiz.

MR. KELLY: Yes, I don't do that. That's the explicit approximation.

VICE CHAIR WALLIS: Okay.

MR. KELLY: And that's one of the more accurate ones for, you know, trying to emulate Colebrook-White without it being an implicit relationship. It's accurate and fairly simple. But you're right. So you have to make sure it's to the power of 2 as an integer and not 2.0 as a real. So to finish with wall drag, that works pretty well for falling films.

And now we're going to talk about interfacial friction and this is something I showed last time, so I'm going to really breeze through this. What I did is I found a database for co-current downflow, the test that Andreussi-Zanelli the air/water co-current downflow. What is great about these tests and the reason I picked them, not only do they measure the film thickness, but they measure the pressure gradient and the fraction of the liquid entrained.

From that, they actually calculate values of the interfacial friction coefficient. So I can do a comparison interfacial friction coefficient instead of just, you know, predicted film thickness. I looked at all of these various models and here are the results. There's one model, in particular, it was by Professor Hanratty at University of Illinois, the one that was with the entrainment was vastly superior to the others. This is what it looks like.

VICE CHAIR WALLIS: How do you know the entrainment?

MR. KELLY: Well, I don't remember how

they measured it.

VICE CHAIR WALLIS: How do you in your TRACE?

MR. KELLY: Oh, that's one of the reasons you're not going to see it in the calculations. It's a correlation by Ishi and I don't know how good it is.

VICE CHAIR WALLIS: Ah, hah.

MR. KELLY: So this is how good the Asali-Hanratty Model is predicted versus calculated for interfacial shear. When I put the model into TRACE and do a simulation, that's slide 21, this is the calculated film thickness with TRACE versus the measured film thickness and I'm using all of the data for which the entrainment was basically zero. And I did that because I wanted an estimate of how good the interfacial drag model was without cluttering it up with the entrainment.

The blue points are with the default TRACE Model and this is the code version and the orange/red points are with the PCCS updates, which is the model I just showed you for wall drag and interfacial friction.

VICE CHAIR WALLIS: It looks like a huge improvement.

MR. KELLY: Yes. And I feel pretty

confident about that. So the next model to talk about is wall-liquid heat transfer. And as uncertain as interfacial friction can be, wall heat transfer can be more uncertain and when we get to interfacial heat transfer even more uncertain. So what do we need? We need a wall heat transfer coefficient. Again, this is wall to liquid that works for laminar films, both smooth and wavy, and it also needs to work for turbulent films.

VICE CHAIR WALLIS: What do you do about entrainment though? If you have any condensation, you start off with no entrainment. Pure steam comes in. It takes a while to develop entrainment, so your entrainment equilibrium correlation shouldn't work very well. You're always going to be less than predicted, it would seem to me.

MR. KELLY: And you would get all of your entrainment at the inlet. And then as you go through it --

VICE CHAIR WALLIS: It doesn't happen instantaneously, because there's no liquid films, there's nothing to entrain.

MR. KELLY: Right. And then you also have to say how is entrainment treated in a two-fluid model. At the moment, we do not have the droplet field.

VICE CHAIR WALLIS: All the experiments on entrainment it takes a lot of l over ds to get to equilibrium entrainment.

MR. KELLY: Right. So I don't think it's terribly important. Even I have simulated some of the NASA tests and, you know, I don't think the entrainment -- it does calculate some.

VICE CHAIR WALLIS: With respect to the entry there's very little entrainment, velocities are so low.

MR. KELLY: Yes, the films are --

VICE CHAIR WALLIS: That's why it works.

MR. KELLY: Yes, the films are modestly sheared. Okay. So how are we going to determine what the appropriate wall heat transfer coefficient is? Well, for laminar films, what I'm going to do is look at falling film condensation data and use that to help me select a correlation. But after I did that, I thought well, maybe I should go back and look at the pure steam condensation data of Kuhn.

Now, I don't have any interfacial heat transfer data in that regime. So what I'm going to do is take the, what I'll call, total heat transfer coefficient which includes both resistances, you know, wall to liquid, liquid to interface, and just split it between the two, because I don't know anything better than that. That's what I'm going to do for laminar. I'll walk you through that.

For turbulent films, I'm going to look at falling film heating data, for just that reason that we have less uncertainty.

VICE CHAIR WALLIS: Are you talking about the NASA transfer causing momentum transfer?

MR. KELLY: Yes, I --

VICE CHAIR WALLIS: That's in this somewhere is it?

MR. KELLY: That's built into the TRACE.

VICE CHAIR WALLIS: Simply added on?

MR. KELLY: There is a gamma v rel in the TRACE equations. Right. So if you look at the field equations, you'll see that there. And what I was showing was the adiabatic interfacial drag. For turbulent films, I'm going to look at falling film heating data and then I'll be considering the interfacial heat transfer separately.

Well, this is an example of falling film condensation from the database I put together. The first thing you will notice is the heat transfer coefficients are averaged over the entire surface.

There are no local values. And so what you are seeing here is a Nusselt no. and what the asterisk means is it's a characteristic link in it, rather than being the film thickness.

It's what I call the Nusselt film thickness. So it's the liquid velocity squared over liquid density times G point to Rho to the 1/3 power. So that's the characteristic link here. The brackets means that it is averaged over the entire heat transfer surface, applied against the film Reynolds no. The solid black line is the Nusselt correlation, so that's the other thing that you're supposed to see, is that due to the presence of waves on these laminar films, there's typically a 15 or 25 percent enhancement. And, of course, in this free-on data out here, when this starts bending back upward, that's because the film is going turbulent.

VICE CHAIR WALLIS: It looks as if almost all of this is Russian data?

MR. KELLY: These two or a lot of it is, yes. And just because I was able to find it, although I had digitized a lot of this stuff. I spent a lot of time typing numbers in. The correlations I looked at are here and I have written them in two different ways. One is the surface average Nusselt no.

referenced to the Nusselt, you know, film.

VICE CHAIR WALLIS: They don't have an effective Reynolds no., but the data don't seem to pull together on the plot of Nusselt versus Roan.

MR. KELLY: It's condensation data. It's highly uncertain. And what I have done over on this side is rewrite all of these correlations in using a Nusselt no. that's a function of the film thickness and say for the Nusselt laminar film theory, you get a Nusselt no. of 1. And so all these others work to be some function of a liquid film Reynolds no. that provide an enhancement overlap.

VICE CHAIR WALLIS: It doesn't make sense. Well, maybe I'm misunderstanding. What's your previous plot, Nusselt no. versus Reynolds no.?

MR. KELLY: Yes, but it's this Nusselt no., which is heat transfer coefficient times that laminar link scale, you know, the viscosity squared over G Delta Rho times Rho to the 1/3 power divided by the liquid connectivity, averaged over the surface. So the first thing you do, the first step is to get rid of the averaging and that's basically -- I mean, you have to do it. You have to -- you know, this has been integrated over the surface. You have to undo the integration.

There is a way, that I took it from Butterworth, to do that and so then that ends up changing this coefficient, leaving the power the same and then what I ended up doing was dividing, changing the link scale that I referenced all these to a smooth laminar film, and that's how you go from this to this.

VICE CHAIR WALLIS: It seems to make more sense. The Nusselt theory is the Nusselt no. is 1.

MR. KELLY: Right.

VICE CHAIR WALLIS: When you plot it versus Reynolds no. in this peculiar way, it doesn't show that.

MR. KELLY: Oh, and that's -- yes. That's because of this. And if you go and look at all of the condensation data, I mean, especially since most of this is old, that's how it is all reported. And in a lot of the papers and handbooks that's how the correlations are given. And the theory, well, part of the rationale is simply that way you don't have to calculate the film thickness.

VICE CHAIR WALLIS: Yes.

MR. KELLY: But since we are calculating the film thickness, we should use it.

VICE CHAIR WALLIS: Okay.

MR. KELLY: So I put together a database

that has almost 900 points on it. Obviously, this is the turbulent region. The reason these are separated is different Prandtl nos., which doesn't show up. Again, this is that same surface average Nusselt no. The black line is Nusselt. It's a lower band of the data. The one by Kutateladze is pretty high and this one by Labuntsov --

VICE CHAIR WALLIS: Well, this is almost like the plot of materials table.

MR. KELLY: Yes.

VICE CHAIR WALLIS: Electronic, and are you saying these numbers are functional, are you? What are you trying to convince me about here?

MR. KELLY: Well, I'm trying to say that it is a function of the film Reynolds no. due to the waviness effect.

VICE CHAIR WALLIS: So they extrapolate out to tender the fourth or something. I got off by order of magnitude easily.

MR. KELLY: Because you didn't consider the turbulent transition.

VICE CHAIR WALLIS: Yes.

MR. KELLY: Now, what you have to do is take the turbulent model.

VICE CHAIR WALLIS: Yes, okay.

MR. KELLY: And again, these are surfaced averages and you have to --

VICE CHAIR WALLIS: In all the books there is another line up there, isn't there?

MR. KELLY: Yes, either that or they chop the plot, one of the two. So we're just talking about laminar films at the moment, and I'm going to get back --

VICE CHAIR WALLIS: It's just a film without much --

MR. KELLY: The laminar, no vapor velocity.

VICE CHAIR WALLIS: Just a falling film?

MR. KELLY: Just a falling film. And this is how uncertain the data is. I'm going to get to some better data. Now, this is what I was using to make a selection, and I had tentatively selected the Labuntsov Model, which is actually a fairly modest increase over Nusselt, but I decided I should look at the UCB-Kuhn test, since that actually bracketed the range of film Reynolds nos. and vapor Reynolds nos. that I was interested in.

This is still the funny Nusselt no. in the sense that I'm using the laminar viscus, if you will, link scale, and so I took all of their data and plotted it in these coordinates and you notice that this is not, in any way, corrected for the effects of interfacial shear. As I've said interfacial shear is fairly modest in these tests. And as you expect, Nusselt under-predicts most of the points, but these other models pretty significantly over-predict.

And even Labuntsov, which was the lowest of them, over-predicts somewhat and this is with no interfacial friction correction. Now, we're going to go to Nusselt no. that you are more familiar with. One based upon the film thickness, but to do that I had to calculate the film thickness, because it was not measured. So I took my best shot at calculating it, switched the Nusselt no. based upon the film thickness, plotted versus film Reynolds no., here is your Nusselt number of 1. Okay?

VICE CHAIR WALLIS: Well, it looks to me as if this is just ripe for a Kelly line.

MR. KELLY: No, no, no.

VICE CHAIR WALLIS: None of them is very good.

MR. KELLY: Right. So what I'm going to do --

VICE CHAIR WALLIS: The Kelly curve.

MR. KELLY: No, no, no. I might want --

VICE CHAIR WALLIS: Kuhn-Schrock.

MR. KELLY: I considered that, okay, because I would have fit this a little bit differently, but I'm going to use the one that the experimenter came up with, and I like it for two reasons. It goes to the value of 1 with zero film Reynolds no., as it well should. And then it very naturally comes up. Now, I think it actually should have a higher slope in here, but that's another thing.

So I was trying to make a model that was pure untainted by using the data that I was going to later compare against, but in this particular case I found that wasn't viable. So I went ahead and used the model that was developed against this data, simply because it was better.

MR. DENNING: Let me try and understand this. You have used the Kuhn-Schrock-Peterson fit of their data, on their data.

MR. KELLY: Right.

MR. DENNING: And that's okay. But are we sure that the Kuhn data is the most applicable to the plant?

MR. KELLY: I am, yes.

MR. DENNING: And it is the best quality control data?

MR. KELLY: Yes. From everything I have seen by looking through the literature and looking at how the data was taken, yes. And what you will see if you look at a lot of the earlier tests, it would go directly and measure condensation with non-condensibles and never measure it with pure steam. So Kuhn is one of the few that have actually done both. And so I'm only looking at the pure steam data here, which, of course, is not the plant condition. But I'm just doing that to get the wall-liquid heat transfer coefficient.

VICE CHAIR WALLIS: You never have fewer steam either.

MR. KELLY: Right.

VICE CHAIR WALLIS: So you're saying the non-condensible fraction is less than some value?

MR. KELLY: Yes, these are reported as zero, but, of course, it's --

VICE CHAIR WALLIS: No, you never have that.

MR. KELLY: Right. Especially if the steam comes from -- well, actually, in this case, the steam did not come from the physical plant. They had a little boiler, you know, where you run it for a long time and try to get the non-condensibles out of the system. So that was the laminar film.

VICE CHAIR WALLIS: So you were distracted by all this Russian work and correlation and you went to some, eventually, thing done locally?

MR. KELLY: True.

VICE CHAIR WALLIS: Essentially, kind of.

MR. KELLY: And also more modern, data from the '60s versus data from the '90s.

VICE CHAIR WALLIS: Right. I'm suspicious of some of this old data. I mean, they didn't control things so well.

MR. KELLY: They did the best, you know, it's hard. If you go in the lab and try and measure condensation data, it's not easy. So we now have a model for laminar film. What are we going to do for a turbulence film?

VICE CHAIR WALLIS: This is about the simplest case you could think about.

MR. KELLY: Yes.

VICE CHAIR WALLIS: And you still got trouble getting feathers that fit together.

MR. KELLY: That's true.

VICE CHAIR WALLIS: Okay.

MR. KELLY: That's why you should never look at more than one data set. For turbulent films, it's more difficult, because we have the same problem in the database, it's all averaged over the entire heat transfer surface. That means a lot of the surfaces in laminar and only part of it is in turbulent.

VICE CHAIR WALLIS: Yes.

MR. KELLY: And you are getting the interval effect. There is no straightforward way to subtract out the laminar and only look at the turbulent to compare turbulent models to. So that's a problem. Then if you look at sheared film data, like the NASA data, where I do have, if you will, "local values" that data itself has very large uncertainties, and then when you go to do an analysis of it, you're relying upon a calculated film thickness and there is an uncertainty with respect to that.

To make it even worse, if you go and look at some of the correlations for turbulent film condensation, they are really all over the map and I'll show you that in just a second. So what am I going to do? I went and looked at turbulent film heating data, because in this case you control the wall heat flux, you can have a much better idea of what the wall to liquid heat transfer coefficient is. Then the interfacial heat transfer, which is the one as I said 4 is highly uncertain, we'll talk about, we'll treat separately and talk about that later.

This is an example of how widely spread the turbulent film condensation models are. I looked at six different models and again the Nusselt no. based upon the laminar link scale versus film Reynolds no. just for two different Prandtl nos. and you're seeing a factor of about 3 just between correlations.

VICE CHAIR WALLIS: These are the same experiments or are they different in some way? Different fluids or different pipe size?

MR. KELLY: These are Prandtl nos. of 1 and 2, so most of these are --

VICE CHAIR WALLIS: They are probably very much the same experiment.

MR. KELLY: Yes.

VICE CHAIR WALLIS: So even in the same experiment, they can't get the same answer.

MR. KELLY: That's true. I mean, a factor of 3 is pretty amazing.

VICE CHAIR WALLIS: Did you try correlating with longitude?

MR. KELLY: So how do you choose which of those is right? Well, I decided to punt and go look at film from heating data, because this is much better control. On this slide, I don't show you the data, but I show you a Nusselt no. based upon film thickness versus film Reynolds no. The LOCA correlation is the one that almost every handbook recommends. It actually has 4 different correlations pieced together. I'm not sure if this is Russian. I'm not sure if it's Gimbutis or Gembutis, but they almost over-raise it, even though it was developed from a different data set. And so that's a good sign.

Gnielinski is nothing more than a single phase, it's like a modern form of Dittus-Boelter. If you're familiar with the Patukoff, it's basically the same. And so what I did is just modify it for liquid film. And what that is you divide by 4 because of the way the hydraulic diameter is calculated. All three of these overlay, so I can use any of those three with the same, you know, degree of veracity.

VICE CHAIR WALLIS: Now, how do we compare this with what was on the previous slide or is it something different we're looking at?

MR. KELLY: Well, the previous slide was following some condensation data.

VICE CHAIR WALLIS: But the number --

MR. KELLY: Not data.

VICE CHAIR WALLIS: -- was quite

different.

MR. KELLY: Okay. That's because those are a different number with the asterisk which uses the laminar viscus link scale.

VICE CHAIR WALLIS: So I can't compare them?

MR. KELLY: Right. And the other is referenced to the film thickness.

VICE CHAIR WALLIS: So be very careful when you use Nusselt no. that you define it each time, because it seems to be different.

MR. KELLY: That's true. I was just trying to shorten the presentation by not having all the extra slides, but you are right, I should have.

VICE CHAIR WALLIS: Well, I didn't know.

MR. KELLY: So this gives you an idea of what the heating correlations look like relative to actual heating data. I have two different sets here. Actually, this is by bay.

VICE CHAIR WALLIS: These are all heating of film?

MR. KELLY: This is all heating of film where you can make a local measurement and you know what the heat flux is, you know, with uncertainty, but a lot better than condensation tests. So in both cases this is the Nusselt no. with the asterisk and this one is just divided by a Prandtl no. to the .34 because that's what Wilke uses and just made the comparison a little bit easier. Both correlations give a pretty good representation of the data.

Wilke is a little bit closer, but of the high film Reynolds nos., I like the behavior of the Gnielinski better.

VICE CHAIR WALLIS: So this asterisk one is sort of comparable with the Chen-Gerner-Tien, Colburn, Kirkbride, Kutateladze, all that stuff you showed earlier?

MR. KELLY: Yes.

VICE CHAIR WALLIS: It's sort of on the same page any way.

MR. KELLY: Yes, that's true. So we now have defined the wall heat transfer, wall-liquid heat transfer coefficient. And now we're going to really jump off into uncertainty space and talk about interfacial heat transfer. I am not going to talk about the vapor to interface, because it's basically negligible in everything we see. So we're going to now talk about the liquid-interface. I should say interface to liquid.

Again, laminar and turbulent films.

Laminar films we have already treated. What I showed you was the Kuhn-Schrock-Peterson correlation or that fit. That was for the total heat transfer resistance across the film, which has the components both of wall to film and interface to film. It has them combined, and so I'm just putting that heat transfer resistance between the two, because I don't know anything better, which should make the liquid film temperature lie halfway between the wall temperature and T_{sat}.

For turbulent film, I just showed you the wall heat transfer. I feel pretty confident about that.

VICE CHAIR WALLIS: This NWU is Bankoff or something?

MR. KELLY: Exactly. And that's where we are going now.

VICE CHAIR WALLIS: So now you've brought in another actor in this.

MR. KELLY: Right.

VICE CHAIR WALLIS: Okay.

MR. KELLY: See if I use everyone's model, then I'll make everyone happy, but not exactly. So we're going to look at his co-current flow data. Unfortunately, there were no tests for vertical co-current flow, only horizontal, so there can be some differences there. And then because this is over a fairly limited film Reynolds no. range, only about 2,000 to 40,000, I'm going to go look at some other data. That's actually above our range, so I'm going to look at some other data to try to get it closer to our Reynolds no. range.

This is an example of some of the correlations I was looking at and I've got seven of them there and now they vary by an order of magnitude. You know, for condensation, it was a factor of 3. For interfacial it's an order of magnitude. And by looking at those, you can also see the Reynolds no. dependence varies from something like three-quarters to about one and a half and that's just in the correlations. The data, of course, is worse.

Well, here is some of the data. Vertical axis is a Nusselt no. based upon film thickness that was measured in these experiments divided by the liquid Prandtl no. to the half power, which is how Bankoff correlated this later. It's for horizontal co-current flow. I have plotted against film Reynolds no. and I have plotted it, identified the individual measurement stations, and of course did that for a reason, and that's to show off an entrance link effect.

It's co-current horizontal the first station is, you know, just immediately downstream of the inlet and you get higher condensation rates. As you go down each progressive measurement station the heat transfer rate, the heat transfer coefficient decreases, but the last two stations, station 4 and 5, pretty much overlay. One of the early models proposed by Bankoff was what he called a "turbulent center model."

VICE CHAIR WALLIS: I wonder if the effective station is as big as the effective Reynolds no.?

MR. KELLY: Yes, yes.

VICE CHAIR WALLIS: So you really ought to have an l over d or something in the correlation. If you're just going to use Reynolds no., it's pretty misleading.

MR. KELLY: Yes. And if you want to try to do a very good job on these experiments, you probably should.

VICE CHAIR WALLIS: Now, you can't use horizontal though.

MR. KELLY: Because that's the only data. I will show you some vertical, but the vertical I'm going to show you is counter-current. Okay? This is an area where I think the code is highly uncertain and where we may consider doing an experiment in the near future with the next round of the Thermal-Hydraulic Institute. So the first model that Bankoff proposed was what he called a "turbulent center model." So the Nusselt no. is based upon the film thickness and the film Reynolds no. that he correlated against he used what he called a "turbulent velocity," but he ended up just saying it was something like 30 percent of the main film velocity.

So what you end up with is this blue line and it is nothing more than a constant times the film Reynolds no. to the three-quarter power times the liquid Prandtl no. to the half.

VICE CHAIR WALLIS: Excuse me. You know all this stuff. You have explored all this. But some regulator from NRR who uses TRACE just to sort of blindly predict something and doesn't know that for this particular application the l over d is so short that he really ought to worry about something, and then, you know, the regulator makes a decision.

MR. KELLY: Well --

VICE CHAIR WALLIS: How do you get this kind of confusing information to the user?

MR. KELLY: To do the job right, okay, and we did the job right, in my estimation we did AP-600, what you do is a very detailed code applicability study.

VICE CHAIR WALLIS: We look at the range of those things?

MR. KELLY: Yes. You start with the part, you identify the important phenomena and the step that most often is overlooked is you identify the ranges over which those phenomena are important. You know, what's the Reynolds no. range, etcetera, etcetera. You go out you identify separate effects tests for each of those. You assess the code against those separate effects tests for all of those highly ranked phenomena over the range where they are important. I'm talking about a big effort. Then you have interval scale, interval data, hopefully at a couple different scales and you run the code against that.

VICE CHAIR WALLIS: This looks so complicated. It seems to me that if you left the Agency, there probably would be nobody who understands it well enough to figure it all out.

MR. KELLY: Well, Wei Dong is my understudy and I'm hoping also to bring Shawn Marshall along that way too. But that's the nature of any code.

VICE CHAIR WALLIS: The problem I see here is that an applicant could be selective in choosing which data set to show agrees with his correlation and could then make it look as if his analysis is very good. Unless you are really experienced and smart, you wouldn't know that he is being very selective in choosing which data set to use in order to make a point.

MR. KELLY: That's true. And, you know, it's the applicant's responsibility to show that the data is in the right -- covers the right ranges and, you know, show how well or not it does. But it's not easy.

CHAIRMAN RANSOM: Well, it seems to me that's an argument for the Agency having a standard that we trust and basically to measure the applicant, how his code or calculations would do relative to that.

MR. KELLY: Yes.

CHAIRMAN RANSOM: And, of course, you're going to document all this, right, so that it can be retrieved.

VICE CHAIR WALLIS: Well, some applicants still use something antiquated like one of these Wallis correlations, which was produced in five minutes by a graduate student in '62, and you have shown there a better correlation. Should we therefore not allow them to do that?

MR. KELLY: Well, historically, what the Agency has done is it's the burden of the licensee to prove that the analysis works for its intended application. And if interfacial friction and annular flow is a no, never mind for the transit of interest, then maybe they shouldn't have to go back and retrofit the code.

VICE CHAIR WALLIS: I don't see that. When we looked at -- maybe I shouldn't be specific, when I looked at huge volumes of code documentation from well-known applicants, well-known vendors, they seem to just agree with any use of correlation here and the rest of the correlation they therefore will make one up, because it has the right limits and we'll do all this stuff and then it's all put together. And then there is a curve. I never say any detailed justification of why these equations were justifiable. And you have to dig very deeply then to find out what they are using.

MR. KELLY: Yes. Which is why I take a different approach.

VICE CHAIR WALLIS: Well, this is why,

perhaps, we should be pretty careful and cautious and conservative in assuming that you can predict things. Very aware of the range of uncertainties.

MR. SIEBER: This is really the first time that we have examined this give and take between correlations and the data that underlie. I think it is very good myself.

MR. KELLY: Thank you.

MR. SIEBER: It will become like other codes, you know, once it goes to some user some place, the user won't have that background and will accept, you know, the results as though it's error free.

MR. KELLY: Based on -- yes, that's true. And, of course, the eventual road map for all this. You know, we had to do the consolidation. Two things surprised us, I would say. Joe may correct me. The first is how much work it was to try to be able to do a translation of a RELAP5 input deck to a TRACE deck. We spent a lot of time on that. We didn't think it was going to be easy, but it was more work than we anticipated, I would say.

The other thing that surprised me is how bad the extent physical models in the TRACE Code were and how much work we're now having to do, now that we're at the point where we can focus on this to, if you will, recover what should have been there to begin with.

VICE CHAIR WALLIS: If you show us the next graph, I mean, here is George Bankoff doing the best he can with his own data. The next figure, page 39.

MR. KELLY: Yes, well, let me back up just a second. This one, this correlation did not come from this data. I think he actually did this from some earlier data and then adjusted the coefficient on it, so it went in the middle of the data. I went and adjusted the coefficient on it so it fit the fully developed data. So then you could add if you decided to the entrance link effect.

VICE CHAIR WALLIS: Right.

MR. KELLY: And then I thought well, this is actually a Reynolds no. range above where we are.

VICE CHAIR WALLIS: Well, this is NWU. This is Bankoff's own data.

MR. KELLY: Yes.

VICE CHAIR WALLIS: Versus his own theory.

MR. KELLY: Well, he had several theories. Okay. Each experiment spawned a different one.

VICE CHAIR WALLIS: Each experiment has a different theory?

MR. KELLY: And what I'm showing is actually one of the simplest and one of the first, and I'm doing that for a couple of reasons. The first correlations they came out with, let's see, is this the right marker for these, do you know? They were of the form.

MR. SIEBER: You can write on this. Whoops.

MR. KELLY: It's sort of hazardous here. They were written in the form of a Nusselt no., a coefficient times the gas Reynolds no. to some power times a film Reynolds no. to power times prandtl no. to the one-half. And he picked one-half because that's mass transfer and he didn't have enough variation in his data to determine it.

Well, there is three things wrong with this correlation that, as far as I'm concerned, make it unsuitable for use. The first is it's multiplicative in the gas and film Reynolds nos. You know, this is supposed to mainly take the effect of interfacial drag. If this is zero and you have a falling film, you've got a zero Nusselt no., so I can't use it.

The next thing is that it's the gas Reynolds no. The interfacial shear is more dependent upon a relative velocity, not the Reynolds no. Like if you look at these tests, you know, there was a container here and so the Reynolds no. is a function of that distance. So you just make the container bigger, same velocity, you get a different Reynolds no. You see, there are a number of papers where they say, you know, well, this guy's Reynolds no. effect was this. We found something completely different. That's part of the reason. That's the second reason.

The third is that by putting them both together like this, there is a confounding effect. If all of your high film Reynolds no. data occurs when you are at a low gas Reynolds no., you can't separate the two unless you've done some kind of parametric.

VICE CHAIR WALLIS: Do they go together? I mean, you increase the flow rate, you increase the flow rate of both phases, because you're condensing.

MR. KELLY: Right.

VICE CHAIR WALLIS: So do they go together?

MR. KELLY: Right. So there are three things. But even when he did this correlation, he, I should say students or a series of students, it's pretty widely scattered. And when Professor Banerjee was here, he said I should use the northwestern correlation, and it was a later one, and it was a later turbulent center model. And when you look at the documents, the NUREGs, it looks great. For condensation data, especially interfacial, it goes right through the middle of the data and the scatter is pretty small.

So I thought well, yes, that looks pretty good. Well, what's wrong with it? Why am I not using it? Again, a Nusselt no. based upon the film thickness, okay, and now it's just very simple correlation, coefficient times what he called a turbulent Reynolds no. to power, Prandtl no. to the one-half.

VICE CHAIR WALLIS: Okay.

MR. KELLY: And it fit the data marvelously. So what's wrong with it? Well, you have to look at what's the definition of the turbulent Reynolds no.? And it's, okay, liquid density, what do you call a turbulent viscosity, film thickness over the liquid viscosity. So far so good. Where does the turbulent velocity scale come from? Move over here.

VICE CHAIR WALLIS: That's effected by interfacial drag.

MR. KELLY: Yes, exactly. He used a friction velocity based upon interfacial drag. It still makes sense. Okay?

VICE CHAIR WALLIS: Yes.

MR. KELLY: With the one caveat that now if you have no interfacial drag, because it's a falling film, this goes to zero. So it still has that defect in it. But boy it still fit his data very well, so why am I not using it? Well, as we talked earlier about interfacial drag --

VICE CHAIR WALLIS: Now, you start with an interfacial drag which depends on the velocity of the vapor, so the Reynolds no. for the liquid actually contains the velocity of the vapor.

MR. KELLY: Right.

VICE CHAIR WALLIS: I find it peculiar.

MR. KELLY: It's worse than that. Whoops, that's a gamma. When we talked about interfacial shear and condensation, that's a sum of two components, an adiabatic term, which we normally call interfacial shear, and a mass transfer term, that's what they did. They used the correlation for this and calculated this one as basically gamma times v rel. Actually, they may have used v gas and then later said they should have used v rel. But then, that's negligible for their test.

So their turbulent velocity scale is

nothing more than the condensation rate. So you've now got your condensation Nusselt no. as a function of your condensation rate.

VICE CHAIR WALLIS: Culminated against itself.

MR. KELLY: That's a good way to get a model that looks pretty good.

VICE CHAIR WALLIS: You're making me very happy on two scores. One is that I stopped working on condensation. The other thing is that I have refused to review papers like this since I joined the ACRS. Because it's extraordinarily difficult to decide whether or not the paper based on this kind of stuff is valid or not.

MR. KELLY: Unless you get in and look at it. You can't do it in a few minutes.

VICE CHAIR WALLIS: No.

MR. KELLY: So --

VICE CHAIR WALLIS: I wonder whether you are giving me confidence or not. You're giving me great confidence in your ability to analyze stuff.

MR. KELLY: Well, then I'm hoping you'll have some confidence in my bottom line.

VICE CHAIR WALLIS: It's just the kind of thing, I mean, you have pointed out something which, you know, you can do if you really dig into it. Gee whiz, they're correlating X against X or whatever, gamma versus against gamma.

MR. KELLY: Yes, I found that more than once.

VICE CHAIR WALLIS: Against itself, no matter what the theory is. And yet, it may well be that this theory is being used in some code to justify a regulatory decision.

MR. KELLY: I don't know of any, but it could be.

VICE CHAIR WALLIS: Right. And the problem then is that someone like an ACRS Member reads all this and says you shouldn't use that, because they call it in gamma against gamma and it's -- then it's very difficult for the Agency to backtrack and say well, we approved this code, therefore, it's okay.

MR. KELLY: Yes.

VICE CHAIR WALLIS: And not pay attention. You should go on.

MR. KELLY: Yes. Okay. So I started with the co-current data because that's the situation I have. But they only had horizontal and I'm, of course, vertical. But the real problem was it's over a fairly limited range of data and that's these blue diamonds, that's what was on the previous slide.

VICE CHAIR WALLIS: Yes.

MR. KELLY: Most of my stuff is down in here for the ESBWR application. So can I trust that line or not? No, of course. Well, so I looked at the other two Bankoff experiments, the above counter-current, one was horizontal, that's the green circles, the yellow triangles are vertical.

VICE CHAIR WALLIS: I would try it all, except the vertical, because vertical is what you have in ESBWR.

MR. KELLY: Yes. And I was amazed that the horizontal and vertical counter-current pretty much overlaid.

VICE CHAIR WALLIS: You have co-current in ESBWR.

MR. KELLY: Yes. And you expect those to be different, but how much?

VICE CHAIR WALLIS: But counter-current tends to make bigger waves, because, you know, trying to hold up the liquid or the vapor.

MR. KELLY: So I would expect higher heat transfer rates. And that was actually one of the surprising things is that these fell away from the horizontal co-current. I expected -- if you had asked me beforehand, I would have said they might be an order of magnitude higher.

VICE CHAIR WALLIS: But also the film tends to get fatter, because it's harder to transport.

MR. KELLY: Exactly. Exactly. And that seems to be what drives it. So I looked at that and I was tempted to put a line through it, you know, and call it a Kelly --

VICE CHAIR WALLIS: Downflow, this has no hope of correlating upflow data. It's a completely different problem.

MR. KELLY: So at any rate, that has a Reynolds no. dependent. But remember, my Nusselt no. here has a film thickness built in to it, and that was measured in the heat test. So that effect is out in the sense of it's an average film thickness, you know, a mean film thickness and, of course, with waves that vary a lot. That's about a Reynolds no. to the 2 power, which none of the correlations are.

And I want to say it's the laminar-turbulent transitions, but that's not right, because this starts at about a Reynolds no. of 10,000 and comes down around 6,000. And actually the laminar-turbulent transition should be over in here. So I still don't have anything, at least not anything I want to put my hat on.

So I went back to the Kuhn test, the pure steam ones. And so they measured the total heat transfer resistance across the film and I simply double it saying that the interfacial is, you know, half of the resistance, so I doubled the Nusselt no., that's why I'm calling this inferred data.

VICE CHAIR WALLIS: That's why it's 2 rather than 1?

MR. KELLY: Right. Because it's from the liquid film temperature to the interface. And the blue line is that Kuhn-Schrock-Peterson Model that I showed you earlier, the curve fit is a better description for it, rather than a model. This is where I am. What am I going to use for a turbulent film? At this point, I don't have a good model.

CHAIRMAN RANSOM: I'm surprised that none of these models have attempted to use the relative velocity in the correlation or to define a Reynolds no. based on the relative velocity. You would think that would be more reasonable.

MR. KELLY: Well, you know, this is kind of that approach.

CHAIRMAN RANSOM: Yes.

MR. KELLY: Where they are going to use a friction velocity on Tal/I, but then it turned out for the conditions of these tests, the mass transfer term was much larger. What you would expect, Vic, is something that's the function of the film Reynolds no., because that's turbulence in the film, multiply it by something, you know, there is like 1+ and then some function of the relative velocity. But I didn't find that.

VICE CHAIR WALLIS: Now, if I were trying to assess ESBWR, it would seem to me that I wouldn't really want to rely on any of these correlations that go through data. I would want to say this thing has to work.

MR. KELLY: Yes.

VICE CHAIR WALLIS: So I want to know some extreme case. Now, the worst it could possibly be is a Reynolds no. which is below all these data. And as long as the system will work for that, I'm pretty assured it will work.

MR. KELLY: Yes.

VICE CHAIR WALLIS: That would make more sense then fiddling around with a whole other correlation that sometimes work and sometimes don't and work for so and so's data and not for somebody else's.

MR. CARUSO: And that's actually what you use.

VICE CHAIR WALLIS: And this is why you need something like the PANTHERS test where they actually measured the real thing. If you can correlate that with no theory at all, it's correlated.

MR. KELLY: Yes.

VICE CHAIR WALLIS: Fit the data. Then that maybe is much more reasonable, because it's a full scale test on the realistic condition.

MR. KELLY: Now, I won't disagree with that.

VICE CHAIR WALLIS: It seems to indicate that we still need full scale tests for reactor licensing for certain phenomena.

MR. SIEBER: I think that's true, but I'm not sure that you can construct a laboratory experiment or a test that will mimic everything you need to know about axial conditions in a reactor. You are bound to go beyond the rate of the data. And so without a first principle's foundation for the correlations that you have, it's not clear to me that you're going to end up in any known condition.

MR. KRESS: Well, you probably can for this sort of thing, commencing in a tube. You can probably make a pretty good simulation.

MR. KELLY: Because it's your diameter.

MR. KRESS: Yes.

MR. KELLY: Right.

MR. KRESS: And that would be too difficult.

MR. KELLY: But if you extrapolate up to a pipe that's a meter in diameter.

MR. KRESS: Oh, then you've got it. Yes.

MR. KELLY: And that's the problem we have. One of the reasons I spent as much time as I did looking at the northwestern data was we had a condensation problem in the code, in the Code Reg, due to the model and basically stratified flow and it was due to the models that were in the code.

VICE CHAIR WALLIS: Yes.

MR. KELLY: And so I wanted to come up with a better estimate of the model and that was part of the reason I started looking at the northwestern data. And I need a model for turbulent film condensation in stratified, in annular film for things like Code Regs and down-comers, not just for PCCS tubes. And so I resisted the urge to draw a line through this. I may, if I don't run a small scale experiment, end up doing that some day.

VICE CHAIR WALLIS: Well, this is just through the Northwestern University data?

MR. KELLY: That's the only data I could find out there with local measurements of interfacial condensation rates, because that's hard to do.

VICE CHAIR WALLIS: But a lot of it is horizontal flow, which isn't necessarily the same thing.

MR. KELLY: Right.

VICE CHAIR WALLIS: It could be the flow tends to be more stratified, presumably.

MR. KELLY: Yes, and I didn't have any vertical co-current.

VICE CHAIR WALLIS: This vertical counter-current flow, what they have is vertical co-current downflow in the PCCS.

MR. KELLY: Right.

VICE CHAIR WALLIS: So none of these data are really for the conditions in the PCCS.

MR. KELLY: Right. But it was what I could find. This is what I chose to use. Again, I already have the Gnielinski Model in the code. We use it for a number of other things. If you multiply by .7 it fits the Bankoff Model at high film Reynolds nos. and what it at least does is fall off, so it doesn't over-predict as badly. And what I found by looking at actual film condensation data, the Kuhn at UCB and in the NASA data, for the Kuhn data you need very low values at the interfacial.

VICE CHAIR WALLIS: Joe, if you use .7 times Gnielinski, you are a factor of 5 or something high on predicting heat transfer coefficient for some of these data.

MR. KELLY: That's right.

VICE CHAIR WALLIS: Well, that's a huge --

MR. KELLY: Yes.

VICE CHAIR WALLIS: And so if they are predicting that all the steam gets condensed using this, it may be true that only 20 percent of it gets condensed. That's a huge effect in something like a PCCS.

MR. KELLY: But there I have the mass transfer to save me, because that's normally the limiting resistance there, not this. Typically, from what I have been able to ascertain, in pure steam condensation about three-quarters of the heat transfer resistance is wall to film. About one-quarter is film to interface. Okay? So that factor of 5 is on that one-quarter.

Then when you add in non-condensibles and the mass transfer becomes the controlling resistance, that one-quarter becomes one-quarter of maybe 10 percent. And that's why I'm not -- I don't like this. I want to do something better. I don't have a database to do anything better, but I'm not going to lose sleep over it, because we're talking about one-quarter of 10 percent and that's way off in the uncertainty bands.

VICE CHAIR WALLIS: So you haven't yet told us how you model the mass transfer resistance.

MR. KELLY: Well, let's go to the next slide. That's the next topic.

MR. FORD: Joe, would you mind just going back one slide just to satisfy me on something? On one of the earlier slides you waved your hand and said that ESBWR was in the order of 1,000 to 3,000 value of the Reynolds no.

MR. KELLY: Well, actually, it's more like 100 to 1,000.

MR. FORD: So if they use the Berkeley data and the correlation, you would be happy?

MR. KELLY: Yes.

MR. FORD: You would pass it? Okay. Even though the TRACE Code uses --

MR. KELLY: As long as you demonstrate

that. You do your system calculations. Okay? You back out from the system calculations what the conditions in the PCCS were when it was important.

MR. FORD: Okay.

MR. KELLY: Now, what were the gas mixture Reynolds nos., what were the non-condensible concentrations and what were the film Reynolds nos.? Then you compare that to this database.

MR. FORD: Okay.

MR. KELLY: And if this database encompasses it, you're home free.

MR. FORD: And so you really wouldn't use TRACE at all for this particular -- because as I understand it, TRACE is based on that dotted line. Is that correct?

MR. KELLY: No.

MR. FORD: Okay.

MR. KELLY: This is the laminar model that I put into TRACE.

MR. FORD: Oh.

MR. KELLY: And this is the turbulent, so that I cover the full spectrum.

MR. FORD: Okay. Okay.

MR. KELLY: Plus in TRACE all of this depends upon the calculated liquid film thickness and that brings in wall drag and interfacial shear.

MR. FORD: Thank you.

MR. KELLY: And then we go to mass transfer, which is actually the controlling resistance most of the time for the non-condensible concentration, you know, if there is much non-condensible at all.

So the approach is to use a mechanistic model similar to the mass transfer conductance model, which was recommended in Kuhn's thesis. Now, he came up with an empirical correlation, but he said you can do a better job than the empirical correlation by going to a mass transfer conductance model, and so that's where I started.

VICE CHAIR WALLIS: Well, Bird, Stewart and Lightfoot has a whole couple of chapters on --

MR. KELLY: Right.

VICE CHAIR WALLIS: -- simultaneous heat and mass transfer and that sort of thing.

MR. KELLY: And that's, basically, what you're going to see here and an older version would be the Colburn-Haugen. It's the same kind of thing. So you have a heat flux from the liquid to the interface. The difference here is I'm not treating liquid to wall. I'm just going liquid to interface and saying that has to equal what's on the other side, the mass transfer part.

So on the gas mixture to interface, it has two components, condensation and sensible heat. I modeled the sensible heat contribution, but it's very small. Condensation heat flux is simply the condensation mass transfer times the latent heat, and what you have to do is set these two equal and that becomes an iterative process, because you have to find what the interface temperature is and that's really what the interface concentration of the non-condensible is.

Now, it turns out you can do that in about three to four iterations if you put together an intelligent scheme. Here's the condensation mass flux, very simply, gas mixture density. This is diffusivity, tube diameter. Beta is this rolling factor, Sherwood no. I have the ratio of 2 molecular weights, the molecular weight of the mixture at the interface and the molecular weight of the mixture in the bulk.

VICE CHAIR WALLIS: How do you know $x_{v,i}$?

MR. KELLY: That's one of the things you have to solve for by --

VICE CHAIR WALLIS: You have to solve for?

MR. KELLY: Yes.

VICE CHAIR WALLIS: It seems to play a role in everything.

MR. KELLY: Yes.

VICE CHAIR WALLIS: So you have to iterate. You have to calculate and then go back and put it in all the equations again?

MR. KELLY: Right, this right here. It takes about three iterations to converge, sometimes four. This is to account for variable properties between the bulk and the interface and it's a property ratio scheme that I pulled out of Kays and Crawford. And then b is the mass transfer driving potential written in terms of the weight fractions.

VICE CHAIR WALLIS: This beta is what you might call a polarization at the interface when you build up the non-condensibles at the interface, and the concentration at the interface can be 10 or 100 times what it is in the main flow, because you're streaming it to the interface and it has to diffuse back again.

MR. KELLY: Yes, right. That's what all this is.

VICE CHAIR WALLIS: It all depends on --

MR. KELLY: Yes, it's just diffusion away from the interface.

VICE CHAIR WALLIS: Diffusion controlled.

MR. KELLY: Yes. And so you see what I used for the Sherwood no., something that would be appropriate for single phase flow in it, too.

VICE CHAIR WALLIS: Well, to calculate $x_{v,i}$, how do you calculate $x_{v,i}$ again?

MR. KELLY: Well --

VICE CHAIR WALLIS: The $x_{v,i}$ is a concentration at the interface.

MR. KELLY: That's correct.

VICE CHAIR WALLIS: Which can be very much more than it is in the main flow?

MR. KELLY: Yes. I go through, I evaluate.

VICE CHAIR WALLIS: How does this get to $x_{v,i}$?

MR. KELLY: I'm getting there. I calculate the interfacial heat transfer coefficient. Okay? I make an estimate of the interface temperature. This is held constant, because this isn't a function of it. So I take a guess for the interface temperature. I go through and evaluate these two contributions by the equations on the next slide, again, for that assumed interface temperature, which correlates to an interfacial concentration. Look at this.

VICE CHAIR WALLIS: Because of a partial pressure of --

MR. KELLY: Exactly, of the vapor, exactly. If this is less than I think it's 100 th of 1 percent difference, I'm converged. Otherwise, I adjust the guess for the interface temperature concentration until it converges.

Sensible heat, I'm not going to waste any time on. It's negligibly small. How well does it do? Well, this is what you saw before. This is against the air-steam data of Kuhn. It looks very good. This is data from 72 different tests.

VICE CHAIR WALLIS: This fog factor was what I was laughing about.

MR. KELLY: Right, it's a fog factor.

VICE CHAIR WALLIS: It's a fog factor of 2?

MR. KELLY: That value was recommended, I think, in Kuhn's thesis.

VICE CHAIR WALLIS: Because he just correlated the data better and multiplied by 2?

MR. KELLY: You see other ones where it's a factor of 6, I mean, a value of 6. It doesn't make any difference. It's times a very small number. But I agree, that is something good to laugh at. So there are 571 data points.

VICE CHAIR WALLIS: Well, it's the sensible. You don't need to worry about the sensible.

MR. KELLY: Right. The average error was 7.7 percent. The RMS, 16.1, is actually better than the empirical model.

VICE CHAIR WALLIS: That you showed us before.

MR. KELLY: That came from this, right. And there's the helium, same thing.

VICE CHAIR WALLIS: So everything is done by the non-condensibles?

MR. KELLY: For this case unless you're at very high gas mixture Reynolds nos.

VICE CHAIR WALLIS: Is that true of ESBWR?

MR. KELLY: It depends on when.

VICE CHAIR WALLIS: Oh.

MR. KELLY: If there's a phase of a transient where the gas mixture Reynolds no. is very high, we're talking tens of thousands and the gas concentration is on the order of a percent or so, then the mass transfer is not the controlling resistance. But if you go 10, 20, 40 percent of non-condensible --

VICE CHAIR WALLIS: Oh, then it's bound to be.

MR. KELLY: Right. So we're finished with all the theoretical stuff. How well does it work when you shove this model into TRACE? This is a test matrix that I did. Again, I worked with three different types of test, a laminar film, I used the Kuhn test for that, a turbulent film, the NASA test, and the non-condensible gas effect, I looked at the air-steam test of Kuhn again.

For the laminar film, I did a parametric. On pressure, from 1 to 5 bar. For the turbulent film, I just ran two tests and I ran those, because that's basically history. They were part of what RELAP5 was assessed against. For the air-steam, I did a parametric on non-condensible gas mass fraction from 1 to 40 percent at the inlet.

So pure steam, laminar film, this is what we had before. I have got the calculated heat transfer coefficient versus the measured perfect agreement. Most of the TRACE calculation dramatically under-predicts and the prediction gets worse as you go to higher pressure.

VICE CHAIR WALLIS: That's what TRAC would do.

MR. KELLY: That's what it would do today without the PCCS Model. With the PCCS Model, you get this, a very good agreement where I over-predict. That's because those film Reynolds nos. were, basically, at the end of the tube and they were in the laminar-turbulent region and my turning on of the turbulent bumped it up too high.

CHAIRMAN RANSOM: Were these tests with TRACE just take a tube and put boundary conditions at the entrance and exit and --

MR. KELLY: And on the secondary side, I used the measured wall temperature as the boundary condition rather than trying to model the secondary, because that gives you then the uncertainty of the convective heat transfer on the secondary side. So I tried to make it so that what we were looking at is the model that we're assessing.

This is heat transfer coefficient versus distance, the old calculation. This is a test at 3 atmospheres, data and TRACE, and this is the new calculation, which is almost better than is creditable. It also gives you a much more realistic calculation of the liquid film thickness. This was the old calculated value. This is the new calculated value. The red curve is what you get if you assume it's a pure falling film, no interfacial drag. So as I said earlier, the amount of interfacial drag is fairly modest in these tests.

Turbulent film. What I have shown is the data, the old calculation and the new calculation. This is for Run 172. The new under-predicts when a film is laminar. There is an inconsistency between the NASA data and the UCB-Kuhn data. This data is higher for the same kind of conditions, and I chose to go with the Kuhn data, because well, first off, I think the quality of the data is higher, but it also covers our range of applicability better.

VICE CHAIR WALLIS: And the NASA has a much higher velocity, so there's more likely to happen, entrainment and other things that may be not well-modeled by Kuhn.

MR. KELLY: Right. And then the same thing here. The one difference here is the old TRACE Model, it's in a more recent version of TRACE where the wall drag had been changed. And you'll notice the calculation got a lot worse, because it switched it between two different condensation models.

VICE CHAIR WALLIS: Then we should say that RELAP and TRAC would do as badly as that, these approved codes from the past, because this is where

TRACE came from.

MR. KELLY: From what I know when we looked at RELAP5 for the AP-600 and for the ESBWR, we're talking about 10 years ago, RELAP5 would do better than this.

VICE CHAIR WALLIS: Okay. So TRAC was bad.

MR. KELLY: Because RELAP5 was assessed against condensation data more recently, because we already went through some of this exercise with RELAP5 and RELAP5 has several different models in it, at least two, for the non-condensible effect. One of those is the Vierow-Schrock correlation. So if you turn that option on, use with caution.

Non-condensible gas effect, I ran five different experiments going from a non-condensible mass fraction, from 1 percent to 40 percent at the inlet, and as far as I'm concerned that's excellent, excellent agreement.

VICE CHAIR WALLIS: You're modeling Kuhn. Well, all along the way you have had Bankoff and all that stuff. If you would go back and model all that, do you presume you don't do so well?

MR. KELLY: I'm sure I wouldn't. For one thing, the Bankoff is not governed by mass transfer.

That is what I feel like I'm modeling most accurately here, is the mass transfer and that is why that looks so good.

VICE CHAIR WALLIS: So we're lucky that there are so many non-condensibles in the ESBWR?

MR. KELLY: If you want to impede condensation, that helps. So the summary is pretty simple. Develop the model. It's applicable to the conditions of the PCCS. It's within a two-fluid framework and I take advantage of the things that TRACE calculates like the liquid film thickness.

The accuracy is pretty much as good as the data. To make it better, we would have to go and do some more modern condensation tests, which maybe for something like reflux condensation in a steam generator tube we need to do at some point.

VICE CHAIR WALLIS: Well, PANTHERS was a full scale test of the ESBWR, PCCS.

MR. KELLY: Right.

VICE CHAIR WALLIS: Over the range of flow rates and non-condensible and all that expected.

MR. KELLY: And that's --

VICE CHAIR WALLIS: That's a real test? Do you have to know?

MR. KELLY: Yes, and next time you will

have Bill Krotiuk up here.

VICE CHAIR WALLIS: Will I still be on the ACRS when you talk about that?

MR. KELLY: I hope so.

VICE CHAIR WALLIS: Well, I'm not sure. I mean, this is taking so long.

MR. KELLY: Well, the model is in the code now and Bill is starting to test it.

VICE CHAIR WALLIS: The real proof of the pudding in terms of applicability would seem to be what you haven't done yet, which is to apply it to PANTHERS.

MR. KELLY: That's true.

VICE CHAIR WALLIS: Is this going to be published in the open literature?

MR. KELLY: I would --

VICE CHAIR WALLIS: It seems the kind of thing that ought to be there.

MR. KELLY: Well, I am going to do the code documentation, although that's going to take a small lag in time. I should be doing it now, but I'm switching my effort to work on 50.46 now, so that's going to make this code documentation on this lag in time some. So for the moment, this presentation is the documentation, which isn't acceptable.

Now, as far as turning this into a journal paper, it's not breaking any tremendous new ground. If you go and look at some of the more recent papers on condensation with non-condensibles, they use mass transfer conductance models similar to this and get similar kinds of results.

VICE CHAIR WALLIS: Well, I think that's okay, but I was more concerned about the other part, the part where you were modeling the pure steam where you get all sorts of different results depending on how you look at the data and whose correlation. That seemed to me was very useful or would be very useful for the technical community to know about. And the fact that non-condensibles govern through your application is a kind of lucky thing in a way. You can thrown away all this other stuff you don't know much about, because the heat transfer resistance is so small for those parts of the problem.

MR. KELLY: Right.

VICE CHAIR WALLIS: And just concentrate on the non-condensibles.

MR. KELLY: Right. But of course, that gets us into trouble in things like large break LOCA for condensation in the cold leg or in the down-comer.

VICE CHAIR WALLIS: Yes.

MR. KELLY: And that's exactly some of the things I have to look at for 50.46 right now for condensation-induced oscillations.

VICE CHAIR WALLIS: Well, condensation, when you're squirting in ECC water and do some weird geometry that has never been really tested, it cannot be assessed with any of these correlations very well.

MR. KELLY: Right.

VICE CHAIR WALLIS: Especially if you get oscillations in there.

MR. KELLY: Well, I'm talking about the code having oscillations that are much larger.

VICE CHAIR WALLIS: Oh, so physical oscillations.

MR. KELLY: Yes, there are physical ones. So one of the things we will be doing is the Northwestern University test will be added to the TRACE assessment matrix. I haven't done them yet, but they will be done. There is a bit of a trick to them, because they are a rectangular channel and we have pipes, but you can either modify the pipe to be rectangular or fudge it by having --

VICE CHAIR WALLIS: The thing with 50.46, 50.46 would seem to be just picking a good pipe size.

MR. KELLY: Well, we have been tasked with making sure that TRACE can give realistic predictions for that to help give some guidance in that, and actually I'm just now becoming involved in that, so I can't answer those questions very intelligently. Either Joe Staudenmeier or Steve Bajorek would be able to do a much better job.

VICE CHAIR WALLIS: We're going to see you in that context.

CHAIRMAN RANSOM: Joe, I have one question on your model with regard to the Bankoff experiments.

MR. KELLY: Yes.

CHAIRMAN RANSOM: Some of those use super heated steam and in early simulations of that we found that if you didn't account for, you know, the heat transfer to bring the steam down to the saturation point at the interface or then, of course, the energy balance predicted the steam to heat up, which was unphysical.

And I'm wondering is your sensible heat term accounting for the heat transfer that's necessary to bring the super heated steam down to the saturation temperature at the interface and then allow mass transfer to take place by the model that you presented?

MR. KELLY: Okay. I haven't simulated

those tests in TRACE yet, so I don't know how it will work in TRACE. I only know how I did the data analysis. Now, my --

CHAIRMAN RANSOM: Haven't you had to modify the sensible or the --

MR. KELLY: Where I think you're going with this --

CHAIRMAN RANSOM: -- base change energy to account for the super heat in the steam?

MR. KELLY: Right. That's a point I glossed over. Where that really is is here.

CHAIRMAN RANSOM: Right.

MR. KELLY: Right. And as far as I know, the code does that right. It uses a donor value.

CHAIRMAN RANSOM: The code does? So it accounts for that more or less implicitly, you might say?

MR. KELLY: Right. Because if you don't, you get exactly that kind of behavior.

CHAIRMAN RANSOM: Right.

MR. KELLY: In sub-cool boiling, the liquid freezes.

CHAIRMAN RANSOM: Yes.

MR. MAHAFFY: This is John Mahaffy. Let me comment on that, Vic, since I know the guts of it.

What's done in TRACE is exactly what you were used to doing in RELAP5 in respect to the sensible heat.

CHAIRMAN RANSOM: I wasn't sure, because it seemed like he was proposing that the mass transfer would be simply based on the HFG.

MR. MAHAFFY: That's what shows on his view graph, but if you dig deeply into what the code is doing, in fact, it's exactly what you're used to seeing in RELAP.

MR. KELLY: Right. And I just didn't want to go off on that tangent. I finished the presentation on time. I would like that to be on the record.

VICE CHAIR WALLIS: Well, you did very well.

MR. SIEBER: Yes.

VICE CHAIR WALLIS: Let's go back to the discussion we had at the beginning with we have got three to five equivalent FTEs working on this problem. I mean, you have done a very substantial job here and it's impressive, but it obviously took a lot of work and this is just one sub-problem associated with TRACE.

MR. KELLY: Yes.

VICE CHAIR WALLIS: Now, there is a

problem with reflood, which is more complicated than this problem.

MR. KELLY: Yes.

VICE CHAIR WALLIS: And that is going to take somebody a year or two to sort out, it seems to me. There are probably other problems associated with the constituents of TRACE, ECC injection or down-comers or something are things where you don't have very good models.

MR. KELLY: In the last year, I told you that I did this development work basically a year ago.

VICE CHAIR WALLIS: Yes.

MR. KELLY: But it was only recently we put it in.

VICE CHAIR WALLIS: Right.

MR. KELLY: But in that last year, some of the other things that we have done, we have modified the two phase wall drag to get rid of this. We modified interfacial drag, both for rod bundles and for tubes, and this is all something I can show you next time. I just didn't have time to prepare it for this meeting.

VICE CHAIR WALLIS: That's too bad. We would have been happy to be here for two or three days to hear it all.

MR. KELLY: No, but then that would be two or three more weeks of my time.

VICE CHAIR WALLIS: Well, probably good for you to pull it together.

MR. KELLY: Right, and we will. And so Joe will at least mention those when he gives his presentation, and we're just going to knock off those things one by one. And I will be going to 50.46 now. I will be looking at reflood heat transfer. I will be looking at steam binding caused by heat transfer in the steam generators. I will be looking at blowdown heat transfer and condensation in the cold leg and the down-comer and, hopefully, won't have to do this kind of work on most of them. I have Wei Dong Wang helping me.

VICE CHAIR WALLIS: You're using this for the problem of the -- well, now, how does it come about whether you simply have a core heating up and the steam generator is uncooled and you have natural convection between them with pure steam, and the question is does the steam generator or the hot leg or something else pop first? Are you doing that problem?

MR. KELLY: No, that's off in severe accident space and I'm not worried about that yet.

VICE CHAIR WALLIS: How about the sub-cool boiling stuff that Vijay Dir is doing, has been doing for sometime, is that being incorporated into TRACE?

MR. KELLY: It has not yet. It's on my to-do list.

VICE CHAIR WALLIS: So what I'm saying is that you gave us a good presentation, obviously a great deal of work. I think there's a whole lot of other sub-problems like this, which need similar amounts of work.

MR. KELLY: Yes.

VICE CHAIR WALLIS: And I'm concerned about it all getting done in a finite time before you retire or whatever.

MR. KELLY: Yes. Well, that's 10 to 15 years.

VICE CHAIR WALLIS: That's why I say, a message for your management and if you keep working all these sub-problems with the intensity that you have worked this one, which is probably very appropriate, it's going to take an awful long time before they are all done, and we may be permanently frustrated with TRACE sort of not really being complete yet.

MR. KELLY: But what you should see is over time a trend for the code to get better, and that's what we're aiming to do.

VICE CHAIR WALLIS: Yes, well, this is also true of a baby.

MR. KELLY: That's true.

VICE CHAIR WALLIS: But it's a long time before it goes to college and graduates and stands on its own two feet and earns a salary.

MR. KELLY: And as I just recently learned, and then they can still come back and live with you.

VICE CHAIR WALLIS: Okay. Well, you're doing a good job here.

MR. KELLY: Well, thank you.

VICE CHAIR WALLIS: I just hope the management understands how much of this sort of thing needs to be done.

MR. KELLY: And it's my experience that if you don't do it, maybe you don't have to do it in quite this step. Everything doesn't have to be a Ph.D defense, but if you don't do it in something similar to this, you miss things like that. And you put a model in the code and then you wonder why it doesn't work, but it works fine for awhile and then someone tries it with something just slightly different and it falls apart.

VICE CHAIR WALLIS: And you have had the time to do it, and one problem is that the NRC will contract a university to do a job like fix up the condensation heat transfer codes. And they give them a contract and the university says oh, one graduate student can do that in two or three years and they get a contract, and then there is pressure to get the job done and, obviously, sometimes there isn't time to go into all this stuff that you have been doing. So something comes up, which is half-baked and it becomes a NUREG or something and it's accepted.

MR. KELLY: That's true.

VICE CHAIR WALLIS: And you have been lucky enough, you can stick with this.

MR. KELLY: I have been very lucky to have the support of my management over the last couple of years to do this kind of work, and what I'm trying to do is bring some of the younger staff along as proteges and have them try to think some of the same things, as well as they learn the code by they do some of the installation and testing for me. I have both Wei Dong Wang and, in the future, Shawn Marshall will be doing the steam generator heat transfer film.

CHAIRMAN RANSOM: Well, thank you very much, Joe. It was a good presentation and, like

Graham has said, there are quite a few areas that need this kind of treatment. But why don't we take a break until after a quarter past and Joe maybe will tell us a little bit more of the status of some of the other areas.

MR. KELLY: Okay.

(Whereupon, at 11:00 a.m. a recess until 11:16 a.m.)

CHAIRMAN RANSOM: We're back on the record and now the plan is to hear from Joe Staudenmeier about some of the future plans for code development on TRACE.

MR. STAUDENMEIER: Okay. Can you pick that up? Can you hear?

COURT REPORTER: Yes.

MR. STAUDENMEIER: Is it better?

COURT REPORTER: It's better if you're closer to the mike.

MR. STAUDENMEIER: Thank you. Yes, I have it on. Maybe I don't know --

UNIDENTIFIED SPEAKER: Put the switch on.

UNIDENTIFIED SPEAKER: Put it on your tie.

MR. STAUDENMEIER: No, the power is off. That's why. Okay. Is that better?

MR. SIEBER: Oh, yes.

MR. STAUDENMEIER: Okay. Okay. I want to give an overview of our recent work, I guess, since the last meeting in our systems code development and our future plans with our codes.

Our four codes we have development going on right now are RELAP5, PARCS, SNAP and TRACE. RELAP5, it's at a low status in terms of development. We maintain it, fix bugs. It's still used quite a bit at the NRC. It has been used recently for PTS and boron dilution. It has been used to do some preliminary calculations in ACR-700 and also, it's going to be used in some risk informing ECCS calculations for break size redefinition. NRR is planning on using it.

VICE CHAIR WALLIS: I thought TRACE was going to replace RELAP5.

MR. STAUDENMEIER: It is.

VICE CHAIR WALLIS: You have already given up the old TRACs and the various TRAC-P and TRAC-M and all that is gone.

MR. STAUDENMEIER: I mean, they still exist, but nobody uses them. TRACE does every -- anything that anybody would have done with those codes, a person would use TRACE now to do that.

VICE CHAIR WALLIS: Okay.

CHAIRMAN RANSOM: Is this effort on RELAP5 still one man at ISL that you're supporting?

MR. STAUDENMEIER: Yes, it's one man at ISL. I mean, if camp members submit code changes or fixes, we consider them, putting them into the code.

VICE CHAIR WALLIS: What's the name of the person at ISL?

MR. STAUDENMEIER: Glen Mortensen. There's actually a couple of people that contribute. I mean, it's a little bit beyond Glen, and sometimes Doug Barber and Rex Shumway also make some contributions.

VICE CHAIR WALLIS: Is Shumway still involved?

MR. STAUDENMEIER: He is involved in TRACE and RELAP until the end of the month. He is retiring at the end of this month. Okay. PARCS, some recent stuff we have worked on for PARCS is eliminate the need for PVM in coupling the TRACE and there is also a camp member --

VICE CHAIR WALLIS: What is PVM?

MR. STAUDENMEIER: PVM is some software that lets the two codes run and talk to each other without being directly linked together, so it's a software technology. Eliminating PVM makes it easier in terms of things the user has to worry about in terms of installation and it will also make things easier in the future in doing some more implicit coupling between TRACE and PARCS.

VICE CHAIR WALLIS: Now, you just couple them directly, do you?

MR. STAUDENMEIER: Right. It will be like a direct sub-routine call instead of this socket-based communications interface. And RELAP5, there is also a camp country that has done the same for RELAP5 and they are going to contribute removal of PVM for the RELAP5/PARCS coupling also.

CHAIRMAN RANSOM: That's with PARCS and RELAP5 coupled?

MR. STAUDENMEIER: Right. And currently, with PARCS we're looking at developing a BWR Stability Methodology and we have been assessing and running tests against some Ringhals data, some Ringhals stability data. Tom Downar will show you a little bit more of that in his presentation later on. We have updated the documentation and we're going to be developing a VEDA runtime interface for PARCS, so that it can be available on all platforms.

Right now, the graphical interface for PARCS only works on Windows. VEDA is part of SNAP.

It's the visualization and post-processing part of SNAP. It replaces what used to be the NPA and we'll be developing an interface in that to control the code at runtime and give a common interface on any platform you want to run on.

SNAP, the main development activity with respect to TRACE and SNAP is improving the ease of use and functionality. We have, essentially, all the functionality or the completeness of things in terms of TRACE and SNAP, and we're working out bugs and improving functionality and next we'll be layering these engineering templates on top of SNAP sometime in the future. And also, we have integrated VEDA into the Model Editor. You will get to see a little more of what that is later.

VICE CHAIR WALLIS: How important is SNAP to TRACE? Can you run TRACE perfectly well without SNAP?

MR. STAUDENMEIER: Yes, TRACE can be run independently of SNAP. What SNAP gives you is model preparation and editing and makes that easier and makes post-processing easier. But yes, TRACE is perfectly functional without SNAP.

The vision in the future is that SNAP was going to be the input processor for TRACE and sometime in the future, the part of TRACE that reads ascii input decks and starts up calculations, that would be moved out of TRACE and it would just be a calculational engine and SNAP would dump this new sort of file format that TRACE would pick up, so it would be more difficult to use in the future or our ultimate vision of using TRACE and SNAP together.

CHAIRMAN RANSOM: Does SNAP also do the plotting of the output data?

MR. STAUDENMEIER: SNAP will plot the output data, yes. There's a tool called XMGR that does the line plots and there's VEDA, which is the NPA-like replacement that's in SNAP for visualization of the whole model and animation of a model.

CHAIRMAN RANSOM: Who's doing the SNAP work?

MR. STAUDENMEIER: Ken Jones.

CHAIRMAN RANSOM: He's an independent contractor. Is that right?

MR. STAUDENMEIER: He's an independent contractor. He has his own company. At one time, he used to work for Scientech and before that, he used to work for INEL. He did work on NPA back at INEL.

CHAIRMAN RANSOM: Right.

MR. STAUDENMEIER: And also RELAP

calculations. He was the one that developed XMGR capability to read RELAP5 graphics files, and so he has worked on NRC projects for a long time.

CHAIRMAN RANSOM: What is that effort, like one FTE pretty much continuously?

MR. STAUDENMEIER: He has two people that work for him not full-time on TRACE. The funding for the year is a little bit more than one FTE, but it's not all TRACE. I mean, now we have built contain plug-ins. We're building MELCOR plug-ins. There is a RELAP5 plug-in that has to be maintained, FRAPTRAN and FRAPCOM, there has been a plug-in developed for that. So the TRACE part, since the TRACE plug-in has been finished, the level on that is a lot less than an FTE. Chester Gingrich could answer those questions a little bit better when he gives his SNAP presentation.

Yes, what I meant about VEDA being integrated into SNAP, you can now take your input model. Here this is an input model constructed with SNAP and there is an automated way to flip that over into an NPA mass that you can automate or animate with very little effort by the user.

The user tells it what variable he wants to animate it with and give it a color range, so the user no longer has to construct NPA mass like they did before. You can still construct your own custom working mass, but it has taken a lot of work out of the hands of the user, so it does a lot of the work for you.

CHAIRMAN RANSOM: Will SNAP now convert a RELAP5 deck to a TRACE deck?

MR. STAUDENMEIER: Simple decks it does convert. I mean, we're up in a level of complexity of typical PWR, so small plant models. The big missing thing right now in conversion is control systems and signal variables. And we haven't done a lot of work on that lately.

The people that were working on that, they got shut off and were moved over to assessment, 50.46 related assessment, and we have a low level of effort going on on continuing the conversion process at ISL. I expect that maybe sometime around the end of the year, we'll be able to convert most models.

CHAIRMAN RANSOM: Does TRACE have full control system and trip simulation capability?

MR. STAUDENMEIER: Yes, TRACE has full control system and trip, so it's just a matter of mapping or, I mean, there are some things that don't mesh up exactly and, in those cases, you have the choice of either trying to do a translation into the existing things or add functionality in TRACE to make it directly aligned and depending on the feature, both approaches have been taken.

Okay, TRACE. Currently, our development and assessment is driven by these following things. ESBWR is a big application project that is coming up. You saw Joe Kelly's presentation. Most of that work was done in response to needs for ESBWR. We're supposed to deliver a code for ESBWR by June, which is timed with the GE application for the ESBWR and right now, in terms of that, his condensation model has been implemented. It's starting to undergo testing now.

ACR-700. There's a fair amount of effort now adding things into TRACE to form the foundation for ACR-700 calculations. There has already been preliminary calculations done with the existing TRACE and it gets reasonable results of the plan and the test facility, the only test facility data that we have, which is one test on an integral test facility.

MR. SIEBER: Since Dominion and AECL have loosened their intentions, does that affect the work on ACR-700?

MR. STAUDENMEIER: It hasn't yet, but it probably will. I anticipate it will. We haven't received direction to shut down the work yet or that the preapplication phase is ending or been re-prioritized, but I expect that that may happen sometime in the future. So until that happens, we have --

MR. SIEBER: It's ongoing.

MR. STAUDENMEIER: Yes, we have dates, target dates that we're trying to hit with the ACR-700 functionality and we'll move ahead on that until they tell us to stop.

MR. SIEBER: Yes, sort of like the bunny with the cymbals.

MR. STAUDENMEIER: Yes.

MR. SIEBER: Keep on going until your battery runs out.

MR. FORD: You mentioned that the TRACE development for the ESBWR, I think, you said was going to be done by June. What is the completion date for the ACR-700? As I understand it, there are some significant technical problems to be overcome with that different geometry reactor?

MR. STAUDENMEIER: Yes, there are a lot of problems with ACR-700. I mean, our first target for TRACE is to implement our base ACR functionality, which can handle the geometry of the ACR-700 reactor, which is horizontal rod bundles and it will allow looking at stratified flow in a rod bundle and allowing some rod bundles, rods, to be uncovered and some to be covered and calculating radiation between various rods and that geometry and the way the water level is and other things, the header tank with all the pipes coming off, implementing a more general capability that will allow better offtake models to be implemented in a header tank, basically what the void fraction offtake pipe is seeing based upon the level in that tank.

But that's just scratching the surface of what you do. That puts in the infrastructure in the code to support other things like you would need two phase flow models for horizontal rod bundles. We don't have that in the code right now, so someone has to find the data to support development of a model or models in the literature to stick in the code to work with the infrastructure.

MR. SIEBER: Is there data for horizontal flow?

MR. STAUDENMEIER: There is some data. I don't know how good it is or extensive it is. Joe Kelly, he has gone already, but he has looked at some of the literature that has been made available by AECL and is in the process of reviewing it to see what would be needed or if there's more data out there or if more experiments would have to be run.

MR. FORD: Now, the idea is that, obviously, AECL have got their own thermal-hydraulics code they qualified against other data of their own. Are you going to have the same problem of not having enough data to qualify TRACE?

MR. STAUDENMEIER: Right now, there is a large lack of data to qualify a code for ACR-700 from what we have seen. As I said, there is only one integral test that has been made available to us by AECL. They are supposedly running some more integral tests right now, but I'm not in charge of reviewing the data and seeing how extensive it is or if it's adequate for developing models and assessing the code.

MR. FORD: It sounds like it could be the achilles heel of this whole development.

MR. STAUDENMEIER: It could be. The timing, getting adequate data is certainly a concern of not only the basic data to put in basic two phase flow models, but integral data to do assessment, I guess.

MR. BAJOREK: Joe, this is Steve Bajorek from research. One of the projects that we have ongoing and just getting the basic mechanisms into

TRACE is a review of AECL's experimental facilities. As part of that they would be making available to us their experimental data from the Quip Facility where they have run and developed their own models for horizontal flow in rod bundles.

One of the things that we would be doing is obtaining that data, data from the other integral facility that they have run, the RD-14, developing our own horizontal flow pattern maps from those.

MR. FORD: Thank you.

MR. STAUDENMEIER: Yes, the one other achilles heel with ACR-700 is by AECL's own calculations, the design can't meet 50.46 requirements as it currently stands without changing the rule or an exemption to the rule. So if there's LOCAs that where you, essentially, melt the fuel bundle, so it's an isolated chance. Well, we don't know how isolated it is, but it will, essentially, melt and it gets fuel damage in at least one of the channels of some LOCAs.

VICE CHAIR WALLIS: Isn't there also a question about positive volume coefficient or something like that?

MR. STAUDENMEIER: That's another question. Yes, there's lots of questions with ACR-700.

CHAIRMAN RANSOM: Joe, is the PUMA data being used to verify TRACE for ESBWR?

MR. STAUDENMEIER: Yes, the PUMA data is one of the data sets that's going to be used for assessment for ESBWR. Also, we'll use the GE PANTHERS data. That won't be an open publication. That will be a proprietary publication, maybe some of the other GE data. Essentially, it won't be ready at the time of code delivery, but by the end of the project there will be a report that will be called something like applicability of TRACE to ESBWR accident calculations or something like that that will form the basis of why TRACE is good for doing ESBWR analysis.

PWR LOCA break size redefinition. We have recently started up, essentially, a crash effort to get ourselves into that process to help provide feedback to the Commission in the rule making for break size redefinition. We're going to be using TRACE and the first calculations are going to be with a 4-Loop Westinghouse 34111 megawatt design operated in power to the current level.

And the first calculations will be small, the intermediate break LOCA type of calculations, so it will be from the limiting small break up until the transition break size and looking at what are the benefits and consequences of where you define your transition break size, which is --

VICE CHAIR WALLIS: These are all realistic calculations?

MR. STAUDENMEIER: Ours will be realistic calculations to the extent possible.

VICE CHAIR WALLIS: You're going to put in the uncertainty then, are you?

MR. STAUDENMEIER: I don't know how we'll deal with uncertainty right now. Right now we're going to be running base level calculations, and we may have some uncertainty multipliers based on break size, critical flow models like discharge coefficients or things like that. But in terms of overall uncertainty, I don't know the strategy for dealing with that.

VICE CHAIR WALLIS: You don't have the capability that some of the industrial people have to just feed in the uncertainties and run 57 or 59 or whatever the number is?

MR. STAUDENMEIER: No, we don't. Actually, there are some things built into TRACE to put multipliers on heat transfer and drag coefficients and they could be used. I think we may need some more extensive support than that. And actually, SNAP can be set up to spawn off a bunch of calculations.

You can define parameters in SNAP and tell it to do 10 or 20 calculations with this parameter ranged from here to here, and so SNAP and TRACE could be run in that sort of mode with the limited capability that's built in to modify things like heat transfer coefficients or wall drag or interfacial drag right now. Whether it's the full functionality you need, that's yet to be seen.

VICE CHAIR WALLIS: I mean, really, Joe Kelly ought to be producing not just a correlation, but he ought to have sort of a statistical distribution for the coefficient in the correlation, so that it can go right into an uncertainty analysis.

MR. STAUDENMEIER: Yes, that actually would be the harder part of the uncertainty analysis. It's relatively straightforward to build in these multipliers on heat transfer or drag correlations, but to actually go through and compare the correlations you have to have data and come up with an uncertainty range, that's an awful lot of work to do that.

VICE CHAIR WALLIS: But that is what's required.

MR. STAUDENMEIER: That is what's required.

VICE CHAIR WALLIS: The law, you know, the Part 50.46, they are going to use realistic calculations. You have got to model the uncertainty. It's required.

MR. SIEBER: You have to.

VICE CHAIR WALLIS: What about the reflood work? We heard about that several times in the past. It seemed to be an important part of TRACE development and it would seem to be very important to make use of all those results that Larry Hochreiter produced before his project stopped.

MR. STAUDENMEIER: Yes.

VICE CHAIR WALLIS: Is that happening or not happening at all?

MR. STAUDENMEIER: Well, we're doing some assessment against the reflood tests. In terms of using the detailed data to develop a new model, that's planned. It hasn't been scheduled yet. Originally, we had planned on Joe Kelly starting to look at that later in the year and starting to develop a new model that would take advantage of the droplet field that's going into TRACE, but with the change in priority to 50.46 in supporting the current calculations and, essentially, debugging and assessing current models in the code, that probably won't start this year I don't think. It will probably be pushed off until at least next year.

VICE CHAIR WALLIS: Because one of the things that has concerned us all along is that this fellow is doing very detailed tests and it's hoped that sometime in the future someone will manage to use that data and put it into TRACE, because really the tests would be much more effective if they were coordinated with this effort to put things into TRACE, so that as this stuff got put into TRACE the analysts could come back and say well, you know, there's a big gap in the data over this range or there is something weird here and you need to investigate it some more. Otherwise, you're just going to cut off the data stream and the analysts are going to have much more difficulty in making sense of it.

MR. STAUDENMEIER: Yes. Ideally, that's what you would want to do and that's what Joe had planned to do. He wanted to start developing the model before all the tests were still running, but that probably won't happen now based on our current priorities, so I don't know how to solve that problem, but it is a possible problem.

MR. MAHAFFY: Joe, John Mahaffy. One thing I don't know if you're aware of, remember that

Joe Kelly was working at two levels. There is this thing he has called the interim reflood model, which is an improvement over the base reflood model and that went into an official code version just a couple of weeks ago.

VICE CHAIR WALLIS: That is in there.

MR. MAHAFFY: That is in there and you can bet that information from the RBHT experiments that Larry Hochreiter fed into his judgment of his interim reflood model. Okay. Now, when they talk about advanced reflood modeling though, you know, Joe Staudenmeier is right. They are sitting around. You know, even if the LOCA size stuff hadn't happened, they are waiting for me to finish the droplet field to give them the capability to extend the power of the reflood modeling.

MR. STAUDENMEIER: Okay. And we're also working on an NRR user need based on --

VICE CHAIR WALLIS: Doesn't this LOCA break size redefinition need a reflood model?

MR. STAUDENMEIER: It will for the eventual power uprate calculations we're going to be looking at based on breaks above the transition break size. For the breaks below the transition break size you really don't get into large break LOCA reflood type of situations. For the very small breaks it's, essentially, a water level moving up and down. So it's not into classical core heat-up and reflood.

VICE CHAIR WALLIS: If you start talking about what level of mitigation you would want for a large break LOCA, assuming you want some, then you need to have a good reflood model.

MR. STAUDENMEIER: Right, and that's the type of calculations we'll be getting into next year. I mean, what he's going to be looking at is making sure the interim reflood model works good enough for those type of calculations that we're doing or at least it's fully debugged and we understand what the performance of that model is and where its shortcomings are.

It has just been implemented into the code and we have been doing some assessments with it in preliminary versions where it was stuck in in preliminary versions and identified a couple of things that aren't working quite right that he will have to look at in addition to some other problems. So that's, essentially, what his time is going to be spent on for at least the next six months, is making sure all the models in the code are working well for PWR LOCA calculations.

For ESBWR development, a decision was made to use coupled TRACE/CONTAIN for the NRC analyses. Joe Kelly had gone through and developed condensation models appropriate for TRACE for the containment, but it was looked at the effort and time that it would take to put that in compared with all the other things we're working on, and a decision was made that TRACE/CONTAIN coupled calculations will be used with CONTAIN handling most of the containment, except for the PCCS tubes and Joe's Kelly's new film condensation model would handle the PCCS tube modeling.

CHAIRMAN RANSOM: How would that work, because if you use TRACE to model the vessel and associated drain tanks -- well, I don't know.

MR. STAUDENMEIER: Yes, the drain tanks will be in TRACE.

CHAIRMAN RANSOM: Are the drain tanks part of the containment or are they part of the TRACE Model?

MR. STAUDENMEIER: Well, we'll have to examine where the boundaries are actually right now and we're looking at that, but --

CHAIRMAN RANSOM: Why would you even use CONTAIN? I don't know that I understand that.

MR. STAUDENMEIER: Well, I don't know if

Bill Krotiuk wants to speak about that.

MR. KROTIUK: It's Bill Krotiuk. I have been working on the models for using TRACE/CONTAIN and, basically, the boundaries were at interfacing between containment functions and primary system functions.

For instance, I originally had the PCCS heat exchanger in CONTAIN, because there is a model there that could handle that, but we believe that Joe's approach would be better and there's other reasons why we want to go to that. At the GDCS tanks, actually it's partially in CONTAIN. The tank itself is in CONTAIN, but the piping is in TRACE, so there's tradeoffs in that.

CHAIRMAN RANSOM: Well, how do you couple those two together? Is this the PVM type coupling tube or --

MR. KROTIUK: It's a coupling similar to-- not the PVM. What do we call it?

MR. STAUDENMEIER: ECI.

MR. KROTIUK: ECI.

MR. STAUDENMEIER: External Communications Interface.

MR. KROTIUK: Right. So basically, the coupling is such that at the coupling between CONTAIN and TRACE, you have, basically, a pressure boundary, a temperature boundary, a flow boundary, mass of liquid, mass of vapor, mass of non-condensible. So all that then is carried across the boundary.

CHAIRMAN RANSOM: I don't know. It surprises me, because there are a lot of phenomena that go in the suppression pool and things like that that I would think that CONTAIN or, I mean, TRACE might be better suited to model than CONTAIN.

MR. SIEBER: Might be.

MR. KROTIUK: Well, the suppression pool and the event flow and all that from the suppression pool is being modeled within CONTAIN. The CONTAIN Model has historically, you know, goes back to contempt LT and all that and so that same approach is basically being handled that way.

CHAIRMAN RANSOM: Thank you.

MR. KROTIUK: Okay.

MR. STAUDENMEIER: Yes, because, I mean, part of the reasoning in the decision was implementation time for the other things and ability to be consistent with NRR's review schedule, because they essentially want the whole review to be completed within a year and a half of the application coming in for review. Whether that can be done or not is another question, but we can't plan on it taking longer than that. So we're going with something that we think will meet the review schedule.

So part of that, as I said, the other condensation, there is a whole document of condensation models that Joe Kelly put together that could have been implemented in TRACE. That work is on hold and will be moved out until long-term possibly if it is needed for something else. And also, we are looking at changing the energy equation on enthalpy formulation. That's on hold, because we don't need to pressurize the containment that's fully within TRACE. It transfers the right information at the boundary to CONTAIN, so we don't need --

CHAIRMAN RANSOM: Enthalpy formulation in TRACE or in CONTAIN?

MR. STAUDENMEIER: TRACE doesn't have an enthalpy formulation for the energy equation. It has internal energy, so there is errors where you go across volumes with big differences in pressure. But what happens in the TRACE/CONTAIN coupling is those things are calculated right at the TRACE boundary and transferred to CONTAIN right, so you don't have to get into that error when we are using the TRACE/CONTAIN coupled calculations.

VICE CHAIR WALLIS: When you say TRACE will handle two condensation phenomena, this is in the PCCS system?

MR. STAUDENMEIER: PCCS and ICS.

VICE CHAIR WALLIS: ICS is Joe's work at ICS?

MR. STAUDENMEIER: Yes, Joe's work for the ICS also, so that's the high pressure heat removal system. ACR-700 development, the main piece of development for that is CANDU channel component for TRACE. What that has is it tracks water level in the horizontal channel. It decides which rods are above or below the water level and calculates radiation heat transfer appropriately between the rods and the can on the outside of the channel. There is also radiation heat transfer between the -- there is a pressure tube boundary and then a calandria boundary with some gas going in between there.

And eventually, we may also need to look at ballooning of the pressure tube boundary and contact with the calandria tube boundary and how that changes heat transfer and even rupture of the pressure tube boundary. But, as I said, it's in question now of whether that development work is going to continue on. Also, the header tank. There is a lot of complicated things that go on in the header tank and that's actually one place where we're always found saying that well, maybe the code doesn't handle momentum fluxes right, but it really doesn't matter in calculations.

That's one place where the preliminary calculations show that it doesn't matter how you treat momentum fluxing. You can get widely different answers, depending on how you treat it. So header tank modeling is an area that will have to be looked into if we do continue on with that in finding a way of -- I mean, it may be unrealistic to expect calculations you believe are real with that maze of big tank with all the tubes coming off, but something that would be bounding in the sense of licensing calculations coming up with a methodology that you believe may be conservative or bounding is something we would have to look into.

Also, we need a new flow map for horizontal flow and rod bundles. The PWR LOCA related development, there has been a few deficiencies identified in the interim reflood model. I'll cover that in another slide beyond this set. I'll go into it a little more. Blowdown heat transfer heat transfer deficiencies, it looks like we are under-predicting blowdown heat transfer. There are some condensation oscillations that may be larger than what is seen in the integral test facilities. Steam binding is a big --

VICE CHAIR WALLIS: Something of a numerical nature or in the model?

MR. STAUDENMEIER: They are related to physical models, bad physical models, correlations in the code. They have been improved to a large extent, but they are still -- we need to look at assessment against the data to see how they compare to the data. Also, for the LOCA calculations, we are going to be looking at coupling of a more advanced fuel model to the code to look at ballooning and rupture of the fuel in large break LOCA calculations.

NRR user needs, requests, they are using TRACE. Well, what they do is they have been running TRAC-G decks in TRACE through a multi-step process where they take their TRAC-G deck, which is very similar to TRAC-BWR format. They run it through a PERL Program that converts it over to TRAC-BWR format. Then they run it into TRACE and sometimes after steady safe calculations, they want to be able to extract another input deck.

And our solution for doing that is SNAP

can read these things called the "TPR file," where it's a new binary dump format. TPR is TRAC Portable Restart, so SNAP can read that and reconstruct an ascii input deck out of that. The ESBWR, ICS and PCCS modeling that is being implemented in the code right now and being tested. They identified a problem with control system computational performance with a big GE input deck that had a thousand control blocks or something like that and we haven't figured out what that problem is yet or identified what the solution is.

TRACE configuration, control and testing. Right now, we're doing testing only on one platform. While our testing is done on Windows with Compaq Visual Fortran as the compiler. We want to move to a multi-platform testing environment and Chris Murray is going to be doing some work on moving towards that this year.

RELAP5 Code consolidation. This is having it so that you can translate RELAP5 decks and run them in TRACE. That process is you feed a RELAP5 deck in the SNAP. It creates this RELAP5 TPR file format and then TRACE reads that in and goes off and runs the calculation. As I said, we can do that for simple input decks. We have a low level effort going on at

ISL to expand the range of input decks that that works for. And by the end of the year, we hope to, I think, have most input decks of interest to run.

Actually, within the NRC, that's not as big a problem, because we have equivalent plant decks for just about any kind of plant there is in TRACE native format. And so we have like right now with this break size redefinition calculations, there is a RELAP5 deck that they are using that is based on the Seabrook prime, but we all have an equivalent TRACE. At first, it was proposed that that be translated over to TRACE using this translation capability, but it has quite a few control systems and things like that which aren't translated well.

But we already have a TRACE native deck or it was actually an old TRAC-P native deck that can be converted fairly easily over to TRACE native input deck. And I have been doing that to get an input deck ready for the calculations.

VICE CHAIR WALLIS: How about these BWR breaks? Is there a plus and all that? There's a fairly big region of instability.

MR. STAUDENMEIER: Yes.

VICE CHAIR WALLIS: Are you capable of assessing the instability region and helping NRR to decide about RF breaks, BWRs?

MR. STAUDENMEIER: No, I don't have an effort. Now, looking at developing a methodology to calculate instabilities, so I think we need more assessment work to show how well it works against the data.

VICE CHAIR WALLIS: So you're developing this methodology? You're comparing it with some Ringhals data? Is that what it is?

MR. STAUDENMEIER: Well, there is more Ringhals data that we haven't compared to that we should compare to and there is also some Peach Bottom stability data that I would like to compare to. So there is also basic hydraulic assessments that you need to do against Frag stability data.

VICE CHAIR WALLIS: When you get these BWR power uprates, which is quite significant, and we ask about instability and we get some sort of assurance that it's okay. It would be good if we could have some turnover on this from you folks to give us assurance that it really is okay.

MR. STAUDENMEIER: Yes.

VICE CHAIR WALLIS: Confirmation.

MR. STAUDENMEIER: Yes, I don't know what NRR is doing in terms of independent calculations for that. I thought they had a contract with ISL to do some independent calculations using TRACE, but I don't know the status of those calculations or what their independent calculations, what they are that they are doing there.

MR. FORD: So NRR don't come back to raise, to ask some questions about this?

MR. STAUDENMEIER: Sometimes they do, not always. So like the stability review for ESBWR, we're not involved in that at all. So, I mean, NRR says we need your help on this, but not on that. Okay.

MR. FORD: So you could have NRR using TRACE inappropriately?

MR. STAUDENMEIER: Yes. I mean, with their ESBWR calculations for the LOCA, we had fairly close contact with those and I think they turned out fairly well and they also -- I mean, our calculations, I think, compare pretty well against GE for not having run the code through a whole set of assessments. And actually their calculations showed some errors that ours didn't have, so comparisons to the TRACE calculations actually turned up some questions for asking GE for additional information and turned up some bugs in their calculations.

MR. FORD: I mean, just to follow-up on

the use of this initiative on the TRACE to actual regulatory aspects, going back to Professor Wallis saying we've got a lot of -- you have no input at all to the question of the analysis by NRR or the instability?

MR. STAUDENMEIER: No.

MR. SIEBER: Only your past.

MR. STAUDENMEIER: Yep. I mean, there was this independent review that was started up at one time looking at, you know, it's called "BWR synergy" and now it's morphed into something else called the "safety margins" or something like that where we were going to look at things like that, but that program got cut off fairly early. There were a lot of resources involved in doing that and it wasn't supported to go on in the future, except at a much lower level and morphed into something else.

So BWR Reflood Model. Sometime this year we're going to be -- right now, the interim reflood model is only hooked up into the vessel component. We need to turn on all those heat transfer -- actually, the interim reflood model is more than just a reflood model. It's a full boiling curve heat transfer model and that's going to become the base heat transfer model in the code. And sometime this year it will be wired into being the heat transfer model for everything.

Right now, there is a separate heat transfer model in the core of the vessel component and there is in the BWR Chen component or in a pipe and that's going to be made consistent this year, so that's one thing we're going to be working on. Sub-channel analysis capability is another thing we have been asked for. That would be a major development effort to develop sub-channel analysis capability. So we haven't decided yet on what the capabilities of the sub-channel analysis needs to be or what the solution would be or what NRR even really wants to use it for, what the full range of applications they want to use it for.

CHAIRMAN RANSOM: That's for BWR?

MR. STAUDENMEIER: It was for PWR that they requested it for actually, because I think Westinghouse COBRA TRAC has sub-channel capabilities.

CHAIRMAN RANSOM: I thought you already had multidimensional capability within TRACE, so why would you want to sub-channel?

MR. STAUDENMEIER: Well, I guess, COBRA TRAC, apparently, has the ability to have terms for sub-channel modeling, which is a transfer term across sub-channels.

CHAIRMAN RANSOM: Assembly to assembly, but that's just because it did not, I don't think, have the multidimensional capability. Am I correct?

MR. STAUDENMEIER: Yes, I'm not sure. That's what we have to get a better definition of what the calculation capability needs to be before we can think about taking on a project like that.

VICE CHAIR WALLIS: It seems to me you are sometimes in the position of playing catch-up. I mean, if GE wants to operate the power of something by 25 percent or something bigger than they have done before and you guys may not be ready to answer the kind of questions that NRR may have about instability.

MR. STAUDENMEIER: That's right. I mean, they generally don't consider --

VICE CHAIR WALLIS: So they go and make a decision anyway.

MR. SIEBER: Maybe, maybe not.

MR. STAUDENMEIER: So, I mean, it depends on, I guess, they have to decide whether they have the current knowledge basis necessary to make the decision. And if they don't, then that has to be factored into the schedule of what research needs to be to support that.

VICE CHAIR WALLIS: They don't usually have the knowledge base. They rely on a presentation from industry.

MR. SIEBER: That's right.

VICE CHAIR WALLIS: And it's only if they start to question that then that you guys get involved.

MR. STAUDENMEIER: Yes.

VICE CHAIR WALLIS: I would have thought there would be some effort to always do independent checks of major things, such as what's the effect of a 25 percent power uprate on the stability.

MR. STAUDENMEIER: Yes, I mean, you would think that. But, I mean, they maintain that they stayed within the current knowledge base in their test data and I didn't review the power uprates, so other than sitting in on some of the ACRS meetings discussing them, so I don't know all the fine details of power uprates, but NRR by signing off on the power uprate maintains they have enough knowledge to approve that and they are within the knowledge base of being able to do that.

Speed up code calculations. There is always a need for faster code calculations. I think we have taken care of most of the runtime problems that have been reported. I'll show you an example of the main source of our runtime problems.

VICE CHAIR WALLIS: How long does it take to do a calculation say on large break LOCA? One realistic calculation.

MR. SIEBER: Well, it depends on how many steps and things.

MR. STAUDENMEIER: Yes, one base calculation 200 seconds. I've been running on my machine lately doing testing these code versions that come out probably about five hours, I think, four or five hours. That's a highly notarized model.

VICE CHAIR WALLIS: 59 statistical tests would take me a long time.

MR. STAUDENMEIER: Well, we have --

MR. SIEBER: There's a lot of machines.

MR. STAUDENMEIER: We have a lack of the Linux clusters and mine is a three year-old machine, so it's -- and it wasn't the fastest machine available at that time.

VICE CHAIR WALLIS: It's much more quickly.

MR. STAUDENMEIER: Yes, I think a current machine would be at least twice as fast as my machine. And if you have 15 of them sitting out there, than it would be fairly quick. Like a large break LOCA, I can run in the range of a half hour or something like that. Okay.

VICE CHAIR WALLIS: Does it have a bulk? I mean, does it never suddenly just say I can't run, because something has happened?

MR. STAUDENMEIER: Well, it does sometimes do that. Actually, with the latest code version coming out, it's very rare that it does do that. And actually, one thing it did more than just saying it couldn't run is it would slow down to a crawl and keep advancing very slowly.

VICE CHAIR WALLIS: I think in the early days of RELAP there were times when the code would just stop running.

MR. STAUDENMEIER: Yes, even in the later days. So, I mean, the codes sometimes do that and you trace it. You have to go and find out what is wrong.

VICE CHAIR WALLIS: Years and years ago talking about this code and how it always mysteriously stopped running or something.

MR. STAUDENMEIER: Well, through like the AP-600 calculations, at the beginning of that RELAP would stop running a lot for AP-600 calculations by the end sorting through all the problems it would run robustly for almost every calculation you would throw at it then.

CHAIRMAN RANSOM: Along that line, does TRACE have a pretty much automated timestep control capability, so when it runs into trouble it can get through that?

MR. STAUDENMEIER: It does. Yes, it does. Well, it does cut down the timestep when it runs into trouble, yes. Whether it makes it through in a timely manner or not, that's another question. There is -- sometimes you just get in to a point where you are into a bad correlation and it just wants to keep the timestep down real low or like if you have a condensation coefficient that's two orders of magnitude higher than it is supposed to be or something, then it will keep the timestep cut down real low or something that is causing oscillations back and forth, that's really a numerical instability driven by a bad correlation, essentially.

VICE CHAIR WALLIS: Does it have under-relaxation and things like that you can put in to stop some of the wilder --

MR. STAUDENMEIER: Actually, right now, the correlations are under relaxed from timestep to timestep. There is weighting between old time and new time of the correlation values. So and that's one thing we're going to have to look at.

VICE CHAIR WALLIS: So when you change flow regime, it doesn't go awhile and say the transfer coefficient is either 1 or 1,000 and it jumps around?

MR. STAUDENMEIER: Right. It will have some weighting based on old time and new time to try and transition it over some reasonable amount of time. That's one thing we're going to do some work this year looking at the transitions and transition times, because we ran into some cases in our assessment where the answers were really highly dependent on how that averaging was performed. And we want to be in a regime where it is not really affecting the answers that much or at least we understand how it affects the answers, what we have done to it.

CHAIRMAN RANSOM: Were there methods to make that weighting sort of timestep independent and that's presumably the way you should do it.

MR. STAUDENMEIER: Yes, yes.

VICE CHAIR WALLIS: Whereas the old time used to be just averaging new time, old time and that one introduced some numerical effects that are almost unquantified.

MR. STAUDENMEIER: Yes, the transition is with respect to a time constant as it is being done in RELAP5. But we're going to look at reexamining the time constants and how it is done.

MR. SIEBER: Do you have any conversions problems?

MR. STAUDENMEIER: Conversions problems? I mean --

MR. SIEBER: You know where your limit is set too tight and it just keeps missing it?

MR. STAUDENMEIER: Well, what it will do if it can't converge is that it will go back to the beginning of the timestep and try again with a lower timestep.

MR. SIEBER: Okay.

MR. STAUDENMEIER: But sometimes you get into the case where it just keeps reducing timestep data. It will hit the minimum timestep and shut down saying I still can't converge at the minimum timestep and that's where you have to go look for correlation problems.

MR. SIEBER: It brings back memories.

MR. STAUDENMEIER: Yes.

VICE CHAIR WALLIS: It's a very funny situation here. I mean, you have one customer who asks a few questions and stops asking some after a while and comes back. If you were a commercial company and had 10,000 customers using your product, you would have to be really on the ball. Here it's sort of uncertain as to what questions you're supposed to be answering, because they change and are driven by the latest crisis or something.

MR. STAUDENMEIER: Well, we have more than, I guess, one customer. We have NRR, who is our regulatory customer. Internally, we have people doing code assessments or analyses like these 50.46 break size redefinitions, so they are providing feedback to the code saying it's not working well here or here.

VICE CHAIR WALLIS: The seniors at Penn State do that too? Do they give you feedback?

MR. STAUDENMEIER: I don't know if in terms of John's class if they give -- I mean, there is feedback that gets provided. I think John probably sticks to problems that run fairly well in terms of that class.

VICE CHAIR WALLIS: In terms of what?

MR. STAUDENMEIER: I don't know.

MR. MAHAFFY: This is John Mahaffy. Let me make a couple of comments here. I mean, you need to distinguish, I guess, between your customer and your user base also. And one thing that you probably don't see is that whether it's of TRACE or RELAP5, the customer user base that you are used to thinking about in terms of hours that the code is run is minuscule compared to the hidden user base inside the Navy Laboratories.

And TRACE is the workhorse code for Knolls Atomic Power Laboratory. RELAP5 is the workhorse code for Bettis. And they have quoted to me up at Knolls, where I normally deal, total number of hours that they run this code in a year, it's astronomical. I mean, it's beyond belief. And we do get feedback from these people. So that, you know, there are things in terms of exercising this code that are well beyond even the bounds of what your normal imagination is.

MR. STAUDENMEIER: And we're also getting camp members starting to use the code and getting feedback from the camp.

VICE CHAIR WALLIS: Now, that's the international effort?

MR. STAUDENMEIER: Yes.

VICE CHAIR WALLIS: So it's quite possible that some user in Grenoble or something would use TRACE and get in touch with you and say how about this? I run this problem.

MR. STAUDENMEIER: Not likely in Grenoble.

MR. MAHAFFY: Not likely.

VICE CHAIR WALLIS: All right. But in Russia somewhere?

MR. STAUDENMEIER: No. Actually, Japan is starting to use it for some calculations. Russians are using it for calculations and actually they have presented some things at the last camp meeting where they had good assessment results versus their test data.

MR. SIEBER: But TRACE doesn't have the pedigrees that commercial codes have, right? Like V&V and all the quality stuff that goes into it?

MR. STAUDENMEIER: I mean, we have a V&V Program in quality assurance that Chris Murray will talk about later of what we do. What we don't have is something like a LOCA Code in the industry. You verify it and do the assessment, then you lock it down.

MR. SIEBER: Right.

MR. STAUDENMEIER: And then you use it in a mode where you throw inputs at it, qualify it and blindly accept the output. You don't care about what -- you don't really examine closely what it is calculating. You are -- it's because it's qualified. If it's under the conditions, you are --

MR. SIEBER: Yes, it's golden.

MR. STAUDENMEIER: Yes.

MR. SIEBER: Okay.

MR. STAUDENMEIER: So, I mean, we're kind of being used in a different manner. Our projects are more unique analysis projects and it's a lot wider scope. So to qualify it for the whole range of things that is being done, if we are to do it on a large project like AP-600, as an example, where RELAP5 was the analysis tool, it went through a lot of assessment through the various transient set it was used for and there is a stack of documentation saying it's good for that. We do have that code version sitting there for that. But that code version is also used for a lot of other things that it wasn't -- it didn't have a giant assessment effort or qualification effort.

MR. SIEBER: Right.

MR. STAUDENMEIER: It's up to the person using it for the application to go off an qualify it for that application. But we do, in terms of putting changes in the code, follow procedures in review of changes. And when this code goes out in its formal release, whichever version that will be, I mean, it will have complete documentation for users manual, assessment manual, theory manual, so it will all have some pedigree behind it.

MR. SIEBER: Yes.

MR. STAUDENMEIER: Every line may not have been reviewed independently by a person, but I don't believe that catches all the errors either. But it will have a wide use base and I think it will be in pretty good shape in terms of using it. But that doesn't mean, you know, I mean, some people in the industry now, utilities or companies, like most of the vendor models are based on NRC Codes. And what they will do is take an NRC Code and stick it, they will put it in their own changes, go through their own --

MR. SIEBER: Pre-processor and post-processor and change the name.

MR. STAUDENMEIER: Yes. Or put in special required models that are required by Appendix K or something like that.

MR. SIEBER: Right.

MR. STAUDENMEIER: And they will qualify it. And there is nothing to stop anyone from doing that. NRR sets the standards on what can be used in regulatory purposes or someone could take TRACE and put it through that type of processing and qualify it as an approved model if they wanted to.

MR. SIEBER: Well, what I think about that is Knolls doing this, you know, they have a qualification program of their own.

MR. STAUDENMEIER: Yes.

MR. SIEBER: So they must do more than just pull it off the shelf and run a bunch of calculations and say I'm okay.

MR. STAUDENMEIER: Yes, they did. I think they have, well, their own modifications they put in the code. Once they get the base version from us and have their own set of things they have to do to qualify the code is being useful.

MR. SIEBER: For their application.

MR. STAUDENMEIER: For their application.

MR. SIEBER: That's right. Okay. Thank you.

MR. STAUDENMEIER: Okay.

CHAIRMAN RANSOM: I think we better move along. We're starting to run a little bit behind.

MR. STAUDENMEIER: Yes, okay.

MR. SIEBER: I'm hungry.

CHAIRMAN RANSOM: I know one thing, Joe, I would like to hear and I don't know from you or from the other presenters, but this development, looking through some of your slides, it looks like 20 years ago we could have written the same slide, you know, about the problems that we're running into. And I'm wondering if you or the others could say a little bit more about why has it taken so long? You know, this is the eighth year of this project actually, and you seem to still be in the development mode.

And there are two questions. I guess one, why is it taking so long? And the second one, what is it going to take to finish the job?

MR. STAUDENMEIER: Okay.

CHAIRMAN RANSOM: So if you kind of quickly maybe --

MR. SIEBER: That would be done.

CHAIRMAN RANSOM: I don't either. If somebody else is going to talk about that, that's fine, but maybe go quickly through the rest of the problems that you have listed on the chart.

MR. STAUDENMEIER: Okay. Yes, I think, well, I guess, one comment now is, I mean, competing interests have kept things from moving forward at a steady clip, like a decision was made for RELAP5 compatibility to be put ahead of updating the models and correlations and making sure those worked okay. So that's one thing. There was quite a bit of effort and time sunk into RELAP5 compatibility.

CHAIRMAN RANSOM: This is so you could use a RELAP5 deck in the TRACE?

MR. STAUDENMEIER: Right. So that's one set of time or effort that was thrown at that. That still hasn't come to fruition where you can run all the deck sets in retrospect. It probably would have been better spent at making sure the base set of models and correlations were working well for the range of conditions we wanted to use them for. So that's probably one of the big stumbling blocks.

I mean, we have always had a code that runs at every step and, I mean, the biggest question was how good are the answers that you are getting and that's probably one thing that should have been done up front more, I would say, in the process. So that's, I think, put a couple year wait on it. I mean, getting a production level code. I mean, the code could run faster or slower, but it was always running and getting answers about the quality.

I wasn't involved in the project from the start. John might have something to say in his presentation later about other things that I'm not aware of. But that's I know the one big thing that probably delayed us a year or two, at least a year, probably closer to two years in the project. Also, assigning people that were supposed to be working on the project to other things that had nothing to do with TRACE. I know that happened in my case. My job was supposed to be working on TRACE, but when I came over to research, I was stuck on everything but TRACE for the first two years, essentially, about three-quarters of my time.

CHAIRMAN RANSOM: Well, I know with the origin of this project, I think like the RELAP5 conversion, the use of SNAP for all of these functions was envisioned, but was it just that that was too ambitious?

MR. STAUDENMEIER: Well, I think, it was changed. The original vision was that you would get some help in moving RELAP5 decks over, but it wasn't going to convert everything and it would tell you what it couldn't convert and what you had to do by hand. And that vision was changed some time into that it would take RELAP5 decks and run them without any user intervention and give you answers that were as good or better than RELAP5, and that was very ambitious, I think, and that's a lot. I mean, the first 90 percent, it probably takes 10 percent of the time, the last 10 percent takes 90 percent of the time type of thing. It's one of those type of situations.

VICE CHAIR WALLIS: Okay. Why don't we

try to quickly get through these and maybe we can by 12:30. Can we do that?

MR. STAUDENMEIER: Okay. Yes.

VICE CHAIR WALLIS: You've spent an hour on half your presentation.

MR. STAUDENMEIER: So portability. It is a portable curve. The testing is done right now on Windows with Compaq Visual Fortran, but we also compiled a code and run it under pretty regularly NAG and Lahey and recently G95 compiler, which is a free compiler on Linux. It's also available on other platforms on MAC OS X, Joe Kelly runs it with xlf, IBM xlf Fortran.

CHAIRMAN RANSOM: What is Lahey? Is that a deck Lahey compiler?

MR. STAUDENMEIER: No, it's -- I can't remember. Thomas Lahey is the guy's name. Their company is out in Nevada, I think. It's a small company. Recent trace assessment. We've been focusing on mostly for the past year large break LOCA assessment, reflood tests and blowdown heat transfer tests. Our findings so far force reflood or force reflood calculations, the peak temperatures are reasonable. The quench front progression is too slow, especially up in the top half of the bundle.

Joe Kelly is going to be looking at that once he moves back into looking at 50.46 related stuff. We have saw excessive condensation oscillations in gravity reflood tests. Our latest code version has gotten rid of a lot of that, but we're going to have to assess it to look at the size of those oscillations with respect to the gravity reflood data. And we're over-predicting temperatures during blowdown heat transfer and the blowdown heat transfer seems to be very sensitive to timestep averaging.

For small break LOCA assessments, both with this ROSA assessment and some past semi-scale assessment, we're over-predicting peak temperatures, but generally we're predicting the parameters of the test facility fairly well in terms of pressure response.

MR. FORD: These are very qualitative statements, over-predicted, under-predicted, etcetera.

MR. STAUDENMEIER: Yes.

MR. FORD: Are there any of these non-predictions of practical importance?

MR. STAUDENMEIER: Well, I mean, one thing we found, I mean, in practical importance, there had been changes made to the code to correct some of the non-predictions, if that's what you mean? Like some of the changes to the code make things better, like a rod bundle interfacial drag model has made the small break LOCA predictions better.

MR. FORD: I guess my question really is are these just academic concerns or are they really practical concerns?

MR. STAUDENMEIER: Well, it depends how accurate you want the answers to be. I think for small break LOCA, I think, it generally predicts the system response and, as I said, temperatures it's over-predicting. Depending on the accuracy you want, I mean, these are kind of -- you could give a whole presentation on small break LOCA assessment calculations and sometime in the future maybe we'll be able to do that.

VICE CHAIR WALLIS: Well, we just had an uprate for Waterford 3, which was the highest uprate for PWR, and I understand they said that they are limiting process for the uprate for break LOCA. That was what stopped them from getting 10 percent rather than 8 percent.

MR. STAUDENMEIER: Yes, compliance can be.

VICE CHAIR WALLIS: Just get the temperature right, because that temperature is what is limiting the power uprate.

MR. STAUDENMEIER: Yes.

VICE CHAIR WALLIS: And I assume when you say over-predicted, you mean that's satisfactorily over-prediction. It's not just a small amount, but significant enough to worry about.

MR. DENNING: Can you give any quantitative feel for that? I realize you can't just give one number, but about how much is it over-predicted?

MR. STAUDENMEIER: I mean, well, a number in my head is semi-scale small break LOCA calculation the peak temperature was in the range of maybe 650 degrees and TRACE was predicting maybe 700 degrees or something like that. But none of those are up in the range of temperatures you get into in small break LOCA licensing calculations. And typically best estimate calculations for small break would give you a lot lower temperature and a licensing calculation with a 20 percent extra to --

MR. SIEBER: In hundreds of degrees.

MR. STAUDENMEIER: Yes, hundreds of degrees, yes.

MR. SIEBER: I think most PWRs are peak clad temperature limited for Pantex K calculations.

MR. STAUDENMEIER: Yes.

MR. SIEBER: And so if you're looking at power uprate, that's what is going to fix your final top power. That doesn't ring true for boilers, however, they have more margin. There is different parameters that control how far they go.

MR. STAUDENMEIER: Yes, because I mean if you took the 20 percent, the K heat that they -- essentially, small break LOCA industry models are close to best estimate models, except they throw on things like 20 percent of K heat.

MR. SIEBER: Right.

MR. STAUDENMEIER: But in terms of other things, they are very close to what we would run with RELAP5 and TRACE.

MR. SIEBER: Yes.

MR. STAUDENMEIER: That the K heat gets it sitting up on matching -- I mean, the only way it can get rid of that energy is through the steam generators and throw the break and 20 percent extra to K heat keeps it up at a higher pressure longer and you get a lot lower level depression and you stick it up. I mean, it's a very non-linear effect as you add power for limiting small break LOCAs.

MR. SIEBER: Well, the rule is pretty

restrictive on correlations and how things shall be treated, so, you know, that's what forces all that conservatives into an Appendix K calculation.

MR. STAUDENMEIER: Okay. BWR transients, Peach Bottom Turbine Trip, Tom Downar will show you a little bit more about our work doing calculations for that and also the density weight, stability. We have been updating our rod bundle interfacial drag and making some wall drag changes to the code that are going to improve those calculations over the base code, but deficiencies in the interfacial drag and wall drag changes in the base code were giving problems in the power profile predictions.

Recent error corrections that have sped up calculations and made them more robust, I should probably skip over them. They are not no big deal. Enhancements that are going into the latest version of the code, we're going to be releasing another version of the code to camp within the next few weeks. Joe Kelly mentioned there is a new wall drag model. We have put our rod bundle interfacial drag model into the code. Some interfacial heat transfer improvements that had to do with condensation problems in the code leg that Joe Kelly mentioned.

We have capability for multiple non-condensible gases, improve the separator component graphics.

CHAIRMAN RANSOM: All of these improvements are finished and they are just being incorporated into the code?

MR. SIEBER: They aren't incorporated yet.

MR. STAUDENMEIER: Some of them are already incorporated.

MR. SIEBER: Some of them are.

MR. STAUDENMEIER: And the ones that aren't will be in the next week or so.

CHAIRMAN RANSOM: Is it just in one flow regime? Don't you have several flow regimes?

MR. STAUDENMEIER: There are multiple flow regimes. I mean --

MR. SIEBER: Right.

MR. STAUDENMEIER: There is like probably slug, essentially, an annular mist, I mean.

CHAIRMAN RANSOM: These improvements are only in one of the flow regimes?

MR. STAUDENMEIER: Well, for the rod bundle interfacial drag that correlation that is being put in is really a bubbly slug type of correlation, the best correlation, so it doesn't -- but the transition to the annular mist regime is going to be changed.

VICE CHAIR WALLIS: So you're going to be going back and improving the really basic things in the code.

MR. STAUDENMEIER: Right.

VICE CHAIR WALLIS: You get some of the simplest things.

MR. STAUDENMEIER: Right. Yes, well, as we knew from TRAC-BWR that we needed a special model for rod bundle interfacial drag and also RELAP5. This is a similar one time problem I was talking about before. A lot of cases, you know, you would get into a case, I mean, you might have a base calculation that progresses steadily in time, but you make some propitiation or a slightly different calculation where the code gets bogged down and you, essentially, jump up your CPU time without progressing through the transient at all.

And some of the fixes we have made that are going into this version, we've gotten rid of all the known cases of that happening. The fixes are fixing that. I mean, when the code runs, it seems to run well, but there were cases where it would get into this type of problem or it just backs off timestep and it keep crunching away. And so the causes of that, there were some improvements John Mahaffy made in our Water Packing logic and also improvements in the physical models and correlations.

Improving automated testing, looking at multiple platforms, we're also adding or looking at a thing that we're going to call a "robustness suite," which does real calculations of plants and test facilities and some of the calculations that have given the code problems in the past in terms of running though and getting good answers, and we're going to be making quantitative comparisons in an automated way that will set up flags when you run through the test suite that will tell you where you need to look into things further or hopefully we're going to be implementing that this year and multi-platform testing.

Chris may show you a little more later. As he runs through it, this whole test suite has some programs that post-process the test data and summarize it, you know, what pages where you can look at changes and performance.

CHAIRMAN RANSOM: Joe, why don't we stop here for lunch and then you can finish up after lunch and tell us more about the future plans, I guess, for the code.

MR. STAUDENMEIER: Okay.

CHAIRMAN RANSOM: So we'll break until about 1:30.

VICE CHAIR WALLIS: 1:15?

CHAIRMAN RANSOM: Quarter after?

VICE CHAIR WALLIS: 1:15? Can we catch up a bit of time?

CHAIRMAN RANSOM: 1:15, we can be back. Okay. 1:15.

UNIDENTIFIED SPEAKER: Okay. Something has to be done here to make it not go to sleep or hibernate, I guess.

(Whereupon, the meeting was recessed at 12:30 p.m. to reconvene at 1:18 p.m. this same day.)

A-F-T-E-R-N-O-O-N S-E-S-S-I-O-N

1:18 p.m.

CHAIRMAN RANSOM: Okay. We're back in session. And Joe is going to finish up with the TRACE development release.

MR. STAUDENMEIER: Okay. Can you hear okay? Okay. We should be coming out with the code release by the end of February. That will also be put on a CD and sent out to camp members. The big changes in this release, since the last release, we proudly have the interim reflood model in the code, which you saw a presentation on about a year and a half ago maybe or about a year ago. We have some changes to the wall drag interfacial drag, interfacial heat transfer.

We have improved the performance of the separator and accumulator models in terms of phase separation in both of those models and improved also Water Packing and some other robust enhancements due to physical model corrections and some choking models fixes.

CHAIRMAN RANSOM: When you say improve separator, is that an empirical model or is it like the BWR separator?

MR. STAUDENMEIER: It's like the TRAC-BWR separator. It had some numerical problems with it, so it's improving the way it works. The TRAC-BWR separator never -- it had some problems of its own, so there were some code robustness problems and also problems where it wasn't separating like it should in some cases. And we think we have worked out those problems and have it working.

VICE CHAIR WALLIS: You say improved Water Packing, you mean improve non-Water Packing? I mean, you don't want it to pack.

MR. STAUDENMEIER: That's right. It improved detection and correction of Water Packing problems and makes the code run a lot smoother. Some problems that we still have that we know about, there were some level tracking problems identified with some ESBWR calculations where the containment was actually modeled in TRACE also. Those problems will be -- we had planned on working on them this spring. They may not be a problem, since the containment is not being done in TRACE anymore, but we're going to look at that, identify that and fix it.

Blowdown heat transfers, there are some accuracy problems in predicting blowdown heat transfer. Joe Kelly is going to start looking at that as part of our 50.46 activities.

VICE CHAIR WALLIS: Cloning Joe Kelly, I think --

MR. STAUDENMEIER: Yes, unfortunately, we can't.

MR. SIEBER: You can try.

MR. STAUDENMEIER: Yes, maybe we should spend our money in cloning research instead of code development.

MR. SIEBER: There you go.

MR. STAUDENMEIER: So and right now the reflood package is restricted to the TRACE vessel component and we're going to move that heat transfer package, so it's the default heat transfer package for the whole code, every component in the code by the end of the year. We want to do that.

Our 2005 development priorities, fix robustness problems and physical model deficiencies. As I said before, our biggest ones now are condensation oscillations and blowdown heat transfer and also some problems with the reflood heat transfer, a few deficiencies, unified physical model package. Right now, also, when you turn on the reflood package, you need to turn it on with a trip set in the code, so it is user activated at a certain time. And when it's a unified package, the code is going to do all the detection logically doing that. It's going to be automatic.

More implicit numerical methods and adding droplet fields to the code.

VICE CHAIR WALLIS: It makes it a three-fluid model?

MR. STAUDENMEIER: There is going to be two droplet fields added.

VICE CHAIR WALLIS: Both fluid model?

MR. STAUDENMEIER: I mean, at least three. It depends if you may want to use more than --

CHAIRMAN RANSOM: They all have four momentum treatment as well as energy and mass for each one of those fields?

MR. STAUDENMEIER: In terms of energy, I think the first implementation is that temperature is going to be the same for all the liquid fields. John is putting that in and he can answer some questions about that in his later talk if you want to ask him that.

VICE CHAIR WALLIS: Are droplets going to impinge on spaces and things like that?

MR. STAUDENMEIER: It will give the ability, yes, to put in --

VICE CHAIR WALLIS: All that stuff is

going to be in there?

MR. STAUDENMEIER: Yes. Well, not when the droplet fields first go in. There has to be some correlations that get implemented that take advantage of the droplet fields. So when the droplet fields are first put into the code, they won't have correlations with them and there will need to be correlations developed to take advantage of the droplet fields.

VICE CHAIR WALLIS: Don't you have more simultaneous equations to solve? You have more conservation, so you have a different solution algorithm of some sort?

MR. STAUDENMEIER: Well, the solution algorithm, I guess, essentially, will be the same, but it will be generalized to more equations.

VICE CHAIR WALLIS: So you formed that?

MR. STAUDENMEIER: Yes. Okay. Features needed for ESBWR, ACR-700. For ESBWR they are --

VICE CHAIR WALLIS: Well, I would think, I'm sorry, that going to a four equation model instead of a two equation model is a really major change. You're going to run into all kinds of development issues.

MR. STAUDENMEIER: Well, the problem is finding data to get good closure models for the correlation models.

VICE CHAIR WALLIS: You need more correlation models, but also --

MR. STAUDENMEIER: You need more closure, yes.

VICE CHAIR WALLIS: -- all kinds of things you haven't yet come into, you know. Matrices have zero determinants and things and you get weird characteristics that change from one --

MR. MAHAFFY: If you would like, I can talk to you about that when I'm up.

VICE CHAIR WALLIS: Okay. Okay.

MR. STAUDENMEIER: But I mean, in a sense it's going back to the future, back in the '80s the NRC had a three field model with droplets that --

VICE CHAIR WALLIS: And one question on --

MR. STAUDENMEIER: Joe Kelly was one of the developers on that code.

CHAIRMAN RANSOM: There are more implicit numerical methods? My understanding is TRACE had this multi-step implicit formulation, which was full implicit.

MR. STAUDENMEIER: Yes, but --

CHAIRMAN RANSOM: What's that?

MR. STAUDENMEIER: Okay. I was going to

say what we're looking at now is implicit interfacial heat transfer, implicit wall heat transfer and essentially full implicit fluid flow with the drag coefficient being --

CHAIRMAN RANSOM: In other words, the temperatures are moved to new time basically, that you used in those correlations?

MR. STAUDENMEIER: And the coefficients.

CHAIRMAN RANSOM: Yes.

MR. STAUDENMEIER: Because right now coefficients are like for wall drag, the drag stuff will be the last stuff that will be made implicit, but yes, they are all. Like right now, there is inconsistency between the wall temperatures and the fluid solution and in the conduction, so where should we move it?

CHAIRMAN RANSOM: Yes.

MR. STAUDENMEIER: Okay. Improve automated testing, multi-platform testing and improve TRAC-B and RELAP5 support. And as I said before, we're offering our first training workshop, which is similar to RELAP5 training workshops that we have been offering for as long as I have been here, essentially. There have been RELAP5 training workshops going on. So it will be in the same form as those workshops.

CHAIRMAN RANSOM: Who all is invited to that? Anybody who wants to come?

MR. STAUDENMEIER: Anybody who wants to come, except its too late if you want to come now, it's full the workshop. But the announcement was sent out to the RELAP5 users group members that ISL runs, to camp members, NRC staff, NRC contractors, but yes, there's a wide range of people coming. There is camp people. There is NRC internal staff. There is --

CHAIRMAN RANSOM: Maybe you ought to invite the ACRS.

MR. STAUDENMEIER: Some contractors, yes, they are free to come. Actually, my branch chief is taking the class, so he's --

CHAIRMAN RANSOM: How long is this?

MR. STAUDENMEIER: Three days.

VICE CHAIR WALLIS: We don't learn very quickly though.

CHAIRMAN RANSOM: We ask too many questions maybe, huh?

MR. SIEBER: That's one of the rules, no questions.

MR. STAUDENMEIER: Yes, actually, the workshop is learning something about the code and models and component models inside the code and there is also hands-on workshops where you are building models and running them and learning functionality of actually running the codes. So this course is just kind of an introduction. It's not going to teach you two phase flow and thermal-hydraulics. It's teaching you essentially what is in the code and how to use the code.

Our first release target for an official release with all the documentation complete is about two years away in our estimate and that will be having a code assessed for the operating PWR and BWR. At least you U-tube PWR. I don't know if we'll be looking at ones through steam generator PWRs in any of the assessments. So far no assessment has been done for that. I will be striving soon upon the feature set and range of assessment to be decided upon soon.

VICE CHAIR WALLIS: The ESBWR Model, too?

MR. STAUDENMEIER: That won't be part of the release. That documentation, I mean, we -- subject to their applicability report for something like ESBWR will be a proprietary type report, so we can't put assessment and applicability of that reactor design in a public release. But the code will be, I mean, it will be being used for ESBWR during that time frame. But, I mean, if someone wants to take it and analyze a design similar to ESBWR, they will have to go out and find out the range of use and make sure all the models work for that design, but we can't send out an approved code for ESBWR or any other reactor for that matter.

But it will be assessed against typical operating PWR and BWR accidents of interest. We will be freezing the feature set in physical models for this code release by the end of the summer and be assessing what that frozen code version and only making corrections and not adding new features that will go in the release, at least the assessed part of the release. There will be features that you would be able to access, but they won't be part of the features that are assessed and approved for use, essentially.

The documentation, it will be complete documentation of the users theory and assessment manual, the programmers manual will arrive later.

CHAIRMAN RANSOM: Who is going to do that documentation?

MR. STAUDENMEIER: In-house. I mean, that's why we have to -- I mean, essentially, it's going to take us probably a year to get all the documentation done with a lot of us working, spending a significant amount of time on that.

CHAIRMAN RANSOM: I know that.

MR. STAUDENMEIER: We'll try to get help from ISL probably to help complete the documentation. But it's essentially --

CHAIRMAN RANSOM: The previous documentation came out from Los Alamos and there was somewhat of a disconnect between it seemed like what you are actually doing or the people who were doing the work and people who wrote the documentation.

MR. STAUDENMEIER: Yes, well, it will be based -- that will be the basis for the documentation. But we'll be making a lot of modifications and make sure everything is up to date. I mean, Chris Murray keeps the users manual up to date. When things go in or they change the users manual, he keeps up the, at least, input format part of that up to date. And if new components are added, he puts that. We have a lot of documentation from things that have been put into the code sitting out in various reports that have to be taken and integrated into the release documentation.

Like when Joe Kelly puts his new condensation model in the code, there will have to be theory manual documentation for that and how to -- and user manual documentation for how to activate that.

So it's a big effort to do documentation.

VICE CHAIR WALLIS: Well, when John Mahaffy was here last time, he told us that TRACE had all the faults of the previous codes, in terms of fundamental equations, momentum equations and so on. We have yet to see code documentation that has a proper element of momentum equations that is convincing. Are we going to see it this time?

MR. STAUDENMEIER: This time as in today?

VICE CHAIR WALLIS: Well, this time. In two years time when you have the documentation.

MR. STAUDENMEIER: Yes, well, I think John is going to cover how his equation of motion is treated in TRACE right now to today.

VICE CHAIR WALLIS: He's going to do --

MR. STAUDENMEIER: But, I mean, we will. That's something that probably will have to be updated over the current theory manual to give a clear explanation of exactly what is done. And as I said, we would like to put out our completed documentation for peer review, contract it our for peer review and also provide it to the ACRS for comments before it gets released.

VICE CHAIR WALLIS: You know the danger then Vic referred to earlier that you put out this documentation two years from now and peer review people look at it and say gee whiz, that's a pretty broad basis for a code and yet he had done it at the end.

MR. STAUDENMEIER: Yes, well, I guess, in terms of documentation it is right now -- I mean, it has been reviewed in the past, the documentation, back at the end of the CSA Project and brought up, I guess, all the comments that were made on the documentation back during that were integrated and the code documentation was updated based on that.

VICE CHAIR WALLIS: A long time ago.

MR. STAUDENMEIER: A long time ago. I mean, we added new features and changes to the code. But I guess, at that time, that level of documentation was accepted as adequate.

VICE CHAIR WALLIS: The original TRAC documentation was terrible. I mean, the code was developed and there wasn't a record of how and why. I'm sure it's better now. Okay. So we will perhaps see this stuff in two years time or you'll know what really goes in --

MR. STAUDENMEIER: Well, you will see it before then, I think. I mean, by the end of the year, I would think or early next year, we would have a pretty good draft documentation. I mean, right now, the documentation is in a reasonable form if you have any glaring deficiencies that come out at you now. If you looked over it, we would be glad to hear your comments about it.

VICE CHAIR WALLIS: With luck, you may see us again by then.

MR. STAUDENMEIER: Okay. Yes, we're just going to try to outlast you all, that's all.

CHAIRMAN RANSOM: Are these documents on the website?

MR. STAUDENMEIER: They are on the code developer's website. And actually, yes, there is a new website called nrccodes.com that is a password protected website that you could get a password for. Because of NRC computer security problems, we are moving our website. A lot of the material outside to ISL, so we don't have to deal with the security requirements of the NRC in terms of supporting a website, a public website like that.

VICE CHAIR WALLIS: So it's a dot com not a dot gov?

MR. STAUDENMEIER: Right. Yes, while we used to have the NRC, the codes website inside the RES and that got closed off after 9/11.

MR. MAHAFFY: Could I kick something in here, Joe? This is John Mahaffy. My recollection from the last time I was at one of your meetings was that you were given access, at least some subset of the ACRS, to the developer's website.

And at the time, we did tell you that documentation was available, so that any time you want to see what's in progress, if you have still got that access, that's the place to go. When I have got a question about documentation on something, you know, I just go onto the developer's site and pull down whatever the latest documentation is.

UNIDENTIFIED SPEAKER: Yes, our code CD releases, I think, have all the current documentation on it, too, so we could send down a CD when we make this latest camp release.

MR. CARUSO: Why don't you send me a copy of that CD, Joe?

UNIDENTIFIED SPEAKER: Okay. Yes, we can even give you -- I think there's even executables on the CD. You could run the code if you want to, SNAP.

MR. STAUDENMEIER: Okay. That completes my presentation. I think next up is Tom Downar.

CHAIRMAN RANSOM: Right. Mr. Downar from Purdue.

MR. DOWNAR: I'm Tom Downar from Purdue University and I have been part of the TRACE Development Team from the very beginning. This is my first time speaking with the ACRS, so I thought it might be appropriate, you know, if I give a little background on PARCS.

You know, over the last seven, eight years I have had several students who have contributed to the code, this past year three students in particular who two of them are now post docs, a former student who is at ISL, Doug Barber, and then Joe, of course, who is our project manager.

And this is a website we maintain. We have now about more than 45 users of PARCS around the world and we have made this website available to them where they can get some of the PARCS-specific things, and then also go to links of my students and get their PowerPoint presentations, see some of the things that the students have been doing.

Just by way of background, of course, what we're solving is the Boltzmann Transport Equation for the neutron distribution. We coupled that to the Nuclide Equation and then to get the feedback from TRACE or RELAP. So we're solving coupled field equations.

PARCS, specifically the features that we have, we're at Version 2.6 and many features have been added over the years. We're solving the steady-state fundamental mode Eigenvalue Problem for the Real and Adjoint. We also do the harmonics, because now we're doing stability plus we're solving the time dependent problem.

We started out just doing the two group form of the diffusion equation. Now, we're doing multigroup and we're also doing transport solution. Numerical schemes, we do Coarse Mesh Finite Difference Acceleration and we solve our linear systems using a Krylov Linear Solver, Pin Power Reconstruction coupled to both TRACE and RELAP5. We also have a processing code to process the cross sections that are generated with our commercial code or now with the code TRITON, and we're doing Fuel Cycle Analysis also.

VICE CHAIR WALLIS: So you have lots of nodes all over the core?

MR. DOWNAR: Yes. I will show you pictures.

VICE CHAIR WALLIS: How many?

MR. DOWNAR: About 40,000 fuel nodes.

VICE CHAIR WALLIS: Is the thermal-hydraulics as precise as that?

MR. DOWNAR: No. That's one of the issues that we have had to address in particular now that we have been doing BWR coupled code analysis. We started out doing Peach Bottom with 33 channels, so there would be 748 neutronics nodes in each plane and then about 24 planes that would map the neutronics into 33 thermal-hydraulics channels.

We started doing stability last year and what we found that now, we needed more channels, so we have even looked at some models where we're doing one thermal-hydraulic channel for each physical BWR channel and I will show you some slides, but obviously the computational time is going to increase dramatically. So I will get to that in a little bit.

CHAIRMAN RANSOM: How about when you apply it to a PWR? Is there still a need for that amount of detail?

MR. DOWNAR: We have done it. I will show you a main steam line break problem. We did TMI core and for a PWR not as much. You know, fidelity in terms of the TH model is required. The problem is easier from a, you know, thermal-hydraulics standpoint. For the stability problem though, if you want to model some of the complex phenomena, your coupling has to increase in fidelity for both fields.

VICE CHAIR WALLIS: You have different kinds of stability, don't you?

MR. DOWNAR: Right.

VICE CHAIR WALLIS: You have some high frequency ones and some low and some very low frequency things.

MR. DOWNAR: Yes. Right. There's, you know, in-phase and out of phase oscillations. I will show you that in a second. We chose to do two points of the Ringhals stability test. There were a total of 40 points. We chose two points. One, because the dominant mode was a regional out of phase oscillation, and then we chose another point where the dominant mode was an in-phase, and I will show you some preliminary results only on the in-phase. We have not yet tackled the out of phase oscillation.

Going back to the neutronics though, what we're doing over the years is adding the sophistication of the solvers tackling, you know, different geometries. We have always been able to do Cartesian geometry, two group nodal, as I mentioned. Over the years we have added the ability to do hexagonal lattices.

This was some work that we did for the Department of Energy several years ago as part of a separate project, and then we have added it to PARCS, appropriately key rated, and now it's part of the NRC PARCS. The people that do VVERs were happy to get that feature.

We also have cylindrical coordinates. Back when Exelon had its prelicensing application for the PBMR, this was added. That stopped. Internally, we kept doing this development and now, we're doing PBMR benchmark problems with South Africa in Penn State. So the functionality in PARCS is pretty broad in terms of applicability for geometries.

I thought I would show you this slide though to give you a picture of the forest before jumping into the trees a little bit more. Of course, here is your temperature fluid field. But in order to do the neutronics calculation, we need to have really two separate codes.

One, we need to have a lattice code to generate the cross sections, the coefficients for the Boltzmann Equation, and then we need a code like PARCS to do the core simulation. So we have written an interface code we call GENPMAXS that takes the output from the lattice code, processes that, puts that into a format then that is read by PARCS.

The next slide, I think, is a little bit more pedagogical and gives you a sense of how we're tackling this problem. This is the Boltzmann Equation and we're separately doing calculations to get the coefficients for the Boltzmann Equation with those coefficients then solving for the flux with fixed coefficients.

So Oak Ridge has developed a code called TRITON, part of their scaled package. This was done as part of the work for MOX fuel analysis and the TRITON Code developed an output, Tranxs. Again, we read that. We generate interface libraries. We call them PMAXS. Then we're prepared to do the core calculation at any set of thermal-hydraulic conditions that we will encounter.

So these calculations here are performed at several different branches, we call them. This is the burnup of the fuel, but then at a particular burnup we then run a range of fuel temperature, moderator temperature, moderator density, soluble Boron controlled by conditions. So we're running different lattice calculations at all of these points.

Then the objective is to set up a cross section model functionalized such that we can then accept from TRACE or RELAP for every particular node the fuel temperatures, the moderator densities at that node and then use this model with these precalculated coefficients to get the exact conditions at that node. So you know, this is kind of giving you the 10,000 foot view of how we do it.

One of the reasons I have shown you this also is that this has been the standard procedure we have used in the light water reactor neutronics business for 20, 30 years and it has worked well. However, there are, you know, some applications that have come up in the last several years where this two-step process, you know, is starting to introduce errors, you know, that are, you know, non-trivial. Specifically, I will show you in a second, there is the business of now looking at cores one-third loaded with MOX fuel and then also the ACR.

The reason this is a problem because when you do this, this step calculation here, you do this lattice calculation for a fuel assembly with zero current or reflective boundary conditions. So you're looking at it with boundary conditions that are going to be different than those that are actually seen in the core.

For light water reactors over the years, not so bad, because you typically have a loading pattern where you have fresh fuel next to slightly burned fuel, but the gradients at that interface are not so bad. But now with MOX fuel next to uranium fuel, this is starting to, you know, be a problem. So that has been part of, you know, the motivation for some of the work that has been done in the MOX fuel area and in the ACR, and I will get to that in a little while.

Once we have the cross sections, now we can get the temperature fluid conditions from TRACE. Over the years, you know, we have changed the coupling, the way these codes have been coupled. We started out back in '98 where we had, you know, RELAP, TRAC-M back then and we had PARCS, and we actually had a separate module here as the interface. So we had three processes running and that was a little bit cumbersome.

Eventually, we took the interface and merged that into PARCS, so then we were only having two processes. These two processes would communicate using Parallel Virtual Memory, PVM. It's just a message passing protocol.

Finally, this past year, as Joe mentioned, we wanted to eliminate the complexity of even having to have the user use PVM, so we have merged PARCS and TRACE. Now, PARCS, basically, is a static library and it's just linked. So it has, you know, considerably simplified this to the user, but I thought it might be interesting just to see the background over the years.

As Joe mentioned, we have our own graphical user interface. This is QuickWin, which works on Windows. And you can see what we're showing here is real time the -- not real time in terms of the transient, but real time in terms of the --

MR. SIEBER: Everything being together.

MR. DOWNAR: Yes, everything being together during the actual simulation of the code. You can just see this is the power, so I think this is Peach Bottom Turbine Trip and you can just see there's maps. We're on schedule. Chester tells me we're on schedule for July now where PARCS will be integrated into SNAP, so this is the kind of thing we have been using on our own for the last several years.

So I thought what I would do is give you some background in terms of the code assessment mentioned briefly in some of the work we have done here as part of the MOX fuel analysis problem. But more importantly, I think, to you is this word down here. I will just briefly mention the steam line break, because I think you have heard about that and that has been finished for several years.

More importantly, down here, is the BWR assessment. I will show you some Peach Bottom and some ongoing work we're doing with Ringhals, and then I will just finish up mentioning what we're doing now for the ACR. Okay.

So first, the MOX project. We started this project three years ago and the notion here is that when you are putting fresh fuel with enriched uranium, fresh fuel with plutonium, when you put those bundles next to each other, again, you're looking at conditions, which are very different than the typical light water reactor where it's uranium next to uranium. The reason is because the absorption cross sections for the plutonium isotopes --

MR. SIEBER: Are different.

MR. DOWNAR: -- are very different from uranium 235. That gives you significant differences in the spectra. The MOX spectra, because of the larger absorption here, more thermal neutrons are being absorbed.

MR. CARUSO: What's the difference between a fresh MOX bundle and a once or twice burned non-MOX bundle?

MR. DOWNAR: In terms of reactivity?

MR. CARUSO: In terms of the absorption

cross section distribution or the neutron spectra.

MR. SIEBER: Quite a bit.

MR. DOWNAR: Yes. I mean, you have to remember we have always, obviously, been analyzing cores with plutonium. We build that in right into -- when we have fresh uranium, we build plutonium in. The difference now is that we're talking about taking these bundles, let me just skip ahead two slides, whoops, one slide, we're now taking fresh fuel and we're loading it with, you know, 4, 3, 2.5 weight percent MOX.

So to answer your question, the reactivity, you know, is similar in some sense but, you know, the neutronics properties are considerably different. Does that answer your question?

MR. SIEBER: MOX fuel is usually made with depleted uranium and because they do that, that gives you a really low concentration of U-235, which exacerbates the effect of the differences in spectrum and the differences in cross section between the plutonium and whatever remaining U-235 content there is. So that makes it more severe than just nearly depleted uranium fuel assemblies. I mean, the spectrums are different. The cross sections are different.

MR. DOWNAR: Yes.

MR. SIEBER: And you have some other things that go on like self shielding.

MR. DOWNAR: Exactly.

MR. CARUSO: But are the methods that much different?

MR. SIEBER: The --

MR. DOWNAR: The methods that I use in the core simulator?

MR. CARUSO: Well, I'm trying to understand why the methods would be much different for --

MR. SIEBER: They are not.

MR. CARUSO: -- for a MOX bundle as opposed to a once or twice burned non-MOX bundle.

MR. DOWNAR: Well, let me just show you a few more slides. Maybe that will help.

MR. CARUSO: And I was wondering here just looking at this diagram here, you're comparing a core that looks like that's entirely UO --

MR. DOWNAR: This is just a fuel assembly.

MR. CARUSO: Oh, that's a fuel assembly. Okay.

MR. SIEBER: Right.

MR. DOWNAR: This is just the fuel

assembly. So what I'm going to show you is you take this fuel assembly and what we do in this two-step method is we're going to generate then homogenized cross sections with our lattice code using a reflective boundary condition, right?

So in other words, in the lattice code we use integral transport, a very fine detailed representation of the pin, the clouding, the moderator. Then what happens, we take those cross sections to the core simulator. So we're going to do it for this bundle. Then we're going to do it for that bundle also.

MR. SIEBER: Right.

MR. DOWNAR: The problem is that when you look at these bundles in isolation, you get spectra that look like this and like this. You put them together, what happens at that interface, that spectrum is not an asymptotic spectrum.

MR. SIEBER: Right.

MR. DOWNAR: It's now neither --

MR. SIEBER: Right.

MR. DOWNAR: -- this one alone or this one alone. So this boundary condition that we assumed to be zero current, it's no longer useful. You see? So it's that interface.

MR. SIEBER: And it depends on where you are within the assembly.

MR. DOWNAR: Right.

MR. SIEBER: What those look like.

MR. DOWNAR: Right.

MR. SIEBER: So once you're past a couple of diffusion links, the spectrum really changes.

MR. DOWNAR: Right. It's changing considerably as you move.

MR. SIEBER: Right.

MR. DOWNAR: So when you begin to put these bundles into the MOX, into a core one-third loaded with MOX fuel, it's a different problem, if you will, than if it was just uranium. So what we did over the years, we tackled that by increasing the number of energy groups. We have normally been using two energy groups.

MR. SIEBER: Right.

MR. DOWNAR: You go to eight energy groups, you mitigate the problem, because now you're allowing neutron transport to exist between, you know, a whole larger number of energy groups. We also added a transport capability. Normally, we use P1 theory. Now, we're using SP_3 and doing pin-by-pin analysis.

So very briefly, I will just show you some assessment we did using critical experiments that were performed in Belgium, and then I will show you a transient benchmark problem that the OECD/NEA, we proposed and was accepted by them.

This is just showing you the equations that we're solving. Normally, we just solve the first two equations, so those would be the P1 equations. Now, what we're doing is we're adding the third and the fourth equations. And you can see what we do is we combine these into two equations such that this first equation is the diffusion equation if I drop, you know, the second moment or if I drop this moment and this moment. That's P1 theory.

What we're doing is now solving an additional equation and putting them in this forum allows us then to use the same machinery, in other words use the same set of solvers that we have had all along in PARCS.

This then we apply, you know, to benchmark problems. This is a classic benchmark problem that has been performed by many of the European countries who have been recycling plutonium into their cores. That was, in fact, the motivation for this benchmark in Belgium. You're looking at plutonium assemblies here. These are uranium and this is a reflector, baffle reflector. So at this interface, you have very significant spectral differences.

MR. SIEBER: It makes a difference where the plutonium came from, too, right?

MR. DOWNAR: Well, this is reactor grade.

MR. SIEBER: Okay.

MR. DOWNAR: This was done with reactor grade.

MR. SIEBER: Well, that's different than weapons grade.

MR. DOWNAR: I agree, yes, absolutely. But we did this problem, because this was the only critical --

MR. SIEBER: Right. That's the only data you have.

MR. DOWNAR: Right.

MR. SIEBER: Okay.

MR. DOWNAR: And this slide is a brief summary that here is the conventional method, and you can see that the errors in prediction of the pin powers is on the order of, you know, 4, 5 percent. By using the higher number of energy groups, you know, the error is significantly decreased.

Down here you're looking at one of the pins that was measured, and this is the axial position, and along the length of the pin you're looking at the difference in the prediction of PARCS compared to the measurement, and you can see plus or minus a couple percent. So, you know, the bottom line here is that these methods were put in to analyze the MOX core and now, there's confidence from doing the critical experiment that we can analyze one-third loaded MOX core.

So the next step was to look at something that might be of interest, specifically a control rod ejection from a one-third loaded MOX core could be more. You know, you don't know, but it could be more severe than a uranium fueled core. Beta effective, you know, will be smaller in a plutonium core. So then you say well, let's take a look and see, you know, what the behavior is.

The way I thought would be appropriate is you get a large community to do the same problem. You develop a benchmark and then you go around the world and you ask users would you do this problem, and you get people with similar methods and see how they agree.

So we proposed this back in 2001 to the OECD. We developed a MOX rod ejection problem. It's from a core, 4-Loop Westinghouse PWR, similar to the core that will actually be used by Duke Power, and we have a scenario again where we're ejecting a control rod, you know, from this core.

We have now more than 10 participants, and I think that's, you know, a critical number if you have at least 10 people doing the problem from Japan, Russia, some European participants, Korea as well as ourselves, and people doing it with methods, various types of methods. So where we are now is we have, as you can see, several results. We're gathering results. We will have by the end of the spring prepared a report. In June there is a meeting in Paris and we will present our results.

Here I'm just showing you some preliminary results for the rod ejection. The rod ejection, as you might remember, is a super prompt critical event where the assertion of more than a dollar Doppler takes over and of interest, of course, is the energy deposition before the Doppler takes over. And I'm just showing you here that we have Korea, the Swiss and ourselves all using two group nodal methods and we agree pretty well.

But now, we ran the same problem with our SP_3 solver and you can see that the prediction here is for a lower peak, little bit delayed. So the results are different depending on, again, the order of the method. So what will be interesting --

VICE CHAIR WALLIS: Doesn't SP_3 have better physics?

MR. DOWNAR: Yes, SP_3 is a transport.

VICE CHAIR WALLIS: Right.

MR. FORD: So does that mean that everybody else is wrong apart from the SP_3?

MR. DOWNAR: It doesn't necessarily. Well, I think, again, two group diffusion theory predicts that and these are different codes predicting the same thing. So we feel that okay, if that's, you know, the accuracy, the order of your method, that's what you're going to get and that's comforting. But obviously, as you use a higher order of method, you know, what we're seeing here is that, in fact, transport solution is saying that the peak is lower.

MR. SIEBER: It makes a lot of difference.

MR. DOWNAR: What we're expecting is we're going to have people doing this problem with integral transport, with SN methods and, you know, by the time we finish we're going to have, I think, a very good picture. So that's --

VICE CHAIR WALLIS: Is there any reason why things should happen slower with SP_3?

MR. DOWNAR: Well, there's a simple model, point kinetics. In the point kinetics, you know, the Nordheim-Fuchs Model in point kinetics allows you to relate the peak and the time of the peak to some of the simple kinetics parameters, the prompt neutron lifetime, you know, the initial reactivity insertion, the beta.

The thing that would cause this to be a little bit slower is the prompt neutron lifetime, see? So what you're doing here is you're saying that, you know, these are solutions with spacial kinetics, but you can take that solution and you can exactly then compute a core averaged prompt neutron lifetime.

And if you do that, I haven't done it, but if you do that, I suspect what you would see here is the prompt neutron lifetime is a little bit, you know, larger, so the core response is being predicted to be slower. But that's something that we should do, is compute these kinetics parameters, you know, for each of these and then compare those.

MR. SIEBER: The time of the peak and the height of the peak is related as much to the thermal-hydraulic properties as it is to the nuclear properties.

MR. DOWNAR: The height of the peak is

very dependent upon the Doppler feedback.

MR. SIEBER: Right.

MR. DOWNAR: Exactly. And the prediction of that -- again, this event is driven solely, you know, by the fuel temperature.

MR. SIEBER: Right.

MR. DOWNAR: None of the moderator effects.

MR. SIEBER: It's too quick for that.

MR. DOWNAR: Just too quick. So you know, but what we have done in this benchmark problem is we specified, you know, the conductivity, heat capacity, so we have, you know, very carefully made sure that that is not a parameter that will be varied by the users. They are going to use that of the specifications.

Their solution of the conduction equation, how they do that, that could be a little bit different. But these are our three solutions, right, so we know that we have been solving the conduction equation the same way.

MR. SIEBER: Okay.

MR. DOWNAR: And that will not cause a difference, so we believe that difference is just because of a transport effect.

MR. SIEBER: Right. Interesting.

MR. DOWNAR: So very quickly, I will just go over their first application, and I think you might have seen this in the past, just showing you what we have done for the PWR and then get to the more interesting problem.

Three, four years ago a problem was designed in order to, you know, do a numerical benchmark of coupled codes and I think, you know, again, you're probably familiar with this, rupture of a steam line on the secondary side causes an overcooling of the primary fluid, which then leads to a positive reactivity insertion driven by this Delta T of that primary fluid.

This is the intact loop. This is the broken loop and this Delta T then is the driver for the positive reactivity. And of course, when you scram, the power comes down then causing positive effect from the Doppler. You're getting back, you know, the Doppler broadening scram. And then the combination of these gives you then a total reactivity. And this is a relatively slow event over 100 seconds.

What's of interest here is what happens in the core as you start to see this reactivity rising.

This problem was designed such that the reactivity never went to zero, never got positive. It's always negative. And they designed the problem, because we're able to, obviously, artificially adjust the worth of the control rods.

This was an interesting problem, because when the problem does not go back critical, you still see the power in the reactor rising. Okay? So the problem never comes back critical, but after scram the reactivity is increasing, cold water continually coming into the core and eventually, the power comes up.

Point kinetics would be able to predict that. Again, your core average power here could be 20, 30 percent. However, we know that this event is analyzed assuming the highest worth rod is stuck out of the core. So therefore, we know that what's going to happen is that 20, 30 percent power will not be distributed symmetrically. In fact, it will be concentrated over here where this stuck rod is out of the core.

So we can do a simulation. This is showing you the initial steady-state condition and these are the representations of the different fuel bundles, and then this is just the time of the event.

So you remember the event runs for 100 seconds. So now, we can just take a look at the simulation and see now the power comes up, scrams and now, the cold water going in.

What's interesting, you can see even though the core average power is only about 30 percent, you can see relative power of the peak bundle went up to over 1. The relative power exceeded 1. And again, this is even though the core was never critical. So this is the average bundle power. Obviously, what we could also do then is reconstruct the pin power. This is one of the advantages of spacial kinetics.

MR. SIEBER: When you say it never went critical, but the power is increasing --

MR. DOWNAR: Right.

MR. SIEBER: -- which to me says it has to be critical during part of that transient.

MR. DOWNAR: It never went.

MR. SIEBER: Why does the power go up?

MR. DOWNAR: Well, there is a simple little prescription. If you go back again to, you know, the point kinetics and you use something we call the prompt jump approximation, eventually you can write an equation. Okay. So this is power.

MR. SIEBER: Okay.

MR. DOWNAR: And this is again out of the point kinetics, but you can write an equation that looks like this where this is the reactivity and, of course, this is the beta effective.

MR. SIEBER: Okay.

MR. DOWNAR: This is the K constant of the delayed neutrons.

MR. SIEBER: Okay.

MR. DOWNAR: This expression shows you that this is, though dark, I mean, this is the -- so this would be a rate, rate of change --

MR. SIEBER: Okay.

MR. DOWNAR: -- of the reactivity.

MR. SIEBER: Okay.

MR. DOWNAR: What happens is we can have this to be negative, but if this is positive, we can have a positive period. Okay? So it's possible. Physically, what's going on is, you know, we have here, this is the instantaneous reactivity. You might think of it, after we have that initial spike, we create, you know, a lot of precursors distributed around the core.

And they are all sitting there, right, with their own little half lives, you know, to decay, to emitters. We have got things like bromine 87, which had a half life of about 70 seconds. So they are just sitting there, you know, waiting to emit neutrons.

MR. SIEBER: Okay.

MR. DOWNAR: They will not emit that neutron until 40, 50, 60 seconds. They will be multiplied depending upon the local conditions when they actually, you know, appear. So the multiplication of those delayed neutron sources, if you will, are going to be dependent upon, you know, the rate of change of the reactivity not just the initial precursor distribution.

So again, this comes out of, you know, any classical kinetics book, but it's possible that the reactivity can never be positive. But if the rate of change is positive, and that's what we saw, you know, from --

MR. SIEBER: Prompt, yes.

MR. STAUDENMEIER: This is Joe Staudenmeier. From classical static reactor analysis, there is something called subcritical multiplication.

MR. SIEBER: Right.

MR. STAUDENMEIER: And if you want to know, you take one neutron. That will turn into 1 over 1 minus K neutrons due to subcritical multiplication. So you're increasing K while keeping it below 1, but the multiplication on a given neutron from the source is increasing as you go through that.

MR. DOWNAR: So it's counterintuitive, but it's interesting. But this again, this was a numerical benchmark.

VICE CHAIR WALLIS: This is all because of the delayed neutrons?

MR. DOWNAR: Right. Exactly. So perhaps more interesting to you is the assessment of the coupled codes for BWRs, which is a lot more challenging.

We started out actually three years ago with doing the Peach Bottom Turbine Trip, which was an NRC sponsored coupled code benchmark. And the Peach Bottom Turbine Trip, I'm sure that you have heard about it, but it's sudden closure of a Turbine Stop Valve.

The pressure generated unabated into the core leads to a void collapse, a reactivity insertion. Then it's mitigated or it's eventually ended when the bypass valve opens. But what we did is we started back in '02 to do this problem with TRAC-M and PARCS, and let me just briefly show you some results from that.

What we used is a TRAC-B input deck. So it's not TRAC-B the code. This is TRACE Version 3950, but it's TRAC-B input deck, which TRACE, you know, can process. And this is the result that was published. In fact, it just came out this past October, worked on it with students. Also, I should mention I have been collaborating on all this BWR assessment work with Professor Ivanov at Penn State and his students, and then Tony Elsis from staff also worked on this with us. So with Version 3950, we got a result.

MR. STAUDENMEIER: Actually, it's not that base 39. There were changes made to 3950.

MR. DOWNAR: Right.

MR. STAUDENMEIER: There were special changes added into the code.

MR. DOWNAR: Right, right. We took some of the two phase models from TRAC-B, correct, and Tony put them into 3950. And then this is just showing you the comparison of the measured LPRM data again to TRAC-M. So the results here were pretty good.

CHAIRMAN RANSOM: This is presumably TRAC before you had all those improved interface drag models and all that stuff?

MR. STAUDENMEIER: No, it isn't. There

were some improvements stuck in a special version and, actually, some of them were implemented incorrectly, so that version has some errors in it, and I think there was some hand-tuning of the results that went on to get results that good.

MR. DOWNAR: Yes. The other part of this is that it's easier to do Peach Bottom than stability. This is very quick. This is a one, two second problem. So you know, this problem compared to what I'll show you, you know, is fairly straightforward and perhaps some of the areas in the models didn't really show up.

What we're doing now is going to a TRACE native input deck and you can see, you know, this is 33 channels, meaning that what we did is we took the 740 some odd bundles in the PARCS model and then mapped them to the 33 channels. So this model now is being used.

The first part of it was to just not do the coupled problem, just used a fixed power distribution versus time in the TRACE. Okay. So this is TRACE stand alone and this was Version 4068. And again, in this model we have also added, let's see, I'm trying to remember, but I think 4068, we added, I think, to this one the wall drag model from TRAC-B.

MR. STAUDENMEIER: And the interfacial. Yes, the interfacial and the wall drag has been changed over the base version and, actually, those changes will be going into code release that will soon happen, so it will be the base version of the code soon.

VICE CHAIR WALLIS: Now, what is this here? This is one code versus another code? Is that what this is?

MR. STAUDENMEIER: No.

MR. DOWNAR: This is actual experimental data. I'm sorry, I didn't show the legend.

MR. SIEBER: Right.

MR. DOWNAR: But this is the actual.

VICE CHAIR WALLIS: That's an experiment?

MR. DOWNAR: Yes. This is actually the measured data from Peach Bottom.

VICE CHAIR WALLIS: A large transient test.

MR. STAUDENMEIER: It's a real reactor. They instrumented it.

VICE CHAIR WALLIS: Are we required to trail the --

MR. SIEBER: No, we don't have to.

MR. DOWNAR: And this is just showing the power pulse. And again, this is from a version. This is coupled TRACE and PARCS. This one, again I didn't show it here, but this is from Version 4111, you know, and what we have in this version again is the TRAC-B wall drag model.

Now, the idea here was for us to be able to set up these models and to foresee with doing some of this analysis. Even though we knew that, you know, some of the models in TRACE would continue to change, we wanted to have, you know, the input decks, all of the methodology there. So now when the new wall drag model, the new models appear, we're ready and all we have to do then is run them.

The more challenging problem has been stability. A very well-instrumented set of data was Ringhals. This was a benchmark and it was actually started back in the early '90s and it's a Swedish BWR. Over cycles 14, 15, 16 and 17 they performed, you know, several tests, I think a total of 41 tests.

And what we did is chose two points, as I mentioned earlier, because we wanted to look at one point that was dominated by in-phase, so point 10, you can see the decay ratio .71, the regional decay ratio, .63. So we wanted to run one point where the global dominates and then another point, .9, where the regional dominates, .99.

And so these two points we have attacked initially. We started this about a year and a half ago and it has taken us until now really just to get this guy. And again, I will show you preliminary results. But more importantly, as I'll show you, the methodology is there. The machinery is there and I will explain in a minute what I mean by that. But .10, the in-phase, is the one we're tackling.

The Ringhals core is quite a bit different than the Peach Bottom core and in that sense it provides a good core to be assessed by TRACE/PARCS. Ringhals, it's an ABB design. This about 2,200 megawatts thermal. Peach Bottom is a GE4, about 3,200, 3,300 megawatts thermal. This is smaller, obviously, about 640 some bundles. This is larger. This one has the internal jet pumps. So there's sufficient differences that they provide, I think, you know, a reasonably good tool to assess TRACE/PARCS. And then obviously, also, both of them are plant data.

So initially, we just took a fixed power distribution, put it into just initial mapping where there were 20 channels and this was, again, trying to first just assess the Ringhals TRACE Model without being coupled to PARCS, and you can see from comparison here that results are pretty good. Actually, Claudio Delfino, I think, did this work when he was still at Penn State a couple years ago.

MR. SIEBER: That, I suppose, is to me almost unbelievable. I mean, it's right on top of --

MR. DOWNAR: Yes. So this is core averaged.

MR. SIEBER: Yes.

MR. DOWNAR: So the distribution, you will see some of the distributions are not -- so that was just to look at the TRACE Model.

Then we wanted to assess the PARCS model, because here what we're talking about is a pretty sophisticated cross section library in order to model this core, and we wanted to make sure that we were processing the library correctly and we also wanted to get a feel for the accuracy of the PARCS solution.

So what we compared this to, their core simulator, when I say experimental that means it's their core simulator from Ringhals, and we input the temperature density from their core simulator into PARCS node-wise, so we wrote a processor to take that information for all the 600 and some nodes times 24 or whatever that number is. So we processed that into PARCS and then we compare, you know, the power distributions as well as the node-wise power.

So this gave us confidence that stand alone PARCS was correct and then stand alone TRACE was correct. Then the problem really became now trying to come up with a model that we could use to analyze the transient. This is where the issue now of how many channels do we need in order to do this? Initially, we used 20 channels. This was the initial model that I showed you with the fixed power distribution.

A Japanese researcher named Hotta was the one who came up with this initial design. On the next slide you can see this is how he came up with these 20 channels. He was interested in modeling this harmonic, .9 there's an azimuthal harmonic. So knowing the harmonic, he decided what he would do is he would then choose the mapping of his neutronics nodes, every one of these is obviously neutronics nodes, at two, in this case 20, you can see it starts from 11 and goes to 30, would map to the thermal-hydraulics using this distribution.

VICE CHAIR WALLIS: If you know the harmonics, and you do, I presume.

MR. DOWNAR: From the steady-state solution --

VICE CHAIR WALLIS: If you don't --

MR. DOWNAR: If you don't, you're out of luck. You're right.

VICE CHAIR WALLIS: Right. And you have to have many more channels.

MR. DOWNAR: Right. So that's the real problem with this mapping. A priori, you have to know.

VICE CHAIR WALLIS: Know the answer.

MR. DOWNAR: You have to know the answer, which makes no sense. So therefore, we began looking at mappings that were using a more fuel property-based, geometric-based mapping. So we started out with 128 and kept adding fidelity. We needed to model each one of the bundles different because of their orificing.

We made it to 486 channels where here every channel had its -- every neutronics channel had its own thermal-hydraulics channel, except for the core periphery, I should have that here, except for the core periphery, so we modeled all the periphery into the same channel, because they would be orificed similarly.

Finally, 645, you just say forget it. Do one to one. So this slide here I just showed you, this is the 204 channel mapping, and you can see we're mapping sectors. 486 channels, now we're grouping, and you can see where this is going, is that you want to try to, you know, increase the number of thermal-hydraulics channels, but your tradeoff is going to be computational time, memory and computational time.

VICE CHAIR WALLIS: Does this get the same answer then?

MR. DOWNAR: No, it doesn't. I will show you two slides. I will show you that. First, I will just show you what happens to your computational time. This is showing you the initialization and this is for .10, so we're just running a 1,000 second null transient, just the null transient and using a PC and this is Version 4.036.

So we start out with 204 channels and we eventually then get all the way to one to one mapping here. This is the runtime of PARCS. This is the runtime of TRACE. And you can see what happens. Initially, you know, PARCS is taking, you know, quite a bit more time, because what we are doing is modeling every individual node, right, whereas here we just have 204 channels.

We increase pretty much asymptotically, because we're still solving the same problem regardless of what TRACE is doing. Our runtime is increasing because what's happening, we're having to process more information as we increase the fidelity of TRACE. Now, we have to process more node-wise, if you will, quantities, right? We're getting more temperature fluid information back.

TRACE increases pretty much exponentially because, you know, John would know better than I, but I think what's happening here is the linear solver, right, we're going as n squared, n is the number of channels, so you expect this to be increasing. So you can see eventually, we get out here to one to one mapping and it's almost a break even.

But there is some good information on the slide, because it's giving you a feel for how much time does it take to initialize, and initialization is the most important or the most time consuming thing and, I guess, most important. It's taking you here about, you know, six, seven hours to initialize, because this is a converged solution. We're about 10 to the minus 4^{th} in the vapor velocity converged here. And you can see that out there at the larger problem, you know, you're up over, you're up close to 17, 18 hours.

Now, I should point out that this is with Version 4.036. What we have found is interesting.

Back then we weren't getting very good decay ratios. Our decay ratio predictions were about .4, .5. The interesting thing is as our decay ratios have gotten closer to the experimental values, about .7. It's taking longer for us just to initialize and that kind of makes sense, right, because as the core becomes less stable, it's going to take a longer null transient in order for a lot of those perturbations to settle out. So now, you know, the runtimes to converge are actually maybe 50 percent higher than this, so it's taking us now, you know, about eight hours, nine hours to initialize.

I will show you some results for the in-phase oscillation. We're using the 204 channel model. Okay. We find that, you know, this is accurate enough for the in-phase. For the out of phase, we'll probably have to, you know, head over there.

So now, the question is in terms of accuracy. What we're showing here is the axial power profile and then just showing here the RMS value for the axially integrated radio power distribution for 204, 486 and 648 channels. And what you'll notice is that, you know, this is their core simulator, Sim-3K. What you will notice is we go from 204 to 486, we see, you know, a nice increase in the accuracy. But we go from here to here, it doesn't improve much.

So again, what we have chosen to do for the in-phase is to stay here, okay, just because of runtime considerations. Eventually, what we will obviously want to do is run the final solution down here. So does that answer your question?

VICE CHAIR WALLIS: So it does sort of converge?

MR. DOWNAR: Yes, yes, it does seem to converge.

VICE CHAIR WALLIS: And Sim-3K is the right answer?

MR. DOWNAR: We're using that as a reality check. Obviously, the right answer is the LPRM data and, you know, we have not compared to the LPRM data yet, but that's the right answer.

And this just shows you how the runtime within PARCS breaks down. This is the 204 channel. You know, we're spending a lot of time processing cross sections. CMFD, this is the course mesh solution. One of the nice features of PARCS is this nodal solver. That's the actual high order solution. We only do the high order solution when, you know, the accuracy requires it, in other words when something is significantly changing in the core. Otherwise, the finite difference solution is adequate. But that's just giving you a breakdown of the PARCS runtimes.

So now, the real proof, if you will, is in the ability of this model to do an actual stability simulation. In order to do that, we had to do some things with the model. One of them was to use a variable axial nodalization. We're using semi-implicit numerics and John will talk more about this, why we're doing this. But we use the variable axial nodalization in order to minimize the numerical diffusion. We want to run with the CFL close to 1. So in order to do that, we increase the node size as we get higher up the core.

To initiate the instability, this is interesting, because most of the participants in this benchmark, they will move the control rod to start the instability and I will show you, you know, results with that, because that's what we did initially also.

However, we wanted to be able to start the instability, you know, with other methods. We wanted to do a pressure perturbation, change the pressure out there, you know, at the end of the steam line and let that propagate into the core. But more importantly, we wanted to be able to do this experiment the same way that they did it at Ringhals, which is basically noise analysis. They just looked at noise and from that extracted the decay ratios and the frequencies.

So one of the things we did with PARCS is we introduced noise into PARCS and did the problem that way. The logic here is obvious, that if you can initiate the instability three different ways and get similar answers, that's going to give you confidence that your solution is not dependent upon how you're starting the instability.

The other thing is post-processing. Decay ratio, obviously one of the possibilities is you just look at the amplitude, you know, of the oscillation and pick off from that, from successive peaks, pick off from that the decay ratio. A more precise method is to use Auto Regressive Moving Average technique and we'll show you results from this.

What we did is we developed a set of post-processing tools at Purdue and our colleagues at Penn State did also, so we both have then post-processing capabilities and we run them independently with the output to make sure that, again, it's not dependent upon this. So that is kind of the methodology that Joe spoke of.

All of this machinery, you want to have it in place and once there is a change in the code, all of this machinery is there already to be able to then look at, you know, how the --

CHAIRMAN RANSOM: Have you ever used the fast 480 transforms to look at the decay ratio and frequency?

MR. DOWNAR: Not to my knowledge. No, I don't think we have.

CHAIRMAN RANSOM: Of course, and what did you mean by noise analysis? Is that --

MR. DOWNAR: I will show you in a second.

CHAIRMAN RANSOM: -- neutronics?

MR. DOWNAR: What we do is we introduce random noise into PARCS using the moderator density variable. We're not physically changing the thermal-hydraulics conditions. We're just introducing noise that, basically, will try to reproduce what was going on in the plant. I will show you that in just a second.

So this is the first solution. I shouldn't say the first solution. Over the last years there have been many solutions, but this is using the control rod perturbation with, again, Version 4111m1 and when I say mod 1, this is the 4111 with the wall drag model from TRAC-B because, again, as the TRACE wall drag model has been changing, we still wanted to be able to continue our work, so we use the TRAC-B wall drag model.

So initially, we run just a null transient for the first 10 seconds. We have initialized the core, so we have run the 1,000, 1,500 second null transient. We now restart and then when we restart, we go for 10 seconds. Now, what we do right here then is, as you're looking at the map, in the lower south center region, we move a control rod in and out. We do that in less than .2 seconds and the worth of that rod, it turns out, is about .60 cents.

So we create a perturbation in the core, and then watch what happens. And you can see that we're getting here then -- this is the power and you can see that we're getting a decay ratio prediction here of about .7 and a frequency of about .56. And then using the ARMA post-processing code, this 30 and 50, these are just different order FITs, if you will, and it's giving similar answers.

That compares then to the final report, the measured values close on the decay ratio, but the frequency, we over-predict the frequency a little bit. In the final report, it has reported the uncertainty in the decay ratio to be about, oh, .05. And these uncertainties are really about this ability to extract from the actual noise the decay ratio, because the actual event, again, is just processing this noise using auto regressive methods. And so therefore, the authors of the benchmark problems indicated the uncertainty would be between 5 to 10 percent as I showed you.

Therefore, what we felt would be appropriate is for us to actually, you know, reproduce the event as best we could using PARCS. And in order to perform noise analysis, the cross sections are perturbed by changing the moderator density, and this change is only for the cross section evaluation. The real thermal-hydraulic properties are not changed.

And when you make this change, you basically are introducing, you know, into the core a perturbation that will be based upon, a priori, you know, knowledge of the fundamental mode, of the power distribution. But also what you want to do is generate random background noise, so there's a random number generator that also generates or contributes to the noise.

So you put this into PARCS and we chose the amplitude and the frequencies based upon what we observed from the actual data, the Ringhals data. The actual Ringhals data was in the range of .01 to 1 hertz. Okay. And you can see this is the PARCS noise simulation, again running a null transient for 5 seconds then generating noise at these amplitudes.

Now, just using ARMA to process that data, and again this is Version 4111m1, so this is TRACE with the TRAC-B wall drag model and the decay ratios are .668, .67. So they are a little bit under-predicted. And the frequencies, again, a little over-predicted from the actual values.

So I guess that's where we are, you know, with the stability. The plan is to continue this work and now in Version 4150, there is a new wall drag model. So that wall drag model will be used and the work would proceed. We have not tackled .9. We did some preliminary initialization, but we'll do .9, begin to try to look at results for the out of phase oscillation.

So I thought what I would do is finish up with some slides looking at, very briefly, modifications we made for advanced fuel types, but then showing you a few slides about the ACR-700. The conventional BWR designs that we have been looking at, the 8x8 GE design, have not had large, internal water holes.

Large, internal water holes now introduce an additional complexity, because in addition to having an external bypass region, now there is this internal water region, so there is some question when you begin to bring a water density into your feedback model, how do you weigh, if you will, the water density around the fuel rods, this water density and the water density in the channels?

You want to make sure that you're getting appropriate feedback. This is true of also the ATRIUM-10 fuel design having a large water region in here as well as the SVEA design. So you know, basically, it's a minor modification, but it's something that NRR requested and we now then, again, treat separately the water region internally and then the water region external to the can in order to get the density that is passed from TRACE to PARCS to then be the feedback into the cross sections.

We have also made modifications to PARCS to be able to model the ACR-700. As you can see, I mean, we're looking at a core that's, you know, very different from those that we have analyzed in the past when it comes to the reactivity control devices. They are coming in from the side and, you know, this is really, you know, something that we have to make modifications to the neutronics in order to do this.

We also have to make modifications to the cross section model, so I will just briefly walk you through some of those.

Again, as I mentioned, the control rods in the PWR, you know, moving in vertically, but you're seeing very different movement of control devices and this requires us to rethink the logic by which we perturb, you know, the cross sections in the core. They have also something that are referred to as Zone Control Units in the ACR. These are units that are sitting there in the core during the depletion and these Zone Control Units again are coming in from the sides, we do have to model these.

So there's several unique things about the ACR that have required us to make changes. We have, you know, had to make our cross section model much more sophisticated, if you will. We have to treat many more feedback variables. The conventional model that we have had for light water reactors has just had five variables.

For the ACR now we have had to add a feedback variable for cooling impurities, for moderator density, because now what we're looking at is the moderator and the coolant are separate. The coolant is light water. The moderator is heavy water and we have to be able to treat those as separate variables. So moderator density, moderator temperature, impurities in the moderator, different soluble poisons in the moderator.

But this 12th and 13th ones are the most interesting, because what became clear last year was that the neutronics problem for the ACR is considerably more sophisticated and complex than anything that we have tackled for the light water reactor.

Specifically, what's interesting, by now I think you might have heard about this "checkerboard" voiding in the ACR. Here I'm showing a 2x2 array of fuel bundles and you're seeing here this is the CANFLEX design and this is the central pin, which has dysprosium and then you have rings of pins, slightly enriched uranium.

And what has happened is that it has become clear that if you voided two channels in a checkerboard configuration and kept these two channels cool, you get a coolant void reactivity that is different than if you entirely void the configuration. So this negative coolant void reactivity, in fact, is positive for practical scenarios that would show up if you get a header break, because you have half of your bundles being cooled in one direction and half in the other direction.

What this does is this complicates the modeling from a neutronics standpoint, because if you go back and you remember that two-step method, what we would normally do is we would take one bundle, isolate it, put zero current boundary conditions on it, right, then homogenize cross sections. We would want to take that bundle then into the core, right, and show it a boundary condition now that is very different. That gets exacerbated by the voiding problem, because if you void one of these channels, the boundary that this bundle sees, if you assumed over here it was cooled, is very different and that led to significant sources of error.

So what we have done is, in collaboration with staff, Don Carlson, we have come up with models to treat this where what we're doing now is we're adding the complexity where you don't just look at the properties of the individual node itself. Now, you have to see who's next to you and then based on who's next to you, you then add an additional perturbation to the cross section.

So in PARCS never before when we did feedback have we cared who's -- all you care about is the properties of the node itself. Now, you have to look next door and then, based on who's next to you, you process the cross sections.

This, I'm just showing you the complexity has increased, because now when we generate cross sections, we have to generate a matrix of cases for all of these possibilities of who's next to you. You run a 2x1 where the other guy, right, has to be an assembly that voided 75, 50, 25, right? The node itself is 75 and then that combination next to it. We also do this for various burnup states. The point being here is that the library that we have to carry now in order to analyze the ACR increases in complexity significantly. So that's the object of that slide.

ISL has developed a TRACE Model of the ACR and here you can see this is the core itself down here where there's 284 channels. 142 are being fed by this header, 142 by the other header, so if there is a header break, half of the channels will be voided. The other half would still be cooled at least for a few seconds, so the checkerboard configuration, in fact, is real.

And then so the PARCS model would look like this and we would be mapping PARCS into TRACE.

So this work has progressed and the coupling of the models should be ready by next month.

VICE CHAIR WALLIS: You don't have the answer yet? No answer yet?

MR. DOWNAR: No answer, no answer yet, no. We have got the cross section model set up. We have the TRACE Model set up and now it's a question of -- and we're working again with staff in order to come up with the scenarios on this.

MR. SIEBER: Doesn't sound like it will be a good answer.

MR. DOWNAR: No, I think there's going to be --

MR. SIEBER: Could be a wild transient.

MR. DOWNAR: Yes. It should look a little bit like, you know, what we have been seeing in the turbine trip. You're going to have a power pulse.

MR. SIEBER: Right.

MR. DOWNAR: That will then be turned around.

MR. FORD: But presumably AECL have got their own model and you know of that model. I'm assuming that's true. How does it differ from this?

MR. DOWNAR: Well, I --

MR. FORD: In terms of physics, in terms

of --

MR. DOWNAR: If I could have Don maybe to talk about what AECL has. I'm not sure. I have not been talking with AECL about their model. I have their reports, but I have not, you know, looked closely at how they are tackling this checkerboard voiding scenario.

MR. FORD: So it comes down at the end of the day, which is correct.

MR. DOWNAR: Yes.

MR. FORD: Is that not true, against observation?

MR. CARLSON: Yes. This is Don Carlson in Research. AECL has their own suite of codes that they have evolved in the past for analyzing conventional CANDUs and, as it turns out, the zero current boundary condition, the simple methods, relatively simple methods that we have used for light water reactors are a lot like what they were able to use for conventional CANDUs.

The move to the ACR has given them significantly different physics and they ran into the same kinds of problems that Tom has described here, and they are doing different things to address it. They seem to recognize the problems and it's a work in progress as to, you know, what exactly they are doing to address it. But yes, they have to evolve their methods much like Tom has had to evolve his.

MR. FORD: Good. They must be considerably further advanced than we are in their methods, no?

MR. CARLSON: No.

MR. DOWNAR: No.

MR. CARLSON: Certainly not.

MR. DOWNAR: So finally, just a couple more slides, some things again that show what we're doing. We have completed our Theory Users Manual. We update those as we have come up with new versions. We're working on a Programmers Manual. We're trying to chip into the CPU time, cross section processing, coupling. TRITON is complete. We're looking to couple to a commercial code. So these are some other things.

I have mentioned that we have a user base. This is just giving you a snapshot of who they are. We're actually over 50 now. I didn't update this slide, but we have got some more users, some in Europe, some laboratories, and they communicate with us through our website. And we have a listserv mailing list, so anyone that wants to participate in that is then disseminated all of the information on the latest updates from PARCS on our website.

VICE CHAIR WALLIS: When you say you have 45 members, are they actively doing work or are they just sort of listening to what you're doing?

MR. DOWNAR: They are actively doing -- many of them are actively doing work. We get a lot of questions that are only coming from people that are actually doing work, because they find bugs. They find problems, you know.

MR. SIEBER: What's Armenia doing?

MR. DOWNAR: I'm sorry?

MR. SIEBER: Do they run plants?

MR. DOWNAR: Who?

MR. SIEBER: Armenia.

MR. DOWNAR: Actually, yes, they have VVER.

MR. SIEBER: Oh, okay.

MR. DOWNAR: Yes, Brookhaven Lab did some training of the Armenians with RELAP/PARCS.

MR. SIEBER: Oh, okay.

MR. DOWNAR: So they have got VVER 440. But there is quite a bit of work going on out there with PARCS coupled to RELAP and with TRACE. Any other questions?

CHAIRMAN RANSOM: Well, thank you, Tom.

MR. SIEBER: Yes, great presentation.

CHAIRMAN RANSOM: Looks like you got us back on schedule.

MR. DOWNAR: Well, John was chomping at the bit over there.

CHAIRMAN RANSOM: Well, we can take a break until 3:15.

MR. SIEBER: Great presentation.

CHAIRMAN RANSOM: And start back with John on schedule.

(Whereupon, at 2:58 p.m. a recess until 3:16 p.m.)

CHAIRMAN RANSOM: Okay. We're ready to start. The next speaker is Professor Mahaffy from Penn State. I think, I guess, responding a lot to some of the anonymous letters that we received here, I don't know that anybody here has really analyzed those, so I hope you will explain to us what it was all about, and I guess what the concern was and whatever resolution there is of it.

MR. MAHAFFY: Well, I had assumed that you paid attention to letters to that and you were ready to ask me questions. But before I go into my prepared material, I would like to pick up on something that came up this morning in the beginning. The question was asked what is taking so long with this TRACE development. And as somebody who has been at it from the beginning, the beginning, in fact, dating back to when you, Rick and I were sitting on an Advisory Committee with Farouk, a very long time ago.

VICE CHAIR WALLIS: Is this where we tried to persuade him to develop a new code instead of pulling together these old ones?

MR. MAHAFFY: Well, in fact, if you really take a serious look at it, it is a new code now.

VICE CHAIR WALLIS: Oh.

MR. MAHAFFY: And, you know, that's a side topic that we can talk about for an instant. But if you look at what was really laid down, there was a two stage process with what has become known as TRACE. There was the consolidation effort and the guidelines that were given initially on consolidation was that this product would capture all the capabilities of the predecessor codes, which have been named.

VICE CHAIR WALLIS: Yes.

MR. MAHAFFY: And when it was done, it would perform as well or better than any of the predecessors. Those were the basic guidelines in a nutshell.

VICE CHAIR WALLIS: Are you talking about the breeding of two dinosaurs will only produce another dinosaur?

MR. MAHAFFY: Yes. And this is stage two now.

VICE CHAIR WALLIS: Right.

MR. MAHAFFY: Okay. Stage two is now that you have something that is manageable, you move on to advanced development and produce things that answer concerns that have been raised by you, Graham, and Vic and lots of other people in the past to try to improve your capabilities.

In terms of delays of the process, you've seen some of them discussed already, and I identify really three of them. If you wanted to say okay, well, they promised this and this done in X number of years and now it's Y number of years out, the first big thing that really introduced extra time and development effort was the change in the statement about capturing capabilities, and that was a rigorous card for card ability to reproduce RELAP5 input, but the TRAC-B actually got added on later on, too.

That, as was indicated, took time. Another key item that has been hinted at is the question of physical model development. When we talk about original technology, the original plan was the kind of mating of dinosaurs that you were talking about. There would be a process by which we would go through and extract the physical models from the predecessor curves that we designated to be the best of those available and plug them into this code.

Now, NRC, and I believe driven by Joe Kelly as much as anyone else, wisely decided at some point let's not do that. Okay. Up front I made a list of models that we could pull out of RELAP5 and I think my list probably aligned with anything Joe Kelly would have said also, and just be there. But what Joe did instead was said all right, and he has given the presentations to you, here are the failings that are there. Let's just move forward, rather than doing an intermediate step, and we get product after product out of Joe.

And what you are really seeing now is a blending there of the consolidation effort that was talked about as the original goal of TRACE and the advance development effort that moves forward.

VICE CHAIR WALLIS: It's hardly vast development. A lot of it is fixing up some of the real weaknesses in that.

MR. MAHAFFY: I understand that, but it is advanced relative to any of the predecessor dinosaurs. Okay. We're pushing the state of the technology past where it has ever been before. Okay.

VICE CHAIR WALLIS: As he showed us this morning some of the predictions from the original TRAC film thickness and that seemed to be way off.

MR. MAHAFFY: But he put certain proviso in there, but you always have to remember and that is the scope of these codes. NRC has never claimed that TRAC was anything other than their large break LOCA Code. Okay. And that drove the selection of models and the funding by the NRC for the extent of the model development. RELAP5 had a different set of transients, so it was supposed to analyze. And Vic could talk about that better than I can, but once you get into things beyond large break LOCA, your capabilities in terms of the details of some of these models have got to be much more advanced.

And I know I was involved in some of the oversight of the AP-600 qualification RELAP5 and there was a lot of very good work that went into looking at how it was assessing them and getting them into that code. So you've got two different paths there. And they said if all we were doing was consolidation and it were a fixed goal, we would have grabbed a whole lot of models straight out of RELAP5, shoved them into this product as is and we would have met the stated goals up front.

But a different path was followed and I will tell you four years ago, I think, I publicly made a statement, and it may have been in this particular venue, that really we didn't call it TRACE then, TRAC-M, whatever we called it, had achieved the goal of all the capabilities of all the predecessor codes. The capabilities were there. Whether they were fully tuned up or not is another question, as you saw with the model issues.

At this point in time, I mean, we have not done a full set of assessment, obviously. Joe Staudenmeier can say more intelligent things, I think, than I can about this. But based on the assessment problems I have seen, I've done, I've seen presented, this code is going to do on the whole a better job than any of the predecessors in matching a reasonable assessment base. Would you agree with that, at this point?

MR. STAUDENMEIER: Yes, I think on a wide scope assessment.

MR. MAHAFFY: Yes. I believe you will find specific assessment problems where say RELAP5 would still do better. But if you took the broad scope, we're in the game now, and there's certain things it can do that people couldn't do before. So I've identified two items that have stretched things out. Okay. The strict compatibility with input. This blending of the consolidation and the advanced development work and that blending transitions into other things.

Okay. I've been working on advanced numerical methods. There was some parallel capabilities we put in. There is a list of these things. But then there is number three, and again Joe handed this, and you don't want to under-rate this, this has to do with digressions. Okay. To the credit of the NRC, and I've been in the code development business for a very long time now and seen a lot of development teams. The NRC has put together internally a first rate development team. Very, very good people involved in this.

But that's a double edged sword. Okay. They can do great development work for you, but if you are an NRC manager, and NRC is a very busy organization with a lot of things to do, okay, you've got a mission critical task you've got to get done. You've got a task that's got a lot of wide visibility.

What you want to do is get your very best people working on these particular little items. And where are your very best people? They are sitting on that development team and so they get pulled off to do various odds and ends that have nothing to do with development.

That has a double impact, okay. Impact number one is the large chunks of time for people, you know. As was said, Chris Murray got dragged off to do a very important thing, computer security, you can't neglect that, and this happened, I think, to everybody on the development team within the NRC. Just the base time you've lost, but there is another thing that you don't normally think about and in co-development, you've got to be focused.

You know about this Vic, having worked with RELAP5, you get on a roll. Now, you're moving forward on a very intensive task and you get pulled away from that, the recovery time is a difficult problem also. You don't simply lose the time that you were allocated to another task. You lose more time than that, because you have to recover to that swing of work, that pattern you are in and that's a problem.

CHAIRMAN RANSOM: Well, is that a problem only with the NRC people or has that been a problem with yourself and other contractors?

MR. MAHAFFY: Outside the NRC, it's a problem to the extent that people are not fully funded. Okay. The ISL people move from one task to another. I'm only paid half-time and there is a period of time where I was devoting full-time to this job and now I'm down to half-time. And when you are not, you know, just two or three weeks straight working on a specific task, you don't do it as efficiently as you could have. So that is an issue.

But within the NRC, I mean, that's where your critical core of developers is and that's probably where you see the biggest impact. It's a different developing technique. One thing I know you were concerned about, Vic, was the impact to the fact that it's a disbursed development operation. And, you know, I've worked in both modes now, okay, and you certainly have seen the concentrated mode of RELAP5. We had the same thing with TRAC.

I was very pleasantly surprised by the way the distributed effort has worked here. The coordination that the NRC has provided for the development team has been excellent. The process by which, you know, they work with information disbursal through this website, people talk to each other on a regular basis, I haven't seen any degradation in terms of unit cohesiveness here versus what we had in the old TRAC Project. I don't think that's a particular issue.

In fact, it's better now than it was then, because people have learned a lot of lessons about how you manage a large project. They have been implemented rather well within the NRC.

Are there any other questions on this particular item?

CHAIRMAN RANSOM: Well, one that I don't expect you to answer it today, but I think it is certainly of interest as they move towards risk-informed regulation and use of these tools, is what is the uncertainty associated with things like numerical methods, one dimensional approximations?

MR. MAHAFFY: Yes.

CHAIRMAN RANSOM: You know, things that this thing can never be perfect, so it would be nice to know what is the uncertainty that is associated with methods like this? And hopefully in time, in a sort of generic way, so that you can bound, you know, the results.

MR. MAHAFFY: Yes. I would have gotten to some of that during the talk, but let's go ahead and address part of that now, and then maybe extend on what I would have said during my regular talk. If you look out there, I don't know if you have been following some of the literature, but there has been a body of literature building up in the area of verification and validation. It's a big issue in CFD. They are starting to relearn lessons that people in the reactor safety community learned a long time ago, because they have been burned so often with problems with CFD results.

A fellow named Over Kampf at Sandia Lab and a bunch of his colleagues have written a whole lot of reports and some journal articles on that. Patrick Roache has got a thick book called Verification and Validation, I think that came out about six or seven years ago actually. But people are giving some fairly deep thought to that. If you get into uncertainty due to the numerical methods themselves, there is a pretty good technology based on Richardson extrapolation analysis that people apply within the CFD community. And we have started to apply it within the concept of TRACE.

In that, you do a systematic timestep sensitivity study, you do systematic mesh refinement studies, and by using the Richardson type analysis, you can put quantitative bounds on what error is being introduced simply by the fact that I picked my mesh size this big or my timestep size of one second versus a tenth of a second.

CHAIRMAN RANSOM: Well, is that being quantified to some extent?

MR. MAHAFFY: Yes. If you go out and look at the literature on Richardson extrapolation analysis, you know, I can go out and I can give you some kind of quantifiable number about the error margin, and again in the field of verification and validation, people tend to try to define things in clearer ways and they will distinguish between error, which is a quantifiable item versus uncertainty, which is, at its roots, due to things we don't know and can't know.

CHAIRMAN RANSOM: Yes.

MR. MAHAFFY: Turbulence. I can't tell you exactly which way the velocity is going to be moving at this point and space in this room and that does things to me.

CHAIRMAN RANSOM: Although, there certainly are things of interest, I think, to --

MR. MAHAFFY: Yes.

CHAIRMAN RANSOM: -- quantifying the

uncertainty in these methods.

MR. MAHAFFY: And so you can do a reasonable job of making some statements with a Richardson-based analysis of how much error is associated with the fact that I've chosen this mesh or this timestep. It's not going to be as rigorous as you like in a code like TRACE or RELAP5, because your timestep size is running up and down and you have to really just take snapshots and be systematic in various snapshots about what is going on.

But I have been fairly encouraged about the ability to understand what my mesh is doing to me. I don't always like the answers.

CHAIRMAN RANSOM: Well, things like one dimensional approximation of pipe flow are independent of even the mesh size, you know, so it's some inherent approximation that may be quite close to the real result at times.

MR. MAHAFFY: Yes.

CHAIRMAN RANSOM: And maybe somewhat further away at other times.

MR. MAHAFFY: Yes. But that's an issue, I think, we can start to cope with. Let's see, Chris, is the report that we did on the sensitivity study on Marviken sitting out on the website now? Yes, you might want to take a look at that. Whereabouts does that one sit?

MR. MURRAY: This is Chris Murray. We have like a library in our website where I drop a lot of the documentation that comes in that doesn't fit any real subject, you know, specific subject, but people can come and pick it up.

MR. MAHAFFY: But, you know, under that there is --

CHAIRMAN RANSOM: This is on the --

MR. MURRAY: This is on our internal site that I gave you a password for last year.

CHAIRMAN RANSOM: Yes, that's fine.

MR. MURRAY: I haven't -- I mean, we set up this other external website for any of the camp members to go to and I've got basic stuff there, documentation, sort of what the progress of the code is and about the tracking system, but I haven't migrated all of the supporting documentation under that site yet.

CHAIRMAN RANSOM: Okay.

MR. MAHAFFY: Anyway, if you go out there, I believe I saw it listed under my student's name, Matt Lazor, L-A-Z-O-R, and it is probably just a copy of his thesis. He did a mesh sensitivity Marviken, which was rather interesting, in that it shows you how you get the right answers for the wrong reasons. He had started with an input deck that was provided to us from the NRC that did a fair job of matching mass flow rates, say at the break on one of the Marviken tests. I don't remember right now which one he did the specific study on.

I said okay, now, what I want you to do is go in and do a Richardson-based analysis to tell me what my error is associated with the mesh. And he went and he did the study and as he refined the mesh, the answers started to get worse. And by the time he reached a converged solution on the mesh, there was a noticeable decay in the quality of the answers in a qualitative way that the mass flow rate curve looked quite different.

I scratched my head and said okay, I understand that. What is happening with Marviken is it's a thermally stratified big tank of water, okay. And the time history of the mass flow rate, at the exit, is sensitive to the temperature of the water that is coming into the nozzle, which in turn is sensitive in effect to things like the diffusion phenomena that's prematurely bringing some of the warmer water down to the nozzle.

CHAIRMAN RANSOM: Right.

MR. MAHAFFY: And what was done in the analysis was that you got the mesh size just right so that the numerical diffusion matched the diffusion processes going on in the system itself. If you think about it, really what is happening is you've got this 3-D tank and the water going down into the exit pipe is it's not coming in some uniform drop down of the level, but it is being sucked in from the middle, so you're getting the warmer temperatures sooner than if the tank had just sort of uniformly come down with all your water coming off the bottom at the time.

So, you know, that's kind of an interesting thing you learn when you do these sensitivity analyses. And I'm hoping people will do a lot more of that kind of stuff in the future.

VICE CHAIR WALLIS: You know, that's numerics. There are all kinds of problems with the one dimensional nature of the assumption that go into the --

MR. MAHAFFY: Yes, I talked a little bit about that. You and I will probably get into discussion on momentum equations here shortly.

VICE CHAIR WALLIS: It was extraordinary to me that Joe talked about some of the Wallis or Hugh or all of these interfacial friction things. They are only valid for a long, long straight pipe. And if they are used in reactor circuits where, you know, the bend is very sharp --

MR. MAHAFFY: Sure.

VICE CHAIR WALLIS: -- obviously, something completely different is happening in there.

MR. MAHAFFY: Yes.

VICE CHAIR WALLIS: It's amazing it works at all.

MR. MAHAFFY: Well, if you think about it long enough, and a lot of these things aren't too amazing, it's just that there are an awful lot of -- it's the old part business. There are an awful lot of phenomena in many cases relevant to reactor safety that, you know, they are not modeled correctly, but they are not important.

VICE CHAIR WALLIS: It don't matter very much.

MR. MAHAFFY: Yes, and that's what it shows you. An awful lot of reactor safety analysis, if you do a decent job of conserving energy and mass and get a good check flow out the backend, one way or another your model flow and get a fairly respectable answer.

Okay. I'm just going to address the second letter that came up. I produced a written response and that castrated into things that the NRC talked about in the first letter, but this --

VICE CHAIR WALLIS: Did you send that out? I didn't receive it.

MR. MAHAFFY: I sent a response to the NRC and they embedded that in whatever they, you know, did. And as Joe said --

CHAIRMAN RANSOM: Did you ever see it?

VICE CHAIR WALLIS: No.

MR. MAHAFFY: Anyway, you are about to see it live. Everything I told the NRC, I'm about to tell you and it's in this view graph presentation. All right? What I've done, first of all, is to identify for you in bullet form the key points that I saw in that letter. Okay. There is a comment, a not too happy comment. It says "TRACE numerical methods are an engineering solution."

VICE CHAIR WALLIS: Yes.

MR. MAHAFFY: Okay. There is another one that says "It seems to me that there is a high probability that convergence of the numerical equations to the continuous equations cannot be demonstrated." Those are related comments. There's

"Generally no multi-step method actually satisfies the original FDEs." The next one, "The numerical method does not solve for the void fraction in a way that can be theoretically justified."

Another one, "When a single phase is in the flow system a linearized version of the basic equations is solved." He says "Neither RELAP5 or TRAC/TRACE attempt to satisfy the non-linear EOS for the two-fluid model." And "Numerical methods have been developed to focus on CPU time needed for the calculation" etcetera. There is some confusion in the letter about a coefficient, we call, "beta" in the momentum equations. And then there is some general comments on the momentum equations in the letter.

First of all, let me talk about the engineering solution. Guilty as charged. I will make a blunt statement.

VICE CHAIR WALLIS: What else could it be?

MR. MAHAFFY: Well, no, I mean, you have got to have good solid mathematics behind your numerical method. If you don't, you're sunk. There is no question about that. But what I want to say is that anyone developing numerical methods for use in production simulation codes is not doing their job if they ignore engineering solutions. Okay. You get the underlying mathematics right, but you have to experiment. There is flexibility within the mathematical formulation. There are a number of options that rise before you that you have to explore to find the one that is going to give you the most robust, but still consistent solution.

VICE CHAIR WALLIS: Well, it's true in two phase flow where there really is no fundamental equation like the Navier-Stokes equation you can turn to and say --

MR. MAHAFFY: Yes.

VICE CHAIR WALLIS: We know the fundamental math and if we were clever enough, we could solve that. You don't have that situation.

MR. MAHAFFY: Yes. And I've given you a list of comments here. You can see them. But the one thing I do want to make point is you do stick to the mathematics. You tinker within the idea that the resulting difference equations are still formally consistent with your original partial differential equations. Okay. And I'm going to step back from part of what you said. I'm worrying about the numerical methods.

Somebody has given me a mathematical problem and we'll talk about that in a minute and then

I'm in the business of doing a numerical solution to that mathematical problem. Okay. Convergence of the differential equations. Okay. Basically, I've got to agree with the author's comment here within certain bounds. Okay. Formal convergence in the concept of numerical methods really is demonstrated by the Lax Equivalence Theorem, which I have quoted here.

Basically, it says if you've got a properly posed initial value problem and you've got difference equations that are formally consistent with the difference equations or the differential equations, excuse me, and stability is a necessary and sufficient condition for convergence. Okay. Well, you know, this was something that was hashed over many years ago. TRACE, TRAC, RELAP5, they are in trouble with the properly posed clause of the Lax Equivalence Theorem.

We admit that. Okay. And as we move forward, things have got to be done about that. I know Vic did some nice work.

CHAIRMAN RANSOM: Now, there is another aspect of that too, though, when you deal with one dimensional average system or whether you deal with multi-dimensional. They are volume average models, when you talk about multi-phase flow. You know, we have averaged over the phases to get more or less the single set of equations, so it seems kind of ridiculous, let's say in the one dimensional sense, to talk about zero Delta X, you know, or any attempt to try to converge to that sort of dimension, which then says it is an area averaged sort of phenomena that you're dealing with.

MR. MAHAFFY: Yes.

CHAIRMAN RANSOM: But in reality, it's inconsistent with say the volume average. So I don't -- do me that's not a big issue. You know, I think, we're never going to deal with zero length meshes and we're more interested in say consistent meshes where maybe the l over d is approximately 1 or something like that, and what sort of uncertainty is associated with the model at that level. That would be more meaningful, I believe.

MR. MAHAFFY: I generally agree with you there. It's just, you know, this issue, the numerical error what we often refer to as numerical diffusion, with first order terms. You need to get some understanding of what that is doing to you with any given mesh.

CHAIRMAN RANSOM: Sure.

MR. MAHAFFY: And realize whether or not

it is getting you into trouble.

CHAIRMAN RANSOM: But this was more or less like the Wolfgang Wullf sort of concept where we always were trying to prove, you know, convergence and the limit.

MR. MAHAFFY: You know, I can show convergence in any reasonable limit. I mean, if you go back to the old literature.

CHAIRMAN RANSOM: Oh, please, do. Right.

MR. MAHAFFY: Yes, you have to get your mesh pretty small before we get into --

VICE CHAIR WALLIS: This is silly. If you take the flow say entrance place in developing bound linear on the wall and all the classical problems, you cannot solve that by taking averages across the pipe.

MR. MAHAFFY: I hear you.

VICE CHAIR WALLIS: What happens is essentially in the other dimension.

MR. MAHAFFY: Right.

VICE CHAIR WALLIS: And it's absurd to then fiddle and fuss about whether or not it's accurate, because it isn't.

MR. MAHAFFY: You've got --

VICE CHAIR WALLIS: You still draw your control volume.

MR. MAHAFFY: That's right.

VICE CHAIR WALLIS: You still make an approximate balance.

MR. MAHAFFY: That's right. But there are inaccuracies due to your discretization that you better understand to make sure they are not greater than the inaccuracies due to your physical approximations. That's where you are.

Another problem with the Lax Equivalence Theorem if you really look into the literature, you'll find that it's only going to be rigorous when you've got a set of linear PDEs to begin with. Okay. It's a guideline. Okay. And that's what I say in the next thing. You know, you've got a guideline here. But in terms of SETS itself, which is the big issue for the author of this letter, one thing I want to make absolutely clear is that from the beginning, and you know SETS was created more years ago than I like to think about, because I was still young then, I realize from the beginning there were all kinds of potentials for difficulties here.

And one of the things that we did, for example, and we continue to do over the years is from time to time do a test. Is SETS, as my timestep gets smaller, coming in to alignment with its answers with the Semi-Implicit method? If both of those methods are there as options, you know, if you don't like SETS, fine, turn the semi-implicit option and run that on TRACE. But what we see is that, yes, indeed, you know, as your timestep size gets smaller, the SETS answers are going into semi-implicit answers or if you are approaching steady-state, even if the timestep size is much bigger with SETS, it goes to the same steady-state that a semi-implicit would and it should.

It's because SETS is a funny multi-step method and multi-step methods, I think, get bad names mainly from some of these flux splitting techniques that are used for multidimensional problems and there --

VICE CHAIR WALLIS: Oh, SETS.

MR. MAHAFFY: What is very enhancing to SETS and at Idaho the flow of SETS was something called "newly-implicit." Anyway, when you get into this flux splitting algorithms, yes, you can get into even serious problems with consistency issues. But this thing, you know, each step is using the same spacial difference operators. There is no attempt with these two steps to establish higher accuracy in your second step than in your first. There is no attempt to simplify your multidimensional spacial operators. There are no shortcuts there.

All you are trying to do is stabilize a basic method which is semi-implicit. The other thing we have done to demonstrate convergence, I have already talked about, and that's to take a look at some of this Richardson extrapolation analysis.

CHAIRMAN RANSOM: I wonder if that's similar to, you know, I did some work on using fastforwarded transforms to look at the output from a numerical method to see how the different wavelengths, you know, of a disturbance would decay. And the conclusion I came to there is all of these methods depend on some numerical stability or damping, you know, the limit.

MR. MAHAFFY: Yes.

CHAIRMAN RANSOM: To kill off the shorter wavelength behavior.

MR. MAHAFFY: Yep.

CHAIRMAN RANSOM: And that's just something you have to have. It goes back to the same Lax Equivalence Theorem that, you know, that method must be stable in order for you to ever achieve consistency or convergence.

MR. MAHAFFY: Yes.

VICE CHAIR WALLIS: Well, it's ludicrous.

I mean, you get to the point where you're worried about stability when you have a millimeter long control volume and a one meter diameter pipe.

MR. MAHAFFY: Sure.

VICE CHAIR WALLIS: It makes absolutely no sense.

CHAIRMAN RANSOM: For the analysis type approach though, it looks at what happens at 2 del vex, because that's the wavelength that you must be stable at in these numerical calculations.

MR. MAHAFFY: Yes.

CHAIRMAN RANSOM: To me it was insightful, but never seemed to be picked up as a general way of demonstrating stability of these methods.

MR. MAHAFFY: Yes. Let me give you an example of the kind of thing we do from time to time and did quite frequently early on with this methodology. I've got a set of four runs here, which you'll have trouble telling the difference between, but I do a base semi-implicit run with TRACE letting the TRACE timestep control do whatever it wants to do. And then the dominant control in the timestep there is the material collapse/stability limit for the semi-implicit.

I looked at it and the semi-implicit,

basically, three milliseconds was where, in this particular case, it wanted to set the material collapse/stability limit most of the time. There were some places where it creeped up. I went ahead and put a ceiling on the semi-implicit at 3 milliseconds for a second run. Here you won't see the difference in the answers at all.

SETS was being run for this problem with an upper time limit of a tenth of a second. And indeed, for a large portion of this transient, it was running at a tenth of a second. Not the whole thing, latent time it didn't. I reran SETS back down really effectively at the dominant material Courant limit. I wouldn't expect an exact match in this case anyway, because if you look at the numerical diffusion associated with these methods, at the material Courant limit, there is a noticeable difference.

I mean, it's there. But in this case break mass flow matched pretty closely. You can see a little bit of difference here. In fact, if you look really carefully, you will see some oscillations in the full up SETS method. Those are instabilities driven off the fact that the choke flow models evaluated explicitly. Okay. Which is something we'll talk about in a minute.

VICE CHAIR WALLIS: As an engineer, one would say look, it isn't changing very much.

MR. MAHAFFY: No.

VICE CHAIR WALLIS: And then second, it makes no sense to have a timestep with 3 milliseconds.

MR. MAHAFFY: That's right, yes.

VICE CHAIR WALLIS: The fact that you put in some momentum equation which has compressibility, which I think will go bezerk, that isn't really physically affecting what happens.

MR. MAHAFFY: In this case, it's not simply the momentum equation. The mass and energy equations along --

VICE CHAIR WALLIS: It must be something.

MR. MAHAFFY: It's the semi-implicit methodology. The fact that, if you look, for instance, at a mass equation, I'll show you an instance in a minute and you'll understand what's happening. I mean, one of the most sensitive items in any kind of LOCA, you take a look at the void fraction next to the break and that's what I'm plotting here. You basically the only place you see a difference here is at the highest timestep level for the SETS method.

You go down to the lower stability limit times step level and everything is just laying over each other.

CHAIRMAN RANSOM: SETS is a tenth?

MR. MAHAFFY: Well, SETS is a tenth. You're getting that green line there. The one that lays low.

CHAIRMAN RANSOM: Right. These don't have the value in there, I guess.

MR. MAHAFFY: Yes. And again, there is some oscillations early on when you are running the high timestep off of the explicit evaluation of the choke flow model. And that's pretty much it. It is doing a pretty good job. There was this comment about multi-step methods having trouble satisfying the FDEs. Here I have a suspicion that there was a typo in the letter. The author was probably talking about satisfying the original PDEs, which goes back to our convergence discussion earlier, and I won't say anything more about that.

If the author really was talking about the FDEs, I mean, they are what they are. And, you know, we go through a process of verification and validation. SETS over the years has gone through a rather substantial verification effort. You've got a 99.999 percent certainty at this point that, you know, the Fortran implementation of the difference equations are as written, because we've done things like do analytic evaluations and numerical evaluations of the Jacobian for the semi-implicit step.

Also, semi-implicit SETS have actually been written in three different completely separable Fortran implementations and in each case we have compared the results of one to the results of the next to make sure they match. So that's pretty solid.

There is another comment here and I've got -- I'll let you read this later on at your leisure, and I just want to hit the highlights in some further view graphs. There are some worries about the way void fraction enters into the solution here with SETS. The author of the letter correctly noted that there is a linearization step at the end of the timestep that generates a final new-time value of the void fraction and was worried about the impact of this.

And I want to explain really how that feeds into things. If you look at the time implementation of the mass equation, for instance, the real key item in the mass and the energy equation, it's really not the microscopic quantities, but the macroscopic densities, the products of Alpha and Rho, the products of Alpha Rho EL, your big grand averages over area and whatnot. And the numerical method, this is, you'll see an analogy of what happens in RELAP5, but let's just talk about SETS in TRACE right now.

Here is the stabilizer mass equation and the last thing you do at the end of the timestep, you stabilize your mass equations. And if you look at this, this looks like a fully implicit equation, except what you have to realize is that these velocities that are labeled new-time have actually been locked in by the semi-implicit step. So all I'm solving for here are these macroscopic densities and in 1-D that's at worst to tridiagonal matrix. In fact, it's normally a lower triangular or an upper triangular matrix, if you've got up-wind differencing and unidirectional flow.

So you get that and the important thing here is that when you move on to the next timestep, you carry through this product as generated by the solution into your old time values. So, for instance, when I go to my semi-implicit step, the old time value here in my time derivative it's just the solution as generated there. The values I'm using in the flux terms, they are the same things that came out of this solution.

At no point have I gone back and, you know, used any separable void fractions, whatever to recalculate these quantities. They are carried over in a consistent way that guarantees mass conservation, energy conservation from one step to the next. Okay.

CHAIRMAN RANSOM: Well, it says complained about. I think you probably do solve for Rho EL from the equation state and then divide out for Rho by that to get Alpha, I would guess.

MR. MAHAFFY: It's a little more complicated than that and I'm going to show you that in a second.

CHAIRMAN RANSOM: Some other variable.

MR. MAHAFFY: But the important thing that you've got to understand is whatever we're doing with this Alpha, and I'll show you what we're doing in a second, it's not feeding into the time evolution of the mass content and the energy content of the flow. Okay. In terms of Graham's discussion, if you look at a semi-implicit equation, the stability comes in from the fact that I'm using an old time macroscopic density in my flux term that naturally introduces a stability restriction that says I can't really run my timestep any higher than would give me a natural flow of density from one cell to the next cell.

If my timestep is high enough, that in one timestep I'm trying to grab macroscopic densities from two cells away for my information and my fluxes, something bad is going to happen and it does. It's a numerical instability.

VICE CHAIR WALLIS: I can see where you get into trouble if Alpha is very close to 1. And when you solve this thing you find that the define point growing has been around for bigger than 1.

MR. MAHAFFY: Well, forget the --

VICE CHAIR WALLIS: A hiking or something that --

MR. MAHAFFY: Well, forget that. I mean, even if Alpha --

VICE CHAIR WALLIS: Has that never happened?

MR. MAHAFFY: Yes. Even if I implement this on a pure single phase equation of state with stability, okay, whether or not Alpha something is not going to influence the stability.

VICE CHAIR WALLIS: With single phase flow Alpha Rho is Rho itself.

MR. MAHAFFY: Yes, yes. But the fact that the void fraction is 0 or 1 or anything in between really has no influence on this crush in the stability associated with semi-implicit. It's, you know, something amenable to numerical analysis. You can go out and do a standard von Neumann analysis and understand why that is happening.

CHAIRMAN RANSOM: Well, the only complaint that they might be getting at is you do need Alpha in the momentum treatment, you know.

MR. MAHAFFY: You do need the Alpha in the momentum treatment, particularly, in various correlations. And here is what we're doing.

CHAIRMAN RANSOM: As well, right.

MR. MAHAFFY: Here is the overall strategy with the use of variables in SETS. You are trying to do things as consistently as possible. This void fraction that the letter's author was concerned about, what happens is at the end of the timestep, you have in any given volume these macroscopic densities for vapor, liquid, non-condensible gas, if it's present, and what you can do, I mean, what you don't know is fundamental variables like void fraction, temperature, pressure, partial pressure variable, whatever.

Okay. You can go in and based on your equation of state, you could do a solution. You know, I've got my final state that says the product Alpha Rho G is equal to 3, you know. That may be the number that I got after I solved all my mass equations. So I've got an equation that says Alpha Rho G equals 3 and I've got another one that says 1-Alpha Rho EL is equal to some other number. And I take this coupled set of equations and I can iteratively solve for void fraction temperatures pressures. Right? It's just math. We do that, but we only --

CHAIRMAN RANSOM: If it's 3, the other one is -2, right?

MR. MAHAFFY: Yes.

CHAIRMAN RANSOM: Which is a little embarrassing.

MR. MAHAFFY: Well, it won't be.

CHAIRMAN RANSOM: All right.

MR. MAHAFFY: If you've solved conservation equations properly with a first order of method, you won't end up with a negative number.

CHAIRMAN RANSOM: But you solve for the two Alphas independently or do you use the --

MR. MAHAFFY: What we did --

CHAIRMAN RANSOM: -- fact that the sum was equal to 1?

MR. MAHAFFY: We used the fact that the sum is equal to 1. Okay. You have to. You know, if you want to think about two independent Alphas, you need an extra equation and that equation is the sum of the two, volume fractions is equal to 1. And you know, you'll end up once you do your anomaly and your iteration getting the same answer.

But all we do, and this concerns the letter's author a bit, and I don't blame him, we only do a one shot linearization of that solution set and it's just from experience. The reason we do that is that there is timestep control on change in void fraction. From one timestep to the next, we're not letting the void fraction change by much anyway. So the linearized approximation, just as it works well in RELAP5 with proper timestep control for the whole set of mass and energy equations, is going to work well here for this limited statement of I know the mass content and the energy content of each volume. Out of that you get a void fraction.

VICE CHAIR WALLIS: This again points up on the uncertainties of being too finicky. If you have something like slow flow, you have liquid and then vapor and then vapor and liquid and vapor.

MR. MAHAFFY: Yes.

VICE CHAIR WALLIS: Obviously, you can't have a given point for a fraction going from 0 to 1. For the quite reason you forced it, because of these pseudo partial differential equations, you forced this flourishing never to be able to happen.

MR. MAHAFFY: Yes. In this case --

VICE CHAIR WALLIS: So, you know, the math is really -- should be regarded as a crude representation of the physics and not taken so tremendously seriously as being absolutely true.

MR. MAHAFFY: Now, let me tell you why we went to this trouble. It's numerically and it's all related to a problem I'm going to talk in a little more detail here, a view graph or two down the line, and it has to do with this pervasive problem that heat transfer and friction coefficient, whether they be interfacial or wall, were evaluated with old time level quantities and there are various instabilities that rise on that.

I mean, because of the fact that these things are on the ragged edge of disaster, I want a void fraction that is going to be as well stabilized as possible to feed into these correlations. And the void fraction that's consistent with my final semi-implicit equation solution is from engineering experience, in effect, on the equations. The one that allows things to run with the least amount of instabilities developing off of those various coefficients. So it's used there.

Now, again, the letter author notes

correctly that he threw everything else away. The void fraction is the only thing we keep out of that. Now, why is that? Okay. I don't use when I roll on to my next timestep temperatures and pressures that I've deduced from the end of that timestep. And there is a logical reason for that. The reason is that if you think about it, the stiffest parts of our equations are solved implicitly within that central semi-implicit step.

Okay. That's whether we have the implicit coupling between the temperatures in the phase change terms, in the interfacial heat transfer and whatnot, and that's where I'm going to get my best relationship between my liquid temperature and vapor temperature and the saturation temperature. When I go back to the solution of the final stabilizer mass and energy equations, things are going to drift off a little bit.

So I've got all these risk correlations that really care where I lie relative to the saturation line and I want to use temperatures and pressures, as a coincidence, that will give me the best relationship, the most stable relationship there when I come in and evaluate my correlations. And that's why there is this funny mix there. Again, it's engineering. But what I argue is that if you look at this thing, they are all, you know, given new time level, the same number within the order of accuracy of my numerical method. Okay.

CHAIRMAN RANSOM: Well, the old time values are -- that's true of them as well.

MR. MAHAFFY: Yes.

CHAIRMAN RANSOM: But it may not be as stable like you said.

MR. MAHAFFY: Yes. So I have a choice given the effective accuracy in my numerical method of a number of things to pick off, and I pick off the ones that stabilize things the best and stability is a requirement for convergence and that's something I've got my eye on, but I'm not doing anything in here that destroys the consistency with my PDEs.

The issue was raised in the letter about the treatment of the single phase and here I need to go back and I will look at it very carefully when I update the documentation to reflect the addition of our implicit methodology. It may be that the documentation is unclear. I will make sure it does clear up here. When TRACE goes single phase, it does do something a little unusual, in that it changes the set of equations that you are solving.

Normally, in two phase form what TRACE

will do is it will have a mean mass equation, a mean energy equation, a vapor mass, a vapor energy and, if necessary, non-condensible mass equations. Okay. And when in the course of the iterative solution of the semi-implicit equation step you discover that your void fraction is headed less than 0 or greater than 1, it's got tests in there that take a careful look at the flux conditions, boiling and condensation conditions. And if they indicate that only one conclusion is possible, and that is you've gone single phase, you change your equations.

You preserve the full mean mass and the full mean energy equation so that on the whole you are still conserving rigorously your total mass and your total energy. And then let's say we have gone to all liquid system, what I do is I instead of my vapor mass equation, I've got an equation that simply says Alpha equals 0. And I've got another equation that simply says T vapor is equal to T_{sat}. Okay. And that's the new set of equations.

And if you look at it, another way of regarding that is mathematically it ends up to being the same thing that would have happened if I would have sat down and very carefully looked at the condensation process, say, over the course of the timestep where vapor disappeared, and I only integrate my mass transfer term over the faction of that timestep size in which there is mass to condense. Okay.

There is an Algebraic equivalency hidden underneath this methodology, but at no point do I throw away or I gain that mass. The equations that remain that I have listed, however, they are not solved simply in linearized form. We take the full non-linear mass, mixture mass and mixture energy equations, we iterate to solve them. Okay. So that's the only correction there.

VICE CHAIR WALLIS: So you might be in trouble at the point where you are changing from two phase to single phase or vice versa where the equations themselves are changing across the note. That might give you some problems. I can see how that might happen. Also, a problem if you have tried to model on a node with multiple connections, some of which may be two phase and some of which may be single phase and you don't know which is which, I think you might have a real problem with that one.

MR. MAHAFFY: You find out which is which. Okay, yes. My words have hidden a block of if tabs that are checking to make sure, you know, I'm understanding that not only is my void fraction zero right now, but every connection to that cell I've assured myself that there is no vapor coming in there.

VICE CHAIR WALLIS: But you don't know ahead of time, I mean.

MR. MAHAFFY: Yes, but, you know, I know it's there, because of the nature of the semi-implicit method. That's all I would like to say on that, unless there are further questions. Simply, we do solve the full non-linear equations there.

CHAIRMAN RANSOM: Don't you have to modify the liquid velocity? I mean, this is related to Water Packing as well, right, when you get this phase transition?

MR. MAHAFFY: Yes. That's a separate issue. It depends on how the phase transition is occurring. If it has occurred from a continuity way moving through the volume, you most assuredly will have to do something with that liquid velocity. And either your Water Packing logic has engaged and if you remember the classic Water Packing correction that, you know, RELAP5 and TRACE probably introduced the same kind of thing independently 20 or 25 years ago. Effectively, all you do is when you detect Water Packing you artificially reduce the inertia of the fluid at the face, so that a small pressure change will allow the velocity to come in to line and fit the discontinuity in the velocity that is moving through.

CHAIRMAN RANSOM: That's still the basis for the Water Packing algorithm?

MR. MAHAFFY: It's one of two bases. What we really like to see is situations where a level tracker can take care of it, because it does a much better job.

CHAIRMAN RANSOM: What will?

MR. MAHAFFY: The level tracking logic.

CHAIRMAN RANSOM: Yes.

MR. MAHAFFY: If I'm fortunate enough, and level tracking is just that, it relies on vertical upflow, if I've got a bunch of liquid flowing up through a vapor filled space, and I have my level tracking logic turned on, except for some glitches that Joe is headed to, it's not 100 percent yet, but it's doing pretty well, it will in general do a much better job, because it really has much more cognizance of the nature of the discontinuity that's moving through the mesh. It does a better job of correcting that liquid velocity and the vapor velocity to get a good solution to a discontinuous process.

CHAIRMAN RANSOM: That one, too, many

bases will have a zero liquid velocity until the liquid gets there and then somehow you have to --

MR. MAHAFFY: TRACE does something a little different than RELAP5 does.

CHAIRMAN RANSOM: I don't know what RELAP5 does.

MR. MAHAFFY: RELAP5, yes, I think, it used to do this business. You talk about if the phase is absent, there is a zero velocity.

CHAIRMAN RANSOM: Right.

MR. MAHAFFY: It may not do that any more, but in TRACE and in TRAC, what we always did was we said okay, if I can't find any liquid to calculate a liquid momentum equation, I'm going to assume one droplet present and I'm going to figure out just so I have advance notice when some liquid appears what the liquid velocity would be. Okay. It's just one approximation. It tends to make things run a little better than if I just always start at zero. It's not a perfect solution by any means, but that's something that happens to be done in there.

Not only your equation mistake, my belief is that we probably just have a semantics problem on this one and my understanding of solving it for the non-linear equation of state is not the same as whoever wrote this particular paper, this letter. But I will say that within the context of the semi-implicit step and certainly when I'm running the semi-implicit method as implemented in TRACE, to the extent my iteration is converged, every quantity that I've got is consistent with the full non-linear equation of state. Okay.

You know, your temperature, pressure, density, energy relationships all in a non-linear way are consistent with your equation of state as they come out of the solution of the mass and energy equations. That's really all I can say on that subject for lack of --

VICE CHAIR WALLIS: The equation of state is discontinuous, I mean, slow when you cross the phase boundary.

MR. MAHAFFY: True.

VICE CHAIR WALLIS: You can handle that okay?

MR. MAHAFFY: Yes, we do. It does cause you heartburn, you know, when you have phase changes, when you're trying to solve a set of couple non-linear equations, and this is back to our little engineering interventions, there are various little if tests in TRACE when it detects things jumping across the saturation line. Sometimes it does some things to your next gas for a temperature as you continue your iteration, so you don't get too far out of line.

CHAIRMAN RANSOM: Well, you have incorporated Martinson's improved equation of state, I think, I mean, that models the super heated liquid and the sub-cooled steam state.

MR. MAHAFFY: Yes, I believe, NRC --

CHAIRMAN RANSOM: Do you have an equation state for each of those?

MR. MAHAFFY: -- can speak to that better. But I think we had the latest.

MR. STAUDENMEIER: You know, we can integrate the RELAP5 equation state tables into TRACE. It's an option.

CHAIRMAN RANSOM: So actually, it's two equations of state?

MR. STAUDENMEIER: Yes.

CHAIRMAN RANSOM: One for length and one for vapor.

MR. MAHAFFY: But the reason we have gone with two equations of state is really a question of runtime. I think if that wasn't an issue, we probably would just lock on to the RELAP5 equations and be done with it. But when you run the old TRAC curve fit equations of state, they are substantially faster.

CHAIRMAN RANSOM: How do you get the curve fit values?

MR. MAHAFFY: Yes, you would have to go find Mangit Sahota and find out exactly the mysteries he went through to create all of that.

CHAIRMAN RANSOM: I think Mortensen went through the Helmholtz function for the steam table --

MR. MAHAFFY: Mortensen did an incredibly good job.

CHAIRMAN RANSOM: -- and differentiated it to get a --

MR. MAHAFFY: And Mangit did a good job within the context again of large break LOCA. Okay. An equation state that really did a bang up great job in large break LOCA and has survived a whole lot of other applications, where it finally fell on its face was Tom Downar's left, but when you get into applications with coupled neutron kinetics and very small errors in your liquid density, can make a big difference in your answer.

You know, you see that when you run the old TRAC equation of state. You know, we first noticed the problem when, it was probably Tom that was saying gee, I can't understand why everybody else is getting this answer and I'm shifted off to something else, and he finally traced it down to say ah-ha.

CHAIRMAN RANSOM: Well, I'm a little unclear what you are saying. What is used in TRACE?

MR. MAHAFFY: Either one.

CHAIRMAN RANSOM: Either one?

MR. MAHAFFY: Either one.

CHAIRMAN RANSOM: You mean the user has a choice?

MR. MAHAFFY: Yes. If you are in RELAP5 mode with TRACE, okay, if it knows the input has come from RELAP5 and, you know, there are flags that will tell it that, it will default to the standard RELAP5 equation of state, no questions asked. If I'm running a TRACE native deck, the default is the old TRAC equation of state, but I can set in my name list variable options, request or run the RELAP5 equation of state with no problem. So they are both there.

CHAIRMAN RANSOM: Well, I'm a little confused what you said about the neutronics coupling. I thought you said it was a problem with minor changes in, say, density.

MR. MAHAFFY: Yes.

CHAIRMAN RANSOM: That resulted from the old TRAC equation of state.

MR. MAHAFFY: Yes, that's right. Yes. If I go into the old TRAC equation of state and I give it, you know, a pressure of 5 atmospheres and a temperature of 500 degrees calvin, I get a density.

CHAIRMAN RANSOM: Yes.

MR. MAHAFFY: And that density may be off by 2 or 3 percent. Okay. And that makes a difference to people doing the -- yes, for the RELAP. For an awful lot of transients it doesn't matter. But if I've got the neutron kinetics feedback, it will make a noticeable difference. The RELAP5 equation of state, that error is way, way down. It is much more precise because Glen did a very good job, you know, and his predecessors.

CHAIRMAN RANSOM: Well, why haven't you just incorporated that as inappropriate?

MR. MAHAFFY: It's there. You know, it is incorporated.

CHAIRMAN RANSOM: No, but I mean, you seem to be saying there are two, two options.

MR. MURRAY: We left the old in there, because the RELAP5 ones were slow, because the runs to be twice -- to run two to three times slower.

CHAIRMAN RANSOM: I can't believe that.

MR. MURRAY: That's why we left the

original in.

MR. STAUDENMEIER: Yes. And actually, the bigger, the newer chips with the larger level 2 caches have made the difference between the old -- the new equations of state smaller and also, in RELAP5, there are still some users hanging onto the old equation of states for RELAP because of runtime speed for the same reason, because it runs --

CHAIRMAN RANSOM: When you say old equation of state, I'm not quite sure what you mean by that.

MR. STAUDENMEIER: Well, the tables. There's much smaller tables.

CHAIRMAN RANSOM: Right.

MR. STAUDENMEIER: There's an old and a new set of tables that can go into RELAP5. One is about a megabyte in size and I think the latest version of the new ones is up around 12 megabytes in size and there's people that still hang onto the old smaller runs to run with.

There's a TP or new and old tables that go into RELAP5. But I think in the future we are going to be switching over to the RELAP5 equation of state as default and, especially, now with these later chips we're seeing a much smaller runtime penalty.

CHAIRMAN RANSOM: Well, they are reasonable, because it seemed to have a good theoretical basis and should lead to more consistent behavior, I would guess, as you cross these saturation curves.

MR. MAHAFFY: I haven't seen any noticeable difference along those lines, but I agree with Joe. I think it's headed that way. One thing I noticed when this whole issue of runtime first arose, I took a little bit of a look at what was going on in the table evaluations with the RELAP5 equation and I saw some things that could definitely be improved and I don't think that has been done yet.

My belief is that between changes in the nature of cache and some relatively minor improvement tasks to the RELAP equation of state, it's going to be a non-issue pretty soon and, you know, that will be it. You know, you turn it on, you get it, you know.

MR. STAUDENMEIER: You know, one thing in NRC codes, every code has a different equation of state and every code has different materials properties, and one thing I would like to have move forward in the future is have an NRC steam properties library, an NRC materials property library that get maintained and kept up to date, that all the codes would link to the same materials and equation of state libraries, so we don't have different ones in each code version.

MR. MAHAFFY: Okay. If I can move on, there was a comment in there about time levels in SETS and it says at a given time -- well, basically, the comment, to paraphrase it, said that there were many time level evaluations in the SETS method. And what I want to make clear is, in fact, at any given timestep you only are worried about old time quantities and new time quantities regardless of what step you're in.

Now, this is not a multi-step method that generates intermediate things. There is no n+1/2 values floating around there. Yes, because it's a two-step method, for a number of the state variables I will have values, two different evaluations of a given state variable at the new time. Okay? But they are evaluated in a way that is formally consistent with the differential equations in each step and within the order of accuracy of the methodology, they are the same numbers.

VICE CHAIR WALLIS: So your times are always an n or n+1?

MR. MAHAFFY: That's right.

VICE CHAIR WALLIS: Your velocities and things seem to be j+1/2.

MR. MAHAFFY: No, the j+1/2 is a spacial index. If you'll look at our indications, if you would like me to go back and I can put it up, but the standard rotation we use, and I think is consistent with RELAP5, is that your superscript is your time level and your subscripts are your spacial locations.

VICE CHAIR WALLIS: But you have velocities at j+1/2.

MR. MAHAFFY: Yes, those are --

CHAIRMAN RANSOM: Those are the sides.

MR. MAHAFFY: Those are the sides or the volume.

CHAIRMAN RANSOM: Or the volume.

VICE CHAIR WALLIS: Well, that makes sense to me if I have got a straight plate, but I'm not quite sure what j+1/2 means when I have got changes of the area.

UNIDENTIFIED SPEAKER: Top, bottom.

CHAIRMAN RANSOM: You've got j and k.

MR. MAHAFFY: Yes, all that is is some indication. It's our way of saying it's the edge between volume j and volume j+1. That's what that notation means.

VICE CHAIR WALLIS: It doesn't mean it's half way along?

MR. MAHAFFY: No, it doesn't, it doesn't. It's just an index notation that I think an awful lot of people --

VICE CHAIR WALLIS: So j is sort of the volume average throughout the thing, j?

MR. MAHAFFY: Yes, when you get a subscript j that's just an index for a volume.

VICE CHAIR WALLIS: The average volume, volume average.

MR. MAHAFFY: Yes.

VICE CHAIR WALLIS: But 1/2 is some sort of area average at the boundary then?

MR. MAHAFFY: Yes.

VICE CHAIR WALLIS: Okay.

MR. MAHAFFY: Now, they are saying the infamous beta. And I can understand anybody being a little concerned about this, and I will revisit the documentation to see if there's anything that I can do to make it clear.

But the first thing you have got to understand about this quantity beta is that it is not part of SETS, per se. Okay? Beta is another one of these little engineering things that we did, again, consistent with the original partial differential equations to improve the stability of the overall solution when the timestep sizes were allowed to get substantially larger due to the fact that the SETS method removed the Courant stability limits. Okay?

And it's very analogous to the linearization that we did on the wall friction and the interfacial friction terms. Vic will probably remember. For example, when you're evaluating an interfacial friction term, you have got some coefficient multiplied by an absolute value of the relative velocity at the old time multiplied by the relative velocity at the new time. Okay.

And that worked pretty well as long as you were in semi-implicit land, although I have got a counter-example I will show you in a few minutes. But certainly, as soon as you're into SETS land, you don't want to do that and what you do is you start out and you say absolute value of V new, relative value -- well, then add the value of V new. It's like a V squared at the new time and you linearize that.

And we do the same thing down here with the V Delta V operator. Okay. If you start out here, at some phase i+1/2, I have gone from Js to Is just to confuse you but, you know, this is just an indication that at phase between two volumes I am evaluating, in effect, my momentum transfer term here. And if I linearize that, okay, in terms of the Delta V between old time and new time, what I end up with is an expression that looks like this. Okay? That's just the result of linearizing this expression.

But you get into trouble here it turns out, because once I have gone through the linearization, okay, I have got in particular this term right here, which is the result of this new time velocity multiplying an old time velocity gradient, if you will, and treated in isolation this is just a forcing term in my momentum equation.

And if you look at this, if this little fellow right here goes negative on me, what will happen is I get into a situation where the faster I go, the more forces trying to act to accelerate me even more. It's a fundamentally unstable mode. And so there is this exception clause in here.

VICE CHAIR WALLIS: A derivative of old time velocity is a spacial derivative.

MR. MAHAFFY: Yes. This is a spacial derivative. So I have got this clause.

VICE CHAIR WALLIS: So the velocity is going to accelerate things.

MR. MAHAFFY: Yes, yes. So what happens to me is that I cannot use this term if this guy is negative. I mean, although formally this linearization should be more stable than a base evaluation of this kind of a mixture, say, I can do this kind of a mixture within the context of my two-step method. In classic RELAP land, that would just be old time on both of these terms.

But I have got to introduce a factor of beta that will kill this term under certain circumstances where it's actually destabilizing, and that's all that is. It's engineering. I have gone in.

CHAIRMAN RANSOM: What is beta, just a multiplier on that?

MR. MAHAFFY: Beta is just a multiplier on this part of the expansion, okay, and it's 01. If this thing is positive, so that I don't have this special destabilizing influence, then I just turn my beta on and I do my full up linearization and it is more stable. Any way I do it, it's still consistent with my underlying differential operator. But there are times when I got to turn beta to zero and I do and that's all it is.

VICE CHAIR WALLIS: It seems very strange to me that simply linearizing should give you instability if one of the things is negative, because I would think it would take care of itself.

MR. MAHAFFY: It can. It's because this is old time. If this thing had been new time, it wouldn't have mattered. I would have this full on linear form here.

VICE CHAIR WALLIS: You could have linearized it in some other way.

MR. MAHAFFY: Well, yes. If I'm working with a full on linear form, it's not an issue, as you say, because it does correct itself in the proper feedback, between the spacial feedback combined with the time feedback can correct itself. But when I do this linearization and I lock in during my timestep size this gradient and I can't get any feedback through that term here, it will destabilize. Okay? That's my hand waving argument for it. Okay.

VICE CHAIR WALLIS: So you have developed an engineering solution for it?

MR. MAHAFFY: It's my engineering solution. But again, I will tell you I have done the mathematical analysis. I am still, you know, formally consistent. My differential operator and my difference operator are formally consistent when I do an error analysis of all of this stuff.

Some other issues were raised and were rather relative to momentum equations. There is a note of the fact that in TRACE documentation and in older TRAC documentation we keep referring to the motion equation, and that is something that I have intentionally done for over 25 years now to clearly indicate we're not evaluating the conservative form of the momentum equation. Okay. This is the non-conservative form.

But more than that, you know, if you think about it, momentum is a vector quantity. Okay. As soon as I try to model, approximate channel flow of any sort, particularly channel flow at variations in area and direction with a one dimensional equation, I can't conserve momentum. At the best what I'm doing is, in fact, I'm solving a kinetic energy equation.

VICE CHAIR WALLIS: I think what you're probably doing, you're following a streamline analysis.

MR. MAHAFFY: Yes.

CHAIRMAN RANSOM: Right.

MR. MAHAFFY: If you think about what happens, you know, when you go through the streamline analysis, really what you're doing is you're varying the kinetic energy equation and that's how you have to look at these things.

VICE CHAIR WALLIS: There is a streamline.

CHAIRMAN RANSOM: Well, it's fine in 1-D and I agree with that, but what happens in 3-D, you know, and you have now to solve for all the vector components?

MR. MAHAFFY: That's right. But what happens in 3-D, if you look at the equations that we implement in 3-D, you know, they are formally consistent with the underlying partial differential equation.

VICE CHAIR WALLIS: They now have three scalers.

MR. MAHAFFY: Hm?

VICE CHAIR WALLIS: They now have three scalers and you can conserve these three scalers in 3-D.

MR. MAHAFFY: Yes.

VICE CHAIR WALLIS: Because you cannot force 1-D method to conserve what's really a multidimensional point.

MR. MAHAFFY: That's right and that's where things like lost coefficients come in.

VICE CHAIR WALLIS: There must be an awful lot of bogus stuff developed where people have tried to do this.

MR. MAHAFFY: Yes. And you know, we have done it ourselves.

VICE CHAIR WALLIS: Because I think it sort of shoves the errors onto the other side of the equation. The fact that you have gotten errors in your left hand side, which is your balance over the volume, somehow they are transferred to lost coefficients on the other side of the equation. So you fix up the left hand side by empirically doing things to the right hand side.

MR. MAHAFFY: Yes. But again, see, my argument is that the empiricism on the right hand side when I do any kind of 1-D approximation, it's required.

VICE CHAIR WALLIS: You have got to be very careful. Otherwise, you get things like predicting a bend is a part and things like that.

MR. MAHAFFY: Yes. And I will tell you, and I'm not going to totally defend the momentum equations we have got here, because I haven't had time to go through on a term-by-term basis and give what you would consider to be a sound mathematical and physical justification term-by-term. We need to do that at some other time.

VICE CHAIR WALLIS: Somewhere in the documentation that eventually is written for this code, everything will become clear?

MR. MAHAFFY: Oh, it's all clearer now, if you look at it. We're not lying about what our equations are. Okay. The terms are all there.

VICE CHAIR WALLIS: But the rationale, the rationale will be clear.

CHAIRMAN RANSOM: The problem is with the rationale.

MR. MAHAFFY: Yes. I believe that we need a better rationale document.

VICE CHAIR WALLIS: Right.

MR. MAHAFFY: And that's something that needs yet to be done, but you can look at it. If you look at the difference equations, I will tell you right now that if I have got one dimensional flow and I have got reasonably continuous flow, I don't have discontinuities like large sludge or liquid coming through, I'm going to get a respectable answer compared to, you know, what your formal differential equations will say.

VICE CHAIR WALLIS: Going from a downcomer into a lower plan and there is a turn of 90 degrees of some sort and everything gets averaged across an area, the momentum equation gets varied as far as that goes.

MR. MAHAFFY: Yes. What happens to you is -- one thing you have got to understand about the three dimensional thing is that unless you're really doing full up CFD where you're properly resolving the surfaces and the boundary layers and everything else, there is always some approximation there. You're never going to get it right.

You know, the 3-D to the extent we do it in TRACE and in RELAP 3-D, it's always a very coarse nodalization. And when I come around that bend, what will happen in TRACE, Graham, is that you will get full loss. If I have got a flow that comes into my 3-D and makes a 90 degree turn, you will get full loss of that momentum, okay, because there is no --

VICE CHAIR WALLIS: What about the build-up of new momentum coming out the other end?

MR. MAHAFFY: The pressure. Okay.

VICE CHAIR WALLIS: There must be a higher pressure on the outside of the bend than on the inside.

MR. MAHAFFY: Yes.

VICE CHAIR WALLIS: But then 1-D can't represent that.

MR. MAHAFFY: No, it's the 3-D. I'm talking about the 3-D issue.

VICE CHAIR WALLIS: Oh, the 3-D, yes.

MR. MAHAFFY: I'm talking about the 3-D.

VICE CHAIR WALLIS: I'm just saying the problems you get with 1-D.

MR. MAHAFFY: Yes.

VICE CHAIR WALLIS: Yes.

MR. MAHAFFY: Well, all you can do, the way it's handled again, it's a kinetic energy equation. The equations are formulated in a way in TRACE and they were in RELAP5, so that you are in the limit of incompressible steady-state flow. You will recover something that looks like a Bernoulli Equation.

VICE CHAIR WALLIS: That has always been a puzzle to me, because people write down like you did on the next slide something that looks like a momentum equation.

MR. MAHAFFY: Ah.

VICE CHAIR WALLIS: But then by hocus pocus it turns into an energy equation. You cannot do that. You have got to do something to it to turn it into an energy equation.

UNIDENTIFIED SPEAKER: You got to multiply it.

VICE CHAIR WALLIS: Multiply the velocity.

MR. MAHAFFY: If you go back a view graph here, basically, if you look at the derivation of the TRACE equations to a point, and I'm going to tell you the point on the next view graph, they really are consistent with a derivation of a kinetic energy equation in a sense. I'm structuring them in a way that my kinetic energy is preserved as I go through whatever, changes in area, bends in direction.

VICE CHAIR WALLIS: Except when there is a lost coefficient.

MR. MAHAFFY: Except when there is a lost coefficient.

VICE CHAIR WALLIS: You fix it up on the other side.

MR. MAHAFFY: You fix it on the other side, because I don't have the resolution. You know, unless I'm doing full up CFD, I don't know what's happening going around that bend.

VICE CHAIR WALLIS: Well, this is where you have got to follow it with things like added mass coefficient. And added mass coefficient, how does that figure into something like this energy equation you're talking about?

MR. MAHAFFY: We don't do added mass coefficients in TRACE. Okay? We have been up front about that all along. And you know, I have said actually here a couple of times before if somebody wants to get a pure example where it makes a meaningful difference, go for it and we'll put it in the code. And it will become less of an issue as we move forward with the advanced methods anyway.

But here, if you look down here at my final point, okay, we have got this quandary. When I have a side connection to my 1-D flow, I have really gone from trying to do a 1-D problem to a 2-D problem. Okay. And now, I have got to decide what am I going to do, because to some degree now, because it's a 2-D flow, I have got a vector quantity and I have got to worry about some of the issues there, that it's really momentum-related.

I could finesse it. I could do something that was more kinetic energy-related that would look more like Bernoulli flow at a fork with lost coefficients, but what we chose to do a long time ago, this was on the TRAC side and it carried through into TRACE, was actually at these kinds of 2-D problems, do something where we worry about out and out momentum equation conservation of momentum.

We need to do that for jet pumps. We need to do that for situations where we have got ECC coming in at a right angle, the flow going through a main pipe, to do a reasonable job of capturing what's going on. If we don't do this, you can get into situations. RELAP5 in some of its earlier incarnations when you zipped two pipes together with a zipper connection, you could get these funny circulation paths build up and feed on themselves.

It was because this exercise was not followed in the original RELAP5 derivation and there were some terms missing in the momentum equation that should have been there to account for the fact that I have got mass entering a flow stream without any momentum in that direction.

CHAIRMAN RANSOM: Well, that was because they were just simple, one dimensional approximations.

MR. MAHAFFY: Yes.

CHAIRMAN RANSOM: That were never meant to be used that way.

MR. MAHAFFY: Yes.

CHAIRMAN RANSOM: But I have heard that in your vessel component that you have encountered artificial circulations as well.

MR. MAHAFFY: Yes. And my belief is there may still be some residual ones. As we have seen them come up --

CHAIRMAN RANSOM: Do you know why that is or, you know, what about the momentum formulation that causes that?

MR. MAHAFFY: The ones I saw in the past were situations where, when they were originally implemented way back when, circa -- the first 3-D was Dennis Liles went on a binge one weekend in 1977, put it all into the code. It then took me six months to get it working right.

CHAIRMAN RANSOM: You mean you have seen phenomena like that?

MR. MAHAFFY: Yes.

CHAIRMAN RANSOM: Dating back to those days?

MR. MAHAFFY: Yes. I will tell you up front that the first implementation of the momentum transfer terms were not such that they captured things quite correctly and you could get some of this spin-up. We fixed that in TRAC probably circa early 1980s. There has been at least one other thing that has arisen since then that was fixed and I don't remember the history there. I'm not going to claim that all spin-ups have been crushed in this code.

VICE CHAIR WALLIS: I think it's a weird thing. I mean, if you take just a T-junction coming from the side you say it's coming in with no momentum, actually you have got a momentum balance. Then if you use your energy method, you don't lose that energy. You have Bernoulli's Equation and all that coming around.

MR. MAHAFFY: Yes.

VICE CHAIR WALLIS: You haven't lost that energy.

MR. MAHAFFY: That's right.

VICE CHAIR WALLIS: Although, you have apparently lost some momentum. So you know, you got to be careful.

MR. MAHAFFY: But it's worse than that if you're not careful.

VICE CHAIR WALLIS: Unless you mix them, you probably need to follow two Bernoulli Equations. You have got two streams in the pipe with different velocities. That's what you do really consistently.

MR. MAHAFFY: Yes, well, let me tell you what the worst case scenario is. This happened in RELAP5 and, to be fair, it happened in very early versions of TRAC also, is that when you're in the situation of this so-called motion equation and to get a feel for the motion equation, look at this equation down here at the bottom of the view graph where, you know, we're doing a dV/dt not d Rho Vdt. Okay? It's legitimate. Okay. I can write this equation and differentiate.

VICE CHAIR WALLIS: I don't understand that at all. Why should dVt j+1/2 depend only on a j-1/2?

MR. MAHAFFY: Ah.

VICE CHAIR WALLIS: I don't understand that.

MR. MAHAFFY: Now, let's get to that in a minute.

VICE CHAIR WALLIS: It started off depending on both of them.

MR. MAHAFFY: Yes.

VICE CHAIR WALLIS: But now it only depends on --

MR. MAHAFFY: No, no, I will get to that in a minute.

VICE CHAIR WALLIS: The velocity here only depends on the area there. It doesn't make sense.

MR. MAHAFFY: Yes, it's a game, but it's a legitimate game again within the order of accuracy of the methods and I will explain that. I'm doing a digression here. If I have got a motion equation form, the place where people get into trouble with side junctions is the following, and that is okay, I have got a right angle junction. I have got flow coming in at right angle to my main flow through a pipe. Ah, okay, it's at right angles. There is no momentum source, so I don't put any source term in this equation. And if you go through the derivation, that's wrong.

It's true you don't put a source term in a pure, fully conservative momentum equation, but when you go to this form of the equation there has to be something that looks like a source term to account for the fact that mass has entered the flow without corresponding velocity in the direction of the main flow stream.

VICE CHAIR WALLIS: And therefore, it has to be accelerated up to that velocity.

MR. MAHAFFY: Yes. When I write down an equation form that, you know, is your rod d velocity, d time plus V Delta V, etcetera, if I don't add some kind of a term in there, I'm not obeying the laws of physics when I have got a side junctions, because in the absence of the --

VICE CHAIR WALLIS: This isn't just a side junction, is it, j-1/2?

MR. MAHAFFY: I will get to this in a minute. Okay? I'm one step behind you. Okay?

VICE CHAIR WALLIS: Well, we could spend forever doing this.

MR. MAHAFFY: We could spend forever, but let me roughly go through this. I assume you buy this first equation in the middle roughly in terms of momentum transfer.

VICE CHAIR WALLIS: I would have to see the figures to see what it refers to.

MR. MAHAFFY: What it is --

VICE CHAIR WALLIS: But it makes more sense than the second equation.

MR. MAHAFFY: Yes. The first equation, all that is is I have drawn some kind of a momentum volume and it's donor cell. Okay?

VICE CHAIR WALLIS: Yes. You have got it going in and coming out.

MR. MAHAFFY: Yes.

VICE CHAIR WALLIS: And staying inside.

MR. MAHAFFY: Yes. In effect, it's a donor cell volume and I'm saying that the downwind velocity is representative of what's going on in that one volume, and I have got the momentum flux out. I have got the momentum flux in. This next equation, okay, the trick I play is as follows. I take over here in this term, I just do a chain rule differentiation.

VICE CHAIR WALLIS: I understand that.

MR. MAHAFFY: And then I go in and I take my mass equation for this volume j and I write the flux terms of mass in and out of that, okay, and I combine them all with these flux terms multiplied here. It's all just Algebra.

So what I have done is I have done a chain rule breakup of this. I have written my finite volume form for the mass equation in a formal way and I have added everything together, then I have divided by my volume times 1 minus Alpha Rho l that was sitting right here. And this is the end result.

VICE CHAIR WALLIS: It doesn't make sense. Anyway, let's not talk about it now.

MR. MAHAFFY: Okay. If you would like, I will give you a detailed step-by-step derivation of that.

VICE CHAIR WALLIS: Okay. That doesn't seem to make sense to me, how a j+1/2 can disappear completely from the equation.

MR. MAHAFFY: It's magic, but it happens.

VICE CHAIR WALLIS: Well, that's okay.

MR. MAHAFFY: I will give you a step-by-step if you would like.

VICE CHAIR WALLIS: Yes.

MR. MAHAFFY: I have got it somewhere. I can find your email address and I will email it to you or would you like to me to email --

VICE CHAIR WALLIS: Okay.

CHAIRMAN RANSOM: This donoring the momentum flux, you know, which has been an argument for a long time, should you donor or should you average across the junction.

MR. MAHAFFY: Yes, in this derivation we're donoring the momentum flux.

CHAIRMAN RANSOM: All right.

MR. MAHAFFY: Okay.

CHAIRMAN RANSOM: Which is the momentum flux is all due to whatever is entering the volume.

MR. MAHAFFY: Yes, yes. Okay. I will send a copy to Ralph for everybody and I will send a direct copy to you, Graham, so you have got the full how do we get from here to here. By the way, this derivation also exists in a hard to find Los Alamos document that Bob Steinke wrote a number of years ago when he documented the T momentum source terms in

TRAC, but I have lost my copy of that. I will have to reproduce this. I did this original derivation 25 years ago or more anyway and it's in old now.

Basically, you do another substitution, which you will have to see the step. It will confuse you even more than the last one did. And you end up with an expression that looks like this. I show you what the T contribution to that is and that is how we do the side junctions with a lot of steps out that I will give you. Okay?

And I also show you how we do a linearized implicit form of the T-junction terms. But yes, we have taken a step there. We have tried to do something that, in some formal sense, will conserve momentum when we have got something that looks like a local 2-D problem.

Let me talk briefly about where we're headed with the numerics and why we're headed that way.

VICE CHAIR WALLIS: Well, this sort of thing concerns me, because you're going to come up with documentation two years from now, which says this is the way we do things and that is going to be it. And then some, I won't use any adjectives, ACRS Member decides to look at this and says gee whiz, something is really strange about what you have done here and says I don't think it's right, what are you going to do?

Are you going to say I don't believe that guy. He can't hold up the whole issuance of this code, because he has now raised a problem with a basic equation. That's because you have never had us review it before.

MR. MAHAFFY: What I'm going to do is this. I'm going to, as my first step, I will go back to the official current version of the Theory Manual and look at the section of the manual that documents these steps that I just summarized in two view graphs.

VICE CHAIR WALLIS: Right.

MR. MAHAFFY: To see how thorough it is. I mean, if it's complete, and I think it won't be, you know, I will just send it to you as is. If it's not complete, not only am I going to give a document to you, but I'm going to use the formal TRACE update procedure and whatever I do for you is going to become part of the Theory Manual immediately. All right? So it will exist online as part of the Theory Manual and, hopefully, that will kill two birds with one stone for you.

VICE CHAIR WALLIS: Because I think I know what the mass conservation equation is and I can see how you do the manipulation, but I can't see how a j+1/2 disappears.

MR. MAHAFFY: Okay. You will see it. There are two steps in between here and I apologize. I was trying to summarize the key terms in here rather than give to you the derivation.

VICE CHAIR WALLIS: Okay.

MR. MAHAFFY: Where we're headed. Basically, SETS itself was developed in the late 1970s just as a quick way to remove the Material Courant stability limit. The real history behind that, and I may be the only one that remembers this anymore, if you go back to the original TRAC Large Break Loss of Coolant Accident Code, the way we dealt with breaks where the flow velocities was very high was we actually had "a fully implicit" component available.

Okay. You could flip a flag and you could get a pipe for however long you wanted where the mass energy and motion equations were evaluated fully implicitly with a caveat that we were still doing old time on the coefficient terms, and it's very analogous to what went on in RETRAN, for example, in retranslator incarnations.

But based on the experience we had there, what I saw was that as we moved into the era of small break LOCAs and I saw that coming even before Three Mile Island, it was natural that we were going to have to do those kinds of calculations.

The amount of effort that was going to be necessary to change the structure of the code and introduce the new coding to go to a real fully implicit numerical method was going to be rather extreme, and the cost per timestep for fully implicit method was going to be a big step also. And I woke up in the middle of the night with a set of equations in my head that became SETS as a way to stabilize the semi-implicit method that was already there.

VICE CHAIR WALLIS: Thinking about it, I'm sorry, I'm going back to a point that Vic made earlier. I think you need a review group that isn't contaminated by all the past thinking on this problem. You need a review group of really smart field dynamists, if they exist in the world, who have not been contaminated by the previous work of RELAP or TRAC or something and you want them to sit, have them review some of this basic stuff and see what they say.

MR. MAHAFFY: Look over at some of those guys. I don't disagree with it.

VICE CHAIR WALLIS: The last thing you

want is for them to shoot something down at the last minute when you are ready to issue the code. It should happen now.

MR. MAHAFFY: The one thing, let me give you one word of caution to that, in that when you get into field dynamists who are uncontaminated by RELAP and TRAC, generally what you are getting into are people who have done single phase CFD. And one experience that I have had over the years is that people who do single phase CFD are very naive about the kinds of problems that you get into when you go two phase. So they are good to a point.

VICE CHAIR WALLIS: Yes.

MR. MAHAFFY: It's very tough to pull together the kind of uncontaminated group of experts with the kind of knowledge of two phase that you need.

VICE CHAIR WALLIS: Well, I think it's very tough to get those who will agree with you, because if you had asked George Batchelor, who is a pretty revered member of the community before, I think he may have died by now, certainly before he had some problems with old age, he was saying 20 years ago a lot of this stuff is nonsense.

MR. MAHAFFY: A lot of people have said that.

VICE CHAIR WALLIS: Right. I don't think we can just brush it off.

CHAIRMAN RANSOM: I think it's tough to find people to serve that role, but I think it would be worth looking for a small group who could do that, that are not associated with say TRACE or RELAP5 and be knowledgeable enough to shed some light on that.

VICE CHAIR WALLIS: People generally would be respected, I think, by a very broad group of professional people.

MR. MAHAFFY: Anyway, let me tell you where we are headed, so we can think about the group. Okay. Because to me, SETS is a thing of the past for large part. It did its job, but that wasn't --

VICE CHAIR WALLIS: Just call it SETS so it gets into the record spelled properly.

MR. MAHAFFY: Yes. But to me when I created that thing, it was a patch until we could get into a log where we could run true, fully implicit calculations. Okay. The French with CATHARE went that direction and they have been pretty darn successful. Basically, you need to look at your implicitness at two levels. There is the issue of the coefficients and as you will see, we can deal with that within the context of the current numerical methods fairly directly, and we have already dealt with part of it.

And then there is the question of stepping up to the issue of evaluating your transfer terms implicitly to bring it up to a fully implicit level. And as I said, only the French with CATHARE have really stepped up and done the full blown problem. It took them a long time. You can't kid yourself. There's a lot of engineering that goes on with this.

The more non-linear terms you get in your equation set, the more problems you get with convergence and the more little bits of tricks you've got to come up with when you sit down and make your initial guesses on the Taylor series expansions that are fundamental to your iterative solutions of your non-linear equations.

What I want to do is show you something. Some of you may have seen this before. It's an example of something I used as a test problem when I was first looking at this business of linearized implicit terms for interfacial drag. And the little plots I'm going to show you actually probably came out of RELAP5 rather than out of TRAC or TRACE. But there is an important lesson here. There we go.

VICE CHAIR WALLIS: Well, what is the

geometry of this column?

MR. MAHAFFY: This is a very simple column. I don't remember the cross sectional area. That doesn't really matter to some degree. It's a straight vertical column. My recollection was it was about 10 feet high and stagnant water injected bubbles at the bottom and you'll see a plot of the void fractions. But it was a low void fraction, 2 or 3 percent. And you let the bubbles rise up through the column.

VICE CHAIR WALLIS: They grow?

MR. MAHAFFY: Yes, they grow as the pressure changes. Again, it was only about 10 feet of water.

VICE CHAIR WALLIS: There's no face change?

MR. MAHAFFY: There is no face change. It's an air bubble problem. I wanted to cleanly separate issues here. I don't want the face change contaminating what is going on with the interfacial drag on the bubbles.

VICE CHAIR WALLIS: From what I know about bubbles, it's rather remarkable, they go in at 1.6 meters a second.

MR. MAHAFFY: Yes, up here? Isn't that

pretty impressive? It's an instability. Okay. It's an instability based off of this term that you get.

VICE CHAIR WALLIS: It must be a momentum equation of some sort.

MR. MAHAFFY: Yes, you've got a momentum equation.

VICE CHAIR WALLIS: You've got no added mass. You've got to have added mass when you're dealing with a bubble, because all that's inertia is in added mass.

MR. MAHAFFY: As long as the bubble is not accelerating.

VICE CHAIR WALLIS: But it is, because you've got a T velocity.

CHAIRMAN RANSOM: Well, is this a problem with linearization of the interfacial drag where you've got absolute value of old time velocity difference times new time velocity difference?

VICE CHAIR WALLIS: It's going to be Vdt. You've got to put a Rho in front of that.

CHAIRMAN RANSOM: And the old time velocity difference goes to zero so the drag goes to zero?

MR. MAHAFFY: Okay.

VICE CHAIR WALLIS: I think the problem is your bubble has no inertia, so any force out of bounds will give it an infinite acceleration.

MR. MAHAFFY: All right. Here's the deal. Now, the root of the problem is this. You've got, numerical, a forcing term here for your drag that's based on an old time.

VICE CHAIR WALLIS: Right.

MR. MAHAFFY: And a new time. Okay. And I actually, at one point, did a derivation to show you can derive some kind of a stability bound on this, but that term is fundamentally numerically unstable.

VICE CHAIR WALLIS: That's supposed to balance gravity essentially in this problem.

MR. MAHAFFY: In this problem, that's what is going to happen.

VICE CHAIR WALLIS: And if it is not quite in balance, your bubble which has no inertia, because you haven't given it any added mass --

CHAIRMAN RANSOM: It shoots up to high velocity.

VICE CHAIR WALLIS: It shoots up to high velocity.

CHAIRMAN RANSOM: Now, the next line --

VICE CHAIR WALLIS: Now, the next time it has a big term.

CHAIRMAN RANSOM: Yes.

MR. MAHAFFY: Yes, even --

VICE CHAIR WALLIS: Give it some added mass.

MR. MAHAFFY: Ignore this. Okay. Pretend this doesn't happen, because it does drop down. But even after it has dropped down, this business has to do with the graphic added frequency. What is happening is you have got an envelop here, you see those velocities --

VICE CHAIR WALLIS: Yes.

MR. MAHAFFY: -- oscillating up and down? You know, there is no question of crossing lines or anything. It has established some kind of mean velocity it wants to run at and it is oscillating back and forth across that. The second line, I've gone in and I've dropped the timestep size, done nothing else. All I've done is I've set the timestep size down to a millisecond, which was under the threshold for this particular instability and it comes up and everything is smooth.

VICE CHAIR WALLIS: What's the terminal velocity of this bubble?

MR. MAHAFFY: Now, you're getting a terminal velocity here of about .15 meters a second.

VICE CHAIR WALLIS: That's what I could calculate on the back of an envelope.

MR. SIEBER: Yes.

MR. MAHAFFY: Yes.

CHAIRMAN RANSOM: Okay.

MR. MAHAFFY: Yes. You could do that if you had --

VICE CHAIR WALLIS: And that's the right answer?

MR. MAHAFFY: I hope so. I don't remember what the correlations were in here.

CHAIRMAN RANSOM: Why did you say you've got this with RELAP5?

MR. MAHAFFY: Yes, I believe these plots were from RELAP5. I've got the same results out of old versions of TRAC.

CHAIRMAN RANSOM: What about TRACE?

MR. MAHAFFY: TRACE? Well, TRACE does the linearized terms and this is gone.

(Whereupon, at 5:00 p.m. the meeting continued into the evening session.)

E-V-E-N-I-N-G S-E-S-S-I-O-N

5:00 p.m.

CHAIRMAN RANSOM: All right.

MR. MAHAFFY: RELAP5 now has the linearized implicit drag term.

CHAIRMAN RANSOM: So it tends to damp this?

MR. MAHAFFY: It will get this answer right here. Okay? Okay. If you run the linearized implicit, it will get this same answer, even at substantially higher timestep sizes. A tenth of a second.

CHAIRMAN RANSOM: Will it oscillate like that?

MR. MAHAFFY: No, it will not.

CHAIRMAN RANSOM: Why did it take 10 seconds oscillating unrealistically?

MR. MAHAFFY: Part of that is an artifact and you will see it in the next view graph. You see that?

CHAIRMAN RANSOM: It's a void fraction that's so low. Is that what it is?

MR. MAHAFFY: Yes, you know, there is less void there. As the void fraction settles out to its final value, this right here is just numerical diffusion really operating.

CHAIRMAN RANSOM: You said something, on that previous slide you had two timesteps. One was .001 and one was .02.

MR. MAHAFFY: Yes.

CHAIRMAN RANSOM: And the solid curve, I guess, is .001.

MR. MAHAFFY: Yes.

CHAIRMAN RANSOM: So there were no oscillations in that case.

MR. MAHAFFY: That's right.

CHAIRMAN RANSOM: Sufficiently small.

MR. MAHAFFY: Yes.

CHAIRMAN RANSOM: That's only when you get the larger timeset you get all numerical.

VICE CHAIR WALLIS: In a way you just stop bubbling at the bottom of the column. You get an expansion wave and the first bubble gets free of the other bubbles and goes rushing out at the highest velocity of all.

MR. MAHAFFY: Yes.

VICE CHAIR WALLIS: Expansion wave of void fraction. That's what's probably happening in the beginning of this whole thing.

MR. MAHAFFY: Yes, and then you're

triggering --

CHAIRMAN RANSOM: These are all numerical.

MR. MAHAFFY: Yes, but this is --

VICE CHAIR WALLIS: It's not entirely numerical. The bubble velocity at the top is higher at the beginning, because the bubble is on its own. When the other bubbles catch up and make a higher void fraction, the bubble velocity drops.

CHAIRMAN RANSOM: If I interpret this right, the bubble in the small timestep case took 10 seconds to reach the top station where you are looking at it.

MR. MAHAFFY: Yes.

CHAIRMAN RANSOM: I'm saying that the trend is be back here when it was predicting these large velocities that reach the top went sooner.

VICE CHAIR WALLIS: Well, others have a velocity at the top of the column when it isn't there.

CHAIRMAN RANSOM: Well, it got there.

MR. MAHAFFY: Yes, it got there.

CHAIRMAN RANSOM: Because of these velocities.

MR. MAHAFFY: You see, it does have a zero velocity.

CHAIRMAN RANSOM: It took this long.

VICE CHAIR WALLIS: Well, I'm saying it does have a higher velocity than the other bubbles.

CHAIRMAN RANSOM: Well, only numerically.

VICE CHAIR WALLIS: No, no, no, it physically does, too.

MR. MAHAFFY: Yes.

VICE CHAIR WALLIS: Because there's more drag when you get more bubbles side by side.

MR. MAHAFFY: My guess is that the physical models in either RELAP5 or TRACE are not sophisticated enough to catch the phenomena you are talking about.

VICE CHAIR WALLIS: Well, this is a very simple example.

MR. MAHAFFY: Yes.

VICE CHAIR WALLIS: We should be able to represent.

MR. MAHAFFY: But two things I want you to take away from this example, I mean, regardless of what's going on. The first is, okay, explicit evaluation of certain terms results in numerical instabilities. They tend to be bounded oscillations. And secondly, the mean value of these bounded oscillations you have no guarantee that it's the correct mean value. And you get a lot of analysts, I've seen this happen over my entire career, saying you'll get these things, any of these codes that you run, you get these funny oscillations in there.

The worst thing I hear analysts say is oh, gee, look there is jitter in the experiment and there is jitter in our calculations. We're doing great. You know, it's two different things all together. But the other thing they assume is that there are oscillations and their results are oscillating about the correct mean value. And you are not assured of that with this class of instability. So it's something you want to avoid.

VICE CHAIR WALLIS: Do you do this with your seniors? I mean, do you have them run TRACE on very simple problems like this one and see if there are any anomalous results?

MR. MAHAFFY: Well, that's the whole purpose of this class.

VICE CHAIR WALLIS: You see what happens in most text books is there are, you know, a hundred problems per chapter, which illustrate the methods. And if TRACE is a really mature code, you ought to be able to have a whole lot of simple, simple problems illustrating the method which give reasonable answers.

MR. MAHAFFY: See the purpose of this

class I teach is not necessarily to focus on the reasonable answers. It's to inform students of the kind of problems that you get into with numerically-based simulations, so that they can make good solid engineering judgments and not just say ah, yes, the computer said this, here is truth.

VICE CHAIR WALLIS: But NRR isn't necessarily going to do that when they use TRACE.

MR. MAHAFFY: Well, send them to Penn State for a semester. Anyway, okay, in terms of --

VICE CHAIR WALLIS: There is one anomaly you should --

MR. MAHAFFY: It's one anomaly. It's just one example. You know, there are lots of instabilities you get off of heat transfer coefficients. Right now, in terms of getting things more implicitly evaluated, the first step that we have taken is there is a switch in TRACE that you can flip and you will evaluate, not quite fully implicitly, I'll tell you the caveat in a second, the interfacial heat transfer coefficients. Okay.

And not quite as simply that right now we're not evaluating any contributions from velocities implicitly, that phases into another part of generating elements to the Jacobian that I didn't want to do right away.

VICE CHAIR WALLIS: Is the bubble detached from the top of this column or and if you had a foaming mixture, the bubble would never burst.

MR. MAHAFFY: Yes.

VICE CHAIR WALLIS: How do you get it out of this column?

MR. MAHAFFY: The column, there's a pressure boundary condition that it just wanders into.

VICE CHAIR WALLIS: Something that lets it wander out?

MR. MAHAFFY: It just wanders out into a pressure boundary condition.

VICE CHAIR WALLIS: If you had a foaming solution, it would never get out of the liquid. How does it know whether it is a foaming solution or not?

MR. MAHAFFY: Physics aren't smart enough in this code to do that.

VICE CHAIR WALLIS: It seems to me it's very important.

MR. MAHAFFY: For this example, no.

VICE CHAIR WALLIS: But if I take two columns, one soapy water and one is still water and I bubble air into one and bubble air into the other, I get complete different answers.

MR. MAHAFFY: Yes, I'm sure you do.

VICE CHAIR WALLIS: Well, there's got to be something that's predictable with TRACE.

MR. MAHAFFY: I would have to change properties in TRACE. I don't have any soapy mixture properties in TRACE right now.

CHAIRMAN RANSOM: TRACE has the numbers knowledge --

VICE CHAIR WALLIS: No, no, it's just a question of whether or not the bubble bursts at the top.

CHAIRMAN RANSOM: Well, that's different circumstances.

MR. MAHAFFY: Yes. Anyway, so right now, we can run with these implicit interfacial heat transfer coefficients. It is not the default option in the code and, in fact, users can't get at it right now, because there are going to be problems. Okay. The biggest problem I found when I implemented this and submitted it, I ran a full regression test set. Understand that every time we create a new code version, there are like 1,400 test problems that are run before the update is accepted.

I ran those 1,400 test problems and I think something like two-thirds of them ran and then one-third of them just died on me. I started working my way through the ones that died and I got through about 20 of them and in every case it had to do with the way the TRACE correlation package right now deals with condensation of a sub-cooled vapor. That needs to be fixed. There are some other outriders.

Joe Staudenmeier has found some cases that are not related to that. I found one just the other day. We have just got to slowly go through there. When you are doing fully implicit evaluations of things, you don't like to have discontinuities. Right? Sometimes even abrupt changes in the wrong direction.

VICE CHAIR WALLIS: Let me see if what you are telling us is that TRACE is still in this other research stage and it's not in the stage of being a tool.

MR. MAHAFFY: It is a tool.

VICE CHAIR WALLIS: An engineering tool.

MR. MAHAFFY: It is an engineering tool to the extent TRAC or RELAP were engineering tools. But now I'm talking --

VICE CHAIR WALLIS: So we're talking fundamental things which can lead to quite anomalous answers.

MR. MAHAFFY: In terms of the picture I just showed you, what I am telling you is that I resolved that one, but there are fundamental things.

VICE CHAIR WALLIS: With the initial timestep.

MR. MAHAFFY: No.

VICE CHAIR WALLIS: No?

MR. MAHAFFY: We did a linearized implicit form of that and I'll be happy to show you the equations if you want to see them. I've got them in an appendix here.

VICE CHAIR WALLIS: I mean, the real thing you need is an added mass.

MR. MAHAFFY: In this case, I would argue that I don't think the added mass would change this.

VICE CHAIR WALLIS: You put in a bubble.

MR. MAHAFFY: It doesn't. Okay?

VICE CHAIR WALLIS: You put in a bubble density of zero, you've got infinite acceleration.

MR. MAHAFFY: No. Let me remind you those curves were generated with RELAP5. It hasn't had a mass term in it.

VICE CHAIR WALLIS: Oh, so that's --

MR. MAHAFFY: Okay?

CHAIRMAN RANSOM: So it may or may not.

I don't know what they have in it. Do you know?

MR. MAHAFFY: When this was run the added mass was definitely there.

VICE CHAIR WALLIS: Okay. Well, then --

CHAIRMAN RANSOM: It is there in mod 2, but I don't know if they have it on mod 3.

MR. MAHAFFY: It's in mod 3 also.

VICE CHAIR WALLIS: It's not in TRACE.

MR. MAHAFFY: No.

VICE CHAIR WALLIS: You said you didn't touch it with a barge pole.

MR. MAHAFFY: Yes, I get the same answers with TRACE, the same oscillations of RELAP5 in TRACE, so it didn't make any difference here. Now, understand that when we're talking about this, we're now into step 2. And again, I'm telling you we've got this overlap between the consolidation and the advanced development. I'm not talking to you about advanced development. Yes, we're in exploratory work. Only the French have been here before and they have done it in a slight different context.

We are going to have to work through a number of numerical issues and correlation package issues to get all of this fully implicit technology to work for us in our numerical methods. When it is done, you're going to see much better behavior. You're going to be seeing much higher timesteps, much more reliable answers, because you don't have these strange boundary instabilities that are doing unpredictable things to you.

Right now, what I'm working on, and I would be working on if I wasn't sitting here in front of you, is getting the implicit evaluation of the mass and energy flux terms. Very shortly, probably next week, I want to get going on the implicit evaluation.

CHAIRMAN RANSOM: It already is implicit now, right?

MR. MAHAFFY: Huh?

CHAIRMAN RANSOM: Well, on the mass and energy terms, the velocities are implicit.

MR. MAHAFFY: The velocities are implicit, but, you know, if you just look --

CHAIRMAN RANSOM: Density and void fraction.

MR. MAHAFFY: The density is explicit in the semi-implicit step and the only way it becomes implicit is with that corrector step.

CHAIRMAN RANSOM: Yes.

MR. MAHAFFY: What I'm talking about now is a true full implicit method. There is just one step. And in that one step, the densities and the velocities are all implicit in the flux term. Okay?

CHAIRMAN RANSOM: Okay.

MR. MAHAFFY: And that's, you know, RETRAN does that.

CHAIRMAN RANSOM: There's some iterative process you have to go through to get to that point.

MR. MAHAFFY: Sure. It's a non-linear set of equations and we have to solve the non-linear equations. No question about it. There is also the question of getting implicit evaluation of the wall heat transfer. Part of that has been done by Jay Spore. I've got to get his update adapted and into the code, and then I've got to push it on to finish the job.

The last stage of this development will be to engage a full implicit evaluation of all the terms in the motion equation, so we get the implicit choke flow model, the implicit interfacial drag coefficients. When this is done, what I see is that there probably will really be three options in TRACE to begin with, and they will be winnowed down. You can run in the old mode that's familiar to people who use RELAP5 and TRAC, which is, you know, semi-implicit type methods or a SETS nearly implicit type method or you can take one step up off of that and you can engage implicit coefficients or you can take the final step and you can engage implicit transfer terms also in mass and energy and momentum and go to a fully implicit method. So that's the direction we are headed with the numerical methods.

MR. DENNING: Now, do you see that, you're talking about a release two years from now, do you see a fully implicit capability two years from now? Is that what you're saying?

MR. MAHAFFY: It will certainly be available. Two years, you know, based on my knowledge in numerical methods and my knowledge of the development history of CATHARE, two years is where we're just beginning to get reasonably robust with the fully implicit method. It's still going to have its problems. There will be odd glitches here and there, because something in some correlation package has still got some odd jump in it that somebody hasn't found yet. And we'll be working through those issues. But it should be close.

CHAIRMAN RANSOM: John, a couple of comments on that. One, even with that capability, there are some problems that, I mean, say at least are material limited in terms of the accuracy of the answers you're going to get.

MR. MAHAFFY: Right.

CHAIRMAN RANSOM: So you would want to restrict the timestep in those cases.

MR. MAHAFFY: You never --

CHAIRMAN RANSOM: And the second one is do you know anything, is there any comparison of let's say CATHARE has been able to achieve to show what the benefits are, you know?

MR. MAHAFFY: Too bad Joe Kelly is not here. He used to work on the CATHARE Team. But CATHARE is run. Right now, it is at the heart of EDF's real time reactor simulator for training operators, for example. It's a very fast code and then I have seen in nodalizations they use on those and, you know, they've got many hundreds of nodes.

CHAIRMAN RANSOM: Well, it would be interesting to see if you have like the TRACE Project, it would be interesting to have some benchmarks against that code, you know, to see basically how you compare.

MR. MAHAFFY: Yes, it would. It would.

CHAIRMAN RANSOM: I don't know how the French feel about that.

MR. MAHAFFY: Yes, the DNRC needs to

negotiate some of agreements with the French or some such to pull that off.

CHAIRMAN RANSOM: In fact, at one time, I think the French --

MR. MAHAFFY: Actually, we have access to CATHARE.

CHAIRMAN RANSOM: -- made that available to the NRC.

MR. STAUDENMEIER: Yes, and I mean, we're even allowed to use their models and correlations if we want to.

CHAIRMAN RANSOM: Yes.

MR. STAUDENMEIER: The one thing that they do want is to be able to review any publications that we have that compare TRACE and CATHARE to make sure that we're not using it in some unreasonable way and bashing the code.

MR. MAHAFFY: Yes.

MR. STAUDENMEIER: And based on our not knowing how to use it essentially.

MR. MAHAFFY: But you bring up a really important point, Vic, and that has to do with problems where the material Courant stability limit says something about the physical phenomena continuity ways and whatnot. And we don't propose to ever eliminate the semi-implicit method from an option set here, because, as you saw, Tom Downar runs it for stability analysis, because it has the least numerical diffusion.

One thing that I have looked at on the side, it's not a project I have reported to you here today, we're going to study some high order methods for use in two phase flow. And what we did was to take some of the ones that show up in some of the advanced CFD quicklist. We also did a leaf method. And I'll tell you, it's dicey at best. Those methodologies when you cut over to two phase flow are just not robust enough to hold up against abuse.

And we've done this is a fully implicit two phase context, which is where you've just about got to do it, they boil down so long in the iterations to do a decent job of solving the problem, that as far as I can tell, you're just about as well off running the semi-implicit method with smaller mesh size to get the same kind of, you know, artificial diffusion that you would with the second order or third order upwind schemes.

CHAIRMAN RANSOM: Well, even the Marviken problem talked about a lot of one characteristic of that is that thermal interface must be propagated realistically. Otherwise, you begin to defuse the energy and change the pressurization in the tank.

MR. MAHAFFY: Yes, but, you know, the point of our studies, and I'll get a publication out of that at some point here, to propagate that interface realistically, it's not a 1-D problem anyway. It's got to be done at least in two dimensions.

CHAIRMAN RANSOM: I would guess that's probably true.

MR. MAHAFFY: Yes.

CHAIRMAN RANSOM: But I don't have any data one way or the other.

MR. MAHAFFY: Yes. We did some preliminary 2-D calculations and you really need them.

CHAIRMAN RANSOM: And I guess what I would encourage you to do is have your timestep algorithm smart enough, I guess, to recognize when it is needed to restrict the timestep, even if you use a full implicit formulation.

MR. MAHAFFY: Oh, yes, that's a given. That's a given. An awful lot of our timestep control algorithm is based on, you know, net changes in various variables, percentage changes. It's got nothing to do with stability limits. I'm not going to let my void fraction change by more than .05 per timestep, for instance.

There are all kinds of restrictions in there. And they have to be refined, but they will always be there. But in the long run, you know, as people have always found it doesn't excuse the user from paying attention and doing timestep sensitivity studies to quantify their error.

Okay. We're past everything I wanted to say.

CHAIRMAN RANSOM: Okay. Well, I guess --

MR. MAHAFFY: Two hours in.

VICE CHAIR WALLIS: Well, you've got this 25 and 26 in talking about momentum equations and you're playing around with the versions of velocity. Just to say that when there is a flow that goes around the bend or something like that, you know, the momentum equation is really written from dVx dt dVy dt.

MR. MAHAFFY: Right.

VICE CHAIR WALLIS: The dVx dt is termed like dy/Vy, dVx dy, a convergence term. When you go around the bend, I don't see how you can throw out the dVy Vx. What is V? Is V in the direction of the pipe? I mean, there's no way you can translate this momentum equation, which has three directions, into velocity in the direction of the pipe.

MR. MAHAFFY: It's a 1-D approximation.

VICE CHAIR WALLIS: There is no way you can do it.

MR. MAHAFFY: There is no y.

VICE CHAIR WALLIS: That's something else.

MR. MAHAFFY: That's right.

VICE CHAIR WALLIS: You have to turn it into a newly equation or something.

MR. MAHAFFY: What you have to do and what I will do at some point is a clear derivation of this equation from the rigorous --

VICE CHAIR WALLIS: Flow goes around a 180 degree bend. It comes in in one direction and goes out the other direction, no friction. It simply has turned its momentum from one direction to the other.

MR. MAHAFFY: That's right.

VICE CHAIR WALLIS: And there is no way you're going to convince me there's no change in momentum.

MR. MAHAFFY: Okay.

VICE CHAIR WALLIS: But according to your equation, there isn't a change in momentum. So there's something, you know, very fundamental about that.

MR. MAHAFFY: See, that goes back, you know, again, I'll speak to you as a licensed physicist and that is, you know, I make mistakes. I said it before, I'll say it again. In one dimension approximation momentum is meaningless.

VICE CHAIR WALLIS: Well, isn't the dimension a long pipe?

MR. MAHAFFY: Yes.

VICE CHAIR WALLIS: Or is it the dimension of X, Y, Z?

MR. MAHAFFY: It needs to be done and hasn't been done and maybe I'll learn something that I didn't suspect the last time I did this. I'll do the derivation for you again. You have to think of these equations as being a form of the kinetic energy equation. That's the only way you can justify it.

VICE CHAIR WALLIS: But it isn't. It's a momentum equation.

MR. MAHAFFY: If it looks like one --

VICE CHAIR WALLIS: Anyway, I'm just saying there is a fundamental problem which we have had before, we've talked about it.

MR. MAHAFFY: Yes.

VICE CHAIR WALLIS: And it doesn't seem to have gone away with your presentation here.

MR. MAHAFFY: I can get you that form of the equation starting with the kinetic energy equation. All right? I have to make some approximations and I will spell them out for you.

VICE CHAIR WALLIS: A single phase, there's no problem, I think, with that.

MR. MAHAFFY: Okay.

VICE CHAIR WALLIS: But two phase you might have a difficult time.

MR. MAHAFFY: It's a little dicey.

VICE CHAIR WALLIS: Right.

MR. MAHAFFY: But, yes, we need to do that. But that's the only theoretical way you can justify this kind of --

VICE CHAIR WALLIS: Okay. And that's the kind of thing that's going to be --

MR. MAHAFFY: -- motion equation.

VICE CHAIR WALLIS: -- in the documentation eventually as a proper derivation, which makes sense, which will convince a reasonable person the backbone for the mechanics?

MR. MAHAFFY: I think that's been promised to you before and I'm sorry I can't do it today. We don't have enough time even if I had it in front of me, I guess.

VICE CHAIR WALLIS: Another problem here that isn't trivial that if you try to present this stuff to the editors of the General Fluid Mechanics or some respectable junk, Physics of Fluids or something. I think many of you have some difficulty with the way things are manipulated.

MR. MAHAFFY: Well, just as --

VICE CHAIR WALLIS: I think we owe it to this professional community to convince them this is an okay thing to do. The argument usually is well, whatever happens with all of this stuff and whatever you think about it, it works. That seems to be the answer that everybody falls back on.

MR. MAHAFFY: It's the ultimate engineering solution.

VICE CHAIR WALLIS: Right.

MR. MAHAFFY: We can actually do better than that.

VICE CHAIR WALLIS: I think you have to do better than that.

MR. MAHAFFY: But as I told you, you know, momentum is three dimensional. It's a vector quantity.

VICE CHAIR WALLIS: Yes.

MR. MAHAFFY: I have to come up with a scale equation for you. And the way you get a scale equation is by starting with the kinetic energy equation and clearly indicating the approximations you have to make to get to the point that you see in these manuals.

CHAIRMAN RANSOM: Thank you. Next is Christopher Murray, who is going to talk about verification issues.

VICE CHAIR WALLIS: Well, we seem to be still on time reasonably.

CHAIRMAN RANSOM: Right. And define what you mean by verification.

MR. MURRAY: It's defined, yes. Where is the microphone?

VICE CHAIR WALLIS: There is one verification that says does the code represent the equation. And the other verification is does the code represent in a reasonable way reality? These are quite different things. And you're going to tell us which verification you are talking about?

MR. MURRAY: Yes, I think. I always drop back to the software engineering view and use that.

VICE CHAIR WALLIS: So you say that the code must represent the equations?

MR. MURRAY: I say that the code has to do what you intended it to do. If you meant to say X equals 1, well, you better have said X equals 1, not X equals 2.

VICE CHAIR WALLIS: Well, that's right. Does it represent the equation.

MR. MURRAY: Yes.

VICE CHAIR WALLIS: But you don't go back and question whether or not the equations are good approximations of reality?

MR. MURRAY: No, no, that's validation in my view.

VICE CHAIR WALLIS: That's fine.

MR. MURRAY: Whether it represents --

VICE CHAIR WALLIS: That's different.

MR. MURRAY: -- reality. Is this working?

VICE CHAIR WALLIS: So all we're interested in here is the software clear representation of the math.

MR. SIEBER: Right.

MR. MURRAY: That's correct.

VICE CHAIR WALLIS: It would be a trivial question.

MR. MURRAY: Yes, I'm here to sort of address some of the issues that the letter had with QA and testing. Like John did, I broke it down into three. There were essentially three bullet points that I took away from the letter. NRC has no approved software quality assurance procedure. The NRC Codes have never undergone verification. And the NRC Codes have not been assessed under an approved qualified procedure with "frozen" versions.

I can't speak to all the previous codes. I don't have the longevity in this business that everybody else in this room has. But I can certainly answer some of these questions about TRACE.

As to the SQA procedure, there is two documents that are in the NRC archives, I guess, on two NUREGs that do govern our quality assurance procedures. The first NUREG is, I think, a more general brochure, but the second one was written in 1999/2000 time frame by Frank Odar or at least that was the point at which it was published as a NUREG. It was written a bit sooner than that, I know, because I saw the drafts when this project first started back in '97/98 time frame.

And this is the document right here, actually both of them, so they do actually exist in paper that we can get. I think the NUREG-1737 is the one we base most of our processes off of. The essential item there calls for development of a project Specific Quality Assurance Plan or an SQAP for each code development effort. And that was done back in '98 by a couple of people that were involved in the development back at that time that are no longer with the NRC.

That document, unfortunately, is not in some archival format. I had to pull it off of some files. I remember getting an email with that plan back when I was at Penn State and started on this project and we went back and dug it up. In addition, some of the contractors do have their own internal quality assurance procedures. This SQAP that was developed for TRACE supersedes all of those. Traditionally, that's what NRC relied on in terms of its code development, was that each lab or institution that was doing its development for NRC had to have one of these quality assurance procedures in place.

The SQAP basically addresses the lifecycle of a code update. It takes you from the time of conception of whatever fix or feature needs to be put into the code through its development, what documentation needs to be there and the amount of testing that gets applied to the code. And then once it has been submitted to the NRC or to the code caretaker at the NRC, what gets done with it at that point to the point that it is put into our configuration management system.

CHAIRMAN RANSOM: Within the development system is there any actual review of an individual's coding?

MR. MURRAY: Yes, I'm going to go through that. That does get done. I think one of the things I can talk to is it doesn't always -- especially for the large updates, we don't always do line-by-line reviews. I certainly am looking at the patch files that come in and through a combination of the tools I use for applying the code updates and just visual inspection, I can catch a lot of errors. It doesn't mean I catch them all. I mean, but the very big updates that come in, I don't always have the knowledge.

Like let's say John's implicit work, if I were to try to do a line-by-line review, I don't have the necessary knowledge to really review that from a line-by-line standpoint.

CHAIRMAN RANSOM: I know one thing that was discussed a long time ago was the concept of Argolis programming or coding in which I guess is used in large software projects and that's where one guy develops it, writes it up and then another person actually recoates it, you know, and presumably would even, if there is any modeling involved, put his own slant on that until finally, you know, there is some agreement between the individuals.

Now, it's a very expensive process, but one that probably results in, you know, higher quality products, and it would be interesting to know whether people like Microsoft what they do in these regards.

MR. MURRAY: The new buzzword they have in the software engineering community and, you know, codevelopment is this "Extreme Programming Model."

CHAIRMAN RANSOM: It's what?

MR. MURRAY: It's called "extreme programming" and in that model they actually have programmers sit side-by-side.

CHAIRMAN RANSOM: Yes.

MR. MURRAY: Program at the same terminal. I think companies like -- another technique that is used widely is they do actually have meetings. I forget the official term for it, but you get a group of five people or so, go into a room and you do line-by-line reviews up on the wall of chunks of about 100 lines at a time, because they found, you know, there is a whole art to this and they find that that's about the amount that you can really reasonably work with in a finite amount of time.

And there are software engineering companies that do that. I don't know that Microsoft does that, but --

CHAIRMAN RANSOM: Any thought about using those kind of methods in your work?

MR. MURRAY: We try. See, I try to inject some of that and I'm going to go through some of that a little bit later. I try to inject as much as I can, the look at individual lines and I'm going to touch upon that a little later.

MR. DENNING: Christopher, I'm having a hard time telling as you're discussing this, is this the way it is supposed to be done at NRC or is this the way it is actually happening right now? I mean, it was obvious when you talked about the plan that it was buried some place.

MR. MURRAY: Yes.

MR. DENNING: So obviously that plan was not in front of everybody being immediately followed. These things you are talking about here, is this the way it is supposed to happen?

MR. MURRAY: Right now, this reflects the plan.

MR. DENNING: That reflects the plan?

MR. MURRAY: Yes.

MR. DENNING: But you dug the plan up from the -- what is actually happening right now or what has been happening over the last four years?

MR. MURRAY: Everything in here does happen. There's a couple of slides afterwards that I'm going to show here is where the weakness is that I see are and here is where we're not meeting all of these all the time. Okay. So I think I'm trying to be honest about that and I'm not claiming that every individual is hitting every element for every single update. You know, this is a people process.

MR. DENNING: I mean, it doesn't surprise me that you haven't reviewed every piece of coding. But I haven't heard that there is an independent review of every piece of coding and that I certainly would think is a minimum. Some technical person must be doing an independent review of every piece of technical coding. Is that happening or is that not happening?

MR. MURRAY: There is in a submittal that I get one of the lines on there is that it needs a reviewer, other than me.

MR. DENNING: Yes.

MR. MURRAY: And I have actually gotten requests to be the reviewer and no, I don't want to be on that line. I'm a third step in the process. And somebody has to review that submittal. Now, I don't always know to what level that reviewer is looking at things. That's why I'm saying I don't claim that every update gets each individual line reviewed. The reviewers are looking at, from an architectural standpoint, does the update meet sort of our goals for the code? Are we missing major features that a user wants to see?

You know, these codes have so many different features or restart where you can restart off of a previous calculation. And if I'm adding a new variable to the code, am I maintaining my ability to restart from previous calculations? So there is logistical type features that need to be there and these reviewers should be looking for that in addition to are the equations correct? There is a requirements document that gets written and they should be looking and saying hey, are we missing something in the equations?

MR. DENNING: But I'm still not hearing you say that confidently somebody is independently reviewing every piece of coding.

MR. MURRAY: Not --

MR. DENNING: That some qualified person is?

MR. MURRAY: Not line-by-line.

MR. DENNING: Not line-by-line?

MR. MURRAY: I mean, it's --

MR. STAUDENMEIER: This is Joe Staudenmeier, NRC. I know when I review stuff I generally either review line-by-line or come very close to line-by-line review of code. I mean, if there is a block, I will kind of look at blocks and understand what they mean. I mean, maybe I missed a typo somewhere in that block, but generally I understand what the blocks are doing. But I can't speak for what everyone does, but I expect that that's the level that people are looking at code and understanding what is going on and checking the test cases that are submitted with it that are done.

I know myself, I'll take the changes and compile them into a local version of my code and run test cases in addition to what the person has done to make sure that it works okay. I'm taking it beyond what is normally done by most people probably, but that's what I tend to do. But I mean, some changes maybe 10,000 lines a code or something like that if a big change came in adding a lot of things like change the whole numerical method or thousands of lines of codes.

So in that type, I can't say that I read every line, but I do read at least at the block level and understand what is going on for that. And even if I read every line, I can't claim that that would keep all errors from going by.

VICE CHAIR WALLIS: We have had this problem with vendors who have brought us codes and documentation. It would be the documentation. Let me go down to some equations and say well, it looks to us as if this j+1/2 should be j-1/2 in this term in this equation. I mean, in some cases you're right, there's an error. We go to another equation. We say this looks as if it should be something else and say oh, yes, you're right. We'll fix that. And then they say but it's okay in the code.

Now, how do I get any assurance that it's okay in the code when it's not okay in the documentation?

MR. SIEBER: The problem is --

MR. MURRAY: Right, that's valid. That's a valid concern.

MR. STAUDENMEIER: Yes. Actually, one

thing that is supposed to be done is that the equations, base equations in the code, are supposed to be confirmed and I think in terms of that, if there is a change to an equation or a correlation, we will be checking that the Theory Manual is consistent with what's in the coding.

VICE CHAIR WALLIS: But even when there is no change, when it's simply somebody typed it wrong in the documentation or something.

MR. STAUDENMEIER: Right.

VICE CHAIR WALLIS: What is the reference, which says sort of explicitly what it really should be? If it's not in the documentation, where is it?

MR. STAUDENMEIER: Well, hopefully, it's traced back to an initial paper that --

VICE CHAIR WALLIS: Is it something that's happening --

MR. STAUDENMEIER: -- in development that's beyond the documentation.

MR. SIEBER: But validating the phenomenology is not what this process is, right?

MR. STAUDENMEIER: No, right.

VICE CHAIR WALLIS: No, but I'm saying --

MR. SIEBER: And so it doesn't make any difference what the physics and the thermodynamics and the hydraulics are of the situation. It's whether whatever somebody decided it was going to be and whatever numerical method they are going to employ is coded properly.

VICE CHAIR WALLIS: As long as you know what should be coded, but if the documentation is itself garbled, how do you know what should be encoded?

MR. SIEBER: My personal experience in writing some pieces of the --

MR. MURRAY: I mean, my approach -- sorry.

MR. SIEBER: -- is that you write the code before you write the documentation and you end up with a lot of comment cards inside the code, so that you don't lose your train of thought in the process of doing that, and then you flowchart all the logic and it won't compile if you have typos in it.

And then after you get it to run, you run all the test cases and somebody else is doing the verification and validation, that's when you start writing the manual and I think a lot of people do it that way.

MR. MURRAY: I mean, there are a lot of different approaches a person can take when they are first writing, you know, a subroutine to perform some task, solve some equations, you know, to verify that that's been coded correctly.

MR. SIEBER: Yes.

MR. MURRAY: And you can create separate driver programs.

MR. SIEBER: Right.

MR. MURRAY: That drive it outside of what you are eventually putting in to show that you're getting a correct answer.

MR. SIEBER: That's right.

MR. MURRAY: And create spreadsheets of values. And I did this for signal variables when I was modifying the signal variable logic in TRACE.

MR. SIEBER: Yes, that's a common --

MR. MURRAY: There are 100 and some odd signal variables and I had to have every single, you know, type with different inputs and then, you know, I had to calculate what should the output be from a certain control block and put it in there and go one-by-one and check what the code prediction was against what my, you know, side calculations showed. And if you don't do that, you have no confidence.

There's other techniques where you can do -- another thing you worry about is well, did the test that you have designed adequately hit every line of code? You have got to do coverage testing and show that every single -- in your test suite that you have built to test that feature, executes every line. Okay? Because if you're missing lines, you don't know whether it's right or not.

And there are some techniques that I advocate and I look for developers to follow, and I have a minimum standard and I really hope that I see more, but I also have to keep the development process moving forward, and so I have a minimum that I look for.

But going on to just what the SQA Plan talks about. It says, basically identifies, there's two kinds of code updates, differentiates between them. There's new features or modeling capabilities and there's bug fixes. By their nature, bug fixes don't require the same level of documentation because, presumably, you're just --

MR. SIEBER: Correcting things.

MR. MURRAY: -- fixing what you already have documentation for. So first we conceive of some desired modeling capability. All right. We identify which bugs get fixed. The developer has a certain set of responsibilities. The first thing they do is they prepare a Software Requirements Specification.

For anybody that's not familiar with what a requirement is, it's what the software should do from a user's perspective, how it should function, you know, what a user sees when they sit down and they are going to work with the code and what task they are trying to perform with it.

They prepare a Test Plan that ties each requirement, they tie a test to each requirement. So generally, in software engineering lingo, you usually, you know, bulletize your requirements, give a number, an ID number to them, and then you write a Test Plan that there is a test for every requirement, so that you can show that you have met all the requirements, and a reviewer would look to see if there are 10 requirements, there should be at least 10 tests.

The developer then also prepares a Software Design and Implementation Document. One thing that's going to happen, and it will become obvious in a second in the next slide, is that the NRC has some responsibilities, and so there's reviews inserted in here. I don't want to make it sound like the developer does all these things sequentially without any feedback.

The design document basically just states what or how the code will meet its stated requirements, and the developer performs the code development. They test their changes according to the Test Plan. They document that testing process in a completion report and they send us the submittal package.

On the NRC side, we're responsible for reviewing that SRS, reviewing the tests, well, reviewing all those documents. We review the coding provided by the contractor. I do some of that as a code caretaker. Some of the NRC staff does that. In some cases, a contractor reviews a contractor's work. We try to search for some independence in these sorts of information reviews.

We repeat and verify the results of the completion report. We incorporate the code changes into the configuration control system and I update our internal development website. We incorporate the code changes into the configuration control system and I update our internal development website. Once it's on the website, that new snapshot of a code is on the website, all the developers have access to it and can pick that up and be using that as a base.

VICE CHAIR WALLIS: Are there comments where if I have the code documentation, I have Equation 3.2.7.1, which is some version of a momentum equation or something, and I go to the code and I can see a comment, which says the following lines, put the coefficients into --

MR. MURRAY: I think some of that, yes, there are comments like that. I'm not going to sit here and tell you that every line is --

VICE CHAIR WALLIS: I can sit down with that documentation and the code and I can have a one-to-one correspondence of everything?

MR. SIEBER: No.

MR. MURRAY: Not everything.

VICE CHAIR WALLIS: It would seem to me you have to have that. Otherwise, there is no guarantee that the code represents what's in the documentation.

MR. STAUDENMEIER: I mean, it's not down to equation numbers from the Theory Manual in some cases.

VICE CHAIR WALLIS: But it would have to be.

MR. STAUDENMEIER: But it will be like interfacial drag for bubbly slug, for this. There will be a comment there and you can see what the equations are in the code and see what the bubbly slug equation was in the Theory Manual, but it may not have the Theory Manual equation number in the coding, and that would be actually pretty difficult to maintain. If you change the equation numbers in the Theory Manual, because you inserted more equations, then you have to have a process to go back into the code and change that equation number.

VICE CHAIR WALLIS: I think you need to do that.

MR. STAUDENMEIER: So if we were to do that, we would have to add some --

VICE CHAIR WALLIS: I think you need to do that, because these are the commonest causes of error. What you think is in the code isn't what you have written down in some authoritative document and you cannot make a one-to-one comparison, so then you get all sorts of errors.

MR. MURRAY: I think the other thing that it does, if you get into doing that, is it limits your code architecture. There's places where, you know, we have modularized things, so you solve things by component.

VICE CHAIR WALLIS: Yes.

MR. MURRAY: And some of these equations for a pipe, let's say, they apply to a pipe and a pressurizer and a pump and a valve, and so in each of those spots for each of those component types, you have got, you know, some, you know, code that repeats itself and you would have to maintain that in all four of those places.

VICE CHAIR WALLIS: Yes.

MR. MURRAY: I think that that would be very difficult.

MR. SIEBER: I think one of the harder things is if you're employing coding that is, basically, numerical methods to solve partial differential equations or simultaneous linear equations or something like that, the description of the problem you're solving in equation form, in mathematic terms, in the technical manual is going to be a lot different than what the coding is. You know, you would look at this and then you would go to, in my case, the IBM Numerical Methods Handbook and say this is the way you solve this kind of an equation. Here is the code set that does it.

VICE CHAIR WALLIS: Not in TRACE, because if you listen to John Mahaffy, he takes his momentum equation and he has all kinds of ways of upwind differencing and doing this and taking the divergence term and writing it in some way, which makes it look better and all that.

And when it gets to a final finite difference form, which is going to be used in the code, it's got to be absolutely explicit, no way you can misinterpret it. There has got to be a way of checking that what you have written down there as the numerical method is actually encoded property. Otherwise, you have chaos.

MR. MURRAY: A code of 100,000 lines. I mean, what I think is more useful in the comment is --

VICE CHAIR WALLIS: Suppose someone in the code has a j-1/2 instead of a j+1/2, how are you going to find it unless you have got some authoritative equation you can go into and you can identify that line of code and make the comparison?

MR. STAUDENMEIER: Usually, those type of bugs turn up in bad calculations and you trace it down to something like that.

VICE CHAIR WALLIS: Could be.

MR. STAUDENMEIER: But I mean, I guarantee any code the size of TRACE or RELAP5 has errors in it, and we know that because we keep getting error reports and fixing them. And you find new ones when you push it into uses that you haven't used it for before and, hopefully, you have wrung out all the major bugs that cause big changes in your calculation results, but there still may be some in there that are causing subtle changes or don't show themselves until you get into some special case or something like that, but that's, essentially, the process.

I mean, you try to put good practice in in developing it and have documentation and we are doing that, but there still are going to be bugs that are going to be found when people are using the code.

VICE CHAIR WALLIS: My experience with writing many codes to solve many problems is that writing down the equations and writing down the first version of the code is the most trivial part of the whole problem. And then there are all kinds of ways that something goes wrong, and finding out what that is takes far longer than constructing the solution method, writing down the equations. It's not a trivial thing at all.

MR. STAUDENMEIER: And I'm not trying to say that it is. And actually, part of our peer review process, I know we have talked about starting up a contract within the branch, we haven't done it yet, is for someone to go in and verify the actual mathematical terms that can be verified like the correlations, that they are consistent with the Theory Manual and coefficients that go into matrix solutions that you have constructed then consistent with the --

VICE CHAIR WALLIS: But when you have, say, nested loops and things, you think the thing is going through these loops in some kind of a way that you have all worked out in your head and all that, and you're damn sure that's what it's doing, but it isn't. There is something else happening there where a half is getting stuck in there or it's not going through the whole loop or, you know, there is some strange glitch. It happens all the time.

MR. STAUDENMEIER: And some of the modern debuggers can step through loops and you can see how they are stepping through loops and verify that when you're doing your code development with the debugger. I mean, you take steps and it goes through each line of the code and you can see what's being calculated at each step, so that has helped the development process, the modern debuggers.

And actually, Fortran 9095, you can use more descriptive variables and the array language logic in there makes it easier to write cleaner looking code that looks more like what the equations are down on paper than older dialects of Fortran.

MR. MURRAY: Generally, what I think is important also in commenting isn't always just tying something back to, you know, here's the equation. I think statements that lets you get inside the developer's head, what they were meaning to do when they put a piece of coding there, not necessarily what they -- sometimes it's so easy to see comments, you know, start loop and, you know, that doesn't help you at all.

But if there is a small two or three sentence comment that says here's why I'm playing this little numerical game, because later on if I do see an anomaly against the manual, I know which one is correct and I can fix it right away. I am not left wondering okay, now do I fix this or if I fix this, am I going to break 20 other test problems? And it's more important to be getting inside the developer's head in terms of commenting code and documenting a code.

And that's where we come to with this slide, is where I see the gaps of that process are and how we implement them. Enforcement of the SQA standards are not always as rigorous as it could be. A lot of that, when I say it's not as rigorous, it's that we're not always enforcing certain of those documents, the SRS or the SDID, are necessarily getting written. That has happened a few times internal to the NRC. We're pretty good about forcing our contractors to write that documentation.

There have been some projects in-house though, ESBWR and AP-1000. Well, it hasn't happened yet for 50.46. I think I can see it's going to happen where we're a little looser on requiring an SRS to be written or a SDID.

MR. SIEBER: So that would be your fault?

MR. MURRAY: To some degree for not forcing it, but that's where I take me to the second slide, is that there is a general desire not to be too heavy-handed in the application of the process.

One of the things I worry about is if I get too particular and say no, you're going to have this document before I ever see it and it's going to be of a quality that I'm willing to accept, then I'm either -- going to isolate that developer. In some cases, with the high priority projects in-house, there's management pressure to make sure that stuff gets done and that, you know, is not something that I can always stand up to, I guess.

MR. SIEBER: Okay.

MR. MURRAY: I mean, what I want to say is that there is sort of a people process in trying to manage people. There is a very social aspect to this development.

MR. STAUDENMEIER: Yes. One example I will give for what he's talking about, like the interim reflood model went into the code. Normally, we wouldn't have put that in the code without full Theory Manual documentation to go along with it, but Joe Kelly got moved on to -- he had to do the ESBWR film condensation model and he has to start that up before he finishes the interim reflood model documentation, and now he is moving on to 50.46. Eventually, the documentation will be completed, but it's not contemporaneous with the models going into the code as we would like it to be.

VICE CHAIR WALLIS: That's the same problem we have with these vendor codes. The documentation is the orphan child. It's done at the very end instead of being done at the beginning while you're developing things and being used as a really hard reference to what exactly you're doing, and then it gets garbled and you get typos in it and all kinds of stuff and incomplete stuff, and the guy is off on something else, never comes back and really fixes it up, but it's absolutely vital to get it right.

MR. MURRAY: Secondly, also follows from the first bullet, the SQA documentation is, I find, of generally poor quality and it doesn't really allow me to know what the developer was thinking all the time. I look to a lot of the software in the SQA documentation so I can say okay, was the developer considering this or was he not? And if he wasn't, then there's a way to fix that. The documents don't always contain sufficient information to filter into the code manuals.

CHAIRMAN RANSOM: You have three sources, I guess, ISL, Penn State and developers here in the NRC.

MR. MURRAY: Yes.

CHAIRMAN RANSOM: You're talking about all of them?

MR. MURRAY: Yes.

CHAIRMAN RANSOM: In general?

MR. MURRAY: LANL-T was another one, another developer that we have had, Purdue.

MR. SIEBER: Purdue.

MR. MURRAY: Is another developer.

CHAIRMAN RANSOM: Purdue, right.

MR. MURRAY: They also do some limited TRACE updating as well. But I mean, what you would like to see is if, to give a good example of the sufficient information not being filtered into code manuals, we have a user guide that is supposed to explain what each of the components are and how they work, what the hidden issues are in using them or what the guidelines should be, you know, where they are applicable and where you have got to watch out for using them.

And that user guideline, there should be some documentation or some information somewhere in either the SRS or that SDID that is sort of outlining that, so that I can just take it and cut and paste and, you know, slap it into the user guide document with some minor editing.

And I find that some of that stuff is just not there at all and that's a problem, as I see it, because now you have to play catchup later on. Now, we're here and saying, you know, we're going to have something two years from now and you're questioning well, can you really get all that documentation done like you're saying you're going to and, you know, that seems like a lot and it is and it's partly because of this.

And you know, a lot of that I blame on two things. I blame on ambitious schedules that are put into place and I think we're not always doing the best job of making sure that our developers understand what expectations are and then holding to that, making sure that there's management backing to hold to that, so that when a developer comes in and says here's my documentation, Chris, and I say no, it doesn't quite have it, it doesn't quite meet what we need it to meet. And they say oh, you know, we don't have anymore time to do it, because, you know, we have got to get this work done. And so I do shoulder some of the responsibility for that.

CHAIRMAN RANSOM: I'm sure you run into the problem, too, that this is probably the least desirable part of the job on the part of the developers.

MR. MURRAY: Yes.

CHAIRMAN RANSOM: They always seem to neglect the documentation.

MR. SIEBER: You know, the typical excuse is don't worry, I'll get to it.

MR. MURRAY: I mean, now, I try to take a practical or a pragmatic approach to some of this and this leads me to the next slide, how do we get it right?

One of the problems with some of that documentation is if it falls out of like the SRS and the SDID, there are standard accepted ways of writing these in the software engineering community, but engineers are not software engineers. You know, nuclear engineers are not software engineers and don't always know this.

What I would like to see are more standard templates, and not just templates, because the templates are out there, but two things. Templates in our Framemaker, we use Framemaker for all our documentation, so we need Framemaker templates, so that people don't have to pull something from word over. And secondly, it ought to have a lot of the text that ought to be there already there.

I mean, there is a lot of boilerplate text that can appear in these documents related to TRACE. There's requirements that will always be there. You can't break the restart system. If you add a variable, you have to be able to back up the timestep successfully and have it maintain that or restore that n-1 variable.

You know, there's different requirements that will always be in the code and those need to just be in these templates, so that the developer doesn't have to rewrite that every single time, and that will reduce the burden on the developer and, I think, allow us to get better documentation each time.

Another thing that I think will help the process is NRC staff should prepare the SRS in-house, and that that should become the basis for the statement of work. Right now, we rely on the developer to tell us what our own requirements are and that, to me, seems to be a choke point. If we're not reviewing that properly, we're starting from a point we can't maintain anyway.

And that SRS really needs to be written by us first, but there is a penalty in that and our contracting process gets stretched out, you know, because we have got to write the document. But if we also have the template, that should minimize that.

CHAIRMAN RANSOM: I think we need to move along.

MR. MURRAY: Okay. I'm sorry.

CHAIRMAN RANSOM: I think we're getting the picture that this is a problem area and it always has been a problem area for the NRC. I don't think they have ever recognized how much it really takes to document and keep the documentation up to date and, as a result of that, we have generally had poor documentation.

MR. MURRAY: Okay. In terms of verification, I will just take you quickly over how we verify the code. I have already gone over a little bit of what our definition is.

For TRACE we do verification in a variety of ways. The developer performs their own targeted verification for their features or code updates that they are working on. It may involve any of these following, line-by-line reviews, that is certainly a form of verification, driver programs, small test problems designed to exercise only the feature being modified or added. This is my minimum standard right here. I look for these.

They should demonstrate 100 percent line coverage of the Fortran being changed, and I don't generally see that. I run a regression suite of 1,300 plus tests. That suite is always growing and that suite is designed for me to ensure that other features that the developer didn't intend to change really don't change.

CHAIRMAN RANSOM: Is that a point-by-point comparison, some metric developed as far as how well this new version agrees with a previous version? In other words, these are all transient problems, right?

MR. MURRAY: Yes. I'm comparing.

CHAIRMAN RANSOM: Are you comparing these timestep or point-by-point?

MR. MURRAY: Yes. There's four. Let me think. There's one, two, three different files that we compare, use a Unix Diff command, just compares differences of characters. And one of those files is -- there is full numerical precision output of some key variables every timestep. And then there is the message file and the output file that we also compare. The output file, because the numerical precision isn't always -- you know, some numbers may only be down to the third decimal place. You can run for 200.

CHAIRMAN RANSOM: What I'm looking for is what information do you finally get out of all this because, clearly, you can't go through 1,300 plots.

MR. MURRAY: Joe showed in his slide, I look at three criteria, three criteria, and I put it on our web page. I have scripts that I run that consolidates all this information on like a web page that I can just move through.

The first thing I look at is were there any test failures. If there are failures, problems that used to run that don't run, that's something I look at. Secondly, I look at all the differences. If there are any differences in test problems that shouldn't show differences, then that's a flag.

CHAIRMAN RANSOM: Now, you're saying

differences. You mean out too eight significant figures or something like that?

MR. MURRAY: Yes, yes, that's right, because we have that one file that looks at things out to that level.

And then there's a set of sort of performance metrics, runtime, the end time of the problem, the number of outer iterations, the number of timestep backups, the mean timestep size, and I look at that across the whole suite and I can get a good measure of code performance just based on those, is the code on the aggregate running better or worse.

CHAIRMAN RANSOM: Okay.

MR. MURRAY: The development projects get reviewed at periodic code coordination meetings once or twice a year. That's where we try to do line by -- I try to encourage developers when they are giving presentations of what they have worked on to sort of get at the code level, so that we can see what the code looks like. You know, the developers are either going to sit in front of us, their other peers, you know, developers, like I'm sitting before you and sort of just present something.

What I try to encourage, and this is something that has changed since I have come here, is show us the data structures, you know, show us your overall design, what your data structures look like, because if we see those details we can see a lot. It doesn't always involve line-by-line reviews though. That's what I was told.

Before being submitted to the code custodian, me, the update and its change summary file gets reviewed by another developer who is independent of the development project of interest. We have gone through that. The changes are reviewed by the code custodian to ensure adherence to standard programming guidelines. We have a standard set of programming guidelines that's up on our website that all developers are expected to follow. I look for that.

The update is integrated in the official source base and then the regression suite is rerun. That sounds a little backwards, but really, we run a regression suite and then once it has passed the mustard we put it in the official source base. We check it into our CVS repository. Any new test problems that the developer created get put into the regression test suite.

In terms of the third concern, which was about assessment and whether it is being done with an approved process with a frozen code version, right now the TRACE assessment is currently underway. Our intent is to deliver a Developmental Assessment Manual that is based on a single frozen code version. So I don't know what was done in the past, but that's not going to be the case for TRACE.

CHAIRMAN RANSOM: That will contain comparisons to cases where you have physical data on accuracy?

MR. MURRAY: That's right. The Assessment Manual is only going to be comparisons to data.

CHAIRMAN RANSOM: Yes.

MR. MURRAY: And we're going to have the facilities, we're going to have automated facilities that do that for us, so that from version to version I'm going to be able to rerun the entire suite of assessment cases on a cluster or a multi-processor machine and it will regenerate all the plots and all those plots are going to get updated in the manual automatically.

Now, the analysis that goes along with those plots will have to be looked at by an engineer, but at any time that we run the assessment set, most all of the manual labor is going to be done for us, and that's what I think is going to allow us to really reach this, being able to say we do it with a frozen code version. It's very hard to do that otherwise.

Requirements for an assessment process for an NRC system code are fundamentally different from those of vendors. We have a need for a more broad-based assessment than vendor codes generally need. Vendors usually only have one reactor type they have got to worry about. We have got to worry about them all, but that doesn't mean that we don't do targeted assessment where we need it right now. And so like, at this point, application to TRACE or of TRACE to current NRC projects is being supported by targeted assessments for specific applications like 50.46 and ESBWR.

The one criticism, I think, I can lay on the process that I have not seen to date is no rigorously documented process for input model development. I haven't seen where our models are being maintained in a central repository the way they probably ought to be. There's not some master guide that says this is how, you know, thou shalt model a steam generator or, you know, a Westinghouse pump or something.

And we have tried to get to that point. There's weekly review meetings that are conducted, that are designed to ensure common nodalization approaches. That way if we model a core in an integral effects facility with 2 foot nodes up the core, that the plant models will also have that same nodalization. So these weekly review meetings are an effort to move towards some institutional knowledge, I guess. The lessons learned will filter into the User's Manual and SNAP as user guidelines.

It seems like there's some lines missing. Here we go. Well, I think the slide will show it, the lines. What this graph or this picture is trying to show is what I, luckily, it's not that long, think our assessment will follow, the time line.

Right now, I think we're still in this Phase I assessment, which is really just gathering input decks and getting all the input models together. Once they are all together, we will have this automated. We'll have a framework that we can automate.

CHAIRMAN RANSOM: So you're going to have more than 1,300 problems when you get all of these in here, too, or what?

MR. MURRAY: Yes. I mean --

CHAIRMAN RANSOM: You're asking --

MR. MURRAY: There's basically two different -- yes, I mean, what will happen is the regression suite is a little different from an assessment suite with the caveat that every assessment problem will be in my regression suite, but I may not necessarily run it out in time as far as I would have for the full assessment, because the regression suite is just meant to show that we're maintaining functionality and reliability of input and that features aren't degrading. But the assessment is really what shows adequacy, you know, against data.

And so every assessment problem will be in my regression suite, as well, but it might be we'll treat the timestep, you know, or the end time, so that it runs faster, because I'm envisioning that once we fully automate our assessment suite, it may take a couple weeks to run the whole thing, maybe longer than that.

But right now what I sort of envision is that right now we're still creating the different facility models and we'll assemble, basically, our automated framework together. And at that point, we'll sort of have a draft, what would amount to a draft manual. But I think that, at this point, you know, there will still be deficiencies in the code that will need to be worked out. And so down here we'll start addressing those deficiencies, but each time we do we're going to be rerunning that whole assessment suite in an automated way and see how we get better.

CHAIRMAN RANSOM: Is that a two year time line?

MR. MURRAY: Huh?

CHAIRMAN RANSOM: Is that a two year time line?

MR. MURRAY: Yes, this is our two years out to the TRACE 5.0 release. And at that point, we would enter -- now, I have a little bit of time here for converting to a published NUREG for our documentation, because that takes time for the NRC process and needs to be planned for. And then we would enter, essentially, a different lifecycle model, which is the only difference between what we're doing now and what this is, this green line up here.

I'm envisioning that what we would do is we would -- at this point, you have your Version 5.0 release. There may be people out there that are running that release and may come across major critical fixes that are killing a code in some way that you are going to need to address, you know, so people can get a job done and this would be sort of that branch, which is the release branch.

But over the course of a year, you would be adding features, new features to the code, but you have got to freeze at some point. You can't add more features, because if you keep doing that you'll just always have a moving target, which is sort of what has been happening up to this point.

And so at this point, you have a feature freeze and then in here is your assessment phase on that frozen version. Well, it's not frozen. It's just chilly at this point. And you will identify issues and you will have to fix those, but because of our automated set we will be able to see the effects of those across the whole set, you know, much quicker than we used to and in that way, we'll iterate to the next release.

But now, the developers are still working, so there's going to be this developer branch that you break off here that the developers may still be working with and, at some point, you're going to have to merge those features back into now your new 6.0 release. So it's sort of a leapfrog effect and that is sort of how I envision the development will happen in the future. And that's it.

CHAIRMAN RANSOM: Okay. Well, thank you, Chris.

MR. MURRAY: Sorry that took longer than it should have.

CHAIRMAN RANSOM: We're short of time, but let's go ahead with the SNAP presentation. We can dispense with my comments. You can probably move fairly quickly. I think that we have all been made aware of kind of the structure of the SNAP capability. Is yours on?

MR. GINGRICH: I think so.

VICE CHAIR WALLIS: This is a whole new presentation, too.

MR. SIEBER: It may be quicker than you think.

UNIDENTIFIED SPEAKER: Oh, that's the first clue.

MR. GINGRICH: My name is Chester Gingrich. I am project manager for SNAP and, as you say, we'll go through this pretty quickly. I just want to talk real quick about the new changes. The system architecture for SNAP has undergone a major change, but the structure is still -- what's going on here?

CHAIRMAN RANSOM: When you say major change, I mean, have you gone to a different software philosophy or coding or support?

MR. GINGRICH: Somewhat different. What we have done is we have come up with this CAFEAN architecture. It's an application programmer's interface that is kind of the layer between the visual and the plug-ins that we have been using in the past.

CHAIRMAN RANSOM: What motivated that? Did the old software just didn't work or wasn't able to do the job?

MR. GINGRICH: The goal of SNAP, as originally intended, was to provide a user interface that was somewhat devoid of having to know about the actual --

CHAIRMAN RANSOM: Right.

MR. GINGRICH: -- format of the cards and stuff like that. And the second thing was to provide a consistent interface for all of the research, well, even the U.S. NRC codes. So we had to make sure that we had something that was easily extendable, so that it could be easily implemented for other codes and something that was easily maintainable. Okay?

So fortunately, I have a contractor that is very good, that knows a lot about code design, has been very good at keeping up with the industry standards. So we have come up with this application programming interface called CAFEAN. It's actually a

JavaBean. I don't know if you have heard the buzzwords, but JavaBean industry standard interfaces and the CAFEAN plug-in interface is actually published or, actually, right now it's in a draft form, but it is semi-frozen at this point. It's published and can be looked at by anyone who has a web browser.

It's very rigorous, but a very simple application interface. I think it went from like 1,200 pages down to like I think the last version was only like 120 pages.

CHAIRMAN RANSOM: 120 pages of coding you mean?

MR. GINGRICH: It's the document, how it's interfaced with the CAFEAN package, and what it provides you is how to use these. I wonder if I have a pointer on here. I guess I can get up and point. How to use the different -- how RELAP or TRACE or any of these codes can actually talk to this application interface.

Oh, you actually have a weapon. Thank you. And basically, right now we have our own layer of standards that we apply to these JavaBeans, so a component bean would be like for a pipe, in CONTAIN it would be a cell. And what we have done is we used to have an application for post-processing, VEDA, very similar to the NPA. We have combined that as a plug-in under this GUI client application, so even the VEDA animation stuff is all done using this architecture. In the future, we're going to be moving AcGrace into this, too, I believe, but we'll get to that.

The CAFEAN stuff, the plug-in also covers how the calculations are done, how the executables actually talk to this runtime and display of data as they are running and as to the database server. Even though it's not shown here, there is an interface to CAFEAN.

MR. SIEBER: Does that exist?

MR. GINGRICH: Which?

MR. SIEBER: The database server.

MR. GINGRICH: Yes. Actually, that is being implemented. We used to have a database interface to Oracle, Sybase, anything that supported SQL. But IBM recently converted or released their -- I forget the name of it. They had a free -- well, it wasn't free. They had a version of a database server that they recently released into the public called DERBY and we're actually implementing that. So you won't even have to have your own database system. You can actually just use SNAP right out of the box and it will keep track of things for you.

Let's see. Yes, next slide. Design improvements. As I talked about, this CAFEAN stands for this Common Application Framework for Engineering Analysis. We have converted the TRACE plug-in into this JavaBean format, which is basically --

CHAIRMAN RANSOM: Now, the TRACE plug-in, does that mean a converter that will read a RELAP5 deck?

MR. GINGRICH: No.

CHAIRMAN RANSOM: And spit out a TRAC deck?

MR. GINGRICH: No, this is just the TRACE plug-in interface, the GUI for building decks and such.

CHAIRMAN RANSOM: You didn't have to rewrite all of that, did you?

MR. GINGRICH: Well, it wasn't rewritten. It was, basically, converted, massaged into this new JavaBean format. As I said, the post-processor has been moved into the interface as a plug-in itself. The database has been converted or is being converted. There is a jEdit text editor that is a very powerful text editor. It's freeware that we directly export to now when you complete creating your model on the graphical side.

Right now SNAP still has some misfeatures that may need to have the user go in and edit by hand, so we can edit. We can export directly to the jEdit editor, so if the user wants to go in and make a change at SNAP this doesn't support yet, he can still do that.

CHAIRMAN RANSOM: Well, one of the criticisms that I have heard of this is that, generally, SNAP and, say, converting a RELAP5 deck or generating, I guess, a TRACE deck, spit out a binary file.

MR. GINGRICH: Right.

CHAIRMAN RANSOM: Which was sort of meaningless to the user, and so it was not possible to go back and edit that in the normal input stream.

MR. GINGRICH: Well, that's one reason we had this. Yes, that's one of the reasons we had this.

CHAIRMAN RANSOM: Are you going to be able to do that?

MR. GINGRICH: Yes. And we actually address that in another way, too. We have an editor that can go right in to look at the raw binary format in a data structure.

CHAIRMAN RANSOM: And interpret it, I guess.

MR. GINGRICH: I'm sorry?

CHAIRMAN RANSOM: Would it interpret it?

MR. GINGRICH: Yes.

CHAIRMAN RANSOM: So that the user would know what was intended?

MR. GINGRICH: Exactly. So that's actually provided with the distribution of staff when you download it. We also support something called reference models. One of the criticisms we have had in the past, when we moved to this new architecture, we got rid of what was called the underlying reality layer, which kept a physical representation in SNAP's memory at all times.

So if you did renodalization of a pipe, for instance, you always kept somewhere in that data structure the original pipe dimensions, so that no matter how many times you renodalized it, you would always be able to go back to the original pipe. Well, we blew that away, because it was a really ugly data structure.

So to recapture the ability to go back to our original pipe, what we did is we make these reference models, which allows you to go right back to the original model for any component in that model, so we have recaptured something we had lost accidentally.

The extensibility, we're actually moving over to the standard industry or rather this industry standard JavaBeans has brought us a lot. We can use any JavaBean that's out there on the web, any JavaBean anywhere anyone has developed for a graphical interface, a plotting package, anything of that nature, we can grab it and jam it right into a directory. That's all you do. You just place these JavaBeans into a directory and when you load SNAP, it reads it.

CHAIRMAN RANSOM: It's all run in Windows on PC?

MR. GINGRICH: It will run under Windows and it runs under Linux. Anything that has a Java virtual machine it will run under. I believe Joe Kelly even uses it on a Mac.

Like I was saying, some of the stuff we have been able to get. Okay. This JavaBean gives us -- the custom beans can be independently developed and shared and we can use a shared repository for contributed beans. Anything that is written is written as an independent and right now, we have -- it's not really an agreement, but kind of a gentleman's agreement, between naval reactors, KAPL and Bettis, and they love this approach, because they have a lot of stuff that we can't see, because they are super secret stuff.

But they can still use this architecture, because any plug-ins they make, they keep, but they get to use all of our graphics, all of our data structures without hurting themselves. Likewise, we get some stuff out of them, too, but I will get into that later.

This Python scripting here, Python is a scripting language, very popular. I'm not sure if you have heard of it. It's kind of like PERL only it's an advanced PERL. We got this, because there is actually a preexisting Jython, which is a Java implementation in PERL -- in Java. Someone help me here.

MR. STAUDENMEIER: Python implementation in Java.

MR. GINGRICH: Thank you. And that was implemented very easily in SNAP because of its subject. You know, it's a very strict, you know, we have a very clean interface. And CORBA, it's just that's old news. We haven't changed any of that.

Plug-ins contain all analysis code specific classes. That means that TRACE knows all about TRACE components. RELAP knows about all of RELAP components. And right now, all of our plug-ins, except RELAP, are in this new format. RELAP, if you will remember, was the first one we had implemented and we changed our architecture, and we just simply haven't gotten back to converting it yet. There's a strong interest in doing so. It wasn't initially in our plan, because we hadn't planned to change the architecture. This will either be done in 2006 or the Navy may end up doing this. They have expressed interest in doing this themselves and paying for it.

There are feature plug-ins. A feature plug-in still fits in the same architecture using the same tools. We have this RELAP to TRACE Vessel Conversion Wizard because, obviously, a TRACE 3-D vessel, you know, you will need help if you're going to be making any migrations in that direction from RELAP. As I said, the API is published. You can actually go to this website and you'll see it. You can read it in great detail.

I'm going to try to go through this pretty quick. I have mentioned a lot of this already. We support different windowing types. We have this multi-window mode where you have each frame, each component is in a different window, but the more popular for window users is to have all of it under one window with each little box capturing the functionality.

CHAIRMAN RANSOM: Now, is that one of the NPA type graphical displays?

MR. GINGRICH: Yes, this is what you're seeing here.

CHAIRMAN RANSOM: And that's just carried over into this then?

MR. GINGRICH: Yes. You can load this. Well, you probably --

CHAIRMAN RANSOM: There is going to be an automated way of producing this mass?

MR. GINGRICH: There is indeed. That's one of the things that we have added, and one of the reasons we want to put the animation, this NPA functionality inside of this GUI, is because now when you bring up -- let me see if I have it. Here is how the component navigator works, just labeling different parts here. This is not really something you need to -- what you could do, now, when you normally build a model or edit a deck, you will be looking at this. To make an animation, to make --

CHAIRMAN RANSOM: Is that a drag and drop type function?

MR. GINGRICH: Yes. To make these things, there is a palette up here. You don't see it all right here. You see some of the tools. You will see pipes and things.

CHAIRMAN RANSOM: Valves and junctions.

MR. GINGRICH: Right. Any component and code supports will be up here. You can just drag and put it on the palette and connect them.

CHAIRMAN RANSOM: You then open up a box to put the data in for that component?

MR. GINGRICH: Yes. What will happen is, let me go back, that will open up a properties view. If you double click on any component you drop, you will see a properties view.

CHAIRMAN RANSOM: Okay.

MR. GINGRICH: Okay. Everything has a property, because everything is a bean. Anything you can see is a bean and all beans have properties.

MR. SIEBER: Baked bean.

MR. GINGRICH: And one of the neat things about beans, if you select several of them, say you select several pipes and you say I want to change the property of the length for all of these pipes all at once or I want to change the coefficient of friction or whatever, whatever property it supports, you can bring up this property thing and only the common properties will be editable, and you really get that for free, because the JavaBeans support that kind of editing. We didn't have to do any extra programming for that.

So anyway, that's how you would change the properties of a pipe or any of these components. And everything is a bean even these connections here are beans. You can, you know, change which components they connect to, you know, different features.

Now, you see, this is an animation model. This is the equivalent of an NPA deck. What we actually did here is we took our -- I keep getting the direction mixed up. We just grabbed this image. There is a "select all" under edit and you drag that over to an animation model canvas and drop it, and it will automatically build this view for you. It will show you the pipe. It will even attach this range dialogue here. You have to tell it what the range is, but it attaches that dialogue for you. Let's see.

CHAIRMAN RANSOM: A previous model had a U-tube in I thought, didn't it, or is that embedded within that blue bar?

MR. GINGRICH: I'm sorry?

CHAIRMAN RANSOM: Well, I thought your previous nodalization diagram had like a U-tube --

MR. GINGRICH: Oh, not that particular

one. No, I'm sorry.

CHAIRMAN RANSOM: Oh, okay.

MR. GINGRICH: I just meant, generally, when you take a nodalization and move it over. Yes, the nodalization this came from looks just like this.

CHAIRMAN RANSOM: Oh, okay.

MR. GINGRICH: So it makes creating animation models extremely easy and all these are customizable. They are just graphics. Let's see. Oh, yes. One of the improvements we have done in these animation models is we can have multiple data sources. One of the data sources is a Python. You can just write your own Python script if you just want to make up, you know, some kind of a sequence to compare your data with or to maybe show you a timing sequence or something.

You can actually hook this up to an experimental data channel and, at the same time, hook this up to a code calculation, so you can compare maybe all three at the same time and you could have your code, your experiment and maybe a test you wrote in Python running side-by-side.

CHAIRMAN RANSOM: Does this NPA capability allow you to interactively run?

MR. GINGRICH: Yes, yes.

CHAIRMAN RANSOM: So you can open valves, close valves as the thing is running?

MR. GINGRICH: Yes, absolutely. This has greatly enhanced NPA. Here you're seeing, this here is a source editor for one of the scripts. You can go in here and edit any variable. You can assign variables, user variables, to components of your input deck. Any value that is editable in the deck you can assign a user variable to and bring it forth.

These are editable dialogues now as you can actually see. You can make an engineering template, if you will, and hand it over to an analyst and say okay, if you have a bunch of these to run, there were a bunch of calculations like this you want to run, just go into this template, change the parameters you want to change, dump the deck or just submit the calculation and that's all he has to do. It greatly simplifies that kind of work. Let's see, did I miss anything?

Parametric constants. Yes, you can even define. Some of the constants you can define to be parametric variables. Code SNAP has an ability to say okay, what range do you want to vary these parameters, these special variables, over and it will submit for you a whole sequence of runs that only varies those variables.

MR. STAUDENMEIER: An example of that might be a break spectrum you could run. You could define variables that define the break size and tell it to run 10 cases in the range of this range and it will automatically fire off 10 runs with various break sizes for you.

MR. GINGRICH: Yes, there is a lot of SNAP I'm not showing you. There is an execution monitor, which actually runs like a daemon. You can put it on your cluster. It talks over the network or it has that ability to talk over the network.

We can set up this execution daemon on our cluster, have the analyst sitting at his desktop PC who is connected to that network and submits 100 runs to the cluster just by selecting parametric run and submitting this job. I haven't done 100 runs, but I have tested the capability. It does work. It needs a little work. Right now, if I start the job as myself and Joe comes along and submits a job to it, it's going to be running it as me on the cluster.

CHAIRMAN RANSOM: Is there any thought or plan to build this as an intelligent interface that would help the model developer?

MR. GINGRICH: Someone's feeding you

information, I think.

CHAIRMAN RANSOM: Pardon?

MR. GINGRICH: This is just one of the things that has come up. You're really hitting the nail on the head. One of the criticisms we have had is that this is still a little bit too hard for an average user to use, but we are looking. In 2006 we are going to be submitting, what do you call that, a request for --

MR. STAUDENMEIER: Request for a proposal.

MR. GINGRICH: Request for a proposal for doing, for creating a more intelligent possibly rule driven -- we haven't really specked out exactly what we would want out of it yet. We don't want to take the analysts out of the loop, because we want to make sure he's doing something intelligent with this analysis.

CHAIRMAN RANSOM: Well, the hope was that, originally, with intelligent interfaces is you bring some standards into it.

MR. GINGRICH: Exactly.

CHAIRMAN RANSOM: And so more people who do things in a similar way.

MR. GINGRICH: Right.

CHAIRMAN RANSOM: And of course, that has a common mode failure mechanism built in.

MR. GINGRICH: There is always that risk, yes.

MR. STAUDENMEIER: Could you go back to that channel template? One thing we envision with these intelligent interfaces are these engineering templates where the analyst puts in physical dimensions of things and then automatically, in the background, that will be noted up to code nodalization guidelines and things like that for that type of component.

MR. GINGRICH: Exactly.

MR. STAUDENMEIER: And for the calculation that you have done, so that the analyst doesn't have to worry about details like that. So you're building an --

CHAIRMAN RANSOM: Presumably, it improves the reliability.

MR. GINGRICH: Right.

CHAIRMAN RANSOM: Or reduces the chance of error, I guess.

MR. GINGRICH: Yes. We can actually build in all of the user recommendations right here, so that gets into it.

MR. CARUSO: Have you thought of including the capability of building piping networks specifying the components and engineering terms like 8 inch ID schedule 80 pipe?

MR. GINGRICH: That is actually what the Navy wants to do. I have said the Navy wants to be able to take a CAD drawing.

MR. CARUSO: Right.

MR. GINGRICH: An actual --

MR. CARUSO: I wasn't so bold as to suggest that, but that's what I was thinking about.

MR. GINGRICH: They will.

MR. STAUDENMEIER: But something like this could build that in. You could have a selection for the specific pipe type that you want, it would be a drop-down menu.

MR. CARUSO: Right.

MR. STAUDENMEIER: And you would pick it and maybe lost coefficients, you could say you have an elbow that bends this much.

MR. CARUSO: Right.

MR. STAUDENMEIER: And things like that.

MR. CARUSO: Standard elbow, a 5D bend.

MR. STAUDENMEIER: Yes, get standard lost coefficients for that geometry or things like that.

MR. GINGRICH: The initial proposal for

the generic GUI, which started way back in '95 and Joe was on the team, I'm not sure if you sat on those teams. You did? Okay. So that's --

MR. CARUSO: That was one of the suggestions I made back then.

MR. GINGRICH: Oh, okay. Yes, that's still on the list. One of the things we want to do is to make a library of components and you could do that now, except that there is no special graphical view of that. It would just be loading a model in. You can cut and paste between models very easily in SNAP, so that is how you would handle that for now, but you could easily.

MR. MURRAY: Yes, I think SNAP allows us to do a lot of things now, a lot of the things that you want. It's just we still have to -- what we have heard is some of our users don't necessarily want to be the ones to do all that, and so we need to do and build a lot of that stuff for them. But a really advanced user can do all that for themselves now if they really want to.

MR. GINGRICH: Yes. The comment was at a recent stakeholders meeting that they didn't want to actually have to learn the code and learn how SNAP worked, and that is a very good criticism. It's an extra layer of work to learn how to use SNAP. Eventually, I would like to just have SNAP replace all of the input processing across the board for all of our codes. Kind of like Joe was talking about having a common materials library and a common steam table, this could be the common user interface.

CHAIRMAN RANSOM: We need to wind this up.

MR. GINGRICH: I have one more slide. Future activities is develop PARCS plug-in. We're currently working on MELCOR. It's almost done. We'll have it a finished version in June.

CHAIRMAN RANSOM: Is MELCOR going to be incorporated into the codes, TRACE?

MR. GINGRICH: No.

MR. KRESS: It's stand alone.

MR. GINGRICH: It's a stand along plug-in, yes. Remember, all of these plug-ins can be stand alone. We don't have to distribute anything with it.

MR. KRESS: Yes, that would affect the runtime.

MR. GINGRICH: Yes. That's another reason to have plug-in type capabilities. You don't want to have to load all of the stuff when you're doing any of this. We want to replace the existing plotting package. The existing plotting package is C based.

It required X-Windows, the biggest killer right here. It requires X-Windows to run on, so we had to put an X-Window emulation on your Windows platform to use it. It's difficult and expensive to maintain. If this was in Java, this would be a no-brainer, but, you know, in C, we have to kind of get out of our nice, clean interface and go into the old interfaces.

This is what we were just talking about. We want to improve these user interfaces, illicit, something a little bit more intuitive to the users, develop a simple graphical user interface. We're going to try to get something together in 2006.

MR. SIEBER: What are the illicit user requirements?

MR. KRESS: Will the MELCOR plug-in have MACCS associated with it?

MR. GINGRICH: No, but that's something we're going to add probably in the future. We need to look not only at MACCS, but there's also others like RADTRAD and some of these other things --

MR. KRESS: Yes.

MR. GINGRICH: -- that the Agency uses that would be useful to be able to move data from one of these codes to another.

MR. KRESS: Then your database could

include all the site specific data.

MR. GINGRICH: Yes, it could. The database is something that we really haven't seen much here, but it's extremely powerful. Anything you can see here is retrievable and has a specific ID associated with it. It's extremely hidden, but it's very powerful.

CHAIRMAN RANSOM: Okay. Well, I would like to thank RES for putting together the presentation today. It has been very informative. I don't know. Can we dispense with the record at this point? We can go off the record. I think we're essentially wound up.

(Whereupon, the meeting was concluded at 6:45 p.m.)