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UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

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MEETING
ADVISORY COMMITTEE ON REACTOR SAFEGUARDS
(ACRS)
SUBCOMMITTEE ON THERMAL-HYDRAULIC PHENOMENA

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THURSDAY,
DECEMBER 12, 2002

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ROCKVILLE, MARYLAND

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

<u>COMMITTEE MEMBERS</u>:

GRAHAM B. WALLIS, Chairman
SANJOY BANERJEE, Consultant
F. PETER FORD, Member
THOMAS S. KRESS, Member
FREDERICK MOODY, Consultant
VICTOR H. RANSOM, Member

ACRS STAFF PRESENT:

PAUL BOEHNERT, Staff Engineer

ALSO PRESENT:

STEPHEN M. BAJOREK, NRC

JOSEPH M. KELLY, NRC

I-N-D-E-X

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

8:33 a.m.

CHAIRMAN WALLIS: The meeting will now come to order. This is a continuation of the ACRS Subcommittee meeting on thermal-hydraulic phenomena. For today's meeting, we will discuss the status of the Office of Nuclear Regulatory Research's TRAC-M Code Consolidation and Documentation Project. The entire meeting will be open to the public. Mr. Paul Boehnert is the cognizant ACRS staff engineer for this meeting.

I call upon Joe Kelly from the NRC's Office of Nuclear Regulatory Research to begin.

MEMBER KRESS: Somebody asked an interesting question that I didn't know the answer to: What does the M stand for? Is it modular? What is it?

MR. KELLY: It stands for modernized.

MEMBER KRESS: Modernized.

MR. KELLY: And it was an interim name, if you will, that we initially came up with to distinguish it from TRAC-P, and we've been looking around for a better name and we've yet to come up with one.

MEMBER KRESS: Okay.

MR. KELLY: So if we do, we'll be changing the code name.

MEMBER KRESS: Okay.

MR. KELLY: And so what I'm going to do is give you a summary of where we are in the code consolidation and also --

CHAIRMAN WALLIS: I thought there was TRAC-paleolithic and TRAC-medieval.

(Laughter.)

MEMBER KRESS: But we could call you Michael now?

MEMBER KRESS: Yes. The comment has been made that since the code was written in Fortran that the manual should be ancient Greek and Latin. So at any rate, I'll talk about the status of the consolidation but also give you an idea of what our long-term development plans are.

First thing I'd like to do is give you a release schedule, then I'm going to review what our development objectives were and the status of those objectives, talk about how we're going to maintain the capability to use legacy input models, describe the current and short-term activities and then, as I said, the long-term development plan.

Code release schedule. We're going to release an alpha version to the internal users at the end of this calendar year, so just in a few weeks. It will be able to run input decks from UF5, TRAC-B and TRAC-P with a little caveat on RELAP5. We haven't quite finished the mapping of the control system. That's kind of being done as we speak. So a very large complicated RELAP5 deck will not, at this time, be able to be run by TRAC.

The documentation. There will be user's guide, a theory manual and developmental assessment manual. The user guide we have a first draft, and this will definitely be missing the RELAP5 translation guide and that's how to take a RELAP5 deck and run it inside of TRAC. The theory manual, there is a first draft. It will not include the BWR models and sections on the new physical models. And the new physical models are basically just the reflood model.

CHAIRMAN WALLIS: This theory manual is going to set the standard for explaining how basic equations lead to the equations actually used?

MR. KELLY: Not the first draft.

CHAIRMAN WALLIS: Not the first draft?

MR. KELLY: But by the time we get to here, it will be done.

CHAIRMAN WALLIS: Oh. It's going to take you a year to figure out the theory?

MR. KELLY: Well, take a year to do that part of the work.

MR. ROSENTHAL: To set the standard of excellence.

MR. KELLY: Good response.

CHAIRMAN WALLIS: Well, okay. Can we see the draft anyway so we can help you?

MR. KELLY: Yes.

CHAIRMAN WALLIS: Okay.

MR. KELLY: The draft will not be terribly different from what you have now. We'll be doing a beta release, which will be our first external release, to the CAMP members in the spring, and that meeting is now at the end of April in Korea. It's the week after the ICAAN meeting in Japan. And for this case, the documentation, the user guide, will then be in final form, the theory manual will now have a complete first draft, so everything I've said is going to be missing here will now be there. It will not be completely rewritten front to back. That's just not going to happen. What is going to happen is as we go in and address a certain part of the code, like in this case replace the reflood model, that section will be completely rewritten. And we've also put a task to John Mahaffy to rewrite the part of the momentum equation. So that will be in there too.

CHAIRMAN WALLIS: All of your cowards.

MR. KELLY: Pardon me? And we'll have a first draft of the DA manual. It will only be partial at that point in time, because we're starting our assessment later than we had initially intended. The official release will be at the end of year '03, and for that it has to meet the success metrics and then the documentation will be in draft form. I'll talk about the success metric on another slide, but basically it's to able to be a complete assessment matrices from all of the predecessor codes with the same degree of fidelity. And you'll notice the little note in red, and it says that the potential with the documentation in some of the assessments will be delayed, and that's due to a reallocation of resources due to AP1000 and also the upcoming ESBWR. That's already happened, and you'll see some of that and the reason that this part of the manual -- the BWR models are not going to be in the manual as both the reallocation of people and also of funding.

MEMBER RANSOM: Joe, who is actually working on this?

MR. KELLY: Well, the in-house team currently has four staff members.

MEMBER RANSOM: That's in the NRC, right?

MR. KELLY: In the NRC.

MEMBER RANSOM: Yourself and who else?

MR. KELLY: Chris Murray, Wei Dong Wang and Joe Staudenmeier. And, of course, we all have multiple assignments, but that is the Code and Models Development Group.

MEMBER RANSOM: Who was the fourth one?

MR. KELLY: Joe Staudenmeier.

MEMBER RANSOM: Joe, but I guess it was the first one you mentioned.

MR. KELLY: Let's see, Chris Murray.

MEMBER RANSOM: Chris Murray.

MR. KELLY: He does the code configuration control and testing. Wei Dong is helping me with the reflood model. That's in addition to being things like the PUMA Project Manager and the RELAP5 Technical Monitor.

MEMBER RANSOM: Right.

MR. KELLY: Joe is kind of our BWR expert. And then myself. Then as far as contractors go, Penn State University, which is really John Mahaffy, and we just put a new contract in place with him which calls for a post-doc and a graduate student and part-time from an undergrad student. LANL has been a contractor. They're now going to be down to about one FTE that's coming in. The other contractor is ISL, and that's part of their -- they have a large contract for a code on maintenance and assessment. And that's the same contract that RELAP is under, but we've been doing some of the TRAC consolidation and the TRAC assessment work there.

MEMBER RANSOM: Yes.

MR. KELLY: But as far as TRAC development goes, it's primarily Birol Aktas and partially Rex Shumway. And then for the mapping of the RELAP5 kinetics, we've had some help from Doug Barber of ISL. And I apologize to anyone whose name I didn't mention.

MEMBER RANSOM: Then there's the interface test too, right? Is that a separate contractor, the GUI?

MR. KELLY: Oh, SNAP, yes. That's completely separate.

MEMBER RANSOM: Is that under your direction too?

MR. KELLY: Well, Chester Gingrich, who's a member of my Code and Models Development Team, is the Technical Monitor for SNAP, and I leave that to Chester.

MR. ROSENTHAL: And then we have the

resources for assessment, which we'll get to later this morning. That's separate from development.

MEMBER RANSOM: Does that include developmental assessment or do you have the developers do the developmental assessment? They run tests problems, I assume.

MR. KELLY: Yes, definitely.

CHAIRMAN WALLIS: Now, who are the users of this code?

MR. KELLY: Well, our primary user, of course, is NRR.

CHAIRMAN WALLIS: So are you going to train NRR?

MR. KELLY: Well, one of the people that used to be in my Code and Models Development Team left for NRR, and since that time we've been getting user needs for TRAC-M. And so --

DR. BANERJEE: Who is that, Joe.

MR. KELLY: Shanlai Lu. And so that's actually part of the reason we had to reallocate some of the funding was we got a user need for advanced BWR fuel in anticipation of the SBWR submittal.

CHAIRMAN WALLIS: I think it's important that the Agency get real experienced with use of this code.

MR. KELLY: Right.

CHAIRMAN WALLIS: So that it becomes an easy thing to use.

MEMBER RANSOM: I guess one other quick question: Has anybody ever reviewed the details of what you're doing from a theory point of view, basic equations and whatever is going on? This Committee, I guess, is not, from my understanding.

MR. KELLY: Okay. Over the last five years, what has primarily been going on is a code modernization, improving the architecture and enabling the capability to be able to, in effect, do old input decks, whether they be from TRAC-B or from RELAP5. And also the space when incorporating the physical models from TRAC-B for certain components. That's it. We haven't made improvements to the code in the sense of better physical models or better numerics, except for, obviously, little fixes occasionally.

So on my watch, which would be the last five years, the fundamental equations, the way they're averaged and differenced, have not been reviewed. Whether they were a subject to a peer review before that time, I don't know. Now, of course, the CSAU study was done with TRAC-PF-1-MOD1, which is the predecessor code here. The fundamental equations have not changed since PF-1-MOD1 with the exception of the change to the momentum equation for how it handles area changes.

MEMBER RANSOM: Well, even that has been an issue in recent code reviews, and it would be, I think, kind of good to get this out and reviewed and either get acceptance or -- you know, it's kind of poor to wait until next year and then find that there's some objection to way it's handled, the view or whatever. So that --

MR. KELLY: If you want to put us on the schedule for, say, sometime in early spring --

MEMBER RANSOM: Well, that's, I think, up to Graham. I personally feel like it would be very good to take a look at this and resolve some of the issues that have been coming up in connection with other codes, for example.

MR. KELLY: I don't have any problem with that at all. Just give us plenty of notice, because to come here and do a good job, we need to spend some time on it, rather than just xerox pages out of the code manual and slap them up on the projector.

DR. BANERJEE: I guess it would help to have whatever documentation you've got in front of us maybe a couple of months early so we can review it and send you questions.

MR. KELLY: Well, basically, the part of the theory manual that describes the equations and how they're differenced will stay the same, so you already have it.

MEMBER RANSOM: We already have it on CD, but it's, substantially, the TRAC-P documentation, and it's as vague as that original documentation, so it's nothing explicit.

MR. KELLY: There's nothing in it whatsoever about how the equations were initially derived or averaged over the control volumes. It basically says, "Here are the equations, here's how we differenced them." And that's what will have to be generated as part of improving the documentation, but it's also what would have to be generated before someone comes here and presents it. So that can actually be synergistic in that effect, because we plan on redoing that part of the documentation in the next six months or so. So if in the middle of the spring or something, that timing would be about right.

DR. BANERJEE: You're aware of the discussion that's been thrown around S-RELAP?

MR. KELLY: Well, I won't address S-RELAP, because I worked for Siemens, okay?

DR. BANERJEE: Right, but --

MR. KELLY: But what I will -- I am aware of that discussion a little bit. I've tried to stay away from it, but I was definitely aware of the discussion with the same kind of topic having to do with RETRAN, which preceded the S-RELAP5 submittal.

CHAIRMAN WALLIS: Actually, we're also aware of the presentation you made when you were employed by Siemens.

MR. KELLY: Right. Which, as I recall, was over six hours.

CHAIRMAN WALLIS: It was very interesting and it seems to have disappeared from the corporate memory of that conference.

MR. ROSENTHAL: We did get the consultants -- the ACRS consultants report such as yours, and several of the staff did read it.

CHAIRMAN WALLIS: Okay. Well, that will be sorted out some time next year.

MR. KELLY: Okay. Now what I want to do is review the development objectives and tell you where we are. The first was to have a modern architecture, the next was to effect the code consolidation, make it easier to use and then improve the accuracy and the numerics. Well, as I said, most of the effort, in fact, has actually been in these first three items.

A lot was to improve the architecture in the beginning but what really has the taken the time was the code consolidation. We initially reviewed the code consolidation as putting in the TRAC-B models so that the PWR code would also be able to do boiling water reactors and then recovering the capabilities of the Ramona code by having coupling to 3-D kinetics and that's coupling to the PARCS code. And I think you've seen results from that, and that's worked out fairly well.

What turned out to be a big job was doing the consolidation for RELAP5. The initial idea was we have a PWR code that's supposed to be a large break code. All we have to do is assess it against small break, find out where the deficiencies are and improve the models. So that was the initial idea for what consolidating RELAP5 capability would be, but along the way it became acknowledged that most of our users are RELAP5 users, our past users, and most of the input decks, whether they're for large experimental facilities or for plants, are RELAP5 input decks. And as you know, putting together an input deck for a plant is no small undertaking. People talk about a staff-year or so, or more.

So the decision was made to retain that investment, and what that means is we wanted to be able to take a RELAP5 deck, which can be 10,000, 20,000 lines of input, run it through -- and run it as if it were a TRAC deck. And that's not just simply translating the input deck, which normally you could do if the two codes were close together. But there were some very fundamental modeling differences between RELAP5 and TRAC that caused other changes to have to be made inside of TRAC.

Very simple example is a pipe. In RELAP5, a pipe has a certain number of control volumes and internal junctions only. It doesn't come with junctions attached to the ends. In TRAC, a pipe automatically has junctions attached to the ends. Now, that's a seemingly trivial thing, but when you're trying to map components from a RELAP5 deck to TRAC components that makes a big difference. So we had to put in a capability called single junctions into TRAC, which didn't exist before, so that we could start making TRAC pipes look more like RELAP5 pipes, et cetera, et cetera, et cetera.

CHAIRMAN WALLIS: Now, the equations you're solving are essentially the same for both codes?

MR. KELLY: Well, if you mean that they're a two-fluid, six-equation model, yes.

CHAIRMAN WALLIS: But the terms in those equations --

MR. KELLY: Are different.

CHAIRMAN WALLIS: -- are the same or they're different?

MR. KELLY: They're different.

CHAIRMAN WALLIS: Yes.

MR. KELLY: And that's especially true in the momentum equation.

MEMBER RANSOM: Along that line, Joe, I know that one of the differences, and I'd be interested in how you resolve it, is TRAC used to view the -- the volume center had an elevation and so the elevation change would be from volume center to volume center, whereas in RELAP5 the elevation was specified at the junction. And then the change in elevation would occur at the junction associated with volumes. And so how do you handle that problem?

MR. KELLY: TRAC can now do both.

MEMBER RANSOM: It can do either?

MR. KELLY: What we basically --

MEMBER RANSOM: I know I've had trouble

with TRAC-2 because they sometimes didn't account for the elevation change from volume center to wherever the junction was on the volume.

MR. KELLY: Yes. Basically, you can now bend it in the middle of a cell so that you can do the RELAP5 and then subtract --

MEMBER RANSOM: Well, that's TRAC. TRAC bends in the middle.

MR. KELLY: Yes. I get it mixed up when I see --

MEMBER RANSOM: You'll find bends at the junction or at the ends of the pipe.

MR. KELLY: Basically, it was put in so it can be done either as part of the input processing and then -- again, that's one of those little things that turned out to be a lot of work to get in and get working right.

MEMBER RANSOM: The other thing, my experience with both codes is that the T-modeling has never been very good. RELAP5 has this idea of a parallel branch which was an expediency, you might say, in development. TRAC had the idea of a volume which you branch off of the volume. And both of those have problems, actually, and I'm wondering if -- you know, that's an area, I think, that ought to be reviewed and maybe some effort ought to go into actually improving the T model. I don't know what your opinion is on that.

MR. KELLY: Well, I'd have to --

MEMBER RANSOM: That's come up here in terms of how the momentum equation is treated and where the losses lie and how you treat the elevation changes in that situation. Really important aspect.

MR. KELLY: Yes. And this is where I'm a little out of my depth, because I haven't looked into exactly how the T is done. But what I can tell you is that when we consolidated the jet pump model from TRAC-B to TRAC-M, TRAC-B had problems conserving momentum in a T, so in order to be able to do a jet pump, that basically didn't work because the momentum equation was dissipated, so they had to go in and put back the source term for the driver, okay? When we did that in TRAC-M, we don't need the extra term because the momentum equation is differenced in a different fashion, and it seems that it does a much better job of conserving momentum in a T.

MEMBER RANSOM: It's more like the modified Bernoulli equation that RELAP5 is based on, I guess, so that you preserve the change in the pressure with increase in area or change of velocity.

MR. KELLY: Yes and no.

MEMBER RANSOM: Del TRAC did not a good job of that.

MR. KELLY: It doesn't use a volume average velocity like RELAP5; it uses the V del V term. But on both the V and the del V there are modifiers based upon the difference between the junction and the volume area. And that seems to work pretty well, and there is a section in the theory manual that explains where that came from.

MEMBER RANSOM: Right. I think TRAC-P, they worked on that concept too, you know, to try to preserve the --

MR. KELLY: In MOD2, not in MOD1.

MEMBER RANSOM: Okay.

MR. KELLY: MOD1 uses the non-conservative form, which TRAC-P does as well --

MEMBER RANSOM: Right.

MR. KELLY: -- the V del V, but it didn't have the fix-up for the area changes, so any time you went through an area change, you would get more than the actual pressure drop. And that's not the case in TRAC-P now.

MEMBER RANSOM: I know from what we've seen in RELAP5 in some of the calculations yesterday, it's not clear that the MOD3 version anymore behaves correctly. So it would be interesting even to look into that aspect.

MR. KELLY: Yes. So far, you know, in the assessments that have been done, we have not seen any very large recirculating flows in a downcomer, so the pumping term or whatever that appears sometimes to occur in RELAP5 we haven't seen.

MEMBER RANSOM: You haven't seen that in --

MR. KELLY: In TRAC, in TRAC-M

MEMBER RANSOM: Okay.

MR. KELLY: Now, on the other hand, we haven't yet gone and done some of the things like the OSU test where you have a large body of water basically sitting still with vapor above it.

MEMBER RANSOM: Right.

MR. KELLY: When we do those kind of tests, one of the things I'll be looking for is did we get any artificial recirculation patterns, and at that time we'll know.

I already mentioned this but it was the success metric, and that was that the simulation fidelity of TRAC-M must be at least as good as or better than each of the predecessor codes for their targeted application.

CHAIRMAN WALLIS: Have you listed the difficulties with the previous codes that need to be resolved, like this sometimes suppressing momentum flux or something? Have you made a list of -- and Ts -- have you made a list of the things that weren't too good about the previous codes, rather than just saying simulation fidelity?

MR. KELLY: Well, this success metric was for the consolidation, and that's actually setting the bar pretty low, so that's not addressing the problems that are there, it's just saying we're going to be at least as bad as the predecessor codes.

CHAIRMAN WALLIS: Well, that's right. That's --

MEMBER RANSOM: And yesterday it seemed like with RELAP5 calculations, or the recirculating flows, which are a problem, there were -- break flows were poorly predicted and even questions about stability that -- there were no actual tests of stability but evidence in the calculations that possible instabilities. And we're wondering, are those -- well, I guess it remains to be seen whether those will be present in the present code, but we're also thinking that under the maintenance aspect some of these ought to be corrected or found out what the root cause is, even in the existing codes.

MR. KELLY: Yes. I've looked at the momentum flux problem in RELAP5 before. I did this during the AP600 for why these recirculating flows. And it has to do with the way you define the volume average velocity. You know, if you're a single phase, you can come -- in a normal number of junctions on each end of the control volume, you can come up with something that makes a lot of sense. But once you go to a two-phase configuration and you have a branch component which sometimes is used for the lower plenum so it will have as many as eight junctions for the downcomer and then several junctions for the core all in this one volume and then you try to transfer momentum from one to the other, I thought that was a bad idea once that is taken to that extent. And so when we did the consolidation of, in effect, the branch component in TRAC, I kind of said, "No, we're not going to do momentum transfer for that. It's a large volume, it's going to act like a plenum, we'd lose the momentum and get a pressure recovery and then come back."

MEMBER RANSOM: That would be -- that's in the TRAC-M?

MR. KELLY: When we map a branch component that's what happens.

MEMBER RANSOM: You just throw away the momentum flux calculation, basically.

MR. KELLY: Well, you recover it as a pressure. But, yes, we don't try to get momentum flux from the junctions that are a downcomer to this branch to the junctions that represent the core.

MEMBER RANSOM: So that volume becomes like a stagnation point, in effect, yes.

MR. KELLY: Yes. Because it may not give the right answer but at least it doesn't go off and give a horrible answer.

MEMBER RANSOM: Interesting.

DR. MOODY: Joe, I just was sitting here thinking you've got a code that's going to be released about this time next year, and it will have been built up of components of whatever the state-of-the-art or the state of the codes are at that time. I think my short experience so far with ACRS shows that all these codes are living documents, they're going to be improved or corrected or changed. Will there then probably be a TRAC-M Plus 1 and thereafter it will a continual --

MR. KELLY: Yes. I'll be answering that

as we go through the presentation. I think that's all I wanted to do here. But during this next year, the major effort is going to be on assessment and tying up the loose ends to release the code. And so next year, during next year, we can come one or two times and start showing assessment results. Steve is going to show the matrix that we're doing, and this is a consolidation matrix, and I don't know if he has some preliminary results or not, but starting, you know, in a few months, we can start coming and actually show you what the code does and doesn't do.

CHAIRMAN WALLIS: So Steve is going to take over part way through this bunch of slides here?

MR. KELLY: Well, I'm going to do that bunch; he has his own bunch.

CHAIRMAN WALLIS: Well, if you're going to do this bunch, then we better move along.

MR. KELLY: Right. And what I'm going to basically do is skip, just hit a few high points in the next one. These are our development objectives. Improve the architecture. And this is basically complete now with the exception of reducing the maintenance and that's improving basically the coding, and that's going to be something that's continuing. And what I mean is when we go in and fix a model or put a new model in the code or a new capability, we're going to do it in a much better way than the legacy code. And so, for example, the reflood model development that I'm doing now is in its own module. We tried to make Fortran look as much like an object-oriented code as we can, and so that's going to help a lot here on readability and also maintenance.

MEMBER RANSOM: When you give that as an example, though --

CHAIRMAN WALLIS: Is Fortran 90?

MR. KELLY: It's 95.

MEMBER RANSOM: Modularity is kind of an abused term, in my mind, because every Fortran code that's ever been written was modular in some sense. When you break into serve routines it has some modularity. But you mentioned the reflood model. Will it do its own fluodynamic calculations then so that everything is contained in that module?

MR. KELLY: No.

MEMBER RANSOM: No. So it has to be patched on to existing modules, to a degree, right?

MR. KELLY: Well, I'll give you an idea, a quick idea. It's going to have all of the constitutive models in it for wall heat transfer, interfacial heat transfer and interfacial drag having to do with reflood. They're all going to be in this module. The interface to track is through what's known as -- well, it's what are known as the module variables. The actual reflood module itself doesn't know that it's part of the TRAC code. It doesn't have to know a single variable name from TRAC.

MEMBER RANSOM: Nothing is passed down into it; is that right? Then how would you compute heat transfer? You need velocities, you need densities for the properties.

MR. KELLY: Well, those are set in TRAC. It's kind of a like a calling argument but a more efficient way of doing it. In TRAC, at the appropriate place, it sets these variables in a module --

MEMBER RANSOM: Okay.

MR. KELLY: -- sends it to the reflood module. So what this means is you can compile and debug and reflood module external to TRAC, because it doesn't have to know anything at all about TRAC, and you can put a little driver code where you send it pressures, mass fluxes and void fractions, check out the models independent of the rest of TRAC to make sure it have the correct expected behavior with those parameters. And that's a very good way of doing things, because you can test things a lot better, and you can also do development in parallel without getting in each other's way.

DR. BANERJEE: How parallelizable will the code be?

MR. KELLY: Well, what we've already done is put in what I'll call coarse grain using PBM, and so you can run multiple instances of TRAC on different processors. So you might have the vessel on one processor, a loop on another, PARCS on another. And then at certain points -- and this is one of the things that was done as part of the architecture. John -- Professor Mahaffy, if you will, formalized the data transfer in the code. He identified the times and there are 13 different times during the time step at which there's the potential for data transfer, and so he made those as synchronization points and identified what information has to be passed at each of those points, and those are now in -- they're actually something called -- I've forgotten the name, it's list control -- list-driven data transfers. So you just put a list of the variable names for each of these synchronization points, and then it makes that data available. So that makes it very easy to do the coarse grain kind of things.

But also because of these synchronization points, between them there's no data transfer, say, between a hydraulic component and a heat structure or between one hydraulic component and another. So between those synchronization points you can then go to a fine grain, and that's what we're going to be looking at in the next year. And we're also, at the moment, looking at getting rid of PBM because it's fairly inefficient and using sockets as a way of doing it.

DR. BANERJEE: Or go directly to NPI, which has the sockets built in.

MR. KELLY: Well, NPI and PBM are the same -- are two colors of the same thing.

DR. BANERJEE: Yes.

MR. KELLY: So they both have the same inefficiencies. The sockets are closer to the hardware, but you really only have to treat two kinds: Posic sockets and then window sockets. And so by doing that, we can gain -- and at our last coordination meeting, John showed the efficiency gains you can get by, in effect, removing the middleman there. And it's substantial.

DR. BANERJEE: Will you be able to ground this on a cluster?

MR. KELLY: Yes.

DR. BANERJEE: It will automatically set up all the processes wherever it has to go.

MR. KELLY: Not automatically.

DR. BANERJEE: You need to tell it.

MR. KELLY: We have to tell it how many processors and what part of the mesh is going to go on which processor. But you can do that.

MEMBER RANSOM: Joe, along the lines of the architecture, how do you handle the numerics? You know, you mentioned the reflood model and the TRAC thermal-hydraulic calculation module, but there are some things like wall temperature and fluid temperature you'd like to be at new time, implicitly coupled. Have you gone to an iterative solution scheme so that you can implicitly couple whatever terms, I guess, that you want to be implicitly coupled?

MR. KELLY: Yes and no. One of the differences between TRAC and RELAP was there is an iteration scheme on the mass energy equations already. You know, RELAP, say if you're in the semi-implicit mode, it just does one shot at the mass energy equations and hopes the linearization was sufficient. In TRAC, it actually checks the convergence and we iterate on the mass energy equations within a time step.

MEMBER RANSOM: So you have opportunity to update implicit features in code then.

MR. KELLY: Well, you could, but that's not done yet. But when you see my long-term development plan it's there, because that is one of the sources of problems sometimes is that you have inconsistencies when you go on to the next time step.

CHAIRMAN WALLIS: When you introduce these improvements does this increase the run time?

MR. KELLY: Yes and no. Typically, when you go to a -- when you look at your run time it's basically composed of two things, something called a grind time, which is how much CPU time it takes to do one time step for one computational cell. Obviously, as you go to a more sophisticated numeric solution, that's more implicit where you talk about iterating or solving a larger matrix equation, the grind time goes up. But if your code performance or robustness improves, and what I mean is you have less numerical oscillations -- well, there's two things, you can increase the time step size in two ways: One is violating the Courant number, which TRAC-M is already able to do because of the SETS method, but sometimes even though in theory you can go to large time steps the code doesn't actually use them because it's having trouble. Say there's a lot of condensation, you know, one cell is going from condensing to flashing, when you improve those kind of things and sometimes the implicit coupling that Professor Ransom talked about will do that, then you're able to take ever larger time steps so the number of time steps you need to get through a given transient goes down, and you have large gains.

I have experience with the CATHARE code because I worked in Grenoble for a couple of years. The 1-D components in that are fully implicit, so when things are running very well -- so like for a forced reflood problem where you don't have to worry about the end oscillating, I've seen it use one-second time steps for a reflood transient, whereas we're more likely to use five milliseconds. That's a huge difference. When you can do a whole reflood transient in a couple hundred time steps as opposed to 20,000 time steps, you know, you can spend some time per time step per cell to get those kind of gains.

CHAIRMAN WALLIS: I think we also get trouble with the traditional way, sometimes with the momentum equation, because of the acoustic waves propagating, and of course that happens pretty rapidly, so you have to go back to very, very short time steps to catch those waves, which really don't affect many of the transients at all.

MR. KELLY: Right.

CHAIRMAN WALLIS: But because of the numerics you have to --

MR. KELLY: Well, actually, yes and no again. If you're interested in sonic wave propagation, you have to go down to a time step that, if you will, would be a sonic Courant number, and that's very, very small. But this is true of both RELAP5. They started out with something called a semi-implicit numerical scheme, which basically means that mass and energy and even momentum flux are conducted explicitly. But the pressure equation is solved implicitly for all of the cells simultaneously. So that gets rid of a sonic criteria, but it also puts in some damping of pressure waves.

So if you're going to do a pressure wave transient in a pipeline, you have to crank the time step way down. Otherwise you don't have to worry about that. Where we get into problems with pressure waves is it changes T-sat a little. And if your constitutive models magnify that, then you can end up with noisier calculations than you'd like to see.

MEMBER RANSOM: Incidentally, along that line, have you put in -- you know, Mortenson, I think, did some -- made some real improvements to the equation of state in RELAP5 in recent years, and have you put that into TRAC?

MR. KELLY: Yes.

MEMBER RANSOM: You have, good, because that eliminates some of the problems of T-sat that can cause a problem with some of the models.

MR. KELLY: Yes. It wasn't so much T-sat but RELAP5 when it extrapolated into the meta-stable region --

MEMBER RANSOM: Yes, meta-stable.

MR. KELLY: -- the properties would be inconsistent and it would get mass errors. Now, with TRAC, we didn't have that problem so much, but what we had was that the equation for the liquid density when you got up to reactor operating conditions was not accurate enough for kinetics calculations. It's plenty accurate for large-break LOCA but not for kinetics.

MEMBER RANSOM: I know in the numerics they had spongy water too which was kind of questionable technique that was basically to try to stabilize some of the situation when you get near incompressibility, and I hope that you haven't -- you've done away with those things.

MR. KELLY: I don't know. You'll have to ask Professor Mahaffy when he's standing here. I don't know everything about the code yet, I'm still learning.

CHAIRMAN WALLIS: I assume that you're answering all these questions is going to gain us time later on?

MR. KELLY: Well, I thought this was going to be the easy presentation, I was going to fly through in no time at all. I've already talked about the consolidation. That's basically complete. The only reason the large-break LOCA part isn't complete is because we judge -- this is the subject of this afternoon -- we judge the TRAC reflood model to be completely unacceptable, and so we're replacing it with an interim model. That development's almost finished, and we'll be testing it starting early next year.

CHAIRMAN WALLIS: That's the subject for this afternoon?

MR. KELLY: Yes. Well, not this afternoon but after the morning break, sorry. Hopefully it won't go to this afternoon.

I won't talk about SNAP. At some point soon, we'll have someone come and give you a demonstration. But that's been very important as far as trying to make this easier to run the code.

MEMBER RANSOM: Now, that's Ken Jones that's working on that?

MR. KELLY: Ken Jones -- Initially, the contractor was ISL.

MEMBER RANSOM: Right.

MR. KELLY: Then we switched to Ken Jones. That contract has expired, and it is now out for a competitive bid. So at the moment, we -- or as of January 1, we won't have a SNAP contractor, but the proposals are coming in.

MEMBER RANSOM: Where is Ken Jones, is he here in Washington?

MR. KELLY: He's in Pennsylvania somewhere, so he's not very far away.

One of the objectives was to improve the accuracy, and we know we're going to have some deficiencies in the physical models. That was not going to be part of the consolidation; that's part of the code improvement. And we've started that because of some deficiencies that we've uncovered and that we'll talk about. An advance two-phase model. This means doing things like adding a droplet field but also looking at putting in interfacial area transport. And that's going to be done during this period. And then we also need to look at quantification of accuracy.

CHAIRMAN WALLIS: This means uncertainties? Are you going to try to carry them along in some way or are you going to evaluate them by running the code many, many times?

MR. KELLY: What I would probably do is what I refer to as the GRS method, which is similar to what the Siemens Framatome approach is.

CHAIRMAN WALLIS: Or the 59 runs type?

MR. KELLY: Or however many you want. But nowadays that's not, at least for large-break LOCA that's not such a burden.

MEMBER RANSOM: Well, there are some things you need to do to the code, actually, because things like maybe interface drag, or whatever models you believe there is uncertainty associated with them, then define a range of uncertainty, I mean it needs to be built into the code so somehow you can sample these things then.

MR. KELLY: Yes. You have to build in

multipliers on those phenomena.

MEMBER RANSOM: Multipliers, whatever.

MR. KELLY: Yes. But the real burden in all of this is not doing -- if you use that type of approach, isn't the uncertainty quantification, you know, turning the crank however many calculations, it's determining the uncertainty in the individual models that you deem to be important. And that's where code assessment comes in, and that's where we really want to spend some time in the next few years.

CHAIRMAN WALLIS: You shouldn't just look at the traditional coefficients. If you're modeling the average B squared, you know it's bigger than the square of the average, so you might put in a coefficient there that you can then vary in some reasonable way.

MEMBER RANSOM: I'm a little concerned with even just putting multipliers on these terms, because multipliers sometimes won't give you, for example, let's say you have a range on a variable, high and low, min/max, but you want a uniform distribution, so statistically you want to sample that in that range uniformally. Now, that either means you've got to calculate a multiplier that will give you the equivalent of that, but it's not as simple as, say, varying the multiplier.

CHAIRMAN WALLIS: No. You've got to get statistical --

MR. KELLY: And that's why the GRS method is kind of nice, because what you do is for each for calculation, say it's 50 parameters which you want to vary, some being plant parameters, some the code parameters, for each one of those you've determined a range but also the shape of the distributions.

MEMBER RANSOM: Right.

MR. KELLY: You know, people typically use flat but --

MEMBER RANSOM: They don't know anything else.

MR. KELLY: Yes. But if they can justify it based on the assessment, you know, a distribution of a certain shape, be it normal or whatever, that's what you should use.

MEMBER RANSOM: Right.

MR. KELLY: So when you, in effect, construct an input deck, you sample all 50 of those at once using their distributions, set them, run the problem, do it again.

CHAIRMAN WALLIS: Flat is the most unlikely, it seems to me, and bell-shaped is the most likely for most of those variables.

MR. KELLY: And flat penalizes you the most. So in default, that's what you go to.

CHAIRMAN WALLIS: Can we move on now?

MR. KELLY: Okay. And this also -- I talked about parallel, and this is things for the future, looking at high-order differencing methods. Level tracking, both 1-D and 3-D, is put in the code. That's a bit of a success story. And we also reinstated the semi-implicit capability in TRAC, because TRAC has sets which kind of semi-implicit's built into. But we put this back in for BWR stability, so that's a case where you don't violate the Courant number.

This is an example, this good old oscillating manometer problem, and what I'm going to do is show you what happens with the new level tracking. So this is a very simple problem. Actually, this looks like it's closed here, but actually it was connected to a pressure source, both of them were. Start with the liquid level displaced and let it go. Well, the analytical solution in this is wall friction's turned off, just goes on with this magnitude. TRAC was the black curve, and you see it's highly damped. You only get a few cycles and it basically stops. Of interest, there have been times with different versions of RELAP where I've seen it start with equal levels --

CHAIRMAN WALLIS: And get bigger.

MR. KELLY: -- and start. Haven't seen that with TRAC.

MEMBER RANSOM: A critical part of this is whether the frequency is correct, because you can analytically predict that.

MR. KELLY: And with the 1-D level tracking model --

MEMBER RANSOM: Okay. So you got the inertia right.

MR. KELLY: Yes. This was Professor Mahaffy and his Ph.D. assistant, Birol Aktas. And they got it working very well for 1-D. More recently they've put it in for 3-D components, and so I can now -- and some of the testing problems, these are actually 2-D components on each side, and do a manometer that way.

CHAIRMAN WALLIS: Well, you should really put a bend with several nodes on the bottom to see what you're getting that way.

MR. KELLY: That may actually have been one of the sample problems.

DR. MOODY: What is the natural frequency for that one or at least the frequency, looks like you have about 11 cycles over 50 seconds. I counted them. So that's about up 0.2 with a period of about five seconds looks like there, and the analytical expressions, do you remember what that --

MR. KELLY: Well, the analytical solution is the orange.

MEMBER RANSOM: It's a function of the length of the column.

DR. MOODY: The square root of G over 2L or something like that, isn't it? I guess it's been checked out.

CHAIRMAN WALLIS: It's like a Froude number, looks like.

DR. BANERJEE: What's interesting is the points are sampled, I suppose, on a much slower sine wave.

MR. KELLY: Well, some of this -- I don't remember what the time step was here, but some of the roughness is just when you plot it.

DR. BANERJEE: What are those little triangles?

MR. KELLY: Oh, those are just curve identifiers. Those aren't the points. You have, you know, 20 or so points coming up and down each of these.

DR. BANERJEE: But because they have some sort of a sine wave.

MR. KELLY: Oh, okay. Well, I --

MEMBER RANSOM: Well, oftentimes they make this comparison and you can compute the analytical solution on as fine a grid as you like, make a nice continuous curve while the other one is -- well, that may that not even be every point that's computed, because if these are plotted using NPA-type of graphic --

MR. KELLY: Right. You don't normally dump every time step to the graphics file.

MEMBER RANSOM: Yes. You sample, actually.

MR. KELLY: And that's what's done here, but I'm not sure what the sampling frequency was.

CHAIRMAN WALLIS: I think Sanjoy is looking at a sampling frequency there where you get an alias and you pick up an artificial frequency.

DR. BANERJEE: I'm just hoping that it wasn't sampled at that frequency.

MR. KELLY: Okay. This slide has to do with how we go about preserving legacy input models.

SNAP is a graphical user interface, and this is TRAC-M. The input processor for TRAC-P, you know, reading an ASCII input deck, still in TRAC-M. At some point, we want to take it out and move it up to here, but for expediency's sake it was left in TRAC-M. When we did the TRAC-B consolidation, the capability of reading a TRAC-B deck directly, which is simply added, and that's because the modeling philosophies, the way the components are done, are the same between the two codes. So if you have either an old TRAC-P or TRAC-B deck, you can read them with TRAC-M and run them. You don't need the graphical user interface.

But if you come in with a RELAP5 deck, it's a much bigger deal. So the RELAP5 deck is read by SNAP, and the RELAP5 model editor, which is finished, can then display that deck, and you can go in and point and click and change things. It exports it in a platform independent binary file, which we call a RELAP5 TPR, for TRAC Portable Restart. There's a part added to TRAC which then is able to read this file and map the RELAP5 components to their TRAC equivalents. Then TRAC can export that back to SNAP using its own version of a TPR file. And the TRAC-M model editor is almost finished now, it's very close to being finished, so then you can bring in the TRAC

-- RELAP deck as a TRAC deck and edit it in SNAP. And then this is a two-way street, and so then you can run it. And the only part that hasn't been for the mapping is the control system, and that's underway right now.

MEMBER RANSOM: One thing of interest: TRAC always had a very poor input philosophy, and how do you build a new model in SNAP to build a TRAC input deck?

MR. KELLY: Well, if you've seen SNAP work at all, it's works basically the same for RELAP5 or TRAC, and what I mean is you --

MEMBER RANSOM: You can tell it to put a TRAC model, basically, and so you start filling in the components?

MR. KELLY: Yes. And so you just drag and drop the components on a palette, but then for each component you have to then go and find the data. So it's better than it was, but it's not as good as it needs to eventually be, because you still have too much information to put in.

MEMBER RANSOM: Too much meaning -- I mean some way you have to get the basic information in.

MR. KELLY: Right. But I mean you know how what you said that the TRAC input was poor?

MEMBER RANSOM: Right. It had a nameless type input and --

MR. KELLY: Some of that's still there, but we're going to evolve to something better as we go.

CHAIRMAN WALLIS: I think, Joe, the rest of this we have seen before, all the consultants have seen it. You can probably move through the next few slides pretty quickly.

MR. KELLY: Okay. Here's where we are on the consolidation. Everything's just about finished except the assessment. We're just basically running a little late, and the so the assessment's going to start later than we had anticipated.

DR. BANERJEE: Are you going to be incorporating things like what B.J. is doing at UCLA?

MR. KELLY: Yes.

DR. BANERJEE: How is that --

MR. KELLY: Not part of that, but I'll show you.

CHAIRMAN WALLIS: We're going to get there in Slide 19.

MR. KELLY: There are only two -- as part of the consolidation, the idea was consolidate the capabilities, use the existing physical models, don't improve them. Well, we ran into two snags. When we were doing the TRAC-PARC coupling in doing a Peach Bottom turbine trip, we weren't able to have a good enough prediction of the axial void profile in a BWR for the kinetics, so we made the decision to implement the TRAC-B interfacial drag in subcooled boiling models. That had actually been done kind of in a hard-wired way, it worked out very well, so now we're putting it in -- you know, coding it in, and that will be in by the end of this year.

Also, as I'm going to talk about later, the reflood model is totally unacceptable, and that's mainly because of large oscillations. And so we're coming up with an interim reflood model. We did not intend to do this, but we're basically forced to in order to have meaningful calculations in the near term.

The first thing was improving the fine mesh model when they upgraded the way the heat structures are done in TRAC, and there were some bad decisions made about how to do this. I'll talk about this more later, so I'll --

MEMBER RANSOM: By fine mesh, you mean in the conduction solution?

MR. KELLY: The way we handle reflood, for the hydrodynamic cells they stay the same. They may be on the order of 20 centimeters long to half a meter depending upon how coarse a description you want to use. And, obviously, that doesn't resolve anything at the quench front, especially axial conduction. So something I did about 25 years ago was come up with something I called the Fine Mesh Rezoning Model, and it's really an adaptive grid technique that's applied to the heat structure, and I just didn't know the term "adaptive grid" at that time. So that's what it does. Based upon local temperature gradients or heat flux gradients, it makes a decision as to whether or not to refine or coarsen the mesh just for the conduction. And that includes the 2-D conduction.

DR. BANERJEE: Now, these oscillations, they are non-physical oscillations, because in reflood there are oscillations that occur.

MR. KELLY: Right. Which in theory are equations and constitutive models should time average out. Excuse me, you're talking about bigger.

DR. BANERJEE: Yes, long-term oscillations.

MR. KELLY: Yes. I'm talking here about --

DR. BANERJEE: With very high frequencies.

MR. KELLY: -- a FLECHT SEASET test with a force flow, no downcomer.

DR. BANERJEE: Okay.

MR. KELLY: And I'll show you this, I'll show you those calculations in the next presentation.

DR. BANERJEE: I have another question: Do you track the front at all in the reflood, because there's a lot of problems with numerical carryover that occur otherwise because of the smearing. And if you have a tube being filled, you get carryover when you shouldn't be getting it.

MR. KELLY: That's the idea of the level tracking, and we don't know yet how that's going to work with reflood, and we'll discover it in the next few months.

CHAIRMAN WALLIS: I think there is physical smearing because of entrainment too, it's not just numerical smearing.

DR. BANERJEE: Well, I mean the real smearing is fine if there's entrainment, but if you try to refill a tube, let's say a cold tube, that's always a good test, you'll see that you get carryover.

MR. KELLY: That's the purpose of the level tracking, and that seems to be that we don't have that. But how level tracking is going to interface with reflood, in a few months I'll be able to answer that.

CHAIRMAN WALLIS: Well, we heard Larry Hochreiter last week or whenever it was, very recently, and I think the Committee felt that you shouldn't wait for two years before someone tried to use the data to influence TRAC-M but that the efficient way to do it was to start right now -- analyze the data and what you need for TRAC-M at the same time as the data was being produced. That was much more efficient.

MR. KELLY: Yes. And that's really the subject of the next presentation -- of my second presentation, so let's discuss that then.

CHAIRMAN WALLIS: Are you going to reassure us on that point?

MR. KELLY: As best I can.

DR. BANERJEE: There's one other point, Joe, that came up in the last few meetings. There were three areas: subcooled boiling, reflood and then this condensation stuff. Yesterday, there was this stuff about the difficulties with the region where the ECI is coming in. And are you going to do something, because it seems that heat transfer is very poorly modeled in that region.

MR. KELLY: Well, we will be doing -- well, there's an Upper Plenum Test Facility cold leg injection test to look at condensation in cold leg. I don't remember the test number. We will be simulating that with TRAC, and we'll see how good or well it does. Later on, as we expand our assessment matrix, there was some -- I don't remember if it was EPRI and B&W, but it was one-third scale cold leg injection test for both accumulator flow rates and HPSI flow rates. And as soon as we can get that data, we'll add it to the assessment matrix as well. And so try to make sure we have a good job of what the condensation that occurs there.

MEMBER RANSOM: Well, along that line, does TRAC-M permit you to -- I mean from yesterday's discussion, it was clear that the cold leg sometimes needs to be modeled multi-dimensionally in order to predict thermal stratification phenomena that are occurring there and mixing. And in TRAC-M, can you have multi-dimensional vessel components so you could model that cold leg, I guess, multi-dimensionally and then hook it to the vessel?

MR. KELLY: In theory, yes. You can have more than one vessel component, and we'll be doing that in the ESBWR, for example. The reality, though, is TRAC is not a CFD code.

MEMBER RANSOM: Right, I understand.

MR. KELLY: And so when you start nodalizing a pipe as if it were a CFD code, there's a lot that the constitutive model -- I mean it doesn't have viscous and turbulent sure stress density to begin with.

MEMBER RANSOM: Oh, I understand that.

MR. KELLY: And if you then let that horizontal pipe go two-phase like we saw in the AP600, your flow regime, yes, it's going to try to -- for each of those small nodes, it's going to try to identify a flow regime, and of course a flow regime is indicative of the entire pipe. So that's a research project, okay?

CHAIRMAN WALLIS: It will put in a wall drag when there isn't any contact with the wall and things like that.

MR. KELLY: Yes. So that would be a research project, and anyone that thinks you can just change the noding and get the right answer, you may get a better answer but you're not going to get the right answer.

CHAIRMAN WALLIS: I think you're getting so many questions you're going to run until the break, and I'm going to have to figure out how we go the rest of the day.

MR. KELLY: And like I said, I thought this was going to the easy presentation.

This is what we're going to do next year, and the idea is we start with the code alpha release. We've taken a first pass through the assessment, we're going to look through, identify where there are deficiencies in the assessment and target whatever, either numerics, either the problem didn't run or ran real slow or a deficiency in a physical model, make those improvements, completely repeat the assessment, then ask the question do we make it through the entire assessment matrix as good or better than the predecessor codes? If the answer is yes, we go to the official code release. It's also during this period of time we have to update the documentation.

Steve is going to talk about the assessment. The only point I want to make here is we looked at the assessment matrices for each of the individual codes, and we basically combined them. And what we will be doing is code-to-code comparisons. This was with reflood and so we don't do reflood for RELAP5 because that wasn't it mission, but we'll be repeating these tests with TRAC-B, TRAC-P and TRAC-M and doing code-to-code-to data comparisons.

DR. BANERJEE: Is the point that you're -- all those are pretty large experiments. There's a lot of careful experiments done with simple geometries like tubes or things like that, and I think it would be worthwhile having at least some subset of those in there, because there the measurements are very precise. We've got void fractions, we've got precise temperatures and stuff like that.

MR. KELLY: I agree.

DR. BANERJEE: And precise measurements of carryover.

MR. KELLY: And you're going to see me use a lot of those tests in the model development in what I'll be talking about this afternoon. And you're right, we should then bring those tests over, and I plan to. They just won't be part of the consolidation matrix, because that basically said what have we done before; let's repeat it. But, obviously, that's not sufficient to be the only assessment we ever do.

DR. BANERJEE: No, no. It can only be complementing this stuff.

MR. KELLY: Right.

MEMBER RANSOM: One thing that I'd like to encourage you to do is to include in the assessment what I'll call phenomenological problems, like the manometer problem that you just showed. These are very instructive in terms of showing the correct behavior of the code under at least situations where we know pretty much what the answer looks like. And Ts, loops, static problems, hydrostatic problems, a lot of these are very simple to run and can be very insightful in terms of just clearing elementary problems.

DR. BANERJEE: Does SCTF have large oscillations?

MEMBER RANSOM: Huh?

MR. KELLY: Well, let me answer his question. I agree completely. There are a few cases that we've brought over from what was done before. We're going to expand that. Like the multi-phase science and technology benchmarking kind of problems.

MEMBER RANSOM: Right.

MR. KELLY: I agree. We need to really expand that and make sure the code's doing fundamentally what's right, whether it's a horizontal stratified flow, whatever.

MEMBER RANSOM: Like a variable area pipe so you know whether or not the diffuser would behave correctly. And a lot of these can be done and checked and give reassurance that fundamentally the thing is okay.

MR. KELLY: I agree.

DR. BANERJEE: Well, there's Graham's famous single-phase problems.

MEMBER RANSOM: Well, sure.

MR. KELLY: And those are very, very good things for university contracts. It's good for the students, and it's a good way for us to get those problems in.

MEMBER RANSOM: Well, it's, I think, important to build up a package of these so you can almost automate the running when you have them in your package.

MR. KELLY: Well, we have an assessment -- I won't call it an assessment but a software quality assurance program which has an automated testing, and there are a lot of those that are in that, and the testing at this point in time is for differences. So if you were to make a code improvement that should have null effects on the answers, then you run this entire suite, and it's like 700 problems now that are run, and then it checks for differences --

MEMBER RANSOM: Right.

MR. KELLY: -- and spits out whichever

problems have significant differences, and then you have to go look at those.

CHAIRMAN WALLIS: Are you going to give this code to universities so that students can just run it and then you will get a lot of input from sort of standard problems, simple problems, and you'll find you may learn a lot that way.

MR. KELLY: Yes. And one of the decisions that we made -- well, you know, we have the PUMA facility at Purdue University, so the ESBWR PUMA assessment is going to be done by students there. It was cost-effective for us, it's the people that know the facility, and so that was a good way to go.

CHAIRMAN WALLIS: You probably give it away --

MR. KELLY: It's published --

CHAIRMAN WALLIS: -- and then the University could run it on non-nuclear problems.

MR. KELLY: Right.

CHAIRMAN WALLIS: Which will also be a good test.

MEMBER RANSOM: And one important thing I think out of all this dispersed effort, though, is that you have a nucleus somewhere, and I guess you're developing it here --

MR. KELLY: We're trying to build that expertise here.

MEMBER RANSOM: -- somehow to retain that capability.

MR. KELLY: And you see people like Steve coming on board, myself coming back and then some of the younger people that we're trying to groom to fit specific areas. That's like why I have Wei Dong working closely with me so he can start coming up to speed on what the physical models are or should be, what the extant database is. So we're trying to grow that capability. Question on SCTF?

DR. BANERJEE: The gravity reflood, did it have oscillations, do you remember?

MR. KELLY: It does. If I recall, they get damped out after several cycles. Then there's a small -- but the large-scale one -- it doesn't have large-scale oscillations except for the first few cycles.

DR. BANERJEE: Is that realistic, because I know that with FLECHT I guess they have to try to damp these, and at Winfrith they did two to keep the constant reflood, which they forced as an inlet condition. But in real life, this thing is going to oscillate because the downcomers and gravity flow.

MR. KELLY: Yes. Well, SCTF, especially the gravity reflood cases here, it's a very large-scale facility. There are 2,000 heater rods. It models eight bundles going from the core center line -- they're in a slab geometry going from the core center line to the downcomer, and then it has a downcomer and a lower plenum that's all correctly volumed and height-scaled or close to it. And so it is a gravity --

DR. BANERJEE: But you need a resistance to the full going out, so is it correctly modeled in order to give you this, do you remember?

MR. HULL: They have a mock-up of the steam generator that looks more like a steam separator, so you don't have the steam binding associated with evaporating the drops, but what you have is, in effect, an orifice plate that they trade in and out to give them different loop resistances on the hot leg. And so there are different tests with different hot leg resistances. So that's there.

DR. BANERJEE: Okay.

MR. KELLY: This is the long-term development plan in a snapshot. There's a color scheme here. Everything in this light blue color is going to be part of the initial code release at the end of next calendar year. Likewise with the green and the yellow and then further out in the future. So the idea is to release a new code version on a yearly cycle.

CHAIRMAN WALLIS: I think you're also going to do work on gas-cooled reactors and that sort of thing.

MR. KELLY: That's up in the air at the moment. We were going to do the HTGR as part of TRAC because of the early submittal with the pebble bed. That isn't going to happen, and so I'm not part of the --

CHAIRMAN WALLIS: It's not part of your plan now.

MR. KELLY: Not part of the plan now. But what is part of the plan is doing assessments and calculations for AP1000, getting the code to work with ESBWR, and you'll notice this little box that got put in with condensation with non-compensable gases, both for the PCCS and the suppression pool, that's for ESBWR. Right after the ESBWR is the STWR-1000. Following right on the heels of that is CANDU, the ACR-700.

CHAIRMAN WALLIS: So it's all light water reactors.

MR. KELLY: And what actually the eventual approach will be --

CHAIRMAN WALLIS: One is the heavy water reactor.

MR. KELLY: Well, actually, it's a light water cooler now, the ACR-700. It's light water-cooled, heavy water-moderated. What the eventual approach will be for, say, the MHTGR, I'm not sure what the decision has been on that.

I had a question earlier about the UCLA work. That's right here, so we'll be putting it in at the beginning of this next calendar year, and it will be part of a code release. The same with the OSU phase separation test, the ATLATS facility. And then I also have to look at low pressure interfacial drag in rod bundles.

I've done this by physical models, numerics and modeling capabilities. One of the things that got added was the capability of advanced BWR fuel, and that's having water rods inside the BWR CHAN component that are actually flow paths, but also having part-length rods and how that will then feed back to the kinetics.

CHAIRMAN WALLIS: Can you advance the reflood into that period too, the reflood modeling?

I think now it's not supposed to start until --

MR. KELLY: Yes. This is the interim.

CHAIRMAN WALLIS: It's the interim, but I mean learning from what's going on at Penn State.

MR. KELLY: The way that interaction has worked, Steve and I are technical monitors for that, so we go up for every meeting, we review the data, we review the test procedures, we design the test matrix, not the experimenters, and we also made changes to the instrumentation that they use.

CHAIRMAN WALLIS: Unless you try to put it into TRAC you won't really know what you need, so you can design the test measurements you like. Until you're working with it you're not really sure if you're getting the right stuff.

MR. KELLY: Delay to the next presentation.

CHAIRMAN WALLIS: Okay.

MR. KELLY: This was going to be done here. It got delayed for a year for two reasons. One is the actual experimental facility is way behind schedule. They were supposed to have a lot more done by now than they have accomplished, and then we've had a funding reduction, so we've stretched their schedule out. So what they're going to deliver experimentally you're going to see dates out to 2007, okay? The other reason this got delayed for a year, we had intended to start it here, is because of this work, because, basically, the same people are going to do that.

MEMBER RANSOM: Who's doing the condensation work?

MR. KELLY: I will be.

MEMBER RANSOM: Do it here, you mean?

MR. KELLY: Yes.

MEMBER RANSOM: So it's being delayed?

MR. KELLY: Oh, no, not the condensation work, the so-called mechanistic reflood development.

MEMBER RANSOM: Yes, right. But weren't you talking about the condensation?

MR. KELLY: Well, the data is already extant. If you look at the tests done at UCB -- well, fundamental tests at UCB and MIT, then there's things like the I'll get the PANTHERS experiment in Italy.

MEMBER RANSOM: Oh, this is just the modeling.

MR. KELLY: Yes.

MEMBER RANSOM: Okay. There are no experimental programs, you're saying.

MR. KELLY: No, this is not an

experimental program for the ESBWR, because that database was developed for the SBWR. When we start looking at the SBWR-1000, then we may -- you know, that horizontal condenser sitting open in the containment with fins are on it -- or, actually, I don't know if the fins are in or out this week -- but the database doesn't exist that I know of for that. And so we may want to have some confirmatory research to help us have a code model for that, but I don't know that yet. Okay?

This is really -- so this was delayed a year because the same people that are going to do this work are now going to do this work for the ESBWR. And this was judged to be of a higher immediacy.

The numerics improvements are here. I had a question earlier from Professor Ransom about making more tightly coupled implicit between the heat structures and the hydrodynamics and also for the interfacial heat transfer. That's going to be done here in 2003. We're also going to add a droplet field, and this is really necessary for this work. And we can talk about it a little bit this afternoon if we need to. We're also going to make improvements to the energy equation so we don't have the energy loss when you go across a junction with high pressure difference, so basically go to an enthalpy form of the energy equation so we don't get that.

We're looking at high-order differencing which will be used both for boron tracking and thermal fronts, because those all have to do with stability kind of things or a shutdown. And then look at making the entire 1-D components fully implicit as they are in the CATHARE code. And that's where you do an iteration through the constitutive models instead of just doing the constitutive models once per time step. And then looking at ways to do the 3-D fluid solution in a more implicit form.

MEMBER FORD: You have a very, very ambitious time schedule there, given the fact that there are several reactors up for certification or pre-application. You also pointed out that a lot of the work is being done at universities. What has your historical experience been in terms of the time limits of that work being completed?

MR. KELLY: I would probably say it depends, and it depends on what work you give them and who the contractor is. For example, at Penn State University, we have John Mahaffy. John was one of the initial developers of the TRAC code, and he was the originator of the SETS numerical method. No one knows

TRAC -- that part of TRAC like John.

MEMBER FORD: Okay.

MR. KELLY: Now, we're trying to bring Joe Staudenmeier, who's a staff member, along to learn that. That's what we've done, we've targeted pieces of the code for each staff member. But John has a great feeling of ownership for the code, as we all do with things we've worked on, and so when we have -- give him a problem whether it's an AP1000 deck or whatever and say, "Look, this thing just isn't working," he'll work on it until the wee hours of the morning. Now, when you talk -- and so a lot of the things turn around very, very quickly, but if you want to look at something like high-order differencing schemes and you want to investigate several different methods, find out which is the best method in, say, a stand-alone mode before you implement it in TRAC, then it's best to give them some amount of time so that the student can have a learning curve and then actually make a significant contribution.

MEMBER FORD: So that's been factored into your time.

MR. KELLY: You just have to try, yes. You know, it's likewise for the parts, which is Professor Downar at Purdue, has done a very good job for us, and he's very responsive to getting things done. And you just do the best you can. But that's where a lot of the talent is now. The people that worked at the labs ended up at universities for a number of reasons.

MEMBER RANSOM: For something as fundamental as high-order differencing, I'd think you'd want to put it on a five- to ten-year time scale. If you want to look at history and what it's taken, you know that -- I mean this thing's been talked about for 20 years and nobody, to my knowledge, has ever successfully implemented it into a systems code. The Germans have been big fans of that. I'm not sure where the French stood, although they liked most of the characteristics and some other more exotic numerical techniques.

Along that line, do you have a good idea of where CATHARE is today? Are they fairly robust and able to do a lot of these problems or are they still having trouble too?

MR. KELLY: I honestly don't know. I haven't been to a CATHARE meeting in a while.

CHAIRMAN WALLIS: It seems to me, Joe, that if these advanced reactors move up their schedule, so suppose gas-cooled reactors come back, then you may be required to give input much earlier than you're planning to be ready in which case can you hire more people or something? Can something be done to move things up if you need to do so?

MR. KELLY: Within limits.

MR. ROSENTHAL: Let me try a little bit. The advanced reactor budget and the code improvement budget are separate budgets. That's not to say that it's -- the hard thing is finding good people. But we'll fund the advanced reactor work that we have to do, and I think it's actually healthy now that we're going to be using TRAC-M for ESBWR. I think that's a good thing. And that we're doing some TRAC-M work for AP1000 I think is a good thing to incorporate to get the Agency using the tools, et cetera. So if that displaces some of the current development in the net, I don't think that's a bad thing.

We do have to prepare, this came up before, for ACR-700. We do have to prepare for HTGR and building infrastructure, we've written the advanced research plan. And one of the, I think, lessons learned from AP1000 and ESBWR is you just can't start too soon on building the infrastructure. So we know that we have to take these things on.

MR. KELLY: I'm constantly lobbying my

manager to get a couple new people because we've lost a couple people over the last year.

DR. BANERJEE: I wanted to ask you about the droplet field, Joe. I mean in many situations that I know of, you don't have things like annular flow where droplets and liquid film coexist, and I don't think that's your intention in the reflood part of this. Where you've got reflood you've basically got something like inverted annular flow, and then you've got some droplet field above. I wonder if it's worth all that work to put the droplet field in. I mean how well can that be justified?

MR. KELLY: Two things: It's not a lot of work to begin with; second, it gives you some enormous benefits. Obviously, once you get to something like the upper plenum where you want to look at carryover to the hot leg and you have drops sweeping across these structures, some of the drops hitting the structures and falling back down to a pool, you really need to be able to model a pool and a missed flow above the pool. That's very hard with just a simple two-fluid code where you start to jimmy up the interfacial drag to make it think it's part of a pool and partially these drops. You can't --

DR. BANERJEE: But this is a multi-dimensional problem you're talking about.

MR. KELLY: Yes, but remember our nodes are pretty big, okay? So we'd have a pool and drops in the same volume. But aside from that, you're right about if you look at an individual channel, that you have a transition between the two, and then above that the drops could just be the normal liquid field. But what you gain with the droplet field is the capability to have an interfacial area transport equation from the droplets, because you need to bring over a mass source and an interfacial area source at the same time in order to be able to do that. And if you do that, you can then trace the evolution of a drop diameter from where it was created as it evaporates and also as some portion of the drops hit the grids and are shattered.

That's what we were able to do in COBRA-TF. With a two-fluid code, without the droplet field, you're always hearing what is the drop diameter, how would I estimate it? And you end up estimating it based on local fluid conditions, which are not necessarily representative at all of where the drop was actually formed or whatever history that drop has undergone between that point and where you see it up here.

DR. BANERJEE: Right. But that's a separate equation. I mean you can capture that with an interfacial area transport equation. You don't need an additional field in the multi-field model to do that. I can write an interfacial area equation, I can write a single-gap vapor equation, I can still do that.

MR. KELLY: It's very hard to do the transition from this continuous liquid to the droplets and get that interfacial area transport right, especially because you do have a situation where you have the liquid coexisting in two completely different forms.

CHAIRMAN WALLIS: You can get the area right, but the velocities are completely different.

MR. KELLY: Well, he excluded the case of like annular mist.

DR. BANERJEE: Yes. That's a different problem.

MR. KELLY: But that's the other reason that you want it. And my experience, because I was part of the Development Team at Patelle when we went from a two-fluid to droplet. The droplet made it much easier to model the physical phenomena correctly, and rather than being a performance penalty the code actually ran faster. You know, the grind time was a little bit higher but not much, but the time step size went up. And the reason it did was we weren't having to play all these games with interfacial drag on these, trying to make liquid look like films and droplets or pools and droplets.

DR. BANERJEE: The problem you run into is you need a lot more in the way of closure relationships once you --

MR. KELLY: I'll argue that one with you, because if you look at the way either RELAP or TRAC handle something like annular mist, they have all the equations, save for annular film, the constitutive models, they have the same set for the droplets, and then they have a weighting factor between the two, which is totally fictitious. Whereas as with the three-field, you have the same set of constitutive models for the film and you have the same set of constitutive models for the drops. You don't need anything different.

DR. BANERJEE: Now you need an entrainment --

MR. KELLY: Entrainment, right.

CHAIRMAN WALLIS: It's the difference between the two.

MR. KELLY: Right, but you're doing an entrainment rate instead of a fraction of liquid entrained. So the number of constitutive models are the same but their implementation is more straightforward.

CHAIRMAN WALLIS: Let's see you do it.

MR. KELLY: I've done it before, and we'll do it again.

DR. BANERJEE: COBRA-TRAC does it already.

MEMBER RANSOM: So this is a three-field model in the vapor field and two-liquid fields?

MR. KELLY: Yes. And what we did in COBRA-TF was the liquid only had -- the two-liquid fields shared the energy equation, the thing being that the interaction between the film and the drops is large enough that you considered the film and the drops to essentially be at the same temperature.

CHAIRMAN WALLIS: This is okay for things like straight pipes. When you get to bends or Ts, the droplets and the film do completely different things, you get re-entrainment and deposition, all sorts of stuff, and you need then to figure out how to handle those things.

DR. MOODY: Could I just reinforce one thing? The offer or the suggestion was made earlier about maybe involved ACRS and just helping you in some way. You've got some high-powered help at some of the universities, really competent it sounds like, and there's apparently a tremendous depth of passion on these various aspects like bubbles and drops and so on. But perhaps there are a few blind spots that this Committee could assist with if we did see the documentation at some stage, and whatever your plan is for that I'd just like to reinforce that I see these phenomena mentioned here and I know some of us get very wiggly inside when we see that and say, "I know something about that, maybe I can help."

MR. KELLY: Yes. I would like to use this Committee, to some extent, as a peer review. And so when I'm talking about this interim reflood model, what you're going to see, I'm going to come probably next spring sometime and we're going to have a day-long meeting and we're going to go through every model I've proposed, where the model came from and how it performs and give you a chance to give us some feedback.

DR. MOODY: Great.

MR. KELLY: Because like you said, there's a lot of expertise here, and we need to mine that whenever we can.

DR. MOODY: Plus the fact that I think this Committee or one like it will be listening to presentations for years to come that have been run on this program, and at least if we disagree strongly with something, we will be able to say we may be disagreeing with ourself in some way.

MR. KELLY: Yes. I think that's a very good suggestion, and I intend to do it.

CHAIRMAN WALLIS: We've got to keep moving.

MR. KELLY: Yes. I'm going to really hit these next three slides very quickly. This was my crystal ball. Now, I showed a development plan, and there were some boxes that said BWR improvements, PWR small-break improvements and large-break improvements. But based upon what I know about the codes as they are today, where do I think we have deficiencies in modeling, and that's this list for BWRs. And you'll note the modern fuel design, the thing that we're actually already doing, is the result of the user need. Small-break LOCA --

CHAIRMAN WALLIS: They're for spacers and all that sort of thing. Spacers have an effect.

MR. KELLY: And grid spacers are a very large impact and that's subsumed into this. Now, actually, in 1984, I worked with Larry Hochreiter at the end of the FLECHT SEASET program where we used COBRA-TF to analyze the FLECHT SEASET with blockages. And so we put in grid spacer models, both for grid rewet, droplet breakup, et cetera, and those models would be the first step for what we would put into TRAC.

As you know, we have four different experimental programs, and we're going to take -- these are all targeted, basically, to a known code deficiency, typically, something that came up during AP600. That's with the exception of the rod bundle, and that was something different. So we targeted these to a known code deficiency, and we are going to put them in, and we'll be starting that in January.

CHAIRMAN WALLIS: Our advice for you was, again, have the code developers work more closely with these codes, particularly John Mahaffy at PSU. You should be working with Hochreiter to see is Hochreiter generating the kind of stuff that needs to go in whatever, the assessment of TRAC. Is TRAC going to have a model which can be assessed with that kind of data, and so on? Put the two together, don't just do a lot of experiments and then two years later someone unearths them and says, "Well, how do we get something useful out of them we can put in TRAC?"

MR. KELLY: Yes. Actually, that's Steve's and my job, and that's what we're trying to do. As I said, we both serve as technical monitors in the Penn State Reflood Program, and Steve is a technical monitor here. We've both been out to UCLA.

CHAIRMAN WALLIS: Yes, but that's part of their work description, job description is to actually get something which can be used in TRAC.

MR. KELLY: Sometimes they do as part of the task.

CHAIRMAN WALLIS: Maybe we should emphasize that.

MR. KELLY: It gets difficult times, like if you have a very good experimenter and his students since they know how to build design, they know the facility and the instrumentation, but they don't know anything about TRAC.

CHAIRMAN WALLIS: Then they shouldn't be doing their Ph.D.

(Laughter.)

MR. KELLY: Well, they can learn what a two-fluid code needs, but going in and having to learn the coding of TRAC is something different. It's gotten better, but --

CHAIRMAN WALLIS: But just running an experiment and not thinking about it is not good enough.

DR. BANERJEE: One concern there is with the OSU face separation, when we heard that it seemed that that really was the least integrated into TRAC, at least the first impression we had, and that a lot of the detailed data that we would have liked, like the slot frequencies and things, were not being measured. I'm just recalling this. And the sort of correlations which were being developed did not seem defensible, and I think that's part of the record and you can look at it. But that was the program which was the least well-integrated.

MR. KELLY: And I think Steve -- Steve has -- now that Steve is on board, he's now become technical monitor for this, and Steve is trying to address those concerns and direct their efforts to make sure that we get what we will need. And I'm sure at another time in a few months he can come back and actually show you what we've put in the code and how well it does or does not work.

DR. BANERJEE: Okay.

MEMBER RANSOM: I'd like to voice a question there too, because I know from experience that the academics just look at these system codes with disdain. You know, they hate to get involved with them. They love to create their own models and their own little computer codes with that, so it's really going to take some pressure from you folks, I think, to tell them that this is the way it has to be.

MR. KELLY: And to some extent, that's why some of the model development is being done in-house. That's one of the reasons for it. One of the other reasons is to create the expertise here.

MEMBER RANSOM: Sure.

MR. KELLY: But you're right, we do have to get closer to the experiments in order to get the value out of them. But that's what we're trying to do.

MEMBER RANSOM: Even planning their experiments, oftentimes you find the experiment is planned in such a way that the data you would get out of it there's basically no way to use that level of detail on a systems code, so you need to be thinking from the start in this framework; otherwise, the data may be useless.

MR. KELLY: Well, that's very well taken.

MEMBER RANSOM: There are many examples of that through the history of this program, you know, where experiments have been run with the data basically never being used.

MR. KELLY: The new reg goes on a shelf and that's that. We're trying not to let that happen here. And the reason it won't happen -- I mean I can't say we'll never mess up, but the reason the situation is going to be better is because now you have some of the staff doing some of the technical work rather than just managing the projects. That's a big difference from the past.

MEMBER RANSOM: Well, I'll use Ishii for an example. After working with him for quite a few years, I know he's always hated these system codes, but finally he started with some of his students using them and found that, well, it's not so bad. And now I think he's actually operating in a more integrated fashion. And the same way with Larry. I mean he's using TRAC -- I mean not TRAC but COBRA-TF because that's something he knew, and so it's easy for him to think in that framework. But from the NRC's point of view, they have to start to thinking in terms of your framework.

MR. KELLY: Yes. We would have forced Professor Hochreiter to use TRAC-M except that the reflow capability in TRAC-M at that time was so poor, and so therefore we let him use COBRA-TF for his pre-test predictions because it would give a better answer. In the future, that won't be the case.

CHAIRMAN WALLIS: So then the summary is essentially what you've already told us.

MR. KELLY: Right.

CHAIRMAN WALLIS: Should we take a break now, Joe --

MR. KELLY: Yes.

CHAIRMAN WALLIS: -- or do you want to emphasize anything more?

MR. KELLY: Just at the end of 2003 we'll have the public release of the consolidated code and that the long-term code development in the experimental programs are going to be driven by the assessment results as well as user needs, and user needs will be the new type of reactors. But it will be doing the assessment, and that's where we really want to spend some effort over the next few years, not just doing code development in a vacuum but assessing it against a wide range of types of experiments, from small fundamental experiments to the larger integral experiments, finding where the code has problems, using that to identify where we spend our resources, both for model development and also experimentation.

CHAIRMAN WALLIS: Okay. We'll take a break -- we will take a break till 10:15.

(Whereupon, the foregoing matter went off the record at 10:04 a.m. and went back on the record at 10:17 a.m.)

CHAIRMAN WALLIS: Let's start again. I just wonder if any of the members had questions which I cut off at the break that they want to ask now as we proceed?

DR. BANERJEE: No, I was just going to say we want to see Ishii's work soon.

CHAIRMAN WALLIS: We haven't heard about that for a long time.

DR. BANERJEE: Yes. Down the primrose path of wherever we're going.

MR. BAJOREK: Good morning. My name is Steve Bajorek. I'm from the Office of Research. What I'd like to talk about is the status and where we're at in our developmental assessment. If you noted in, I think, the third to last overhead that Joe had up there, we had two different assessment matrices that we're going to be dealing with, and, actually, I think what I should do before the end of the day, or I can e-mail it to the new people on the Committee, what all those specific tests are.

What I'm talking about is the first one where we just had a developmental assessment matrix, which we are calling the code consolidation matrix versus something else that we called a PIRT DA matrix. The difference between those is the intensity and the number of tests that go into some of the specific phenomena. For the code consolidation part of this, our interest is just showing that TRAC-M is giving you about the same results as RELAP and the other TRAC codes. When we get to the PIRT-based assessment, as we called it, we went to PIRTs for BWR, PWR, large break and small break and said that, hey, some of these phenomena we have to really study in depth so the number of cases on, as I say, critical flow, some of the reflood heat transfer levels will have the increase in that matrix compared to what we want to do just to show that the code has been successfully consolidated.

What I'd like to cover this morning and go over briefly is summarize the work that we have ongoing and give you some typical results where we've been able to take RELAP, TRAC-B, TRAC-M for a test and show the comparative agreement between the three codes and test data, let you know what work we have in progress, and, actually, a better phrase for that is work that we have just started in order to try to address some of concerns for BWRs, and then point out the cases that we're going to be working on in the first part of 2003 to hopefully complete the consolidation and really set the stage then for us to start improving models.

As we've mentioned, the purpose of the code consolidation DA is really to demonstrate that TRAC-M is giving us what the other codes could produce. At this point, we try to make some comparisons to data, but this is really a code-to-code comparison exercise. However, as we go through this exercise, what we've been doing is we've been setting up scripts so that as we change the code and after we've already extracted that we want to make comparisons to, we can do this automatically and it will make it much, much faster the next time around when maybe the comparison will be an existing version of TRAC-M to one with a model change in it to the data, as opposed to bringing in RELAP and TRAC-B into the mix.

Now, we had a fairly late start in getting going on this code consolidated developmental assessment work this year. We've had some problems in getting SNAP moving, we don't have interim reflood model yet, so a lot of our work to date has been focused on unheated, relatively small-scale separate effects tests, things that we could regenerate a TRAC-M input deck by hand as opposed to relying on SNAP to take the RELAP deck, crank it through and get the equivalent in a TRAC-M format.

DR. MOODY: I should have asked Joe, but what is SNAP, what is that acronym again?

MR. BAJOREK: Symbolic Nuclear Analysis Program Package.

DR. MOODY: Thank you.

MR. BAJOREK: And what it is it's a convenience tool that allows you to take flow areas, volumes, dimensions and put them into a TRAC-M or RELAP type of format. Ideally, you'd like to be able to take the RELAP input deck, send it through SNAP and come out with TRAC-M. That isn't working at this point, and that's what's caused some of the delays.

Now, the tests that we have been working on are shown here. Since we think that the blowdown heat transfer heat transfer package may not change considerably, we went ahead and we've done some of the work looking at the Oak Ridge THTF tests. We've got a case looking at the FRIGG subcooled boiling, a simple tube model with a phase separation in the CISE single-tube test.

One of the problems, or at least one of the things that we would be very much concerned about, is that since TRAC has been used primarily as a large-break tool, is what is it's performance going to be as we start to extend it into small-break applications? So we've been paying particular attention to a set of Oak Ridge THTF level swell tests to try to see how is TRAC-M going to compare to tests that you would think that RELAP would do very well. We've got some other tests on a large scale at lower pressure.

As we start to look at AP1000, ESBWR at some of the advanced plants, the basic idea that all of them have is to depressurize very rapidly to something near containment pressure to allow another large volume of water to be able to gravity-feed into the reactor. Well, getting a level swell right at high pressure is one thing, but getting it right when you have low pressure tends to be more difficult for a code. So we're working in a set of THETIS boil-off tests.

We're looking at the critical flow model, and we've done some preliminary work in running some of the UPTF by pass tests. We ran those over the summer, and we have additional cases that we're going to use next year. And we've run SCTF Test 719. I'll show you some of those results, but we haven't made a whole lot of progress, and we haven't put a lot of emphasis on a test like this, because what we're doing we want to wait for the interim reflood model. We know that we're not going to get good results --

CHAIRMAN WALLIS: Do any of these tests consider entrainment from a boiling pool or a swelling pool? Is there entrainment from the surface?

MR. BAJOREK: THETIS has some, there's a very small amount in the Oak Ridge THTF tests, SCTF would have some, but the best place for looking at that, I think, is in the FLECHT SEASET, FLECHT skewed power test. Those are on the schedule, but those won't be happening until the interim reflood model is complete in early part of 2003.

Most of these go through and make a comparison between TRAC-M, the data, and one other code. Let me show you some of the results that we've been getting for the Oak Ridge level swell tests. These tests, the tests themselves were run in an eight by eight bundle, full height that was -- they were run in several different modes. There's a lot of -- it's a nice test to simulate because once you get an input deck setup for this, you can vary it from the blowdown to a level swell to some of the small-break reflood tests that were run there.

In this series of tests, they tried to freeze the quench front by controlling the flow into the bundle so it will reach more or less a steady state and you could get a steady state void fraction distribution of the bundle. This shows the results for one of the tests, and what I'm comparing on this is TRAC-B, which has the squares, RELAP, the round circles, the experimental data shown by the triangles, and TRAC-M with the diamonds. And I'm getting this type of a comparison. Our conclusion from this is that TRAC-M is doing about the same as the other tests at this point. In this case, it looks like it's doing a better job at picking out what I might call the two-phase mixture level than opposed to RELAP, but that's not true for all of the cases.

CHAIRMAN WALLIS: Now, can you make it simulate RELAP or is TRAC-M always going to be itself --

MR. BAJOREK: It's always going to be itself.

CHAIRMAN WALLIS: -- and somewhat different from all other codes?

MR. BAJOREK: Yes. Not all the tests come out that good in comparison to the data. In this case, we see that TRAC-M tends to overpredict the void fraction, underpredicting the total collapse liquid level in the --

CHAIRMAN WALLIS: So what did you do to make TRAC-M so different from TRAC-B in this case? I would have thought they would have been close since they're both TRACs, only one's derived as a consolidation of the other codes.

MR. KELLY: Steve, would you like for me to answer that?

MR. BAJOREK: Yes, go ahead, because I'm not --

MR. KELLY: They're derived from the same code, but the interfacial drag packages in the two codes are completely different. So TRAC has one based upon small bubbles and bubbles with the size of hydraulic diameter and fresh rim between the two as you go from bubble slug. Whereas TRAC-B, and this is one of the improvements made for BWRS, is it basically takes a drift flux correlation and converts it into an interfacial drag coefficient.

CHAIRMAN WALLIS: So which one should we -- there's no real measure of excellence here. They're both different from the data in different ways.

MR. BAJOREK: That's another one and part of my conclusions here is one of the things that we need to do early in 2003 is to define a measure of goodness for each one of these comparisons in terms of the scatter plot or a comparison to -- you know, in terms of a bias and uncertainty. How would we be doing that for these set of tests will be looking at the two-phase level as predicted in each one of these, how close that comes to the data, the collapse level, which we have, and we can also go and make a comparison at the locations where void fractions were related in order to get a bias and uncertainty at each of these various locations for each of these parameters to try to put a numerical value on how good the code is doing.

I'll skip a couple of the Oak Ridge comparisons on there. My point with those is in some cases TRAC-M is doing probably as good a job as any of the other codes; in some cases, there is a need for model improvement. This is for one of the FRIGG tests where the liquid was entering subcooled to the bundle, and boiling would not start until roughly a meter above the inlet. Here, when we make a comparison to TRAC-B, RELAP and TRAC-M, essentially, we are doing a fairly decent job of following the experimental data.

But we do see one out here, a little bit of an outlier. This is one that had been run with TRAC-B, and this was a summary of the sensitivity studies that were being done with this one, because there were some problems in trying to make sure somebody else's TRAC-B deck was really the same as your TRAC-M or your RELAP deck. We want to make sure we get all the volumes and all the areas correct so it was a fair comparison between each of the codes, try to get the input.

MEMBER RANSOM: What pressure was this at?

MR. BAJOREK: I can't remember.

(Off-mic comment.)

MR. BAJOREK: Yes, I think most of them were fairly high.

(Off-mic comment.)

MEMBER RANSOM: One thing that would be interesting is to see the need for the subcooled boiling research or model development that's going on, which I guess is driven by low pressure?

MR. BAJOREK: Yes. Yes.

MEMBER RANSOM: But I've never seen the data on it. It would be interesting to see the motivation for that.

MR. BAJOREK: Okay.

MEMBER KRESS: When you have a test like this with difference in pressure gauges and then you're going to model the thing with say TRAC-M, which has nodes in it where you get a void fraction in a node, how do you relate the nodes to the differential pressures? Do you sort of draw a line through or do you actually calculate pressure and say what would the pressure have been here and here and compare it with the differential pressures?

MR. BAJOREK: The way I've normally done it is I'll look at two points where I'm getting a void fraction, and if I wanted to get a comparison to a DP cell where it's tap may have been in the middle, I'll average those void fractures or I'll do a linear interpolation between what the code nodalization is and what the actual location in the test was.

MEMBER KRESS: Another way to have done that is to actually calculate the differential pressure.

MR. BAJOREK: Yes.

MEMBER KRESS: And compare it with what --

CHAIRMAN WALLIS: If you look at TRAC-M out from here, it looks rather strange, that the high void fraction seems to be stepping up the staircase.

MEMBER KRESS: Yes. Something seems to be going wrong.

CHAIRMAN WALLIS: Is that something to do with the nodalization or is it something to do with the physical model?

MEMBER KRESS: That's actually why I asked because I worry about that.

CHAIRMAN WALLIS: It's doing something which is not physical and it doesn't seem that that's the way it's going to actually be. The other codes don't do that.

MEMBER RANSOM: Are you talking about the oscillation?

MEMBER KRESS: Yes. It looks a little strange. That's why I wondered how you actually did the --

MEMBER RANSOM: That's why it would be interesting to see some phenomenological tests with TRAC-M where they're kind of pure situations and make sure that the code behavior passes those tests. Then apply it to the data experiment.

MR. BAJOREK: When we get to putting in the UCLA models, we'd like to make use of the FRIGG data, we'd like to make use of the UCLA rod bundle, but also just a simple tube with a heat flux where you can go on a piece of paper and calculate your quality and temperatures along the way .

MEMBER RANSOM: Well, even simpler than that. I'm talking about these like the manometer Joe showed this morning, and there are a whole host of problems like that that just say, okay, it looks like phenomenologically it's behaving correctly, and now let's move on to data comparisons.

MR. BAJOREK: Let me get you our full test matrix. I hadn't planned on walking through that today because we had gone through that last year, but right off the bat we have a series of about ten what I like to consider thought problems. They're ones which physically don't have a whole lot of relevance to some of the phenomena that's going on in a plant, but they really help you understand whether the code is conserving mass, momentum and energy.

As we're getting close to completing the work on SNAP, we're starting another series of assessments which have been driven primarily for the need to get the code ready to do ESBWR. So some of the tests which have been towards the end of our, at least our priorities in terms of the developmental assessment matrix, we've moved up. Those being FIX, ROSA, FIST, and we're also starting to get LOFT ready. We want to do some small-break tests with LOFT in addition to some of the large-break tests.

CHAIRMAN WALLIS: Now ESBWR has a chimney that drives the natural convection, and it's not clear what the flow regimes will be in there, if the bubbles will have gone right into some swells or big bubbles. I'm not sure you have a very good basis for knowing just what happens in that chimney. It's a big scale and it's a chemical reaction --

MR. BAJOREK: This is a shorter, wider core in it.

CHAIRMAN WALLIS: -- that tends to get non-one dimensional phenomena where the bubbles squirt up one side or something.

DR. BANERJEE: I think they're putting sort of --

CHAIRMAN WALLIS: Are they going to put some guides in there? I just wondered if we have a good database for evaluating that.

MR. BAJOREK: Not for that specific effect, no.

CHAIRMAN WALLIS: Or for natural convection in a large, really large chamber.

MR. BAJOREK: It does tell me that when we model a plant like that, I think it's going to be particularly important to isolate and be able to model a hot assembly and get the radial power distributions.

Work that we have upcoming, for large break we want to continue the work on UPTF. We're also adding in some additional tests because we don't want to be overly large break-centric in what goes on. We want to look at a test like UPTF Test 25 where you would be looking at perhaps long-term cooling or events very late in reflood or perhaps like you'd see in an intermediate array. But for small-break, where bypass now becomes water being swept away from the top of the downcomer as opposed to the sweeping out of the lower plenum, a prevention of SI from reaching the bottom of the downcomer, as you see in Test 6 and Test 7.

We started this but we've also put a little bit on hold, and we're working with trying to get an agreement with the Korean Ministry of Science and Technology. They would like to send someone over here to work with us for at least a year. They're particularly interested in bypass phenomena probably because of its behavior in the CE system AD Plus Plant where they have an injection port above the cold leg. And the concern is will that sweep out more water than what might have been predicted from UPTF type tests, even with the direct vessel injection in its location.

They've run a series of tests they've called MIDAS where they've put the ports above the cold leg and have studied bypass phenomena in a smaller-scale facility. So we've sort of stopped going on here in anticipation that we're going to have an analyst from Korea to pick up this work over the course of 2003.

As soon as we get SNAP to the point where it can take a RELAP deck and generate a TRAC-M deck, we want to get going very quickly on the small-break and long-term cooling type tests which are important to AP600, AP1000. Those would be the SPES, the small-break LOCA tests, the tests that had been run in the APEX facility for AP600. I'm particularly interested in running these tests, what we call the "no reserve" or the beyond-design-basis tests, primarily because those that have some conditions, some tests that help us to understand upper plenum entrainment phenomena better than what we would from a typical integral test where everything is going on at once. And we'd also want to start getting the ROSA-IV deck up to speed and running some of those small-break tests.

As Joe had pointed out, we're working on to get to an interim reflood model. That's why in the work that we've done to date we haven't made a tremendous amount of progress on things like FLECHT SEASET, modeling the RBHT or the SCTF or CCTF cases.

MEMBER FORD: Steve, you say depending on available resources. What's the risk if you don't have the resources in terms of your capability to assess some of these pre-applications for advance reactors and also for the AP1000?

MR. BAJOREK: Well, I think the risk is if we don't get the resources to get the right models in the code, feel confident that we've put them in there correctly, and that takes a while, and run a very wide spectrum of tests, we're not going to be able to go to NRR and say, "You have a tool by which you can audit the --

MEMBER FORD: So does that mean that the whole advance reactor commercialization stops?

MR. BAJOREK: No, because NRR, I believe, would say, "We can make our judgment on the safety of that plant by just looking at what the vendor gives us."

MEMBER FORD: So in other words, you're not an informed reviewer in that case.

MR. BAJOREK: I agree with you. I think

that's the risk. I think that people who have to perform the review to a certain schedule would say, "I'll just have to take what I see from the vendor and make my decision on that." Whereas I think a better decision can be made is if you can take an independent tool and do your own individual, independent calculations of their tests in those plants itself. That's why we've been trying hard to move up the BWR assessment. The reflood model development that we would do with Penn State, that's been pushed out in order to accommodate that. That's clearly a resource problem, because the same people who are going to put non-condensible models in the codes are the same ones that are going to be hooking up new grid models based on the RBHT data. So if you compare what we had presented today in terms of development to last year, you'll see this mixing or moving up of BWR activities at the expense of things like RBHT and some of the assessments.

So at this point, with regards to the assessment, we feel that we've started a significant number of cases. We're getting pretty much like we would expect, because we have a different package in TRAC-M. Apart from what's in RELAP or TRAC-B or TRAC-P, we don't expect the results to come right on top of one of the other codes. But in the cases we've looked at, it seems as though we're about as close to the data, in general, as those other codes have been. And I tried to show you an example for a level swell, a case where if this code falls down, that's a place where we'd sort of expect it because it really hasn't been used in that capacity previously.

Now, I think an important step before we start doing much in terms of the model improvement is now is the time for us to start thinking of what do we mean by code accuracy? We're going to go through, for example, with the level swells, and we're going to develop a bias and uncertainty for collapse level, for two-phase level, for the individual void fraction measurements, and possibly another scheme that I've used before in this series is, okay, what multiplier would it take to correct your prediction to bring you in line with the data? And if we conclude that the reason those TRAC-M calculations were off because of interfacial drag, we can go in, put an interfacial drag multiplier on this, see if that really and truly brings us back to the data, and if we got the right model, then we have a distribution of multipliers that need to be accounted for with the larger-scale tests.

This is something that we need to start

working on fairly soon so we can develop these for TRAC-M today and hopefully a year from now we can say, ah, the bias has improved this much as we replace the subcooled boiling model, the T-phase separation model or the reflood heat transfer model.

In addition, in 2003, we're going to start focusing more on the integral effects tests. In a way, this is probably a better way of getting a nice code-to-code comparison because we'll have lots of processes going on at once. But we would be looking at OSU, ROSA, SPES, possibly BETHSY, some of these larger-scale tests in 2003. But, again, that's sort of a resource issue as well. If we don't have SNAP functioning the way we were hoping to, that means we're going to have to put together this TRAC-M deck almost the old-fashioned way. It's a big help to have the RELAP deck to give me the processed areas and volumes and flow diameters and use SNAP to produce the TRAC-M model, but it's not the nice clean-cut send in a RELAP and get a TRAC model out that we had been hoping for.

MEMBER RANSOM: Steve, let me ask you a question on that. We heard earlier that the SNAP work is being put up for bid now or rebid. Is that a result of unhappiness with the present contractor or what's the reason for it?

MR. ROSENTHAL: No, not at all -- sorry, Jack Rosenthal, Safety Margins and Systems Analysis Branch, RESA, excuse me for interrupting this evening. I think I feel more comfortable with the money, and right now we're right in the middle of several commercial bids. We have several contracts that just came to an end and we had to put out new commercial bids, and we have to bid within a competitive process. The fact that we've been on continuing resolution in Congress has impacted our ability to place funds at what turns out to be a critical time, and we just have to live with it.

MEMBER RANSOM: All right.

MR. BAJOREK: I think the problem with SNAP is it was a very ambitious undertaking.

MEMBER RANSOM: Which?

MR. BAJOREK: The development of SNAP. We have to take all of the RELAP decks, send them through and produce a TRAC-M deck.

MEMBER RANSOM: Well, the whole TRAC-M project was pretty ambitious. Kind of like fusion, you know, it's, what, a 20-year project that's in its 40th year.

DR. BANERJEE: As long as it's not like

cold fusion.

(Laughter.)

MR. BAJOREK: Another part of the code consolidation work has also been the work using the phase separation data at ATLATS and the UCLA subcooled boiling. We heard you when you guys said in June and July, I guess it was, "You need to integrate your code work with the experimental work." We agree 100 percent on that. It hasn't been scheduled that way, we're trying to move that up. It still remains a bit of a resource problem to try to cover some of our other areas.

MEMBER RANSOM: Remind me, ATLATS is the Penn State facility, is that right?

MR. BAJOREK: I'm sorry. ATLATS is the facility at Oregon State --

MEMBER RANSOM: Oh, Oregon State.

MR. BAJOREK: -- that's being used to develop models for entrainment and carryover to a relatively large-sized branch line.

MEMBER RANSOM: Got it.

MR. BAJOREK: We know what the facility looks like, we have a number of tests, we've got questions on the old models, we still have questions on the new models. But to get started on this, we're setting up a TRAC-M model of the facility, we're going to simulate it with both TRAC-M and RELAP. Not that we're real particularly interested in adding new models to RELAP, but we think at this point when we do AP1000 audit calculations, we want to get a better model in the RELAP, because we don't think TRAC-M is going to be up to snuff, okay, in the right time frame. So we intend to get facility models of ATLATS, make simulations with TRAC-M and RELAP. Both the models are identical at this --

CHAIRMAN WALLIS: What happens to RELAP when TRAC-M is really operational and used a lot, do you stop maintaining RELAP or what happens to it?

MR. BAJOREK: No. We intend to maintain RELAP for sometime into the future. We think that RELAP is still a tool that a lot of people are going to use, including the staff.

CHAIRMAN WALLIS: You wouldn't maintain TRAC-B and so on.

MR. BAJOREK: No. But one of the big differences is when we come up with new models for grid models or new reflood models, we're going to put those to the TRAC-M. We aren't going to try to put them in both RELAP and TRAC-M. That's why I say this one may be the exception just because of where we're at in TRAC-M development. We want to make sure that we can make a good estimate on what this higher carry over in the hot leg does to AP1000. So we're going to try to do both TRAC and RELAP at this point.

But as we go to other tests, including the UCLA work, those models are going to go right into TRAC-M, they won't go into RELAP. And that's the other project that we did start I guess it was around the September time frame. We've been working with UCLA to develop sub-routines that we can take and put right into TRAC-M to replace the subcooled boiling model that's in there now. We've iterated with them on, "Hey, here's what the code can give you, here's what we expect to get back out from the sub-routines," and we haven't had a problem with that. There's nothing new that the code can't handle, and there's nothing that we have to supply to these calculations that the code isn't already using in some capacity.

So I just wanted to let you know that outside of the code consolidation we are starting to take advantage of these experimental programs, and I think if you look on Joe's overall schedule, we intended to start that about now and hopefully we'll get these models functional, understand them through the developmental assessment and in the REV, I guess

0.0 release at the end of 2003, TRAC-M would have both of these available to the user.

CHAIRMAN WALLIS: Thanks very much, Steve. We'll move back to Joe Kelly. This looks like a fat package. Ends up at Page 78.

DR. BANERJEE: Seventy-five?

MR. KELLY: You just handed out the first package. Do you want to hand out the second one?

CHAIRMAN WALLIS: Jose had 120, I think, yesterday. We're having a real contest in getting through a large number of transparencies or slides.

MR. KELLY: I know how to work the mouse, I just don't know how to turn it on. I just saw a green light flash. It flashed but then it's back off. Technology's great when it works. Ah, I saw a glimmer. Thanks, Paul.

CHAIRMAN WALLIS: It's now warming up.

MR. BOEHNERT: It's warming up. It's coming up.

MR. KELLY: Okay. A good part of this presentation I actually gave to this Subcommittee about four and a half years ago at the beginning of the RBHT Program.

CHAIRMAN WALLIS: There's been no progress since then?

MR. KELLY: Well, this Committee has changed substantially since that point in time, so I thought I would repeat some of it. But in the interest of time, a lot of the first 20 or so slides I'm going to skip through very quickly and not argue about the rationale for the program but just hit a few highlights.

What I was going to basically talk about is what the program is, why we're doing it, very brief description of the current reflood model that's in TRAC, show you some of the results that are the reason that we are ditching that model and part of the reason for doing the RBHT test, and then talk about how we're going to use the data from the RBHT facility to develop the models. I'll skip this.

When I talked about the program, it was really two things: A model improvement effort and an experimental program. And this effort actually started at the same time as our RBHT Program, but it was interrupted by things like me leaving the NRC and also me getting a lot of other assignments. So we've done some work here but then you know about the test facility.

The one thing I'd point out here is the intent was to have a small-scale reflood and blowdown rod bundle test facility, and so what we were doing initially was all of the piping at the RBHT facility was going to be designed for high pressure, and a lot of it is, so that later on we could go in and do, say, a blowdown rewet test there. But as cost escalated, some of the components, like some of the tanks, are not sized for high pressure, but some of the loop is. Obviously, the bundle housing isn't, but we knew that.

I will go through this. We've planned a number of different types of tests in the facility. First, obviously, you do the bundle characterization once you build it. Single-phase, steady state flow to get the bundle and grid spacer pressure drops. That's been done. I also wanted a series of tests with steady state two-phase flow, and the point here is to measure the void fraction, again using DP cells, normal flow regimes, talking bubble, bubbly slug, churn turbulence, in order to get a database at low pressure, low flow, decayed heat levels for the passive plants. Well, that was not done as part of the bundle characterization. We're going to be doing it during calendar year '03. They did measure bundle heat loss, which we'll be using when we simulate the test. That's something that's needed there. They also did radiation tests with the evacuated bundle, and you can use that as part of the assessments of your BWR channel radiation model, structure radiation.

Starting later in 2004, I guess now, are the steam and mist cooling tests, and these are really unique to the facility and one of the big reasons for doing it. There's nothing really major about single-phase steam cooling in a rod bundle except that there's not a whole lot of data out there, and the data is widely scattered. So the idea here is to look at turbulent and mixed convection, because we're in a Reynolds number range like starting down as low as 2,000 up to about 20,000, but also look at the grid spacer enhancement in single-phase conditions to be able to use this as part of a baseline for what you then see when you're two-phase.

Then at the same steam flow rates inject droplets near the bottom of the bundle in each sub-channel with a known droplet mass flux and size distribution, because we've designed these injectors and tested them previously. We can use that to look at two-phase enhancement of the convected heat transfer. This is the kind of thing like if you have particle gas flows where the particles can increase the heat transfer, except with drops it's a little more complicated process.

CHAIRMAN WALLIS: These are going to be with very hot rods so that you can get into Leidenfrost-type --

MR. KELLY: Yes. You'll be beyond that.

CHAIRMAN WALLIS: Or Forslund-Rohsenow or whatever?

MR. KELLY: Yes. And we'll talk about that some more later.

MEMBER RANSOM: I have a question relative to that. How do you plan to use that in the code? That's sort of an artificial situation you create in the experiment, and I'm wondering how do you use that to help you with the code model?

MR. KELLY: Well, when you're developing a model for this first flow from boiling and all you have are, say, reflood data, you're always going, well, what is the vapor flow rate, what is the droplet flow rate, what is the droplet diameter when you're trying to make judgments about which model to use. Here we know those things.

MEMBER RANSOM: Are you going to put that into the code as an input condition in a way, like a boundary condition, and then some way say, okay, do my heat transfer correlations predict the correct behavior with this situation?

MR. KELLY: Well, yes and no. You do it both ways. You'll have detailed local condition data here.

MEMBER RANSOM: Right, but you created an artificial condition, and I'm wondering how do you relate that to reality?

MR. KELLY: Well, two ways. One is you know the local conditions now, because you made them easy, okay? You can take those local conditions and use them to judge how good correlations you find in the literature are for those local conditions. So they can make a difference in which model you select. But then you can also use it in code validation, and that's where we would do exactly what you're saying, we would set up and run the test exactly the way the test is run, injecting the liquid in droplet forms.

MEMBER KRESS: Are you going to have multiple grids and series in these tests --

MR. KELLY: Yes.

MEMBER KRESS: -- so that the drops actually do change as they go through the grids?

MR. KELLY: Yes.

MEMBER KRESS: So then you'll have to recharacterize the droplets after each grid?

MR. KELLY: Well, what you find, and this is based more on my experience with the FLECHT SEASET Program, the Sauter mean diameter of the total droplet population only changes a small amount as it goes past each grid, they're not large changes. What you do see is some fraction of the drops have hit the grid, and typically the fraction of the drop that's within the projected of the area of the grid is shattered into microdrops, and those microdrops evaporate very rapidly just downstream of the grid, and that's what provides the superheating of the vapor.

MEMBER KRESS: Yes. And that gives you some clues as to how to model it.

MR. KELLY: Right. And we did this once before in 1984 with COBRA-TF and FLECHT SEASET, and it's the lessons that we learned in doing that work that have helped define some of this.

MEMBER KRESS: Yes. I think that's a reasonable view of what happens.

MR. KELLY: So this is what we can get from these tests: Information on the convective two-phase enhancement, and I'll describe what that is more later, we'll get some information on the interfacial heat transfer, and this is superheated steam to droplets, because we'll see the axially evolution of the vapor temperature, again, given that we know what the droplet flow rate is. And we'll also be able to look at the grid spacer effect under two-phase conditions. Under two-phase conditions, there's really two. One is if the grid is wet. Imagine a liquid film completely covering a grid that's about two inches long and covers the middle of each sub-channel. That's an awful lot of surface area. And if you then blow superheated steam past this wet surface area, you get a pretty large heat transfer coefficient times a pretty large area. So that's a real good sink of heat from the vapor.

The other way is the process I described before, which is droplet shattering. Droplet shattering becomes more important when the grids are dry. Whereas when the grids are wet that's such a good heat sink already the droplet shattering becomes secondary. In the tests that we've run to date, we've kept the peak clad temperatures down to about -- to about 1800 F is the maximum that's been run. Under those conditions, for most of our tests, the grids rewet very quickly and stay wet. That's part of the reason the grid effect you see in RBHT is so large is that the grids are wet. If you want to go to best estimate plus two sigma, 95th percentile type calculations where you can have up to 2000.100 F, then that's non-prototypic. Under those kind of conditions, the grids would be dry, and that's what we saw in FLECHT SEASET, and that's why the second series of reflood tests in the RBHT we're going to bump the rod temperatures up high enough to get those grids dry, and then we'll be able to compare behavior with wet grids versus dry grids.

I already said something about the forced reflood test, but what we did when we designed the test matrix is kind of split it into two parts. One part we wanted to look at what happens in what we'll call froth region or inverted annual are boiling. And so we tried to do a parametric. Now, in the bundle, starting at around 48 inches, we started having a fairly fine mesh of DP cells, because the void fraction is very important for this regime.

And so what I did was pick the point in the transient when the quench front would be up into that and then vary the subcooling at that elevation. So that would be the parametric is changing the subcooling at, say, 53 inches. For dispersed flow film boiling what's more important is the void fraction of the quench front. So, again, do a parametric on pressure and mass flows of a quench front void fraction, again, at that level where we have the DP cells.

CHAIRMAN WALLIS: We talked with Hochreiter about whether or not DP was a good measure of alpha.

MR. KELLY: It depends on where you are. If there's a lot of water around, like say if you're in bubbly, bubbly slug under these kind of low flow rate conditions --

CHAIRMAN WALLIS: At low velocities, at low velocities.

MR. KELLY: -- at low velocities, there's no problem whatsoever. And the highest flow rates we go up to are six inches a second. That's about 150 kilograms per meter squared per second, so that's very low.

CHAIRMAN WALLIS: But when you're at high alpha and you're looking for the whole number of drops, it's not so clear you can do that.

MR. KELLY: The way I look at it if there's a grid spacer in your Delta P span, then you really have to look carefully because the pressure drop across that grid is very large. So let's set those aside. You would still have then say dispersed flow conditions, about plus or minus five percent void fraction, not five percent of the void but five percent just because your frictional pressure drop is there.

CHAIRMAN WALLIS: Presumably, you accelerate the drops downstream of the grid too. There's other components that --

MR. KELLY: Right. So anything above 90 percent void fracture don't believe DP cells, just throw that away, just say it may be an indication but it's probably more an indication of the frictional pressure drops. But between zero and, say, 80, they're pretty good, but once you get higher than about 80, especially if there's a grid spacer around, DP cells are not that great.

DR. BANERJEE: Do we have any direct evidence of their performance like against densitometers? I mean one of the issues is that in regions where you get rapid vaporization, you've got very high acceleration of the pressure drops. So it's not very convincing unless you have some other way to corroborate this.

MR. KELLY: Well, the rapid vaporization which appears is actually pretty small compared to what you're really talking about. You can do hand calculations on what the acceleration losses are, and they're pretty small for this. That's been done before. We did look at putting a --

DR. BANERJEE: So do you have anything written up on this or does Hochreiter have something?

MR. KELLY: I've seen it before. I don't know if he did it for the RBHT. I've seen it as part of other experimental programs. We did look into using a gamma-densitometer, and the one that they wanted to use was a low-energy one that had been part of another NRC program, but there were some fairly substantial costs associated with getting it and getting it back to working. But what was worse, because it was -- they wanted low energy so they didn't have to worry about radiation shielding and so on. The real problem was that the low energy one needed a special window because it can't see through metal, so it can only see down through the gaps.

And, apparently, the -- for some reason, it didn't work with the quartz windows, so we had to have somebody put a ruby insert in the quartz windows. And if you followed along with this program, we've had enough trouble with the quartz windows, and if we had put another insert inside of it, it just -- it would have been awful. So we didn't do it.

DR. BANERJEE: So the reason you didn't use it was difficulty.

MR. KELLY: Cost and difficulty.

DR. BANERJEE: But one thing needs to validate that this measurement technique actually is okay or not on what its limits are. And I think that in Karlstein they're using densitometers, so you put a direct measure there. Franz Mayinger, the guy who developed this stuff, was looking straight through. So there might be a database there that could help you to validate this.

MR. KELLY: Yes. I saw your comment about that in the notes from the RBHT, and certainly when I start doing the data analysis, if Hochreiter's team has not done a better analysis of that, then I will take a look at it to have some idea of the accuracy.

DR. BANERJEE: And maybe you can analyze the problem away.

MEMBER RANSOM: Well, as a matter of fact, if you just took a stead state calculation for a droplet drag model and wall friction model and the gas at the flow rates that you're talking about and show that the DP in that case how it compares with what you would interpret from a hydrostatic pressure difference in terms of void fraction. And I would think that wouldn't be too hard a calculation to make, and if it doesn't correlate well, why it's an indication that this method really does not work.

MR. KELLY: Yes. Well, I've done that in the past, and that's why I say throw away anything with a void fraction more than about 90 percent.

MEMBER RANSOM: Right.

MR. KELLY: Because it's just -- at that point, it's just giving you an indication.

MEMBER RANSOM: Yes.

MR. KELLY: Yes. Now, you're not going to measure droplet volume fractions. In dispersed flow, the droplet fractions we're talking about are half of a percent. You're not going to measure that -- we're not going to measure it with a gamma-densitometer, and you're not going to measure it with a Delta P cell.

DR. BANERJEE: The only way you can get them is by neutrons capturing, which is accurately done.

MEMBER RANSOM: So, really, I think what you're saying is it's very useful, because you either have liquid, primarily, or small void fraction in the subcooled boiling region and then highly dispersed.

DR. BANERJEE: No, no, you have an inverted annulary region.

MEMBER RANSOM: You have an inverted annular, which that one too would be critical.

MR. KELLY: Let me get about 50 view graphs from here and we're going to talk a lot about that. And that I'll show you why we have the DP cells, because that's one of the big things here.

What we were hoping to do was have a second bundle build where we would go in and change the grid spacer design or put guide tubes in, because now we just have heater rods except for the four corners. And the point of this is if you put in guide tubes, I've seen large-break LOCA calculations, the guide tubes, because they don't have a heat source, can rewet, and they provide a radiation sink for the fuel rods. And in something like a Westinghouse 17 by 17 design, you're never more that I think it's two sub-panels away from a guide tube. So you can get about 25 degrees K on your PCT by modeling radiation to guide tubes.

CHAIRMAN WALLIS: They have water rods and things like that too, don't they?

MR. KELLY: But water rods are in BWRs. And BWR radiation is very important because of the canister housing. So we were hoping to do this, and we were also planning on doing gravity reflood tests to try to look at the effects of oscillation on entrainment, I know that's something that Professor

Banerjee talked about, and also do a parametric on outlet resistance. Notice these are all grayed out, and the reason is that the cost of the facility construction and schedule were both much more than we expected, but also the costs of the ongoing operation, because this isn't run by graduate students, it's run by professional staff at the ARL. That's much higher than we're used to in our other facilities, and with everything else that's going on in the different reactors, et cetera, we've had to make funding reductions to this program. So we're basically not going to do this unless there's some dramatic need or funding made available.

MEMBER KRESS: But you would still have the guidance in the code to deal with those.

MR. KELLY: Yes. This was just to try to put some of this in perspective. When they did the CSAU study and quantified the uncertainty in TRAC-PF1/MOD 1 back in the late '80s, they came up with a very large safety margin for large-break LOCA. It was about 350 degrees K. That was for a formula plant with a peak linear heat generation of about 9.4 kilowatts per foot. Now, in the submittals you've seen recently, you see numbers up more like 15 kilowatts per foot, and when you do that, when you do your best estimate prediction with two sigma uncertainty, you no longer have this very large margin. Instead the margin is very decreased, and you start getting temperatures up to what you get for an Appendix K calc. I'm going to show those on the next slide.

And so this is an example of a best estimate plus uncertainty calculation at 15.1 kilowatts per foot. So this is a 95th percentile calc. This was a CSAU study at a nominal temperature. This was basically the result that shut down a lot of the thermal hydraulic research because the margin from here to the Appendix K limit was so large that it was called a "never mind." But as you add the uncertainty, which was up to about here, and then you go to the higher power levels with uncertainty, you've shrunk the margin. And in fact today, if you look at the best estimate submittal for the AP1000, it's right about here.

CHAIRMAN WALLIS: One has to wonder whether 95 percent certainty is good enough if you're so close to the limit. Depends on the risk and so on. I mean there's nothing magical about 95 percent.

MR. KELLY: Right.

MEMBER KRESS: Well, I guess you ask the

question of what are you risking, what are you losing if you exceed the limit? And here I don't think that limit is -- I don't think you lose much if you exceed that. Is that right?

MR. KELLY: That's a whole other question. For me, I'm saying this is the law, we're getting up very close to it with the vendor calcs, our code at the moment isn't good enough to really audit those calcs. We want to have a code with a low enough uncertainty that we can do a parallel calculation and if we get a number close to what they have, it makes NRR's job a little bit easier.

MEMBER KRESS: What bothers us quite often about that is that uncertainty is generally a parameter uncertainty, and we know that hidden in those codes is something called model uncertainty and we don't know how to deal with it. And we don't know whether the realistic best estimates of our version of Appendix K are supposed to include that model uncertainty.

MR. KELLY: By modeling, do you mean physical models or an interim model?

MEMBER KRESS: Well, that's the question of lack of knowledge uncertainty where you miss something that --

MR. KELLY: And you just have to do with the best you can with a comprehensive assessment.

DR. BANERJEE: Well, there's also the built-in scale uncertainty. You know, the assessment tries to address that, but there's nothing really that's been ever done on full scale completely. Bits and pieces have been.

MR. KELLY: Right. You do the best you can.

This was talking about where we are with TRAC. When they did TRAC-PF1/MOD1, this was back in the late '80s, the CSAU study did quantify the uncertainty a little bit in a hand waiving way compared to what you see with the Westinghouse and Framatome submittals, but they did quantify. But they also identified a number of areas of TRAC modeling deficiencies and high uncertainties and said that there was a potential for a significantly larger margin than they had identified.

Well, what that led to was a development program to improve the reflood models in TRAC, and they came up with a completely new, which they would call, mechanistic reflood model and it was based primarily on data from tubes, for example, the Winfrith hot patch test. There was minimal assessment against rod bundle data, only a couple of the large-scale facilities, no FLECHT SEASET test at all. And just the way things worked, there was less and less interest at this time, so there was very little assessment, and at that point, the code kind of sat around for a while. Then the AP600 came and we wanted to do large-break LOCA calculations with TRAC then, and that's when we started having some problems with it and realized that it wasn't good enough. And, actually, the contractor did try to improve the code but was unsuccessful. So the point here is that the pedigree that was generated for MOD1, and what I'm talking about is all the separate effects reflood tests, all of the integral tests, the SCTF, CCTF, UPTF, LOFT, all of the assessment that was done here doesn't apply to MOD2. And the models that are in MOD2 are what are in TRAC-M today.

MEMBER RANSOM: Joe, along that line, then RELAP5 they went to a drift flux model or at least the interface drag that they used in rod bundles was based on the EPRI drift flux modeling, which I always felt was a step back. But is TRAC using that same kind of philosophy?

MR. KELLY: No. TRAC uses --

MEMBER RANSOM: More mechanistic?

MR. KELLY: Well, I won't call it more mechanistic. It uses the kind of thing that we did in COBRA-TF. Basically, you know, you say there are small bubbles at a certain diameter, large bubbles at a hydraulic diameter, and there's some kind of alpha ramp between the two to get you to transition from bubbly to slug. And there is a little profile correction factor in that's taking from a drift flux model, but that's an area that needs work. If void fraction under those situations are important, that's what needs work. And that's one of the reasons we want to do the interfacial drag test in the RBHT facility this coming year is to give us a database at low pressure conditions. By more mechanistic, what I would really think of it is the interfacial area transport type models where you start modeling the bubble coalescence and breakup processes as driving your flow regime transitions. And we're not there yet. That's a few years away.

DR. BANERJEE: But you wouldn't be modeling the transition in breakup, I think you'd simply put source terms in.

MR. KELLY: Right.

DR. BANERJEE: I mean it's going to be very empirical.

MR. KELLY: Yes. You just move the empiricism one level down.

DR. BANERJEE: Down.

MR. KELLY: Yes. But it also gives you evolutions over length and time scales that we don't have now. That's one of the real keys.

DR. BANERJEE: Right.

MR. KELLY: And that's why I will typically put quotes around mechanistic any time, because at some point they're all empirical. We don't have droplet trajectories and drops flattening against walls and everything.

So then I came on the scene and the RBHT Program started to come about. So I went and started to do some separate effects assessment using what I'll just call TRAC-M. Anywhere you see MOD2 I'll say TRAC-M. And so what I looked at was FLECHT SEASET, and I did some calculations for a low flooding rate test, and they're completely unrealistic, highly, highly conservative, and you're going to see those in a second. The reason they were is they have extremely large oscillations. It was a very good model of Vesuvius. And this is for a test with fixed inlet flow rate.

DR. BANERJEE: Low pressure, right?

MR. KELLY: Two point seven bar. And so the conclusion was that significant model improvement was needed before we started doing any kind of quantification of uncertainty for TRAC-M. And so this is kind of my philosophy here, so I will go over this slide. Obviously, it needs improvement. The current model is overly complicated, we need something that's a little bit more simple, at least more straightforward. We've got to reduce the oscillatory behavior, and then we have to improve the accuracy of the predictions.

In the past, and I'm going to criticize this because I've done this, not just other code developers, I've done it, what you would do is go to the literature, find some other correlations, try some different correlations, maybe tune a few coefficients or put in some different smoothing ramps to try to smooth things out so you don't see so much. You end up with something that's overly complicated, and it has compensating errors in it. So you have no assurance -- even if you're matching the right temperature for part of the transient, you have no assurance that you're matching it for the right reason. And, typically, you aren't. You may get the right heat transfer, but you've got the wrong fluid conditions, so that means your heat transfer model can't really be right.

So what we want to do here is have an experimental program that to the extent that we can with the instrumentation we have available today gives us the detailed data that you need for model development. And, of course, you want to be able to supplement this with more fundamental tests like Professor Banerjee talked about earlier.

Then try to select or, if you have to, develop those models looking at the underlying things. So don't just get the heat transfer right, get the heat transfer and the void fraction right. Or if you're in the dispersed floor regime, get the heat transfer and the vapor temperature right. And then --

CHAIRMAN WALLIS: Or measure the droplet velocity and get that right too, among other things.

MR. KELLY: We can talk about that later. Let's say the droplet flow rate, okay, the entrainment rate. So try to get the right local fluid conditions and the right heat transfer, but also when you're doing this try to make sure that the models you're developing for the code are consistent with the two-fluid model in the way the numerics in the code are.

Okay. We can skip that, skip that. I

basically just said this, but if we have to develop a model, it has to be accurate, but you also want it to be consistent, and what I mean here is if the flow regime map you're using for your interfacial drag and interfacial heat transfer tells you you're in this regime, well, when you calculate the wall heat transfer it should be in the same regime. A lot of the codes, typically you'll have, in effect, different flow regimes for interfacial drag, interfacial heat transfer and wall heat transfer for the same node.

So you need to try to make those consistent and try to get rid of ad hoc models, especially if they're important. So, for example, in interfacial drag for inverted annular film boiling, at the time we did COBRA-TF, we didn't really know anything. So if I recall, that interfacial drag coefficient is 0.01. And when we did simulations it kind of, sort of gave void fractions that weren't too bad, so it was left that way. But when you can, you know, especially if there's more fundamental data available, go and get that data, reduce it and come up with a model that's not just, well, what Dennis Wallis used to call a six-pack correlation.

DR. BANERJEE: But, you know, the friction factor, which is constant, is reasonably good for wall anyway --

MR. KELLY: Yes.

DR. BANERJEE: -- so it's probably not too bad.

MEMBER RANSOM: You know, I never understood the NRC's use of the word, "ad hoc." I think officially it means special case, and I've never seen anything wrong with using special case models that apply to a specific situation. But it's always used in a negative sense here, it seems, meaning picked out of the air, but that's not what it really means.

DR. BANERJEE: Or lashed together.

MEMBER RANSOM: Huh?

DR. BANERJEE: Or lashed together.

MEMBER RANSOM: Or lashed together, right. But "ad hoc," I think, in the English language means special purpose.

MR. KELLY: Well, I may have to change that. I may end up putting "six-pack" where it says, "ad hoc."

And the other thing is when you develop a model you need to think about its numerical characteristics. I'll give you a very quick example. Whether it's the Chen Nuclear Boiling Correlation or a lot of others, you use the inverse Martinelli parameter, and so it has a quality over one minus quality as one of the terms in it. I may have it upside down, but I always do that. One of my -- X over X.

And what this really comes from is a force balance on interfacial drag and wall drag to give you a film thickness. Well, the code calculates void fraction; it does that calculation itself. And so the void fraction is the film thickness. But when you go to low flow rate conditions, basically stagnant pool-type thing with boiling, what is the quality? It really becomes undefined.

Like, for example, if you're a pot on your stove boiling, the vapor flow rate comes up but the liquid is actually falling down. What is your flow quality? You know, flow quality is a very nice correlating parameter for a steady state one-dimensional experiment. It's not a very good parameter to use in a two-fluid code, because now your quality can go between zero and one, and one minus X over X, it can go anywhere. And if that's multiplying either a suppression factor or a flow factor in a Chen correlation, you can get big oscillations from something that you would never dream would give you a problem.

So when you can base your correlations on integral variables. And what I mean by an integral variable is something like the void fraction. The volume fraction in a control volume results from the conservation equations, so it takes some amount of time for that void fraction to change. Whereas a velocity at a junction can flip-flop very quickly. Well, you can read the rest of that.

There are some things unique about the experiment, and this is where if you think this is a worthwhile program for the NRC to pursue, you need to make that message very clear to our Management. Budget for this program has been reduced, it could go to zero, so if you think it's a worthwhile program, make sure our Management knows that.

CHAIRMAN WALLIS: It seems to be the rule. As soon as something that's been expensive starts to give useful results, you stop it. So all our previous investment is thrown away .

MR. KELLY: Yes. Please give that message very clearly.

DR. BANERJEE: There was a reason, though, that was sort of -- you know, there were presentations that were made before the RBHT or after, I'm not sure, to us where the analysis showed that it really didn't matter what the details of the droplet field were. There were some bounding sort of assumptions made. I was trying to remember, was it S-RELAP or which way, whether it was Forslund-Rohsenow or you -- it seemed that the peak clad temperature was remarkably insensitive to details of the model in this dispersed boiling region, or whatever, that was taken.

MEMBER RANSOM: That was in the quench region.

DR. BANERJEE: Well, no, it was for the PCT, which is not necessarily in the quench region somewhere downstream being steam-cooled. And if they took, say, just steam cooling and said that -- and film was still important, of course, but if you say everything became steam or it stayed as droplets, it didn't really make too much difference. This was the argument made to us that the PCT is remarkably insensitive to the details of these models. So if that's the case, then the case has to be made on a different basis. I don't know what the truth is here, because that was being argued by some group of people who are trying to get approved as it was.

MR. BAJOREK: This is Steve Bajorek from Research. Then that must be something that's incident to that code only, because I've done lots of calculations with COBRA TRAC and it is very sensitive to your assumptions on droplets, the droplet size and the local flux that you have near the PCT location. The reason for that is depending on your droplet size and how it interacts with the grids, that is what's providing a lot of the cooling to the steam itself, and it will have a very large impact on the steam temperature.

DR. BANERJEE: Then we should have or you should document it because the analysis that was presented to us for S-RELAP was that the, at least for the large-break LOCA, the details of the correlations that were assumed did not have a really large effect on PCT which was why they could afford to be rather cavalier about what they used. And they showed some graphs, right. And that left, at least me, with the feeling, maybe incorrectly, that this is something new, I haven't seen this before, so maybe if this is the case, then all this stuff isn't all that useful.

MR. KELLY: Now, we can talk about the Forslund-Rohsenow a little bit later because it has a very checkered history having to do with the CSAU study. But --

DR. BANERJEE: Maybe you have to make your case better.

MR. KELLY: Yes. I agree with Steve but there are some compensating effects for an integrated system. You know, if you have more heat transfer in the core, you tend to carry more liquid up. If it gets to the steam generators and you have steam binding, you reduce the reflood rate, then you get less heat -- so there are some global parameters that tend to make the models less sensitive than they appear in the separate effects tests. But how sensitive I don't know, and today I can't tell you with TRAC-M because the oscillations that we see are so large that the results just simply aren't meaningful.

DR. BANERJEE: I think the argument, Joe, was that let's say that there was a certain amount of liquid in train, that's important, of course. If it all turns to steam, then it gives you some enhanced steam flow rate and some reduction in steam temperature. Convective cooling to that steam then is quite effective. And then if these don't turn to steam but stay as droplets, they enhance the heat transfer due to various effects. And the upshot of all this is that you get roughly the same answer. So the details of the droplet field are less important than the details of the entrainment, how much is entrained.

MR. KELLY: What I'd say is that one catch in all this is you have to say where do those droplets turn into steam? If you'll allow those droplets to turn to steam in the core, then the steam cooling argument works. But if instead you really do have a good model for this steam generator and the various processes that occur between the upper plenum hot leg to steam generator, if one of those droplets that turn into steam is in the steam generator so that they provide a back pressure and limit your reflood rate, that's entirely different.

DR. BANERJEE: They were letting it go into the channel itself. They were saying, "Here we have droplets and here we have steam, and the answer is about the same." But that's only from the cooling point of view.

MR. KELLY: Yes. But suppose rather than letting droplets turn into steam there you have the droplets that carry off and provide your back pressure by evaporating in the steam generator. Then the models that you used from the droplet are very important, just as Steve said. Now, it depends on what heat you take out and where you take it out is what's really important.

MR. BAJOREK: That sounds like a compensating error somewhere in their formulation.

DR. BANERJEE: Could be, depending on the position, Steve.

MR. BAJOREK: Because even the FLECHT SEASET tests with one inch per second, and there's a couple down at 0.8 inch per second, all showed very high carryover, 70, 80, 90 percent. And those are droplets which if they don't become de-entrained in the upper plenum are going to contribute to a steam binding effect.

DR. BANERJEE: You may be completely right. All I'm saying is it needs to be documented, because there was a school of thought put forward that it wasn't terribly important whether you used Forslund-Rohsenow or this or that or whatever.

MR. KELLY: I just looked at my watch and saw what time it is, so I'm going to skip a whole bunch of viewgraphs here on why we're doing this and why I think it's a good idea and try to get you to some of the stuff I think is more important.

Schedule, everything for the test has been pushed back due to the budget cuts. Basically, we're only going to be able to afford to run the facility for about half of the year, and so things that we're planning on doing in two years are now going to be stretched out to four to five. We already discussed that.

CHAIRMAN WALLIS: You already had a five-year plan just to spread out for ten years.

MR. KELLY: In effect, due to taking longer to build the facility and now the budget reduction.

MEMBER KRESS: Is all this due to the continuing resolution?

MR. KELLY: No. Reallocation of resources --

MEMBER KRESS: Reallocation.

MR. KELLY: -- to the advanced plants.

MEMBER KRESS: Okay.

MR. KELLY: When I talk about what's in MOD2 and the development they did, I would say that development is well-intentioned, and what I mean here is they used data from fundamental experiments, ones that were in tubes, they did things like look at the jet breakup experiments by Ishii and DeJarlais. They did give a consistent treatment of flow regimes between interfacial sheer drag and heat transfer, and the way they did that was with a position-dependent inverted annular flow regime map, which I'm going to show in the next slide. So it's based upon distance from the quench front. And there was a big -- I should probably have used a different font here.

The model that was installed in the code is very, very complicated, and the level of detail there is not at all supported by experimental evidence. It has 48 coefficients which can be adjusted, and they actually went through a three-year long program trying to, using a non-linear optimizer, adjust these coefficients in order to improve their calculations, but that was a dismal failure.

CHAIRMAN WALLIS: How many data points are there?

DR. BANERJEE: Forty-eight.

MR. KELLY: And you'll see where some of those come in just a second. It also contains multiple moving functions, so you're never really sure what's being used. You know, what correlation is being used here at this point in the transit, no idea. It ignores differences between rod bundles and tubes, and it is susceptible to very large numerical oscillations.

MEMBER FORD: Joe, excuse me, you said that this is not -- likely will not be funded because of reallocation of money to the advance reactors?

MR. KELLY: Well, the funding has been reduced.

MEMBER FORD: Yes.

MR. KELLY: Actually, I may have misstated that. That's probably a better question for my Management to answer. But at any rate --

MEMBER FORD: But this is relevant to the advance reactors.

MR. KELLY: Well, if you're talking about ESBWR, for example, if in a large-break LOCA it does not dry out, which I believe is the claim, then refloods in their mind doesn't occur. It is very relevant to AP1000, but of course we're just coming in a little late for that. This would have been great if we had done it five years ago.

And beyond that, it would be relevant for CANDU, ACR-700 but that's sideways, so you'd need a whole different set of reflood experiments and a lot of other things for us to accurately model a CANDU.

So the reflood flow regimes what they did is they used the location of the quench front, which is Zchf, as a trigger for the flow regimes and calculated distances downstream of that at which you would go through each of these flow regimes. And that's bad enough, but what's worse is they made it a function of a capillary number where the capillary number has the liquid velocity at the quench front in it. And if any of you have ever looked in detail at a TRAC or RELAP5 calculation and looked in a reactor-type system, looked at the liquid velocity at this point, it wildly oscillates. So if you do that and all these links are functions of that, all of these links are going to wildly oscillate, and the net result is your vapor generation rate wildly oscillates. And in this particular case, what happens is the water would come in, you just blow it all out the bundle. For FLECHT SEASET, the upper plenum acts like a phase separator, so once you blow that water up in the upper plenum it's gone. The bundle sits there quiescent for a while until enough inventory's built up and it does it again.

CHAIRMAN WALLIS: Coffee percolator.

MR. KELLY: It's more violent. Let me put it this way: If you turn the critical flow model on at the top of the bundle, it affects the answer.

CHAIRMAN WALLIS: You actually have critical flow at the top?

MR. KELLY: Yes. That's how bad it was.

DR. BANERJEE: Sonic velocities.

CHAIRMAN WALLIS: Whose fantasy was this?

MR. KELLY: Well, they didn't realize that's why the answers weren't quite as bad as they really were.

This just shows you the distance for some -- if these velocities were constant, what some of those links would be in centimeters, and it shows you the degree of resolution they were trying to get when the best -- smallest node sizes you typically will ever use will be about 25 centimeters.

For each of those regimes, there are different correlations for heat transfer to the wall, from the wall to the vapor or wall to liquid. And so all these different models, and then there are these various different ramps. So I mean I took a look at this and tried to figure out what's salvageable and what isn't, and I ended up deciding to throw it all away.

CHAIRMAN WALLIS: Where was this done?

MR. KELLY: Los Alamos.

CHAIRMAN WALLIS: It wasn't done in an academic environment, was it?

MR. KELLY: But this model got a best paper award at a conference.

So at any rate, I started doing assessment against FLECHT SEASET Test 31504, which is basically just a one-inch per second reflood case. This is axial profile of the clad temperature, this is the code calculation, this is the data at the time of the PCT, and we're already overpredicting by almost 100 degrees C, but the real difference is the quench front is lagging behind about half a meter. It gets absolutely ridiculous as you go a little bit further in time. This is the quench front propagation. The TRAC calculation is so slow because you're throwing so much water away. It's turning a one-inch per second reflood rate test into about a 0.2-inch per second reflood test.

CHAIRMAN WALLIS: It's being conservative.

MR. KELLY: Yes. And then some. But you can't say that if you were to apply it to an integral test, because then when you throw the water in the upper plenum, the next time step when all is quiescent, the water falls back down. So the answers won't look this bad but I still sure wouldn't believe them.

And this is an indication of the oscillation. This is the vapor temperature as a function of time. This is the data. This is somewhat similar to what you'll see from RBHT, except the RBHT microthermocouples actually seem to work a little bit better. And this is 400 to 800 degrees K in the vapor temperature, and that's just an indication of how bad things are.

MEMBER RANSOM: These kind of things only happened in RELAP5.

MR. KELLY: So, obviously, we need a better model with emphasis on let's get rid of this oscillatory behavior and then let's improve the accuracy.

CHAIRMAN WALLIS: There is some real oscillatory behavior in the real --

MR. KELLY: No question. Both the large-scale downcomer to core oscillations but there's also a little high frequency that you probably saw in the movie. That one --

CHAIRMAN WALLIS: Bursts of liquid.

MR. KELLY: Yes. That one will probably end up time averaging out, because that's such a localized phenomena drive, that we're not going to resolve them in the next few years. We may resolve them before I retire but not in the next few years.

To give us a vocabulary as I go into the more interesting part of the presentation, I'm going to talk about four different regimes. This is a little cartoon that I scanned out of a FLECHT SEASET document. Transition boiling, it occurs at the quench front, so this is where you go from pre-CHF nuclear boiling type heat transfer to film boiling. It's only one to two centimeters long, okay? What's labeled film boiling on here is what's often called and what I'll call inverted annular. You typically see this for high flow rate and highly subcooled conditions, conditions like when you had accumulator injection . You won't see this regime when you have, say, HPSI injection and your low plenum's almost saturated and your flow rates are low. This regime won't exist at all.

It's what they called a transition regime, which actually covers a much larger part of the bundle. It's typically between void fractions of like 40 to 90 percent. It's also called inverted slug, which is what I'll typically say -- agitated, inverted, annular or froth. It's a mixture of liquid fragments and droplets and basically occurs when -- as you go axially in the bundle, your vapor flow rate is increasing as you go up, because there's just more and more boiling. At some point your vapor velocities get to be substantial enough, they can break up this liquid core, and that's what triggers this regime.

And then above this, the disperse flow regime, which I'll call a disperse flow film boiling. When film boiling is stuck as a tag on something here, we don't really mean annular films. What it really just means is that the liquid can't touch the surface. The surface is beyond the Leidenfrost point.

And this just shows clad temperature and heat transfer coefficient versus time mapped against those regimes for a typical low flooding rate reflood case. In the disperse flow region, the heat transfer coefficient gradually increases as the quench front approaches. That's simply because the steam temperature has gone down.

At some point quite often in these temperature versus time traces, you'll notice a distinct discontinuity in the slope, and that's at the onset of either this inverted slug or inverted annular where the heat transfer coefficient dramatically increases over a fairly short distance. Then you actually have the quench front, and that's when you need the log scale for the heat transfer coefficient. Downstream this can be nuclear boiling or just conduction to liquid. And all you're doing is removing the decay heat, so the heat flux is on the order of five watts per centimeter squared, it's very small.

So I was going to talk, of course I'm not, but I was going to talk about five different regimes, and then for each regime why it's important, give you some background that I've looked at, talk about the constitutive models that are needed and then how we'll use the RBHT data. We're obviously not going to get through what I've prepared. I think I'm going to do the first one in detail so you can see where I'm coming from and then we can check the time and then maybe come back another day.

CHAIRMAN WALLIS: My suspicion is that these are fairly complicated models, so even though you don't want 48 coefficients, you've got to figure out what's the essential physics that ought to be represented without undo complication. But looking at the whole picture, it looks as if it's not a very simple thing you're going to try to describe. So I would think that you really have to, as we said before, do it in coordination with the experiment, because you will be asking questions as you develop the model which the experiment may not have answered, and this may tell you what you need to measure that you haven't measured.

MR. KELLY: Well, the one thing is I've

been in this part of the business for a long time now, so I have a fair amount of experience, as does Steve, and we've both worked not only with the experimental programs in the past but we've both worked with the code, so we know what the code needs and expects. And so we tend to serve as technical monitors on these, and especially with the RBHT Program we've made a lot of changes in the program based on this, and I'll point out a couple of those.

CHAIRMAN WALLIS: Okay.

MR. KELLY: But I'll do the first regime in detail, and then depending upon how much time we have we'll see where we can go from there, because this will give you an idea of exactly what you're talking about.

So I'm going to talk about inverted annular or film boiling, so this is a regime that occurs typically just downstream of the quench front, and it occurs when the liquid is subcooled. It's largely responsible for the quench front propagation, the rate at which you're quenching the bundle. You know, people talk about axial conduction governing it. It's really only the case for like an idealized falling film situation. What you really have here, if I go back to this slide, is you have a region of enhanced heat transfer that rapidly cools the rods to the point at which the rod can rewet. And, of course, once it gets to that point, it quenches instantly. But it's this precursory cooling that really governs the rate.

And if you compare the quench front velocities that you see in tests like this with ones that will be governed by axial conduction, it's more than an order of magnitude faster, sometimes it's two orders of magnitude faster. So it's getting this heat transfer right. This greatly augmented heat transfer coefficient just downstream of the quench front is what really --

CHAIRMAN WALLIS: Is that what you mean by inverted annular film boiling, what you call the frost region in the --

MR. KELLY: In some slides, yes. It depends on when I made the slide.

So it controls the quench front propagation, but also it gives you the vapor -- in concert with the regime just above it, the inverted -- what I'll call inverted slug, it gives you the vapor generation rate that ends up providing the vapor mass flux, the vapor temperature and the entrainment rate downstream to the dispersed flow region which is where you calculate the PCT. So this serves as a boundary condition for your dispersed flow film boiling region. And so these are the two reasons it's important.

To get into the background, when I first started doing this five years ago, I looked at some PERICLES tests, and these were tests I had started to look at when I worked in Grenoble. It's a fairly large rod bundle, and my conclusions from looking with these tests, and I'm going to show you why, or at least partially, is that the most important effect for this regime was the void fraction profile just downstream of the quench front.

More recently, I've looked at more fundamental tests, and these are steady state, low-quality film boiling in a tube done by Fung. And so it's a tube with a hot patch which is used to freeze the quench front near the inlet and then so you're able to actually do a steady state film boiling experiment with low-quality conditions and the type of heat fluxes in the range that you would see in a reflood case. They also included a gamma densitometer to measure the void fraction.

When I looked at these, I realized that the subcooling was also highly important and that we were going to have to do a much better job of the interface to liquid heat transfer -- interfacial heat transfer coefficient. And I'm going to show you these now. That's just the conclusions I had coming into this.

PERICLES, as I said, is a large bundle, mixing grids, typical co-sign power profile, a three-bar high inlet subcooling, range of mass fluxes, instrumentation was fairly standard, just rod thermocouples and Delta P cells, but the Delta P cells were over by half a meter, whereas in FLECHT SEASET they were about 25 centimeters and in RBHT we go all the way down to eight centimeters.

So what I did I went out to a point in time where the quench front was just a little below the mid-plane, and at that instant in time, I did an axial scan of all the clad temperatures in the center parts of the bundle. And by doing an inverse conduction solution generated the heat transfer coefficients for all those points. So what I'm plotting is the heat transfer coefficient versus distance, and this is distance downstream of the quench front, okay? And what you see is -- oh, when I say heat transfer coefficient in this context, I'm always referencing it to T-sat just so we have a common basis, because you have superheated steam, et cetera. So it's reference to T-sat. You see a large value just downstream of the quench front and an axial decay.

DR. BANERJEE: How did you do the heat flux? Were there any direct measurements of heat flux?

MR. KELLY: No, inverse conduction.

DR. BANERJEE: Okay. So you inferred them.

MR. KELLY: Which isn't too bad as long as it's not changing too rapidly. But one of the things I wanted to look at, because a lot of the correlations -- typically what's done is people grab off Bromley because it has a nice pedigree and it gives you the right order of magnitude, and then there are void fraction modifiers, sometimes mass flux modifiers and sometimes subcooling modifiers all stuck on top of it. But it's really the wrong -- it doesn't describe the right phenomena. It simply is wrong here. So I knew I wanted to look at the liquid mass flux effect. So three different tests at 80, 130 and 190 kilograms meters squared per second, where 130 is about six inches per second, just to correlate that. And so this is the same plot, and what you see is there's some effect but it's not terribly large, but it is there.

What I wanted to then do was go and check the void fraction. Now, remember, I only get the void fraction with all the caveats based upon Delta P cells that are fairly large, so these are the three different tests, going from 80 to 190 kilograms per meters squared per second. And the individual points here are the actual void fractions, if you will, or a representation of how much liquid was in that Delta P span. Between those I'm just doing a simple linear interpolation. This is simply not right, it just gives me an idea of what the void fraction may have been, and I'm explaining this so you know the caveats.

Then I went and replotted this as heat transfer coefficient versus this interpolated void fraction. In all three -- data from all three mass fluxes went away, within a lot of scatter but still the mass flux effect went away. So my conclusion was that the axial profile of the void fraction just downstream of the quench front is what was the most critical parameter for us to get correct.

One of the deficiencies in previous reflood tests, like FLECHT SEASET, is the Delta P spans were, say, one foot, 25 centimeters -- well, 30 centimeters, excuse me. And when you do that, this regime quite often is only a few inches long. You know, it certainly isn't always multiple feet, and so if you want to get an axial profile, you need much smaller. And so we went down to three inches and we did some hand calculations showing that we thought with three inches, with the accuracies of the cell, we'd get meaningful data, and it seems that we are if you look at the traces. Unfortunately, I don't have any to show you, but they seem to make sense.

CHAIRMAN WALLIS: You're really plotting heat transfer coefficient against Delta P or something because the void fraction comes from Delta P.

MR. KELLY: Right. It's an indication of the collapsed liquid level in that span. So it's just an indication. But that's why we, if you will, forced Penn State to use such a fine array of Delta P cells over the middle section of this bundle was so we could look at this and see if this was indeed the case.

I've already said this, and this was the result. In the middle part of the bundle, we put 11 DP cells with a span between eight and 12 centimeters.

CHAIRMAN WALLIS: Well, it might be that Delta P in your void fraction is a measure of the mixing of the turbine set up by the relative velocity which is holding up this liquid at the boiling void fraction, rather than the absolute velocity turning to the mixing. I don't know, it just --

MR. KELLY: Yes. You can do --

CHAIRMAN WALLIS: There's got to be some kind of velocity, it seems to me, that's giving you the heat transfer coefficient. Maybe it's the relative loss that it's holding up the job that's giving the void fraction rather than the absolute total velocity.

MR. KELLY: Yes. In this case, it's more a liquid column, and we'll get to that. You can do estimates of what the acceleration and frictional pressure drops are for this regime, and they're relatively small compared to the gravitational head.

DR. BANERJEE: But if the liquid column has to be carried out at some point, the thing has to look almost like a fluidized bed, right, with a pressure drop?

MR. KELLY: That's what I think happens downstream of this.

DR. BANERJEE: Okay. Balance the gravitational head.

MR. KELLY: Yes. And so you can take your equations, get rid of some of the terms and do the backout, you know, alpha one minus alpha Delta G equals --

CHAIRMAN WALLIS: I'll just say for the record I'm going to give the gavel to my colleague, Dr. Kress, here. I'm going to go and get an airplane.

MR. KELLY: Okay.

CHAIRMAN WALLIS: This has been very, very interesting.

MR. KELLY: Well, thank you.

CHAIRMAN WALLIS: I'm sorry I won't see the rest of it, but I can read it.

MR. KELLY: Well, I'll probably just do this one and then go near the end, and then we can revisit this at another time.

But before you go I'm going to try to get -- this won't let you go so fast. Never mind. I wanted to show you my last slide, but they'll just have to tell you about it.

MEMBER KRESS: He's got the slides.

MR. KELLY: Well, there's one of those that isn't on there. I don't even know what this is. I'm going to go back to my presentation. So I apologize for that. I should have known better than to --

DR. MOODY: Somebody said the best laid plans of mice and men often --

MR. KELLY: Does anyone know what this application is?

DR. BANERJEE: Never saw it. Why don't you just go back to your slide show and just select your last one?

MR. KELLY: That's what I'm trying to do but it --

MR. ROSENTHAL: Slide 39.

MR. KELLY: Somehow it's going on Internet Explorer.

MR. ROSENTHAL: -- live in fear that that's what will happen in the control group.

MR. KELLY: I should not have tried to go too fast. Okay. Now we're back to where I left. Sorry about that distraction here.

Now this is something I looked at more recently as part of the interim reflood model development, and it's a low-quality film boiling experiment done by Fung. I think it was done in AECL, and so it's a tube, an Inconel tube, with inside diameter about what you get from a 17 by 17 rod bundle. There's a hot patch down near the bottom that's basically a temperature control where you freeze the quench front here so you create film boiling conditions and you don't let the tube quench even though you're at relatively low powers. So you have an array of thermocouples but also a gamma densitometer. So you would take void fraction measurements at five locations. The only bad thing about these tests were they were at atmospheric pressure. I wish they'd been a little bit higher.

And so this is the void fraction dependence of the heat transfer coefficient. So I've got the heat transfer coefficient, the same metric units, versus void fraction, and I'm only showing it for data that would be subcooled where the equilibrium quality would be negative. And the various points go with tests from a mass flux from 100 to 500. Now, for each of these tests, at most it would be five points per test, and in fact in the subcooled regime there's typically only one or two. So when you see a test at 500, maybe two or three of these are from the same test. The other ones are from a different test at the same pressure and mass flux but maybe a different inlets temperature or a different power.

The fact that these line up as well as they do to a void fraction is really pretty amazing that there's that many different tests and you can put them all on there, and they're not too very different. But if you close your eyes and have a good imagination, you can kind of see there might be a little mass flux effect, but tests at 500 kilograms per meter squared per second tend to be more near the top and the other ones a little bit lower, but it's very small; it certainly is secondary.

And something to really note, because this was really a surprise to me, we're still at negative equilibrium qualities here. This is subcooled, and yet the void fraction is up near 70 percent. That's really pretty amazing, because your view of inverted annular film boiling is very, very vapor films which give you void fractions on the order of five or ten percent. And that's what you get if you take the Bromley equation and convert those film thicknesses from it into a void fraction. So we're totally -- something totally different than Bromley.

Now, if you convert -- take these void fractions, turn them into film thickness and make the same assumption that Bromley made of her conduction across that thin film, turns out you come very close here, and you get something that looks like this. And then if you allow that film to be turbulent and figure out what the velocity must have been to support the column of liquid, et cetera, you get something down here. So something else is clearly going on.

And there have been other tests, like Costigan and Wade did neutron radiography on a quenching test and they looked at this inverted annular region. And you always view it as just very nice co-axial cylinders sitting there well-behaved. And then people will talk about waves on the surface of the core, and all that happens, but what also happens is the whole core, especially as you get out to here, this whole core just moves around and comes very close to one side, then back and forth. And when it does that, it enhances the heat transfer.

DR. BANERJEE: It also breaks up, goes up and falls back is what you see in all these tests.

MR. KELLY: Yes. And that could be happening here as well.

DR. BANERJEE: Yes.

MR. KELLY: Although this is steady state.

DR. BANERJEE: No, I mean even in steady state.

MR. KELLY: Yes. Especially at one atmosphere.

MEMBER RANSOM: Well, Joe, are you planning to use the void fraction as an independent variable in the heat transfer coefficient establishment?

MR. KELLY: Of course.

MEMBER RANSOM: The problems I see with that is void fraction is generally very hard to predict with these codes. Do you have another model for void fraction at the quench front or --

MR. KELLY: We just have to do a better job of predicting the void fraction.

MEMBER RANSOM: But the void fraction is, like you said, maybe 20 centimeters averaged over that that you're going to predict with the code, unless you subdivide the region some way.

MR. KELLY: Well, a couple things. We will be using smaller nodes because we're able to do that nowadays because of computer power. It's, of course, still not the scale you need to, and we'll just have to do an interpolation of the void fraction between cells to simulate this axial profile and see how we do. You know, the codes tends now, even if they use Bromley, they put some kind of void fraction weighting on it where they get the void fraction from the two-fluid solution.

DR. BANERJEE: The coarseness of the noding is true for any correlation you use, whether you use void fraction or anything else.

MEMBER RANSOM: That's true but mass flux and some things like are more stable parameters, actually, and void fractions tend to be wild, all over the place in most code calculations so that it's going to feedback into the heat transfer.

MR. KELLY: And I'd say it depends on which mass flux you're talking about. If you're talking about the vapor mass flux and you don't have the problems that I described earlier, I agree, that's relatively stable. If you're talking about the liquid mass flux, then you have what Professor Banerjee talked about. Both in reality and in the code, it's quite often positive/negative. And, actually, the void fraction is much more smooth than that or at least it can be.

MEMBER RANSOM: How far are you away from installing some of these models and trying it out?

MR. KELLY: The models are being coded now. They'll be finished by the end of this month, and we'll be doing the testing starting in January. And maybe later this spring we'll be back here showing you what the results are and reviewing each model individually.

MEMBER RANSOM: I think it be interesting to follow how this goes.

MR. KELLY: So this was the first way I

plotted it, because I still remembered what I saw of PERICLES, so I had heat transfer coefficient versus void fraction. And, actually, if you were to override the two, they would be closer than you would have ever thought.

Then I went back and replotted it against equilibrium quality, and I got the idea from a paper by a Professor Takanaka. I don't remember which university he's at in Japan, but he did very similar tests using freon, and he was able to do a very, very nice parametric study, because with freon, because the temperatures aren't so high, you can change mass flux like this but hold your heat flux and your inlet subcooling constant. Whereas here, for every test, as you change mass flux, those parameters have changed. So there's a lot of -- even though I'm plotting this all on the same thing, they're really only at the same pressure and mass flux. The points for any two different tests can be at different wall heat fluxes, different wall temperatures or different -- well, I'm going to plot it versus this, so that takes subcooling out.

Now, what I want you to focus on at the moment is the negative quality part of this. We're going to talk about the positive quality part later.

Maybe not today, I'll mention it briefly at any rate. And what you see is all of these things come together, remarkably so. And my conclusion after I thought about this a long time is what's really happening is it's the ability of this subcooled liquid core to absorb the wall heat transfer, you know, the heat from the wall. It's a sensible heat makeup in the liquid core that's driving your heat transfer coefficient.

And I'll talk about that a little bit more in the next slide, but my conclusions from that were that the interfacial heat transfer, and now we're talking about from the interface to the subcooled liquid, needs to be a model accurately, and the result of this was we'll modify the test matrix for the RBHT to use subcooling at the quench front as one of our parameters. And this just shows the matrix and so I -- these are all at six inches a second, and we just did a parameter in this case on pressure, 20, 40, 60, with minus 12 percent quality. This is at the 53-inch rubble. You can get that just from the heat balance. And this is when we're into the region of the fine Delta P cells.

Then at the same flooding rate we did a characterization on subcooling. This is what the inlet subcooling would be, but the point was we went to six percent and basically zero quality because I wanted to look for the regime change and the quench front. And in one case where the quality was positive so the void fraction was about 56 percent. Then we would change the flooding rate to three inches per second. We couldn't get the minus 12 because the water just couldn't be cold enough at the inlet, but we could do the minus six, the zero and then the 56 percent. On the matrix, we put some ten-inch per second tests, but we were actually not able to run those tests. The facility had problems with those. That's something we'll look at again. I wanted to do that just to extend the mass flux beyond where we though it would be so that it would extrapolate more in the correct direction.

Now, this is where I'm going to talk about what the underlying phenomena actually are. So here's my little cartoon. We have a wall, we have a vapor film which is, you know, it's on the order of millimeters, it's not very thick, and a liquid core, vapor flow and liquid, subcooled liquid. The heat transfer process is from the wall to the vapor. Some of this will go to superheat the vapor. The majority of it is going to go to the liquid interface, and this is the assumption in Bromley, for example.

At the liquid interface, it can do one of two things: It either generate vapor or it can be conducted into a subcooled liquid. This flow split, you know, how much heat generates vapor versus how much goes into raising the temperature of the liquid, in effect, is going to determine how thick this film is. So if you have a very high interfacial heat transfer rate here, you're simply going to move this liquid over and get this film thinner and thinner and thinner until you're able to conduct enough heat through this vapor film to balance that. That's why the liquid temperature is so important in trying to predict the heat transfer. That's why all those points line up with equilibrium quality, because at that point in negative quality, equilibrium quality is nothing more than a liquid subcoolant. But put this in a -- there also is radiation to the liquid, and that can be ten, 20 percent type number. So that's something you want to model as well.

So what do you need? Wall-to-vapor heat transfer, you need something here; vapor-to-interface, liquid-to-interface, and I'm saying this may be one of the more important ones; wall-to-liquid radiation. If you're going to predict the interfacial drag, you know, once this part has given you a vapor generation rate, which in effect will then give you a vapor velocity and a liquid velocity just from your mass and energy, then the interfacial drag here is what gives you the relative velocity and in effect will determine what your void fraction is.

We also need a criteria for the regime transition. At which point does this break up and become something a little bit more similar to a fluidized bed with lots of liquid fragments going up and down? Now, traditionally, the assumption has been vapor film is a laminar and you just have conduction across it. And so the heat transfer coefficient is nothing more than the vapor connectivity divided by the film thickness. And that works as long as the film is very, very thin, but when you start getting up to void fractions of 15, 20, certainly by the time you get to 70 percent it doesn't work any more.

A lot of papers in the literature started to say, well, it's probably turbulence, but more and more recently what they go to is that it's something to do with the surface, and part of it is the surface becomes wavy. You can have low axisymmetric waves or the big helical kind. You can also have this whole core moving. So it has something to do with the waviness of the core is going to enhance this heat transfer coefficient above the K over delta. It's going to be larger than that. But also it's going to affect the interfacial drag coefficient. And you need to get all those models working together in order to be able to predict both the heat transfer and the void fraction right. And that's going to be a challenge, but that's what we're trying to do.

And the question is how are we going to use RBHT data for this? And this, unfortunately, isn't the best example, but I can tell you what we're going to do. In a typical test, what we're going to have available is the wall temperature in the heat flux, the liquid mass flux. You can infer that from a mass and energy balance. And the void fraction over about an eight centimeter interval. It's a possibility we'll get to liquid temperature because we have these liquid probes -- well, excuse me, fluid probes hanging down from each grid spacer, and we also have those little rakes that measure the center temperatures of three sub-channels. Now, when they're in this liquid core how well they're going to work I don't know yet because they haven't processed the data and looked at it.

But should we be able to get an axial profile of that liquid subcooling and be able to see the rate at which that subcooling diminishes as you go up the bundle in this film boiling region, then at least we'd be able to get an order of magnitude estimate of what the interfacial heat transfer coefficient is. And to be honest, that's pretty good. You know, if you look at interfacial heat transfer coefficients, they tend to go over about five orders of magnitude, and so if you can get within an order of magnitude, we'll be a lot better off than where we are today.

So looking at the RBHT, it will give us the data we need to validate the performance of these combined models. You put all these models together, do a simulation, then you can compare the wall heat transfer coefficient and the void fraction, did we get both of those right? But it's probably not going to give us the detailed -- you know, for this regime, the detailed data we would need for model development. It can help us in what I'll call a model selection process. If I go to more fundamental tests, such as the one by Fung, and, actually, I would like to do one using freon because you can do much cleaner parametrics or some kind of refrigerant, and you can also do a simulation from higher pressure, which is one of the holes in our database -- is this type of regime at high pressure? But it can get us some information to help us select models.

And that brings us to the regime that Professor Hochreiter talked a lot about, and that was what I call disperse flow film boiling. And now it's almost quarter after 12, so how long do you want me to go? Do you have any --

MEMBER KRESS: I think my preference is to continue on and finish it before we go to lunch. What does the rest of the Committee thing?

MEMBER FORD: Yes. I agree.

MEMBER KRESS: Okay. Why don't we just continue on?

MR. KELLY: Okay. Some of this I won't go through in quite as much as excruciating detail, just so we get there, because we're only on Slide Number 46. But I'm going to try to at least give you the essence.

Dispersed flow film boiling is obviously important because peak clad temperature occurs there. Now, there is some background. This is where I say we could talk about Forslund-Rohsenow and its very checkered history. Sometimes you'll see large overpredictions of the heat transfer. This is especially during the blowdown phase. When the drop-wall contact was put into TRAC-PF1, Forslund-Rohsenow came about from some liquid nitrogen experiments, and so what they did -- so they were doing film boiling using liquid nitrogen as the coolant, and they made some guesses as to what the droplet diameter would be and calculated the evolution down the tube of what the vapor superheating would be, the rate at which the droplets would evaporate and then force conducted heat transfer from the wall to that superheated steam. So you've already got a lot of assumptions built in the model, what the drop diameter is, et cetera.

Then wherever the model did not agree with data, they assumed it was due to drop-wall contact, and they took a model developed by Bailey for the evaporation of liquid drop on a horizonal surface and said, "Okay. This gives us an idea of what the heat transfer would be, but we know it has some approximations in it because we don't have gravity holding our drops against the wall here. We also know that we don't know what fraction of the surface is covered by drops." So they came up with an estimate of what the fraction of the surface might be based upon the drop volume fraction and then put a coefficient -- actually, they put two coefficients in front of them and lumped them into one. And the original correlation on that was, I believe, 0.2 and in a book by Professor Siu, he said for water it should be two.

I don't know where that came from, but the value of two is what was put in TRAC-PF1, and if you look at the void fractions that would have occurred in those type tests, they're very -- the drop fractions are very, very small. But if you go through like a blowdown rewet and put a lot of water through the core, all of a sudden you're a completely different regime and especially with that two this gave a huge heat flux and you could quench a lot of the core. And so it was non-conservative when you look at LOFT and some of the other tests. So this was one of the big deals that came out of the CSAU study was to scale back that coefficient and to actually put a bias of a fairly substantial penalty was put on because of this. So that's kind of the background there.

Now, when you look at dispersed flow film boiling data, and I've been doing that a lot lately, we have experiments similar to Fung, but that one was geared towards inverted annular but there are ones geared towards disperse flow where they measure the superheated vapor temperature with the same kind of test. If you -- this appears not to be relevant, and if all of the heat transfer is now from the wall to this superheated vapor, you know, plus a small radiated component which you can estimate, well, they don't line up with any kind of force convective heat transfer correlation you and I have seen. I'm going to show the results of that in a second.

They can be quite a bit larger, and the reason they're larger is that the presence of this first phase somehow enhances the heat transfer, and it can do it any number of ways. One of the theories is that it's through turbulent enhancement, the weights behind these drops actually enhance the free string turbulence level, enhance the heat transfer coefficient. Of course, I'm nervous talking anything about turbulence in front of you.

But the other thing, the other way to look about it is that if the drops are really small, think of them as this distributed heat sinks in this channel, and you can actually calculate that for laminar flow, and you get pretty large enhancements just by having changed the temperature profile over the channel.

So this is a reality, and this is something codes don't model. The only code I know of that has a model for this in it is COBRA-TRAC, and that was one we put in based upon something that had been done for BART back in 1984, and it was a turbulence enhancement model or a very crude approach to turbulence enhancement.

The other thing you see, and you see it in RBHT, FLECHT SEASET or FEBA, is that there's a very large heat transfer enhancement due to the presence of the grid spacers. And in RBHT we've seen as much as a 200 degree C drop as you go past the grid spacer in the rod temperature. And part of that is because these are mixing vane grids so they're very effective. They're either capturing or breaking up rocks.

Here's my cartoon for dispersed flow, and what I'm going to talk about now is how it's traditional in model. Three modes of heat transfer: conduction, radiation and drop-wall contact. So this is very similar to what I showed for inverted annular. Convection from the wall to the superheated vapor. Some of that is sensible heat to the vapor. Some of it is then retransmitted from the hotbed to the surface of the drop through interfacial heat transfer where it causes evaporation of the drops.

And this is the primary heat transfer mode. The drops are a pretty effective heat -- a fairly effective heat sink. There's also radiation to the drops and radiation to the vapor, but for low-pressure vapor, this is really negligible. This one, on the other hand, it can be -- typically, it's around five percent, but if you have a co-signed power shape in the bundle, up near the top of the bundle the vapor can start to get hotter even in the rods, and then the radiation can become a larger and larger component of the heat flux.

The third one is drop-wall contact. Obviously, if you've ever looked and you have seen the movies, droplets don't just travel nice right down the middle of the channel, everything is moving around flat, hitting against -- now, they can't contact but a drop can come over, spread out and underneath it you can get an enhanced region of heat transfer. And this is where the logic behind like Forslund-Rohsenow came. But since that time, Forslund-Rohsenow must have been in the early '60s. I don't remember the year but it's pretty old. Since that time there have been a lot of separate effects tests, you know, dropping little drops from a syringe down on a hot plate kind of test where they then measure how much either the plate cools off or how much of the drop evaporates.

And what it turns out is as long as your wall temperature is greater than some mythical temperature which I'll call Tmin, you know, hot enough that the drop can't wet the surface, only a small fraction of a percent of this drop is going to evaporate. And this heat transfer rate then is actually smaller than the uncertainty in this one. So I don't see the point of putting a very large detailed model in for this when this is smaller than the uncertainty in this one. So I'm going to leave it out, at least for now.

MEMBER KRESS: Good choice.

MR. KELLY: Now we're going to talk about two-phase enhancement.

DR. BANERJEE: Before you leave it, I didn't realize you were -- I guess it really depends on the population of drops and for most of these problems there are very few drops around. So in that case, the convection is likely to be most important. But if you had a higher population of drops, the drop wall contact and enhancement there may be significant. So I suppose it depends on the void fraction, really. So how are you defining the boundaries of this in terms of the void fraction?

MR. KELLY: When we get to the inverted slug --

DR. BANERJEE: Right.

MR. KELLY: -- let's talk about that then.

DR. BANERJEE: Okay.

MR. KELLY: Because I agree. I mean, obviously, in --

DR. BANERJEE: If you've got a bed of drops, this is going to be quite high, if it's drops and not big chunks of liquid.

MR. KELLY: I don't agree -- I mean I agree with what you're saying but only up to a point, and I'll argue why later.

DR. BANERJEE: Okay.

MR. KELLY: But I do agree, though, like say, for example, if you're in a place where you're rolling out aluminum and you have sheets of aluminum coming out of a mill and you have high velocity water sprays directed on the surface, you can get pretty high heat transfer coefficients due to this, because they found that was a more economical way to quench it than ducking in a pool. So that's actually done in industry. But you look at the jets and you have very large masses -- mass fluxes -- mass flow rates of water being driven under the surface at high velocity. And the degree to which this droplet spreads, how close it gets to the wall and how much heat it can remove from the wall is dependent, if you will, like on a droplet Weber number that's normal to the wall.

So our droplets, which zigzag around, they may approach the wall, but they're never slammed against it at several meters per second. Or if they are, it's only down near a quench front. It's certainly not several meters away.

DR. BANERJEE: And there are actually very few of them around, surprising enough.

MR. KELLY: In the dispersed region, I once calculated what the drop volume fraction for a rainstorm at one inch per second is -- I mean one inch per hour rain, which, you know, you can get very, very wet if you walk outside. It's like less than ten to the minus five is the volume fraction of drops. So you're right, there's very little, but you can sure get wet if you get in it.

Two-phase enhancement, this is of the vapor conductive heat transfer. And what you'll see is at that the presence of a dispersed phase enhances that component of the heat transfer over and above what you would normally calculate. I mean in retrospect it's obvious. You're obviously changing the temperature profile and the velocity profiles and maybe the free stream turbulence level. So, of course, it had to be in effect, but it's actually a pretty large effect at the lower Reynolds numbers that we see in reflood calculations.

And what you also see in current models is every now and then you'll see a calculation where for at least this one reflood test they will nail the peak temperature for the hot rod. It will be just dead on. But when you do that and you don't have this effect in, what you'll find is the convective heat transfer coefficient is actually too small, and what they've compensated for is by the vapor temperature will be cooler so that the product is the same. And when you do that, sure, you can get that one temperature right, but then you can't predict the temperature histories at both the center line and in the upper elevations. You can't have it both ways. You can tune it to one location but not for all.

DR. BANERJEE: I think this is essentially what these S-RELAP people, or whatever, did is they got the PCT right, or whatever, but at completely the wrong time. It has nothing to do with the real experiment. But they said it at least bounds the PCT because they're just redistributing the -- they're using the superheat of the vapor. That was the argument we went through. So if PCT is the only criteria, which I don't think it should be, then, you know, they say it doesn't really matter. That was sort of --

MR. KELLY: Okay. I can show, and I'm going to show in the next slides, that the enhancement does exist. The question is what's the mechanism for it, and you can read any number of papers on either. If you look at gas particle turbulence, what you'll find is quite often they talk more about turbulence modulation rather than enhancement, and that's especially true of particles in, say, the 100-millimeter range -- excuse me, 100 micron range. When you start getting up to a millimeter or more, then you might have enough weight turbulence to actually enhance it, and that's where our droplets are. So maybe this occurs, but for laminar flow you can show this occurs. But it's more likely a combination of the two and maybe even something we haven't thought of yet. But these are the two theories that are out there. But the bottom line is the vapor conductive heat transfer coefficient is not just a function of the vapor Reynolds number and the fluid properties like we normally would calculate it.

MEMBER KRESS: Are you already interpreting flow about the droplet enhancements?

MR. KELLY: No. You can be but more than likely you're in the transition regime and down near where you would expect a transition towards laminar.

This is the Lehigh three by three rod bundle, so it's a little rod bundle, it's only a little bit more than a meter tall, and they tried to build a hot patch on the bottom of each rod so they could emulate these two tests. Again, the drawback here is they're all at one bar in a fairly limited range of conditions. They couldn't go very much or they couldn't keep the rods from quenching.

But they do measure the vapor temperature. They come in with a doubly-aspirated steam probe and measure the temperature out near the center of this little bundle. So at that location, it's a steady state test,so you know the mass flux. You can do a mass energy balance using that measured temperature and get the actual quality here. So you know the vapor mass flux and the vapor temperature and the wall temperature and the heat flux.

So using all that, you can calculate a Nusselt number, divide it by your Prandtl to the one-third, and here is the data. This is against vapor Reynolds number. This is the Dittus-Boelter correlation which you wouldn't really expect to apply for a rod bundle so much. This is a FLECHT SEASET 161 rod. This is from their steam cooling test. So in the transition region, this is 1,000, this is 10,000. Most of this data is between about 500 and 6,000 or so. So this kind of gives a lower bound and what it kind of looks like is if you continued this out to higher Reynolds numbers, maybe it would merge to just the force convective component. And, certainly, down near laminar this is 100 percent higher.

Same kind of test but now this is in a tube, also done at Lehigh. Wider pressure range, mass flux and heat flux range. In this case, it's a Nusselt number that I'm plotting, not divided by the Prandtl, against the vapor Reynolds number. The orange triangles are the dispersed flow data. The little blue asterisks are the single-phase Nusselt number calculated using a more correlation, one due to Gnielinski, it's a Russian author. And that's why there's some scatter here. These include the variation of the fluid properties, the variable property effect. And since each of these data points are at different local conditions, this is a function of the fluid properties as well as the Reynolds number.

But what you see is the dispersed flow data at higher Reynolds numbers, about 20,000 or so, it's about 20 percent enhanced. But, again, as you move down into the transition region, down towards the laminar, you come up with 100 percent or so.

DR. BANERJEE: There's more scatter down there, so there's some other effects.

MR. KELLY: Well, these are at different liquid volume fractions, if nothing else. It's not just a function of this, it's a function of some other things.

DR. BANERJEE: Other variables.

MR. KELLY: The point of this is that and also that it can be sizable and it's something we should look at.

This is actually poorly organized in the sense that I don't directly go from what I just talked about to how we're going to do the RBHT. Instead I was going to talk about drop diameter. So what I will try to do is skip forward a little and go to -- these are the models that we're going to end up needing, and I'll talk about each of these in turn until you get tired of hearing my voice. But what I'm going to talk about now is just the wall-to-vapor convective heat transfer.

So this is how we're going to model it: Force convection heat transfer coefficient by something like Gnielinski, wall minus vapor temperature, it's going to be one plus this two-phase enhancement factor, and then we're going to add the radiated heat flux to it.

So the first thing you need is you need that single-phase coefficient. The second thing you need is the enhancement factor. So this is single-phase data from the steam cooling test at FLECHT SEASET from the 161 rod bundle, vapor Reynolds number, Nusselt over Prandtl number to the one-third, and these -- basically, each Reynolds number is pretty much one test. And then the reason for all the asterisks is they use the sub-channel analysis code to back out the vapor temperature for each sub-channel, calculated a heat transfer coefficient for that. So that's where this spread comes in.

The blue line is Dittus-Boelter. This is the FLECHT SEASET. So that doesn't look too bad, so we need something that's close to that, and that's just to capture the rod bundle effect, because rod bundles are not tubes. So you show a correlation that is a function of the rod bundle geometry.

But you also want to look at the convected enhancement, and here's where RBHT can come in. In addition to the steam cooling test helping us select a rod bundle heat transfer correlation --

MEMBER KRESS: Now, is your rod bundle geometry captured in your Nusselt number by the equivalent diameter that goes in there?

MR. KELLY: No.

MEMBER KRESS: No? That doesn't do it.

MR. KELLY: If you look in the literature, there's a lot of correlations. They don't necessarily agree with each other, but there tends to be a 20 to 30 percent enhancement of the convective heat transfer just by the fact that it's a rod bundle. Of course, the grids --

DR. BANERJEE: More so with Reynolds numbers.

MR. KELLY: More so, yes. The steam cooling test will help us select that correlation, because it's going to be turbulence down to the transition region. You're going to have to worry about mixed convection effects as well. But what's really of interest are the droplet injection tests. So in steam cooling, there will be 16 tests, two pressures and eight different Reynolds numbers. For the droplet injection, 48. We'll repeat those pressures and Reynolds numbers but then come in with droplet flow rates at three different ratios of the liquid flow rate to the vapor flow rate, because if you look at like, say, gas particle heat transfer tests, it's the mass loading ratio that's important. So that's what we're going to do.

Unfortunately, this has all slipped further and further out in time. What we'll get, you know, the data that will be available from these tests is you'll get the rod heat flux and temperature, the liquid and vapor flow rates. These are steady state tests, you can do mass energy balances and get a good estimate or at least a decent estimate of what your local fluid conditions are. That's very hard to do in a reflood test. We'll also know the superheated vapor temperature and the droplet diameter. It turns out, the drop diameter doesn't change a whole lot. These drops move relatively fast. Their velocity is on the order of one or two meters a second, so they get through the bundle very quickly. So unless they hit a grid and are shattered, they just zip out. They're very important for controlling the superheat level of the vapor, but the actual drop diameter only changes a little bit. Only a fairly small fraction of it will evaporate.

And so when you have this, you can then do a direct evaluation of what this two-phase enhancement factor really is or would be. The wall heat flux minus the radiation heat flux over your single-phase heat transfer coefficient and the temperature difference you measure minus one. And the only assumption is what this radiated heat transfer component is. And at the moment, we'll be using something like a model do to Sun-Gonzales-Tien that's been around for, say, 30 years now and pretty much, in one form or another, shows up in almost every code. How good it is, I don't know, but it's very hard to measure what the radiated component to droplet mist is.

MEMBER KRESS: If you miss it, the value you come up with your two-phase enhancement will compensate.

MR. KELLY: Yes. From the data I've looked at, there is so much spread in this, and I've looked at like six different theories, and it's very, very -- or six different type models. It's very difficult to come up with one that minimizes the uncertainty in here, and so that's one of the reasons I want to have these tests, because for --

DR. BANERJEE: It could certainly be a function of the volume fraction of the drops, because what you've got is a flow with distributed heat sinks in it. So the temperature profile is not turbulent, it's below, it's been sunk down by these. And that's why you're getting the enhanced heat transfer in some way, you're getting a temperature profile near the wall.

MR. KELLY: Exactly, especially near the laminar end and when the liquid loading isn't too high.

DR. BANERJEE: Yes.

MR. KELLY: And if you write the equations for this laminar flow, say you're going to do a numerical solution, when you non-dimensionalize the equations, two groups falls out. One of them I call a heat sink factor, and it's a ratio of -- well, it's basically the liquid volume fraction, one minus alpha, times the vapor-to-drop Nusselt number, divided by the Nusselt number of a wall-to-vapor. So it's kind of a ratio of how much heat can go to the drops versus how much heat goes to the vapor.

The other one is a superheat factor, and it's basically the vapor-specific heat times the vapor superheat, divided by the latent heat. And those become -- so those are your non-dimensional number things when you're doing this laminar flow solution. So I've tried to use those as a correlating factor. It works very well for some data, but when you go to very heavily-laden, you know, where the mass loading factors are ten or 100 or 1,000, and you get up to those in some of these regimes, then it doesn't seem to correlate at all.

DR. BANERJEE: But then you've got other factors. You've got probably turbulent enhancement and other things happening.

MR. KELLY: And that's what I'm going to try to use there, and there you go to something like what a Theophanos and Sullivan paper --

DR. BANERJEE: That is bubbly flow.

MR. KELLY: But they turned it around and the equation said you could do it for dispersement. But you can do something like that.

DR. BANERJEE: Right.

MR. KELLY: So it's basically a ratio of a drag coefficient in interfacial area to the wall friction factor.

DR. BANERJEE: Right.

MEMBER KRESS: For these drops, the convective Nusselt number, it's probably just the --

MR. KELLY: The conduction?

MEMBER KRESS: -- the conduction in a solid sphere. We don't have any -- they're so small we don't have any enhancement over that, right?

MR. KELLY: Well, we're talking about not inside the drop, we're talking about between the vapor and the drop. And so, quite often, it is close to a conduction limit. If you look at the formulas, the traditional one is Lee and Ryley, but there are more modern versions.

MEMBER KRESS: Yes. I was ignoring the inside of the drop, I was still talking about vapor to the drop.

MR. KELLY: Yes. They all say the Nusselt number is something like two plus a square root -- a constant times the square root of the Reynolds number times the Prandtl number. And, quite often, the value is between two and ten.

MEMBER KRESS: Okay. So it's not just two.

MR. KELLY: It's seldom more than ten, but it's not just two. It depends on the flow condition.

DR. BANERJEE: It depends on the size of the drop. I mean if it's too big, then you get internal circulations.

MEMBER KRESS: Yes. I'm assuming for this size drop that's not a big factor, though.

MR. KELLY: The drops will become distorted, they won't stay spherical.

DR. BANERJEE: They'll oscillate a little bit.

MEMBER KRESS: Well, it could be higher then.

DR. BANERJEE: You can get an analytical solution to this problem as a bounding calculation by just taking a heat sink in your equations and integrating them, and that will at least show you an upper bound.

MR. KELLY: There is an analytical solution out in the literature. There was one done by Jens Andersen, and there was an older one before that but I can't remember the author's name. But Yao at, I think, the University of Pittsburgh has done a lot of work on this.

DR. BANERJEE: Yes, maybe.

MR. KELLY: And I did a numerical solution for laminar flow on this, and that's where I came up with the heat sink factor and the superheat factor. And those results, actually for certain conditions, give you the kind of numbers that you see in the data.

MEMBER KRESS: Well, if this two-phase enhancement factor turns out to be a relatively strong function of the liquid vapor mass loading ratios, which you expect it to be, I worry about only having three values for those.

MR. KELLY: Well, I would like to have more, but if we can only operate the facility for half a year, and they're actually saying it's going to take them two years to do this series of tests --

MEMBER KRESS: Yes. You take what you can get, I guess.

MR. KELLY: Yes.

DR. BANERJEE: Also, if it turns out to be really important, you can probably go back and do that.

MEMBER KRESS: Probably go back and do that, yes.

DR. BANERJEE: If it is.

MR. KELLY: So Steve and I are both very interested in this program and trying to follow it along and also to encourage it and direct it to what we think is important.

MEMBER KRESS: You're going to choose these three liquid vapor mass loading ratios to span what you expect in the real case, I guess, so that you can extrapolate in between them.

MR. KELLY: At least up to the point for the dispersed flow regime.

MEMBER KRESS: Yes, okay.

MR. KELLY: Now, one of the regimes we're going to talk about later, if we get there, is what I call inverted slug, and that's why I'm thinking of more as like a fluidized bed where the volume fractions are on the order of 50 to 90 percent, and you have these liquid drops and fragments going every which way, and the heat transfer can be pretty significantly enhanced. And the loading ratios there get up to about 1,000. And if we were to spray that kind of liquid mass flux in these little droplet injectors, there's no way we wouldn't quench the bundle. We wouldn't be able to do the steady state dispersed flow test. So we'll push it as far as we can, but we'll at least make sure we end up with a good model for disperse flow. And this is how we would use that data that we would get from the facility to generate the model we need. Okay.

Now we're going to back up, because I skipped some slides, and go back to the background and talk about drop diameter, because it's primary role is its effect on vapor superheat, and that's really crucial because that's going to be your sink temperature. But it also affects the grid space-to-drop breakup. This two-phase conductive enhancement factor is going to be a function of a drop diameter.

And also water drop radiation heat transfer is a function of the drop volume fraction and diameter. So it affects all of these.

We don't even know what mechanisms forms these drops. If you look in the literature, there's a lot of speculations. You know, is it aerodynamic breakup of these liquid slugs? Well, maybe and probably at least some of the -- maybe the majority of the drops come from that. You'll see papers where they waive entrainment from that inverted annular core. Now, if you're going to develop waves on it, you can strip drops off of it. Or if you go to a low flooding rate, what you have is actually an annular film down below the quench front. You can develop waves in that film and entrain drops actually before you get to the quench front. But this wouldn't too often happen just because of the heat flux levels below the quench front.

MEMBER KRESS: Aren't you producing vapor at the quench front? And when the vapor breaks to a liquid interface, doesn't it carry liquid with it?

DR. BANERJEE: That's a splattering.

MEMBER KRESS: Is that what you mean by splattering?

MR. KELLY: Yes.

MEMBER KRESS: Oh, okay.

MR. KELLY: It depends on your liquid flow rate and your liquid subcooling. If you're at high flow rates and high subcooling, yes, you generate vapor at the quench front, but you immediately condense a lot of it. So you get your onset of film boiling there, but the eventual -- and that's how you have the liquid inverted annular core downstream of the quench front is because you've condensed the majority of that vapor.

You can also generate droplets by wall-to-drop interactions. As we talked about, if you slam a drop up against this hot wall, it's going to flatten out, you're going to be generating under the drop, instability is -- as you start to push the drop away, you'll have instabilities on that vapor surface, and the drops will tend to break up with some critical wavelength that will be the size of the drop. And I don't remember what the formula is, but that's there.

You also have drops colliding with other drops, with the grids, of course, and sputtering is what happens if, say, for example, you have an actual annular flow, we're talking about low flooding rate cases, so below the quench front it's two-phase and you actually have an annular flow regime near the quench front that then this film trips over the quench front itself where you go from basically a cool wall to red hot and blows the liquid film off, and the name for that is sputtering.

But if you look at the current database, there is some data for drop diameters in reflood from both experiments in tubes and rod bundles. But, typically, the local conditions are not reported. All you get are the droplet diameters. And so what happens, and I guess now I can't use "ad hoc" anymore, I'm going to have to go check that definition. What you'll see in a lot of codes is they'll take a critical value for the Weber number based upon the local conditions, not where the drop was actually created, and they'll tune that critical Weber number so they'll match the PCT for a particular experiment. And you'll end up seeing things like Weber numbers of one or two years.

MEMBER KRESS: That seems backwards to me.

MR. KELLY: But part of it's a limitation that if you don't have some kind of interfacial transport mechanism for the droplets, you have to do it based upon local conditions, because the reality isn't steady state. Normally, you know, you can analytically you can say, "Well, I know where it was," but that doesn't happen in the codes. And this is not an idea, this is what gets done, and that's what we want to improve on.

This gives you an example of what's out there now. You know, I searched, I looked for droplet data, and there's not a whole lot. I've got two set -- ah, my legend went away here. That happens sometimes when you cut and paste things in.

MEMBER KRESS: We have it on ours.

MR. KELLY: So the black triangles are from FLECHT SEASET, and these were done optically, high-speed movie, looking through a window, then you project it on graph paper and your graduate student draws circles around the drops and gets out. So in this case, each one of these triangles represents an individual reflood test. You notice most of them are at 40 psi, one is at 20.

Typically, the number of drops measured range between about 50 and 300. So these populations that -- each of these represents a population, and what this is is the Sauter mean diameter in millimeters, but they're fairly small populations, so you wouldn't really trust the shape of it and even the value. It give you a pretty good idea. But, again, we don't know what the flow conditions were. We know the pressure, but we don't know what the vapor velocity and we don't really know where the drops came from.

MEMBER KRESS: But those size droplets are, for example, too large to get Kelvin Helmholtz stripping off -- you don't get droplets that big, do you?

MR. KELLY: No, and you don't get droplets this big with a Weber number of 12.

MEMBER KRESS: With a Weber number of 12, you get a really small --

MR. KELLY: Well, no, actually, let me back up. You don't get -- these drops are too small for the Kelvin Helmholtz thing if you use the droplet terminal velocity. That's why you need to go to a Weber number of like one or two to get these.

DR. MOODY: Larry Hochreiter showed droplet data. Did he make predictions of those droplet diameters that he measured?

MR. KELLY: No.

DR. MOODY: He just measured them and there they are for --

MR. KELLY: Yes. And I'm going to talk about that in just a minute, about how we're going to use that data. I'll finish with this. These orange diamonds are from the ACHILLES test which were done in Great Britain, and in this case they did two reflood tests. This is the only -- they have more data, but this is the only that I had access to. So there are two different tests, and what you're looking at is each diamond is a value measured for one sub-channel. And I don't remember how these were measured.

And then we have some tube test data, some tests done at the University of Berkeley, I think it was an Inconel tube, I don't remember. And in tests done in Britain by -- I think it was Britain -- by Ardron and Hall, and these are quenching of a quartz tube with a little wire wrapped around it. And so here -- in both cases, they were using optical techniques. Each one of these represents a point in a reflood test, and these are sometimes as many as 1,000 drops in each one of these. Again, this is Sauter mean diameter. And so these were taken at a couple axial elevations in the tube, so at different distances from the quench front. Whereas these were all at the exit of the tube. So we have a very large difference between the two, and one of the questions is why, how can you measure droplets that are --

DR. BANERJEE: It's hard to get a nine millimeter drop.

MEMBER KRESS: Yes.

MR. KELLY: Well, we talked about Kelvin Helmholtz. Let's throw Rayleigh-Taylor into this. These are about as large as a drop can be and remain stable.

DR. BANERJEE: They must be these big chunks. I think Keith Ardron must have been calling those drops, he was English. Anything which has sort of a circular shape is okay.

MR. KELLY: Well, but --

DR. BANERJEE: They're big chunks of liquid, I think.

MR. KELLY: But, you know, that's what they were measuring.

DR. BANERJEE: Yes.

MR. KELLY: That's what was there. And this is actually Sauter mean, so they saw things even bigger, but you can't get much bigger than this. You just can't. You know, a Rayleigh-Taylor limit is about four times over -- which is about ten millimeters at these conditions, so you're not going to get much bigger than that.

So at any rate, if you were to ask me what's a droplet correlation --

DR. BANERJEE: One to two millimeters.

MEMBER KRESS: Yes. That's what I was thinking.

MR. KELLY: Well, except what we're measuring in RBHT, at least the couple of tests I looked at, are about half a millimeter. And that's one of the things we're going to have to look at is what's the difference here.

DR. BANERJEE: But maybe there's a spacer effect.

MR. KELLY: These have spacers too. Egg crate, not mixing vane, but definitely there's a spacer effect. But part of it may be the flow conditions being different, you know, vapor velocities being higher in these tests. Part of it may be the measurement technique. Here we're actually using an automated software to measure the drops. And the laser camera, the digital camera here has a very high resolution, better than what was available back in the 1970s. So if you look in the test report for these, they say that they -- I don't remember the number, but there's a certain diameter drop that they can't see. Anything below that, they can't see. Whereas here we can see some of those very small drops, and, of course, if what you're doing is Sauter mean, you know, ratio of the volume to the area for the population, if you're not counting small drops, it's very easy for you to overestimate the droplet size.

DR. MOODY: I guess in every case the droplets are formed from a bigger body of liquid, whether they're ripped off, stripped off, coaxed off.

MR. KELLY: Somehow. And probably several different mechanisms build a population, and we simply don't know.

DR. MOODY: Isn't that amazing, here we are 100 years later and we still don't know.

MR. KELLY: You know, there's thousands of papers out there on inverted annular or dispersed flow film boiling, and when you have to sit down and put their model for a code, you're scratching your head sometimes, and it's surprising. There's a lot of inconsistency between the papers that are there.

Okay. I talked about this. Okay. This was how are we going to get the interfacial heat transfer between the vapor and the drop. And the point is you can't really. I mean we're not measuring the rate at which droplets are evaporating, but we can get an indication of it by looking at the axial profile of the vapor temperature. So we can use the models that we get -- excuse me, we can use the data that we're going to get, the superheated vapor temperatures, the drop diameter and the vapor-entrained liquid flow rates.

MEMBER KRESS: Does the change in Sauter mean diameter give you any information on that?

MR. KELLY: It's --

DR. BANERJEE: Too small.

MEMBER KRESS: Too small?

MR. KELLY: The uncertainty in what you're measuring is much larger.

DR. BANERJEE: But the superheated vapor temperature is a function of the heat transfer from the wall and a whole lot of stuff going into that.

MR. KELLY: Right. But it can give you -- you can at least use it to help you select which models, and then once you have a set of models in and are doing a comparison, you can then validate their integral effect.

MEMBER KRESS: I would be tempted there to use existing correlations for single drops and swarms. I think some of those exist, don't they?

MR. KELLY: Yes.

MEMBER KRESS: I think I'd be tempted to say, "All right, we'll just put those in for that."

MR. KELLY: Yes. Whenever you think you know something, use it. That's what I'm doing. And so, for example, for the vapor-to-drop interfacial heat transfer, there are experiments where they either put a little sphere and coat it with a liquid film and put it in a wind tunnel and --

MEMBER KRESS: Yes. They've done a lot of that.

MR. KELLY: -- come up with those correlations, so that's what I'll use. The only catch is the multi-particle effect.

MEMBER KRESS: Now, it may be that your loading is so small that these act like single particles, but I don't know that.

MR. KELLY: Well, not quite. We're not at the dense solution where you have to worry about clusters like in fuel ignitors. So we're not having to worry about penetrating clouds of drops. But on the other hand, we have enough drops around that the rate's going to be a little bit more than the single particle. And that's where you might look like and what I've been doing is looking at the correlations for fluidized beds for the vapor-to-particle heat transfer in a fluidized bed.

DR. BANERJEE: Are you talking of the heat transfer coefficient on the vapor side or on the liquid side?

MR. KELLY: Vapor side.

MEMBER KRESS: Yes. I think --

MR. KELLY: Between the vapor and the particle.

MEMBER KRESS: Yes. I think you would generally neglect the liquid side for this size particle.

MR. KELLY: The drops are in saturation and it's high enough because the drops are small.

DR. BANERJEE: But, usually, the liquid side heat transfer can vary, of course, by a factor of two or three.

MR. KELLY: Yes. Here it's so much larger than the vapor side that it's really a no "never mind," and if you just do a conduction on the drop and have a fairly constant number, you're close enough, because it doesn't limit the rate process.

DR. BANERJEE: Well, the flow around the drop is turbulent, correct, by then?

MR. KELLY: But it's fairly -- if you look at the Nusselt number, you get a Nusselt number of about ten or less on the vapor side.

DR. BANERJEE: You see, if you had a very high conduction heat transfer inside the liquid compared to the convective heat transfer outside, you won't get any vaporization.

MR. KELLY: But these drops are saturated.

DR. BANERJEE: They are saturated.

MR. KELLY: By this point, you know, the liquid is broken up and everything, and now you've got little small drops. They're basically saturated.

DR. BANERJEE: If that's the case, then all that heat transfer will just go to vaporization.

MR. KELLY: Right.

DR. BANERJEE: And you don't care what happens.

MR. KELLY: Yes. We don't care what happens on the liquid side. I should have said that at the outset.

Now we're going to talk about drop diameter again. Sorry for the aside in interfacial heat transfer. What I said in the existing database, as you see some drop diameters, is there's a large disparity but you don't have a local fluid conditions, so you can't go and make any judgments. Well, this one set of data by Ardron & Hall they do report at least the exit conditions. So at the end of their tube, they give you the steam velocity. And so if I assume that steam mass flux were constant all the way back to where they made their measurements, I don't have enough data to make any other assumption, but if I assume that, I can do this plot, which is a non-dimensional drop diameter, it's a drop diameter divided by the cost number, so that's the square root of the surface tension over G delta rho, versus a modified Weber number.

It's modified in two ways. It uses the vapor superficial velocity rather than the relative velocity, so I don't know the relative velocity, I only know the vapor superficial. And, actually, you see that in a lot of annular mist things for droplet diameter. The other way it's modified is instead of using the droplet diameter, it uses the LaPlauce number. So that's what meant by modified Weber number here.

And because you're plotting it that way, and I picked this up with some annular mist stuff, you can draw these dashed lines that are straight, and what you'll see in your handout is that it says Weber number equals 12, Weber number equals four. There are two sets of data here. What I'd like for you to look at first are the diamonds. Those are the drop diameters that they measured for locations that were more than I believe it was 0.7 meters away from the quench front -- or maybe it was one meter. It's in your handout. So what you're basically seeing for these Sauter mean diameters is something that you would get for the Weber number criterion based on vapor superficial velocity of about four. Well, you would tend to believe the number of 12, but 12 would give you the maximum size drop. In the Sauter mean, because there's a population, a distribution, it's typically three to four times smaller than the maximum, which brings you right into this value.

Now, if you look at the open orange triangles, those were taken at a distance of a tenth of a meter, only ten centimeters, away from the quench front in these tests. So these drops haven't had much time to accelerate, haven't even had much time to break up, but they tend to be bounded by that Weber number value of 12 and then move down towards this limit. So this is an indication of something you might be able to use as a correlating factor, and that's one of the things I'll be looking at --

MEMBER KRESS: Droplet size versus position along the tube, without consideration of evaporation?

MR. KELLY: Well, actually, I didn't mean that. What I meant as a correlating factor was the vapor superficial velocity or the vapor momentum flux.

Okay?

MEMBER KRESS: Would you use the four or the 12 or the --

MR. KELLY: Well, for the maximum, you would use a 12, for the Sauter mean, a value more like the four. And this is just first order model here.

DR. BANERJEE: Anyway, the vapor velocity is very close to superficial, right? There aren't that many problems.

MR. KELLY: Right. And also -- well, yes. But I'm ignoring the relative velocity here.

MEMBER KRESS: The relative velocity is pretty low.

MR. KELLY: Yes. There's a difference between the vapor superficial and the relative, and what I'll be saying in this model, if I were to use this, is that where the drops are actually created the drops are initially standing still. They haven't accelerated to the terminal velocity up here. So their velocity is basically zero so that that vapor superficial is indicative of the relative, the relative at the top of this, say, fluidized bed before it becomes fully dispersed.

MEMBER KRESS: Even if it wasn't that way, you almost have an empirical factor.

MR. KELLY: Oh, it's going to be empirical.

MEMBER KRESS: Yes. So it wouldn't matter if that was the right interpretation or not would be a good way to look at it.

DR. BANERJEE: It's more or less in line with what you expect.

MEMBER KRESS: Yes.

MR. KELLY: Yes. And the point of showing the 12 and this is to show where some of those very large drops came from. These are drops close to the quench front that haven't had the chance to really accelerate and break up.

MEMBER KRESS: And they're not going to do much, I don't think, are they?

MR. KELLY: They're going to stay down.

MEMBER KRESS: Yes. So we don't know really a whole lot about them.

MR. KELLY: Yes. Eventually, they'll break up and then become important.

MEMBER KRESS: Yes.

DR. BANERJEE: It's a mess down there.

MR. KELLY: Yes. And one that I'm not going to model the details of for a long, long time.

MEMBER KRESS: But it looks like a Weber

number might be a good shot at getting --

MR. KELLY: That's what I'm going to try. And I got this from the annular mist literature, and I'm just adapting it for a different situation. And then I'm going to try to actually get other data to check this, and I'll explain that when I come back and give you the models, okay?

This is what Professor Hochreiter showed you from the RBHT --

MEMBER KRESS: I think we're going to lose one of our members here very shortly.

DR. MOODY: Your audience is shrinking, it's nothing personal.

MR. KELLY: I'll tell you what: Before I lose all my audience, let me go to my last slide. It's not in your handout.

MR. BOEHNERT: Powerpoint poisoning.

(Dilbert Cartoon.)

(Laughter.)

MR. KELLY: Since I'm noted for standing up here for hours on end and boring my audience with hundreds of viewgraphs, I just couldn't resist. This is what I was trying to desperately get to just before Professor Wallis left, because I thought he would enjoy this.

MR. ROSENTHAL: Let me be serious for just a second, and that is we read the consultant's report, and to some extent I think we have to agree that the experimental program and the analytic program wasn't tucked together as tightly as everybody would have liked, just what was funded when and who was on staff when and what not. I mean even we recognize that we could have done better, but I think that we're playing catch up but we're getting better. So I'm sure that you have to -- or would be writing additional consultant's reports, and if in those reports you included your views, having read this presentation, I'd appreciate it.

MEMBER KRESS: I think that may be all you get out of this meeting is a consultant's report, unless Graham wants to write a summary.

MR. ROSENTHAL: I'm not asking for anything more, but what I'm saying is that the prior reports were based on the Hochreiter were fair but negative. So if you have whatever -- if you change your views or have additional views, we'd appreciate seeing what they are.

MEMBER KRESS: Well, the other issue -- I share your concern about losing support for the rod bundle heat transfer test, and I don't know how to convey that concern at the moment, because I don't think we intended to have a letter.

MR. BOEHNERT: Well, you're writing a research report. Maybe you want to think about that, getting some --

MEMBER KRESS: But the research is what were supposed to focus on advance reactors, but I think this is probably --

MR. BOEHNERT: I haven't seen it, so I don't know.

MEMBER KRESS: -- appropriate.

MR. ROSENTHAL: Or even in your lesser reports. If you think that --

MR. BOEHNERT: Well, but the research report would be good because that's going to elevate this right to the top.

MEMBER KRESS: Okay. That's a good point.

MR. BOEHNERT: Yes.

MR. KELLY: And from my perspective, even in a consultant report, you know, you may even see a sentence that says, "This is a pretty interesting test series. We think we're going to get some valuable data." But then there might be 20 different ways in which it could be better, and they may be very true, and maybe we can make the program better, but when couple levels of Management above sees this, they see 20 and one, and they come away with, "Well, this -- my staff doesn't know what we're doing, our experimenters don't know what we're doing, let's just kill the program." So if there's a cover letter that goes with the consultant reports, I mean if you really feel this is a worthwhile program, just make it very clear in the front, and then tell us how to do it better. We don't mind that, because --

MR. BOEHNERT: Let me give some input here, because you have to keep in mind what are the intents of the reports the consultants provide the Subcommittee. It's basically for internal use, and in fact we kind of grapple with, gee, should we give you guys these reports? And I tend to say you ought to see this stuff because I think it's useful, but I always have to get the permission of the Chairman to do that. And he generally says, "Sure, go ahead." So that's why it's -- it's a different audience and that's why they tend to be maybe not as positive as you'd like, but it's basically for internal use.

MR. ROSENTHAL: I think that given the presentations that were made, I think that the reports that came in were fair. And we're all saying we need to do better. Having heard this presentation, if you have additional comments x

DR. BANERJEE: Well, you have to know that that when we listen to the RBHT Program, this program which is going on in parallel wasn't presented.

MR. ROSENTHAL: Right.

DR. BANERJEE: And maybe the right thing would have been to make it one more day at that point and put those two together.

MR. ROSENTHAL: Fair enough.

DR. BANERJEE: That would have made a difference.

MR. ROSENTHAL: A little bit of a history, by the way, if we go back like six months to a year, we would come in and have these like summary presentations, you know, a one-day or two-day marathon. And Professor Wallis said it would be more useful if we came in instead of with these big overview presentations where you got into no detail on anything is if you can have more detailed ones on specific topics. So Steve brought Vijay Dhir, Hochreiter, et cetera, and I guess we're losing something in maybe we're being too fragmentary. So I'm just saying some combination.

DR. BANERJEE: Yes. In fact, if there was even an hour presentation by Joe or Steve or something to put this in some context, yes, that would have been different.

MR. ROSENTHAL: Fair enough. Fair enough.

DR. BANERJEE: The only thing that still bothers me, to some extent, and I think it's a crucial issue, is this maybe we need to see something of what Steve and Joe have done in terms of sensitivity of these temperatures to the sort of modeling assumptions you were saying that you had made. Because we are sort of getting conflicting information on this, and I can see how it's coming about, because there are people who want to get S-RELAP or whatever the next code applicable for their fuel reload analysis or whatever they're doing, and so they're going to present a case that nothing needs improvement in these codes, we can do everything with it, right? I mean even if you --

MR. ROSENTHAL: I think you're still bleeding from yesterday.

DR. BANERJEE: Yes. If you take that at face value, then there's no program needed of any sort whatsoever. We know that's not true. But there is something there which is sort of in the middle ground I think that they've been maintaining that a lot of the dispersed flow, heat transfer flow, the nuances and so on, don't matter. What we've got is good enough to give us PCT, which I disagree with personally, because I think the work should be done. But we need to have some evidence presented to us that we can make the case stronger, because I believe this is a good program too.

MR. ROSENTHAL: We've had a lot of discussion, by the way, and you've heard a little bit from Joe, a little bit from Steve about developing metric again. The one thing that I think we're all convinced of is that PCT should not be the only metric.

MR. ROSENTHAL: Right.

MR. BAJOREK: When we did the best estimate methodology for the Westinghouse model, that was our original attack was to, hey, if we can get the PCT correct, everything might be all right. And that was thrown out and rightfully so, because when we did take a look at what the code was doing, we did start to find compensating errors. You're getting the right reason but for the wrong -- you're getting the right answer but for the wrong reasons. Where that comes back to haunt you is in a full-scale PWR analysis where if you might be correct for a test, which runs either at steady state or over a short time scale, now you think you have an answer that's got a good bias, small uncertainty. But it becomes very important if you take that uncertainty and propagate it over time. So if you think your answer is good and you do it only on PCT, you may be missing the fact that your heat transfer coefficient may be off by ten, 20 percent. And when you propagate that in a code that goes for several hundred seconds, then you could be mispredicting your PCT by hundreds of degrees.

MR. ROSENTHAL: And along that line, we're trying to -- you know, this idea, you heard the expression, large-break LOCA center, and that is that if on probability you dismiss the double-ended guillotine break, I don't think that you'll ever dismiss breaks that depressurize the plant, you know like surge line. Then people will immediately take the margin that they've gained by that, you'll be up against new limits, and then you have to ask is your code capable of these other issues? So for all these --

DR. BANERJEE: Well, one of the points I made in my last report was that NRC, now I don't know which appropriate branch of NRC it should be, should develop more than just the PCT criteria for evaluating a code. Maybe it should have -- this is up to NRC to decide what is the most important factors, but say time to PCT could be important too or there could be a number of other things which I can think of, and I'm sure you can, which are sort of would give more credibility to these calculations, which have been presented by all the vendors and people like that in licensing their codes. And there should be a short list of four or five things that they have to get right, more or less, before we sign off on these things. Because PCT is -- you know, they adjust stuff and they finally get the PCT and they say, "Well, we've assessed 59 experiments now" or whatever the number is, "and we're fine."

MR. BAJOREK: If you'd like, I'll give you part of a presentation we made December last year, and we covered exactly some of those concerns where we said quantification of code performance it's conservative, you compare the PCT, and we basically said that's unacceptable. For reflood heat transfer, we would look at more of a list of parameters which would go from steam cooling heat transfer coefficient, dispersed flow heat transfer coefficient, inverted annular heat transfer coefficient. Minimum film boiling temperature has a very big effect in your blowdown cooling, a carryover fraction. We haven't said anything on that yet, but if you remember watching that movie from RBHT, we were still well above the minimum film boiling point, but we saw lots of water in this, very high carryover fractions. We need to get that correct and level swell to make sure that you aren't frothing up your quench front to a higher elevation than it should be.

So I can give you this, and that's when it comes to assessment and in our model development we're not going to use PCT except as a --

DR. BANERJEE: But somehow it has to get through to NRR, and they have to say, "Okay, these are five or six variables that we look at."

MEMBER KRESS: You've got to change the rule.

MR. BOEHNERT: You have to change the rule.

MEMBER KRESS: That might be a problem. I guess given the hour and the time, I want to thank you guys for a very interesting, productive meeting, and I think at this point I'll declare the meeting adjourned.

(Whereupon, at 1:20 p.m., the ACRS meeting was concluded.)