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UNITED STATES NUCLEAR REGULATORY COMMISSION'S

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

NOVEMBER 28, 2001

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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

(ACRS)

THERMAL-HYDRAULIC PHENOMENA SUBCOMMITTEE

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WEDNESDAY,

NOVEMBER 28, 2001

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ROCKVILLE, MARYLAND

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The Subcommittee met at the Nuclear Regulatory Commission, Two White Flint North, T2B1, 11545 Rockville Pike, at 8:30 a.m., Graham B. Wallis, Chairman, presiding.

COMMITTEE MEMBERS:

GRAHAM B. WALLIS, Chairman

THOMAS S. KRESS

F. PETER FORD

GRAHAM M. LEITCH

WILLIAM J. SHACK

VIRGIL L. SCHROCK

PAUL A. BOEHNERT, Staff

RICHARD LOBEL, Staff

ALSO PRESENT:

STEVE BAJOREK

HARV HANNEMAN

ROBERT HENRY

JOSEPH M. KELLY

NORM LAUBEN

JOHN MAHAFTY

JOE STAUDENMEIER

MIKE TESTA

JENNIFER L. UHLE

TOM ULLSES

WEIDONG WANG

A-G-E-N-D-A

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

(8:32 a.m.)

CHAIRMAN WALLIS: This is a meeting of the ACRS Subcommittee on Thermal-Hydraulic Phenomena. I am Graham Wallis, the Chairman of the Subcommittee.

Other ACRS members in attendance are Peter Ford, Thomas Kress, Graham Leitch and William Shack.

ACRS consultant in attendance is Virgil Schrock.

The purpose of this meeting is for the Subcommittee, firstly, to continue review of the NRC Office of Nuclear Regulatory Research Activities pertaining to thermal-hydraulic phenomena in support of the ACRS annual report for the Commission on the NRC Safety Research Program.

And secondly, discuss a proposal by the licensees of the Point Beach and Beaver Valley plant to perform more realistic analysis of the containment design basis accident EPRI/MAAP code.

The Subcommittee will gather information, analyze relevant issues and facts and formulate proposed positions and actions as appropriate for deliberation by the full committee.

Paul Boehnert is the cognizant ACRS staff engineer for this meeting.

The rules for participation in today's meeting have been announced as part of the notice of this meeting previously published in the *Federal Register*, November 15, 2001.

Portions of the meeting will be closed to the public as necessary to discuss information considered proprietary to the electric power concerns. A transcript of this meeting will be kept. And the open portions of this transcript will be made available, as stated in the *Federal Register* notice.

It is requested that speakers first identify themselves and speak with sufficient clarity and volume so that they can be readily heard.

We have received no written comments or requests for time to make oral statements from the public.

Now, our hope as a Subcommittee is that today's meeting will be the highlight of the year as we hear about all this great work which is going on. I call upon Jack Rosenthal to get us started.

MR. ROSENTHAL: Thank you. I'm Jack Rosenthal. I'm the branch chief of the Safety Margins and Systems Analysis branch in the Office of Research, and I just have some introductory remarks and then, as you can see from your agenda, Jennifer will talk about applications, Jennifer Uhle. And then Joe Kelly about code consolidation and Steve Bajorek about our experimental program, and I'll get help from much of the other staff.

But I wanted to make some introductory remarks in a few areas. The easiest one is that we've accumulated a fair amount of hardware now, and so at least the capability to run the codes. And we're proud of a new PC cluster that we're doing in CFDR.

The next thing, more important, and I don't want to embarrass my staff, but we have now in fact I think a world premier staff of people that have come on board, and several recent ones.

Joe Kelly was at the NRC and has returned.

Steve Bajorek was at Westinghouse in Kansas State and is now with us.

Joe Staudenmeier and Tony Ullses were in NRR and have joined us.

Chris Murray was at Penn State and has joined us.

And so we have a staff that's now capable of analyzing experiments, developing the codes and doing the safety analysis. And we should be proud of the staff.

In terms of products, okay, we are using our codes to make regulatory decisions. We're using MELCOR to come up with a source term for 50.44, combustible gas control.

We're using RELAP and TRAC to modify 50.46 ECCS.

There was another subcommittee out of OSU, and you heard that we're using RELAP and CFD and REMIX to do PTS.

We're using RELAP SCDAP to do steam generator high temperature severe accident work, and you had a separate briefing on that.

We did some work on combined injected rod LOCA as part of the CRDM issues, and we used RELAP and PARCS and for AP1000 we'll be using RELAP and TRAC. For our work on synergy we're going to be using TRAC and PARCS.

So we're actually using these codes to make regulatory decisions, and that's very healthy. And much of that work is being done in-house, and that's very healthy.

The last point that I wanted to make is that in prior years it was typical to have a vendor come in with some calculations and what we would cause our contractors to do some calculations to check vendor calculations. But the regulatory decision, to a great extent, was based on what the vendor came in with. But for changing the rules, we're looking at this synergy issue or the objective art issue. These are safety issues that are before us. And we're using our analysis to make those regulatory decisions. We don't have a vendor to balance this stuff off against, except in the AP1000 case. And that puts a greater burden on us.

The entire Office of Research is paying far greater attention to QA than it did in the past, because we're using this for regulatory decisions and we're trying to do the code development and configuration management, etcetera, to modern standards in support of those regulatory decisions.

It's the first time that somebody gave me a microphone. I didn't realize I talked softly.

With that short introduction, I'd like to turn it over to Jennifer for about an hour -- Jennifer Uhle who is the assistant branch chief now in our branch.

MS. UHLE: We're going to do something a little different. Usually we talk about the status of our code development efforts and then talk a little bit about applications. But I think you guys are tired of hearing it in that order, so today I'm going to start off with what we're currently using our tools for. The question gets asked what do you use the codes for; they're, of course, time-consuming to develop and we have an invested effort in that. And so we're going to be answering this question for you, hopefully.

So I'm just going to talk a little bit about the branch mission, the current uses of the codes at NRC. You know, the current applications we have for licensee submittals, generic issues, risk-informing regulation, design certification. And we just draw the conclusion that you'll find on the summary side now, and that is that you do utilize the codes, they are used at NRC for field application. And it is our goal to continue to improve this analytical capability to respond to these emerging issues.

We always discuss about the consolidation effort. That effort, of course, sometimes gets in trouble for the fact that we are not making improvements to the physics as quickly as some people may want. And I just want to focus or make the statement that we are consolidating first, we are making improvements as we need to respond to these applications as they arise. But by the end of 2002 we'll be in a prime position to have one code. At that point in time we'll really focus on improving the physical models and as well as the very detailed developmental assessment. And Joe Kelly and Steve Bajorek will be discussing that further.

I'm going to skip over this, because I'll do that on the summary side, but Jack Rosenthal had pointed out that we do have five recent hires that have really added to the capability of the branch, and you'll be hearing from them.

MEMBER LEITCH: Jennifer, maybe it's obvious, but I'm not sure I understand. What is the advantage of a consolidated code?

MS. UHLE: We used to have four thermal-hydraulic codes. And we used RELAP for PWR small break loss of coolant accidents and transients. We used the TRAC-B code for large break loss of coolant accidents for PWRs. We used the TRAC-B code for BWR applications that only required one 1-D kinetics. And then we used the Ramona code for places that required 3-D kinetics capabilities. And because of that each of these codes have very similar features. They're not that different, and so we had a lot of maintenance points; that wasn't an efficient way to operate. It was more costly than it needed to be. So when we needed to make improvements we, in a sense, had to do it four times over. So improvements weren't made as fast.

Additionally, the user base was distributed across these four codes. So, again, instead of moving forward we were sort of moving in parallel and not making improvements as fast as we would have liked.

Additionally, each of the codes had a different input deck. And so when you're looking at maintaining these large input decks, these very complex models, you would have to do it for two different inputs, because the PWRs would use RELAP and TRAC, the BWRs would use Ramona and TRAC-B. So it just wasn't an efficient way to proceed, especially with the budget reductions and the fact that we wanted to bring the technology in-house and have in-house staff to develop and maintain and use the codes for the regulatory applications.

So that was the decision to go with the consolidated code. And what we did is we selected TRAC-B as the base of that consolidation, and we modernized it so it's a new architecture. It's very easy to modify, very easy to extend to other applications and to couple to other tools like a CFD code. We haven't done that yet, but this is where we're heading.

And what we've done is we've just taken the features that were in the different codes, all of the four different codes, and we've only taken the different things that the other codes could do and brought them into TRAC-B. We now call it TRAC-M because it's modernized, and we're trying to find a name for the code. It's a very sore subject.

MEMBER LEITCH: And this will be consolidating code that NRC --

MS. UHLE: Yes. Yes. It's in-house expertise. We work with contractors. Gil Actess is in the back of the room. He's at ISL, Information System Laboratories. John Mahafty is Penn State University. He was an original developer of TRAC-B at Los Alamos. He's at Penn State now. He is our numerics guru. And Tom Downar at Purdue University is working on -- is more of the original developer of the PARCS code. Now we don't use the code as stand-alone in PARCS; we've coupled just the kinetics routines to TRAC. So it's a modular, so it's the PARCS modular. But, again, we work alongside of the contractors, the staff does, and we've really developed in-house expertise. Tony Ullses is now starting to do PARCS development so that we can rely more on in-house staff and rely on contractors for specialized skills, so it's not part of the staffing plan to have a full-time employee on one of those particular skills.

MEMBER LEITCH: Now, when I think of the consolidated code, I think in terms of simplicity and efficiency. That raises sort of the feeling that maybe there's some compromise of precision for the individual codes, a specific code for Bs and Ps and small break. large break and so forth. Is any of that precision compromised?

MS. UHLE: That was a concern. I think Dr. Shack is of that mind. I think that Dr. Zuberg was of that mind as well. And the bottom line is we just couldn't continue to operate that way. We couldn't make any more improvements to the codes because we were spending all of our resources on maintenance. And so as these issues were identified, we just didn't have the staff or the budget to be able to make the changes. So in a perfect world maybe that would be the best way to go, if you had infinite resources and infinite time.

So the consolidation plan is that we are forming the consolidation activities. We can read all of the input decks from all the other codes, so we've recovered the input decks. And at this point or shortly we'll be starting the developmental assessment phase. And for the targeted applications of each of the predecessor codes, we will be comparing the results of the modernized code to the other codes to make sure that we're as good as the other codes for those applications.

And the way that the architecture is set up, it's really the physical models; wall drag, interfacial drag, interfacial heat transfer, et cetera. Those are the points that made the codes different. And, for instance, the solution of setting up the numerics, solving the matrix, performing input processing, performing or exporting the answer to a graphical tool; those are all common things. So really the only big differences between the codes is the physical models. The architecture of the modernized code is allowing us to do component-specific physical models.

If I'm a pipe, I'm going to use this interfacial drag, this wall drag. If I'm a channel component in a BWR; okay, now I have a rod bundle there, the interfacial drag is going to be different than it would be in a pipe of the same hydraulic diameter. So because of the architecture it's set up to very easily incorporate component specific physical models which will allow us to be as good, and then eventually better than the old codes. So we're accommodating that concern.

We have to prove that to you, but that is our goal.

CHAIRMAN WALLIS: Of course, you're also checking that this pipe really is a pipe and isn't a pump because --

MS. UHLE: Yes, of course.

John wanted to say something. John Mahafty.

MR. MAHAFTY: This is John Mahafty.

I'd just like to make a comment. I've been kicking around with computers since they took up the whole room and they had the kind of computer power you have in your watch right now, and I understand the concerns about efficiency from that kind of ancient perspective. But the fact is now memory on computers is massive and it's cheap. Disk space on computers is massive and cheap. So that it doesn't matter to me if I've got a large code with a bunch of special subroutines for interfacial guide and BWR and another set for interfacial drag and PWR core; if I'm running BWR, that stuff never gets swapped in where the action is, which is your real local memory on your chip. It sits out somewhere and gets swapped into virtual memory. It's not impacting you from the standpoint of the efficiency of operation of the code, but it's there when you need it and it's tied together with all these things that everybody needs to make the maintenance and the improvement of the package important. And things don't get overlooked as much.

Now I can remember the old days. It used to drive me nuts. We'd find some problem with TRAC-E and we knew that it was an important issue that the people in BWR side, maybe we only had five of them looking at it and communicating that and getting all of this information to run off, it would sometimes take years. But now it's in one place and there are people thinking about it as a whole, so you don't lose improvements that are applicable to everything, and it's a big advantage.

MR. ROSENTHAL: Let me do a follow-up then, if I may. And, John, you're absolutely right. Every time we turn around, of course, it's a tenth of what it did before for more horsepower, computer horsepower. But people are expensive, and it takes over a staff-year to create an input deck, one of these really big input decks. And so you really gain some efficiencies by being able to use decks that were previously created or have common decks for purposes, etc.

So I think we're really going to achieve some efficiencies.

MS. UHLE: I just want to add to the idea of taking a year to develop a plant deck, and that was again in Jack's old time frame, in the olden times, in the time of the dinosaur. That's how long it used to take.

I'll be doing a bit of a presentation on the graphical user interface, which we've also recognized the inefficiency associated with plant modeling and feel we have a program to handle that. We've demonstrated that before to the ACRS, but I'll be touching on some of those points that I think bring that to light, that we have improved the efficiency of the plant modeling.

MEMBER SHACK: Just out of curiosity. What language have you settled on? I mean, these were originally --

MS. UHLE: Fortran 90.

MEMBER SHACK: Fortran 90.

CHAIRMAN WALLIS: Well, the plant modeling involves people looking at a lot of drawings and then turning this into computerese. I would think with a new plant and the plant is already a computer model before it's even being built and you don't have that problem; having to look at drawings and figure out where the pipes go and --

MS. UHLE: You're assuming that we communicate to the licensee.

CHAIRMAN WALLIS: Well, if that's the problem, you need to fix it.

MS. UHLE: At this point that is something that we have thought about, being able to scan in something from the architectural engineers.

CHAIRMAN WALLIS: That's the way that industry does it.

MS. UHLE: Right. Right. Well, they don't build an input deck by scanning in the graphics --

CHAIRMAN WALLIS: But in the automobile industry, if they want to get a piece from a supplier, they just send them a computer model of the stuff that they need to know and they've got it.

MS. UHLE: But the computer model's not going to have lost coefficients, reverse and forward lost coefficients. I mean they're going to have geometry, and that's what we can recover.

CHAIRMAN WALLIS: So you have to figure that out.

MS. UHLE: But the rest of it is going to still require somebody knowing the code, knowing what each of the input is required.

Well, I mean we have talked about that as an ideal way to go, being able to recover any of the geometric information.

CHAIRMAN WALLIS: Right.

MS. UHLE: We have talked about that.

MEMBER SHACK: They probably don't have computerized geometric models in most of these plants.

MS. UHLE: They'll have like tech -- what is it called -- CAD drawings. They'll have CAD drawings. And so we've thought about being able to take in the data from the CAD drawings and getting the geometry. And that is somewhere we want to head, we're not there yet. And of course, at NRC we don't have CAD drawings, so it would require interface with the industry.

I want to talk to you about the mission of the branch, to give you an idea that this is the Safety Margins and Systems Analysis Branch. So we are tasked with the idea of maintaining these analytical tools. We're also tasked with maintaining the infrastructure for the understanding of the phenomenology to help out NRR on more complex issues. And this is applied to severe accidents as well as the thermal-hydraulics, and as well as the field behavior. What you're hearing from us today is the thermal-hydraulics, but we are hoping to follow suit in the severe-accident and field-behavior areas so that the team can seamlessly interact throughout the branch; and that includes coupling the computer tools, the field behavior code to the thermal-hydraulics code, the severe accident code to the thermal-hydraulics code, and bringing in-house expertise. And so it's an exciting time in the branch.

Hopefully, if all the good things you hear today, you can think that's going to be applying to severe accident. And if it's something you don't like, well then tell us so we don't make the same mistake twice.

MEMBER KRESS: When you say criticality safety, what all is wrapped up in that?

MS. UHLE: Criticality safety originally-- well, for instance in the dry cask PRA they have asked the branch --

MEMBER KRESS: Okay. You're not just limiting this to reactors then?

MS. UHLE: No. No. For instance, the most --

MR. ROSENTHAL: The burn-up credit comes from in our branch analytically and we provide, as a user need --

MEMBER KRESS: I understand what you mean.

MS. UHLE: Tony Ullses, in the back of the room, is currently running some calcs with the dry cask PRA to just double check that there's, obviously, very -- I don't know what the word is -- low, low, low probability that anything could happen and cause a criticality accident. And so he's doing that in the branch, because we have the reactor physics tools. We've coupled the reactor with some kinetics tools, but we're getting the reactor physics and with that there is quantum PYLAR codes for criticality.

MEMBER FORD: You mentioned safety margins on this slide. Is there any plans in the future to incorporate, for instance, aging phenomena for construction materials?

MS. UHLE: We are going to talk -- I will actually talk a little bit about that with respect to the power uprate synergy program that we're undergoing at this point. And Joe Staudenmeir is the lead on that. But, additionally, we do interact with the engineering division, well for instance, through the pressurized thermal shock rule we are looking at risk informing the PTS rule in the way that we're giving them thermal-hydraulic information and then they're putting it into their FAVA code for the fracture mechanics.

So the whole office, really, I think it's a nice tie and we're all starting to interact a bit more. There's a lot of cross-division, cross-branch, as well as in the branch cross-section interaction.

MEMBER FORD: Forgive me, because I'm new to this organization. Is this a new mission or has this been a mission you've had for ten years?

MS. UHLE: I think this is a mission that we've always had, but I think the way NRC is currently operating we're trying to do it in a more efficient, more --

CHAIRMAN WALLIS: Integrated.

MS. UHLE: Yes, integrated and more of a outcome-oriented, and all these management buzzwords that make you sick. But, you know, looking at the user offices as our customers, looking at the fact that we're supporting the PRA work as our customers. And because of that, I think this has helped as far as people understanding who is doing what and who to go to talk instead of not knowing and calling their professor or, you know, and not knowing what NCR is currently doing.

CHAIRMAN WALLIS: It also helps if your customer is really listening and is in on that decision making.

MS. UHLE: Oh, yes, right, and that goes to the user need process.

Okay. So we're getting into the activity, because now I'm talking about power uprates synergy. So you read my mind here.

This was actually I think at one point discussed by the ACRS, the full committee, looking at the potential for synergy. Synergy coming from the fact that we're operating with higher burnups, higher power and plant aging. And we are currently looking at license amendments for BWRs up to 20 percent power uprate.

Also the Office of Research -- I don't want to be giving you a full review of this program because I'm not the lead on this program, but I just want to talk about our branch's use of the codes to support this program.

We've got an independent study we'll be doing; the best most rigorous method we could do would be a level 3 PRA before and after the results, but we don't have the time or the staffing, or the funding to do that. So we're trying to do this in an efficient way, so due to the time and funding limitations we're going to focus on components and the scenarios of high risk significance, and using the knowledge that we have in the field to point to the things that are most sensitive to the changes. We're looking at the results of NUREG-1150 as a guide. And we're going to be looking at generic safety issues and reviewing them to see that if there was something that is affected by any of these changes within the operations.

MEMBER KRESS: Now let me see if I understand that. You will do a level 3, but for just selective sequences?

MS. UHLE: Yes.

MEMBER KRESS: And those sequences will be the ones you feel are more important?

MS. UHLE: Yes.

MEMBER KRESS: And you'll pick out a number of plants to do this with?

MR. ROSENTHAL: The level 3 would include consequence analysis.

MS. UHLE: Right.

MEMBER KRESS: Yes, you'll forget about

LERF and go to the full consequence.

MS. UHLE: Well, we're looking at consequence on the synergy program after listening to the advice from Joe Staudenmeier.

CHAIRMAN WALLIS: So you're going to look at casualties in the surrounding countryside and things like that? I mean is that part of your mission?

MS. UHLE: Yes, I mean it's going to result in a source term and then --

MEMBER KRESS: Well, before we get carried away, I think I'd like to lend the Subcommittee's support to your doing that. Because LERF can only do it when you are talking about power upgrades.

MS. UHLE: Well, I mean, the focus is looking at the source term.

MEMBER KRESS: Yes, absolutely.

MS. UHLE: And we are going out to source term.

MEMBER KRESS: And we really ought to do the level 3 in this case rather than stop at LERF.

MS. UHLE: And if we have source term going to, you know, the health effects, I mean that's -- I don't see how that's a big step.

MEMBER KRESS: Will you use specific sites for this or some sort of a --

MS. UHLE: Joe, do you want to stand up and talk? Joe Staudenmeier is the lead on this. I mean, maybe a lot of it could just be my misunderstanding, but I mean if we're doing source term, I don't see why we wouldn't do the final health effect. I mean, that's just a matter of running the Max code, which takes 5 seconds. But maybe I'm offering work that the office isn't willing to do. I don't know.

MEMBER KRESS: Stick by it, I hope you do. Go ahead.

MR. STAUDENMEIER: Joe Staudenmeier.

Tentatively we had planned to do consequence analysis. We don't really have all the details of this whole study all worked out yet, but tentatively we'll look at the consequence analysis with the PRA people. We're going to provide guidance based on NUREG-1150 study on what sort of sequences we should be looking at and also engineering is providing information on materials and things like that.

MEMBER KRESS: Okay.

MR. STAUDENMEIER: It's hopefully going to be an integrated study that gives consequence numbers, or at least what we think may be resulting consequence numbers being more like a prioritization analysis rather than a full level analysis.

MEMBER KRESS: Would you use the SPAR models for this or --

MR. STAUDENMEIER: I don't know the details of what GRA would be

MEMBER KRESS: All right.

MEMBER FORD: Could you -- and you ought to be able to put the government timing and funding limitations off. In light of, for instance, synergism between higher power flux and plant aging from a physics point of view, there's a lot of things which are not understood in a quantitative sense. So there is a lot to do beforehand. So far as timing and funding limitations would you have these for is it 3 years? --

MS. UHLE: I think we have funding out for another 3 years. Is that right, Joe? Three years?

MR. STAUDENMEIER: Yes, the program is scheduled to go over three years. The total contracting money for the first two years, I think, is about $800,000, and from last year about $1500.

CHAIRMAN WALLIS: So probably three years most of these BWRs will already have had power operation approved?

MS. UHLE: That's coming out of NRR. What we're doing is an independent analysis.

CHAIRMAN WALLIS: I know, but it's so interesting. So your report will come out after the fact and then --

MR. STAUDENMEIER: We are working on a confirmatory report and we are not going to concern ourselves with the licensing process. Unless we do find something. If we do find something it will affect licensing, obviously, we'll provide that information.

MEMBER KRESS: Better late than never.

MEMBER SCHROCK: So the BWR presentations, these upgrades claimed that the bundle power is not increased, and the flux therefore is not increased. So you have a situation in which the total power in a system is increased by --

MS. UHLE: Right.

MEMBER SCHROCK: -- working the bundle so they're both hanging over the mark. But it doesn't come through clearly to me how you're dealing with the increased total power, I mean in the context of source term and things of this sort. You don't have a higher local power density, and so the onset of failures is not changed in the sense of local conditions, but the amount of the core that's involved in the onset of failures is increased.

MS. UHLE: Right.

MEMBER SCHROCK: How is that --

MS. UHLE: Affecting source term?

MEMBER SCHROCK: Yes.

MS. UHLE: Well, I mean with the higher power the higher fission productivity and then of course if you're getting --

MEMBER SCHROCK: Well, of course. But the issue is how much of it gets out.

MS. UHLE: Right. Right.

MEMBER SCHROCK: And how does the failure propagate?

MS. UHLE: Right. But if we're looking at on the very unlikely situation where you'd have a core melt, then you know it's going to be the average of the core that's determining the source term, not just the hot bundle.

MEMBER SCHROCK: Yes --

MEMBER KRESS: You would get more out sooner.

MEMBER SCHROCK: Oh, I'm sure you'd get more out, but my question is how it's being determined in these new evaluations.

MEMBER KRESS: Well, it depends on how they nodalize the core.

MEMBER SCHROCK: Because as I read the stuff that we received, I was reading there's an increase in flux, there's an increase in temperature, there's an increase in this and that, which we heard in the arguments in favor of the uprates it didn't exist because we don't have an increase in bundle power, we don't have an increase in center line temperature of the fuel, we don't have this, we don't have that. Whereas, the description that I read sounds to me like it's contrary to the claims that were made in the evidence supporting the approval of these 20 percent uprates.

MS. UHLE: I think Joe wants to make a statement here.

MEMBER SCHROCK:

MS. UHLE: He's behind you.

MR. STAUDENMEIER: The source of the core-melt progression in source term release is something we're going to be evaluating under this program. We plan on planning some severe accident calculations. I think we'll probably be talking in more detail about this program, coming up with a presentation sometime, I imagine being the first half of next year coming down to explain what the parts of our program are and schedule a presentation just describing this in more detail. Right now Jennifer has a long way to go, and this may not be a good time to discuss it any further.

CHAIRMAN WALLIS: She has a long way to go in terms of the slides she's going to cover. You're going to cover 19 or 20 or so?

MS. UHLE: Well, I don't know. I'm trying.

Along those lines, though, I just want to point out that we will be using the codes in the branches, the severe accident analyses with melt core, talking about the melt progression and then the thermal-hydraulic codes. And so we'll be focusing on the risk-significant events and the risk-significant components providing input as success criteria, operator action times, stating the case of ATWS, and also different component failure modes.

If it's a DET the division of engineering to look at the effect of additional hydraulic loads on the components, crunch the numbers and come out with a new risk value.

So I'll skip the next one there, because I think we've talked about that.

One thing I want to talk about, though, is the fact that we're using this code and how can you be assured that we are getting an okay answer for, say, the BWR cases. The next stage in the consolidation is very consistent with the fact that we need to do a developmental assessment. And so what we're going to do is that we are focusing on the BWR models first. We'll be looking at them in the consolidation matrix, the DA matrix, the BWR models. And that's, of course, good timing with respect to the BWR synergy. So we will be running a developmental assessment for BWRs with the code, and we'll be using the TRAC code for that.

We are currently involved in the Peach Bottom Turbine Trap using the TRAC-M code in the PARCS 3-D kinetics module. And Tony Ullses -- he's in the back of the room -- he is the lead on that.

Based on the results, and I have a few results for you to show you, we found that we know we're going to have to do some BWR specific physical models. And what was put in was an interfacial drag model was changed and -- I think it was the two phase loss multiplier for -- I'm sorry, the two phase multiplier for the wall drag that was important in the BWR sense.

Once we replaced those models and reran

the Peach Bottom Turbine Trip for just the CHAN, you know the BWR channel component in the core, we got very good answers. We're still looking at it again to focus on what models we need to change to improve those answers. And I just want to --

CHAIRMAN WALLIS: If you take the RELAP models and put them in TRAC, do you predict the same answers as RELAP predicted?

MS. UHLE: If we were to do that, it would take time to do it. We haven't done that. But in general -- in general you would say yes.

CHAIRMAN WALLIS: You would expect --

MS. UHLE: If we run in the semi-implicit numeric scheme.

CHAIRMAN WALLIS: It's a test that we probably should run, isn't it, so that there isn't something peculiar about TRAC which gives different answers from RELAP with the same models?

MS. UHLE: Well, that's where the developmental assessment work will --

CHAIRMAN WALLIS: You haven't done that yet?

MS. UHLE: That's what the next stage is.

CHAIRMAN WALLIS: I mean, it's related in a way to Graham's question; when you consolidate these codes, the question will arise probably about whether or not you're recapturing what the codes could do before.

MS. UHLE: Right. Right. And so that's why the next phase of consolidation is the most important phase.

CHAIRMAN WALLIS: So you won't really find out if there's a hitch to consolidation until you get to that point?

MS. UHLE: You have no faith.

MEMBER SCHROCK: I've expressed concern for years about using interfacial drag as a tuning device in the codes. And what can you say about what you're doing now that's any different than what's been done before? In terms of the physics, isn't it necessary to have a clearer explanation as to why you needed a different model --

MS. UHLE: Yes, we do.

MEMBER SCHROCK: -- for one reactor as compared to the other?

MS. UHLE: Yes. In particular the CHAN component. The CHAN component is essentially a pipe. And if you put in your hydraulic diameter --

MEMBER SCHROCK: That's in the code, but in the reactors they're rod bundles.

MS. UHLE: I know. Right. Exactly. So currently in the code, in the TRAC-M code, if you're going to have a CHAN component, it is a pipe with a different hydraulic diameter. So your interfacial drag is going to be much -- you know, calculated to be very high. Because in reality you have this channel there -- sorry. You have this rod bundle there and you know with the same hydraulic diameter you have a much lower interfacial drag. So in that particular instance we have to put in an interfacial drag model that reflects the fact that there is a rod bundle in this pipe. That's physically based.

MEMBER SCHROCK: Well, I didn't, I guess, fully understand the argument.

In both reactor systems you have rod bundles. You do have pipes. And so now you're --

MS. UHLE: In the PWR we have a 3-D hydraulic model, so it's not a pipe because the hydraulic is three-dimensional -- you can have cross flow, what have you, you don't have the channel boxes.

MEMBER SCHROCK: Well, there's a scheme for accounting for cross flow. Calling it three-dimensional is a stretch.

MS. UHLE: Not in the TRAC code. It's a three-dimensional model, three-dimensional hydraulic model.

MEMBER KRESS: They don't use these little --

MS. UHLE: We don't use the cross flow connections.

I think Joe Kelly wants to say a few words, maybe clear it up.

MR. KELLY: This is Joe Kelly, from research. And I think I can clear that up, Professor Schrock.

In TRAC-P, its mission was for large break LOCA. So consequently, interfacial drag in the core for normal, you know, bubbly flow was never considered a priority. They were always worried about reflux, first of all, from boiling etcetera. So the models that were developed for that actually were fairly crude, based on bubbles and slugs where the slug size is limited by the hydraulic diameter of the channel. But, as you know, in an actual LOCA configuration the vapor structures actually span a number of subchannels, and it can lead to much higher slip than you would get if you only took into account the hydraulic diameter of a rod bundle.

So, because the modeling TRAC-P is relatively crude, it was in fact never extensively accessed against rod bundle void fracture data. There's no expectation that it would do a good job. And what we've found is, yes, indeed it does not do a very good job when you apply it to BWR operating conditions. And so we needed to implement a model, and what we chose was the one from TRAC-B that actually does try to model the interfacial drag in a rod bundle. And that's what was done for Beach Bottom Turbine Trip, and I'll talk a little more about that.

MEMBER SCHROCK: Okay.

CHAIRMAN WALLIS: When we review other codes, we've been reviewing other codes over the past few years, we get a stack of stuff like this, you know, the documentation. All the equations are spelled out, justified, and the verifications are explained. Are we going to get that for your code?

MS. UHLE: Yes.

CHAIRMAN WALLIS: When do we get that?

MS. UHLE: End of 2002.

CHAIRMAN WALLIS: That's a long way.

MS. UHLE: Well, we won't know what physical models we're putting in the code until the end of 2002, when we've done the developmental assessment to make sure.

CHAIRMAN WALLIS: Well, do you have a

draft --

MS. UHLE: We have a theory manual for the--

CHAIRMAN WALLIS: If you had a draft version of the theory manual or something, we might give you some useful input before end of 2002. And if we're going to raise any problems --

MS. UHLE: Are you offering?

CHAIRMAN WALLIS: -- the sooner we do it, the better.

MS. UHLE: So you're offering to be a contractor?

CHAIRMAN WALLIS: Well, it just turns out that in a peculiar way we should never fault the ACRS. We act as sometimes reviewers of these codes and we find what look like -- not what I should call errors, but --

MS. UHLE: Right. We have a theory manual for the base TRAC-P code. We can provide that to you as well as --

CHAIRMAN WALLIS: It might be useful if we saw that before you think you've got the final version.

MS. UHLE: Right. I'll report that back, although my management is here now.

CHAIRMAN WALLIS: Because that would be really embarrassing if we found an error in some fundamental thing after you think it's final.

MEMBER SCHROCK: We used to complain about the lack of recommendation on TRAC. And I remember at a meeting in Saratoga Springs -- from Las Alamos I guess. Said that the latest version was fully documented and I said "Well, I've never seen it." And so there was some correspondence between he and four others.

I think that he's under the impression that it's been reviewed by the ACRS. I don't think it ever appeared at the ACRS.

MS. UHLE: Okay. Well, I mean that would be very helpful to us if you're willing to do that.

CHAIRMAN WALLIS: But you're going to write your own documentation for these facts, right? you're not just going to pick some original TRAC document --

MS. UHLE: We're going to redo what needs to be redone, yes.

CHAIRMAN WALLIS: Right.

MS. UHLE: Sure. As our developmental work has been proceeding, we have quality assurance guidelines and we've generated more documentation than you can imagine.

CHAIRMAN WALLIS: If you go back to the very original TRAC documentation, such as it was, it was extraordinary. It was extraordinary, and it was a maze trying to figure out what was happening.

MS. UHLE: I think of "extraordinary" as good.

CHAIRMAN WALLIS: Oh, no, no. It was extraordinary. I'll try to choose a word that's neutral.

MR. ROSENTHAL: Let me just chime in. I think what's going to happen, the goal and reality would be that by the time we're done, this code will have more review and more scrutiny than anything else out there with a large user community, both domestically and internationally. And we share source code as well as compiled code. And we put it to the user community so that it will be far better reviewed and understood than I think the commercial code.

CHAIRMAN WALLIS: I was just trying to -- and you might think about how the ACRS could be most helpful to you in that process. We don't have the time to read every line and all that, but as you know we do look at selected parts of this code documentation and assure ourselves that it's credible.

MS. UHLE: And I guess you're interested in the momentum equation?

CHAIRMAN WALLIS: We want to be helpful. The last thing we want to do is to shoot you down in some way.

MS. UHLE: Yes. I think the --

CHAIRMAN WALLIS: And the last time you want to do it is at the end of the process.

MS. UHLE: Yes, I mean if you're willing to do that, it would be great. I would think that we would be accepting that.

CHAIRMAN WALLIS: Why don't you think about how we might be helpful there.

MS. UHLE: I'm not important enough to make that decision. It's these other people.

MR. ROSENTHAL: Sure you are. Sure you are.

MS. UHLE: You have the results here, I think, in your slides. I'll just skip over them. If you want to pursue them, because I think we're running out of time.

MEMBER LEITCH: This Peach Bottom Turbine Trip, is that the generator breaker openings or how is this -- or does that make a difference? In other words, we run them along in the turbine trips, is that--

MS. UHLE: It was a task scheduled at the Peach Bottom facility. Tony Ullses can elaborate on that; he's the lead, as well as Bajorek helped out originally. Go ahead.

MR. ULLSES: It was actually a cycled test that they ran at the facility during coasting down gradually from 100 percent power.

MEMBER LEITCH: Okay. From a 100 percent power? You say they were coasting down? They weren't--

MR. ULLSES: Actually they were at low power and they -- they actually had multiple trips but they were down in the 60 percent power when they started the trip and they actually disabled the initial stops on the valve position --

MEMBER LEITCH: Oh, I sure. Okay. So they closed the stop valves at 60 percent power. Okay. Thanks.

The other question I had related to that was you mentioned that in the previous LOCA, and I guess you didn't slide 5, but you referred to the Brown's Ferry ATWS. I guess is that a full-blown ATWS, or is that the partial ATWS that occurred at Brown's Ferry in '76 or something?

MS. UHLE: This is just on the matter from the BWR synergy.

MR. ULLSES: That was a partial ATWS.

MR. KELLY: Partial ATWS, yes.

MEMBER LEITCH: So you're not using a full-blown ATWS for this reference here?

MR. KELLY: Well, we're doing the plant calculations on a full-blown ATWS, but we're going to start with -- the deck was developed for the partial ATWS, which is what that was for, and there were some modern calculations on a former ATWS that we were evaluating and we're going to start off by rephrasing those calculations on TRAC-M using that deck as a surrogate high power BWR4 deck as a full ATWS.

MEMBER LEITCH: Well, I guess I'm just a little confused as to why you would use the Brown's Ferry rather than a full-blown ATWS.

MR. KELLY: Well, we are going to run a full-blown ATWS. What Brown's Ferry had was a development responsible partial ATWS but there's nothing in the input that would keep it running at a full ATWS.

MEMBER LEITCH: Okay. Okay. Thank you.

MS. UHLE: Okay. I'm going to skip now to the MOX fuel issue. I think we have talked about this before, but this is the idea of developing our kinetics capabilities to deal with MOX fuel.

The PARCS, the Purdue Advanced Reactor Core Simulator, that's the PARCS. What we do is we coupled to just the kinetics features in the code. So we use it as a module. And we are improving the kinetics module to be able to handle MOX fuel. We're adding the ability to do any number of energy groups because of the fact that plutonium has huge capture and fission resonances, and the beta is much lower than in uranium. So you have to be much closer because -- you have to be much more accurate because you can be closer to prompt critical.

The way that the MOX core will be run is we will be, we think, be using eight groups for the MOX assemblies and two groups for the uranium assemblies.

CHAIRMAN WALLIS: These are delayed neutron group of the N?

MS. UHLE: Yes, beta delayed neutron fraction.

CHAIRMAN WALLIS: You only need two for U?

MS. UHLE: I'm sorry?

CHAIRMAN WALLIS: You only need two for U?

MS. UHLE: Oh, sorry. The groups. No,

these are two energy groups for --

CHAIRMAN WALLIS: These are energy groups? This is something else you're talking about?

MS. UHLE: Well, N groups. Additional energy groups. N groups.

CHAIRMAN WALLIS: I don't know what an N group is.

MS. UHLE: That means N number of groups, how many groups.

CHAIRMAN WALLIS: Well, group of what?

MS. UHLE: How many bins of energy the neutrons can be in.

CHAIRMAN WALLIS: I see. I see. Okay. Okay. Thank you.

MS. UHLE: Two fields of neutrons, like the vapor and the liquid.

CHAIRMAN WALLIS: You have two groups there and eight groups here.

MS. UHLE: Yes. I think Dr. Kress can help you on that.

CHAIRMAN WALLIS: Well, it seemed funny, but I mean I guess this is a subgroup -- sub-sub title. This is a sub of the title. The neutron fraction isn't a subtitle of energy groups. Okay. Never mind.

There are new problems with MOX, so we really can't be surprised.

MS. UHLE: Yes.

CHAIRMAN WALLIS: New neutronic problems.

MS. UHLE: I'm glad I got that across.

MEMBER SCHROCK: Your bullet on reactivity difference due to mix of plutonium in the range is a little confusing. Error in reactivity can be closer to prompt critical in MOX.

MS. UHLE: Yes.

CHAIRMAN WALLIS: That's because of the delayed neutron fraction.

MEMBER SCHROCK: You need a comma there somewhere? Error in reactivity still can be closer.

MS. UHLE: Can be closer to prompt critical.

CHAIRMAN WALLIS: Well, you worry about error because you don't have this cushion from the delayed neutron, isn't that the idea?

MS. UHLE: Delayed neutrons. So your prompt critical with --

MEMBER SCHROCK: Well, I understand the problem, what I'm trying to understand is what message am I to get out of this statement.

MS. UHLE: Okay. Take a step back here.

Okay.

Additional energy groups, there is a need to have additional energy groups, more than just two, that we currently use for uranium cores. Okay?

Why do we need additional energy groups? We need them because of the fact that plutonium has a lot of resonancy, and so around the epithermal range and at the 1 eV range, and around the -- in Pu-241 you get capture and fission resonances at the 1 eV to KeV range.

So you have these resonances, whereas in uranium you don't. You pretty much can bend your energy groups of your neutrons into fast neutrons and thermal neutrons because there's none of these big resonances on the way scattering down to the thermal.

Additionally, you worry about error in reactivity. We could have used the two energy groups and --

CHAIRMAN WALLIS: Would you bend your betas? The beta is an average of a whole lot of different betas, isn't it?

MS. UHLE: It's an average of the betas.

CHAIRMAN WALLIS: And do you have to worry about individual betas with plutonium?

MS. UHLE: Yes. In the 3-D -- yes. In

the code you do. I took off the 235 beta and the --

CHAIRMAN WALLIS: Well, that beta's just an average for you, isn't it?

MS. UHLE: It's a beta for that isotope.

CHAIRMAN WALLIS: There are different groups. Right. So there are different groups in the beta --

MS. UHLE: Yes.

CHAIRMAN WALLIS: -- itself it subdivides. Okay. You didn't worry about that now, because you've got such a lower beta?

MS. UHLE: Well, in the fission event you're -- I guess I don't understand what you're asking. Do you understand what he's asking, Tony?

CHAIRMAN WALLIS: There are separate groups. I get confused about the groups.

MR. ULLSES: Yes, the code itself, Dr. Wallis, it actually on a node-to-node basis will maintain an individual amount of the actual related neutron.

CHAIRMAN WALLIS: But it looks at the simpler fractions of the separate groups?

MR. ULLSES: Right.

CHAIRMAN WALLIS: Okay.

MS. UHLE: Just to give you an idea that

we have to be very accurate, more accurate than we do in uranium cores because of the fact that we are closer to prompt critical because of the --

CHAIRMAN WALLIS: So it's not just the average, it's also the group which is slowest -- which is governing in a rapid transit, isn't it? So it's not just the average you worry about?

MS. UHLE: Well, it's the most dominant, the most dominant group.

CHAIRMAN WALLIS: But, I guess Tony's got it all under control. Tony's got it all under control, certainly.

MS. UHLE: I'm sorry?

CHAIRMAN WALLIS: I said Tony has it under control; that's all I'd really like to know.

MS. UHLE: All right. Great. So does that explain this slide any better?

MEMBER SCHROCK: Well, no. The language is what I'm criticizing, as in that statement error in reactivity can be closer to --

MS. UHLE: Okay. Okay.

MEMBER SCHROCK: There is a reactivity evaluation problem which is rather complex. POR is big, it behaves pretty much like several critical assemblies loosely coupled and each one has different average values, of the delayed neutron fractions owing to the fact that it has different composition at that point in time, different weighting both the effect of plutonium versus uranium neutronic properties and the neutron fraction specifically. And so you're rolling an awful lot of important information into a simplistic statement here.

I've raised questions about this in the context of other codes in the last year and I haven't heard crisp clear answers to those questions. I don't know that you're doing the calculation better than some of the industry codes where they make claims that they're doing it right.

Somewhere I'd like to hear a clear explanation of how one keeps track of the local compositions and how that information is then impacting the calculation of such things as the 3D kinetics. I haven't heard any of it yet.

CHAIRMAN WALLIS: You need to see the POX -- you need to see the POX documentation.

MS. UHLE: We can provide that to you. We have it written up, if you'd like that.

MEMBER SCHROCK: I'd like to see it.

MR. KELLY: Yes, we can do that. Sure.

MS. UHLE: Oh, sure. Or we could have a

separate briefing on the MOX development if that's --

MEMBER SCHROCK: You see, in the documentation you're offering here, our code is like the government's code, and therefore it's okay. You guys can't challenge that because you've developed it, it's your documentation and we're doing the same kind of inadequate documentation as you do, but you've judged it's good enough and therefore you've got to accept the fact that we say it's good. What I'm telling you is that it is not good engineering practice. And I'm going to keep asking the question until I hear some better engineering answers.

MS. UHLE: With respect to the MOX capabilities?

MEMBER SCHROCK: It has to do with the calculation of the reactor kinetics in a 3-D situation in which the composition of the core is nonuniform and evolving, it's different at different points in time--

MR. ULLSES: I understand. Right. Okay. I can get back to you on that.

MS. UHLE: I mean I have a --

MR. ULLSES: I could take a stab at it now or we can do it later.

MEMBER SCHROCK: No, I think we need to get back.

MR. ULLSES: Okay. We'll get back to you.

MEMBER SCHROCK: Right.

MR. ULLSES: I'll bring you the documentation.

MS. UHLE: Okay. I can skip over the other slide. I was going to get more into 3-D kinetics methodologies for MOX, but I think we're going to have a more detailed description of that provided to you at a different date, if that's all right.

MEMBER SCHROCK: See, the term MOX is generally interpreted as being -- as situations in which the fuel is designed to be mixed oxide. Whereas, what you really have in all reactors is some form of MOX. And my problem with the calculations that I see done is that this level of complication gets getting short-shrift in describing what the codes actually do. With the physics it is relatively straight forward to understand in principle, but complicated to deal with in the calculations.

MS. UHLE: Right. And I can tell you that the way we're going to be handling the MOX cores is that the uranium assemblies, the $U0_2$ assemblies, they will be homogenized so that each -- the node -- the power --

MEMBER SCHROCK: Have you asked yourself

the question of why does this issue of error in reactivity arise when you're talking about mixed-oxide fuel and not for reactors that have initial uranium fuel?

MS. UHLE: Okay. With the reactor physics, I mean you get three different types of errors -- well, I mean stemming from three different phenomena.

One is the number of energy groups that you have because, of course, there are -- you don't want to get into this.

CHAIRMAN WALLIS: No, his question is different. I'm sorry. He said why is MOX different from regular reactor because when you've got high --

MEMBER SCHROCK: In principle it's all MOX.

CHAIRMAN WALLIS: -- burnoff, there's a lot of plutonium there already.

MS. UHLE: MOX is because you're going to have uranium dioxide fuel assemblies sitting next to a MOX of plutonium dioxide assembly.

CHAIRMAN WALLIS: So there's increased heterogeneity?

MS. UHLE: And so -- and you get very different energy spectrums coming out of the plutonium side because of the different cross sections for the resonances. And so you get this very strong neutron, this gradient in neutron flux between the assemblies.

MEMBER SCHROCK: Clearly the more you complicate the spacial variation in fuel composition, the harder the calculation becomes.

MS. UHLE: Yes.

MEMBER SCHROCK: And in mixed-oxide fuel meaning that you have bundles of different composition loaded into the reactor initially, it's going to be more complex then if you load it uniformly and let it generate its nonuniformity as it burns up. But you get the same phenomena occurring to different degrees. The relative consequences become more important when you're talking about what you're characterizing as mixed-oxide fuel cores.

MS. UHLE: The orders of magnitude --

MEMBER SCHROCK: But the phenomena are always there.

MS. UHLE: Right.

MEMBER SCHROCK: And the codes need to deal with the phenomena.

MS. UHLE: They deal with the phenomena.

MEMBER SCHROCK: Yes. My question is how do they deal with the phenomena.

CHAIRMAN WALLIS: I think that's where you have to look at the documentation.

MR. ULLSES: Yes, I understand the question, Dr. Schrock. I mean, I can go through an excruciatingly long discussion right now about hydrogen --

CHAIRMAN WALLIS: I don't think we need that. I think --

MEMBER SCHROCK: What I'd like is to be given something to read that tells the story in a clean cut fashion.

CHAIRMAN WALLIS: So would you agree to give him something to read and then we can move on?

MS. UHLE: Yes. That is an action item for us. By Monday we will have a clear --

MEMBER SCHROCK: Okay.

MS. UHLE: We have it written up. It's upstairs. It's upstairs. We can go get it if you want it.

CHAIRMAN WALLIS: Okay. Let's move on.

MS. UHLE: We'll give you a brief tomorrow.

CHAIRMAN WALLIS: Let's move on.

MS. UHLE: Why don't we go get it.

MR. ROSENTHAL: Why don't we provide him

with the documentation, okay. And then after he's had an opportunity to look at the documentation, at his discretion we'll schedule a morning session and we'll talk about MOX. When we talk about MOX, we not only talk about the physics, but we'll also talk the neutron physics --

MEMBER SCHROCK: See, my emphasis --

MR. ROSENTHAL: -- we'll also talk about source term and other related issues.

MEMBER SCHROCK: Jack, my emphasis is not on MOX. It's on the fact that I look at old documentation, which continues to be referenced, and what I find is that people say you do these things with delayed neutron yields and there's a table of delayed neutron yields for U-235 presented in the documentation in the early versions of RELAP5, for example. And nothing's said one way or the other about does this deal with the problem that the core contains some other fissile nuclides and what are the delayed neutron fractions from those.

It's the latter that I'm concerned with. Why did they get lost in the shuffle?

When I raised it in connection with review of another code, I'm told that it's all done correctly, you just don't view it in -- yes, right.

Well, I'll believe it when I see it in a clean cut --

CHAIRMAN WALLIS: So you're going to see it, Virgil.

MEMBER SCHROCK: Thanks.

CHAIRMAN WALLIS: And we're going to move on. You're going to satisfy him with some documentation, otherwise the question will just come up.

So, can we move on?

MS. UHLE: I think everyone was aware of the control rod drive mechanism issue. The Oconee Unit 3 spring 2001 outage, there were circumferential cracking on the CRDMs. We looked at the idea that there's this potential for a rod ejection because of the circumferential cracking.

The question was raised that you could result in, perhaps, an ATLAS because of the fact that you have collateral damage with the CRDM ripping off and taking out a bunch of the other CRDMs in the area. So Research performed a worst case scenario calculations on the off chance that for some very improbable reason there was a full ATLAS. And we did a 3-D kinetic, 3-D hydraulics model using the TRAC code. Jack had said it was a RELAP, but we had used TRAC with this because we, again, want to keep exercising the TRAC code. And we used the Boron tracking to determine the effect of the RWST injection shutting down the reactor.

The results of this actually confirmed NRR from the analysis that NRR had done with RELAP5. And what it showed was that there was no new phenomena identified bounded by the current design basis and no fuel heat up was expected, no core damage was expected.

We did this as part of a confirmatory analysis for which that was an activity that we did.

One thing to point out was that based on the results of in running these codes is that they, again, there are still bugs in the codes. And one that we found was with respect to the Boron reactivity coefficient.

In the PWR people don't picture -- well, typically you're thinking of normal operation, you're not picturing any boiling in the core. And the reactivity coefficient for the boron, assuming no voiding and it was based on parts per million versus parts of boron per parts of liquid. And so it could deal with boiling. And what we have done is, of course, change it to what it should be, which is moles of boron per the volume of the cell that you're talking about. And this was actually identified also in the TRAC-B code as well for the point kinetics model.

So, every time we use these codes it helps us.

MEMBER LEITCH: So in your calculations you assumed that there was a partial --

MS. UHLE: A full ATWS.

MEMBER LEITCH: Oh, a full ATWS?

MS. UHLE: Yes. And so you're getting the heat up, you're turning back around and with the depressurization you're injecting the RWST water with the high boron concentration and it's shutting it down.

MEMBER LEITCH: Okay. So even with the full ATWS you're still reaching these same conclusions.

MS. UHLE: Yes.

CHAIRMAN WALLIS: It's a full ATWS and a LOCA at the same time.

MS. UHLE: Yes. And Tony also said the network.

Steam-generator tube integrity. You've heard that, a briefing on that before. I think I'm going to skip that for reasons of time. You will please note that we will be using the thermal-hydraulic code in the branch to look at those DPO issues.

Let me get into risk-informing activities that we have in the branch and the use of the codes in those areas.

Of course, I think that you understand what we mean by risk-informing regulation. The current activities we have with respect to thermal-hydraulics is risk-informing the ECCS rule and the pressurized thermal shock rule. So 50.46 for the ECCS and 50.61 for the PTS.

You have seen or the full committee has seen a briefing in our risk-informing of 50.46.

I wouldn't say that it's really risk-informing, the activities are more looking at any modifications that can be made to Appendix K based on the industry's desire to reduce regulatory burden. And Ron Lauben and Steve Bajorek are the technical leads on this in the branch.

So what has been looked at as an idea to look at the Appendix K evaluation models and note the real conservatisms in the code, and based on better science can we replace the oxidation model for heat generation to Cathcart-Pawel, because Cathcart-Pawel does a better job as far as the heat generation.

We also have better science now with the decay heat curve of 1994 standard versus the '71 standard. We were looking at that as an option. We've been running code calculations to get an idea of the change in the PCT based on these changes going to the '94 standard or using Cathcart-Pawel versus Baker-Just.

MEMBER SCHROCK: I guess we're going to hear more about that?

MS. UHLE: Yes, in detail.

MEMBER SCHROCK: In details, but in my mind it's just kind of strange that suddenly there's a large activity going on to revise what has to go through Congress to get approval, I think. Appendix K is in 10 CFR, it's got to be -- it's part of the legislation is involved here.

MS. UHLE: Yes.

MEMBER SCHROCK: There are lots of complexity, but the background that's covered, evidently, in SECY 01-133 seems to be totally lacking. I don't understand how a decision can be made that we must deal with a modification in Appendix K without the technical evaluation that leads to the decision to do that. Where is it?

MS. UHLE: That's our stance, though, the division position, Research position, and we've had a discussion with NRR in this manner that we're leaning towards the idea of not modifying Appendix K because of the fact that we have found nonconservatisms in Appendix K. And the person who came up with the 71 times 1.2 was very good because they accounted for those, in a sense, conservatisms.

MEMBER SCHROCK: Well, I read that so I know what it is.

MS. UHLE: So you're the one.

MEMBER SCHROCK: Well, I'm not "the" one, I was involved.

MR. KELLY: One of the ones.

MS. UHLE: One of the ones.

MEMBER SCHROCK: But what I'm hearing and what I'm reading isn't a very accurate account of that; not that that's a terribly important thing. But what I'm getting at here is why is a lot of activity going on here to revise?

MS. UHLE: What is the initiative?

MEMBER SCHROCK: What is the impetus to revise Appendix K?

MS. UHLE: Appendix K --

MEMBER SCHROCK: What is the technical

basis for it?

MS. UHLE: Well, why this started was a petition submitted by NEI looking at replacing the '71 standard with the '94 standard. And so the idea of reducing unnecessary regulatory burden or --

MEMBER SCHROCK: They're totally different things. You're comparing apples and oranges.

MS. UHLE: I think -- can I finish what I was saying?

MEMBER SCHROCK: Yes.

MS. UHLE: I think it'll -- okay.

That's why this, we started looking at this one here with this idea to a risk-informed Part 50 is where a lot of -- we were looking at changing Part 50, changing the regulations under this risk initiative, this risk-informing initiative. And this work here was put in with that based on the petition.

MR. BAJOREK: Jennifer, can I jump in?

MS. UHLE: Yes, sure, Steve.

MR. BAJOREK: This is Steve Bajorek.

One of the things that we're trying to deal with is accuracy in the various models; the decay heat or Cathcart model versus Baker-Just versus the expectation that those can be changed in an evaluation model.

I think there's been a recognition in the SECY paper that the '79 or the '94 standard is technically better than the '71 decay heat standard, more accurate with regards to more recent data. And likewise, with the Cathcart-Pawel versus Baker-Just.

The expectation that seems to have been raised in the SECY paper is that we can just simply replace those in Appendix K. The work that we have been doing in our branch has been twofold: (1) To take a look at what do you need to go from this decay heat standard to the '94, and there's more complications involved in dealing with the uncertainties. Norm Lauben has been looking at that. But the other issue is to what extent do the present day Appendix K evaluation models depend upon the conservatism that was inherent in the '71 plus 20 percent to cover other issues.

Now, when we start to delve into this what we have been finding are things like downcomer boiling and fuel relocation would result in increases in the peak cladding temperature that would almost offset any kind of benefit that would be gained with the 1971 model.

MEMBER SCHROCK: Well, do you really believe that the people that drafted 10CFR back in the early '70s brought the uncertainty in decay power as taking care of unrelated uncertainties?

MR. BAJOREK: No.

MEMBER SCHROCK: No. Okay. So why is that brought up as an issue here?

MR. LAUBEN: Norm Lauben.

There was an evolution and it didn't start out that nobody thought the decay heat multiplier, as you say, we dropped another degree but as time went on different things were discovered that was discovered that there was a larger conservatism in the '71 than was originally thought, but at the same time there were -- how do I want to say this -- there was creeping reduction in conservatism in Appendix K evaluation models that ate away at some of the increased margin that was perceived as time went by.

So, people then began to think, "Ah, well there is extra conservatism in the decay heat model." But it truth at the beginning we did not believe that.

MEMBER SCHROCK: Well, yes, I think that's a historical fact that people have thought that way that expressed their view, etcetera.

MR. LAUBEN: Yes, right.

MEMBER SCHROCK: But it's not something that's documented as a basis for licensing evaluation.

MR. LAUBEN: And in fact --

MEMBER SCHROCK: So it's not something that has anything to do with issues of whether you're going to change it or not.

Those rules were created when there was a lot of information that was still, basically, unknown.

MR. LAUBEN: Right. Right.

MEMBER SCHROCK: And did a remarkably good job under the circumstances.

MR. LAUBEN: And may be lucky, too.

MEMBER SCHROCK: I think it was --

CHAIRMAN WALLIS: Well, I guess, one of the things said here is that it could change the regulations and became more realistic about decay heat; it would look good and industry would think they had gained something. It turns out you've got to be realistic about some other things, which take away the gains from the decay heat.

MEMBER SCHROCK: Yes.

CHAIRMAN WALLIS: And so that it's not clear that there's a gain to anybody by changing the regulations, except the new regulations would be more based on more realistic physics, and that's probably a good thing.

MEMBER SCHROCK: Well, I've probably

gotten into this at the wrong time in our discussions. I know you have a presentation coming up on it. But it does seem to me the starting point is thrown at this committee in a very strange way. This SECY paper has not been reviewed yet by this group. Okay. I don't know what in the world it says or why they think there's a sound basis. All I hear is rumors to the effect that it is something that was initiated by NEI.

MR. LAUBEN: Is that true with the activities with the --

MEMBER KRESS: Yes, we have. Not this subcommittee.

MR. BOEHNERT: Yes, not this Subcommittee. The full committee of ACRS I think, because it's handled under subcommittee.

MR. LAUBEN: Have you reviewed all the other copies of this?

MEMBER KRESS: Yes.

MR. BOEHNERT: It was handled by another subcommittee, that's my --

MEMBER SCHROCK: Well, what I'm challenging here is why does the Research branch of NRR get deeply engrossed in a lot of considerations, it's obviously an expensive thing to do, to address a problem which somebody has told them is a change that has to be made? On what basis can a decision such as that be made without the technical work preceding the decision?

MR. LAUBEN: Of course the technical work has to be done.

MEMBER SCHROCK: Yes.

MR. LAUBEN: And I think 01-133 says the technical work must be done. And if the decision comes that we shouldn't change it, then we won't change it.

MEMBER SCHROCK: My concern was, pure and simple, that this is something that's going to get railroaded through despite everything. And you're saying that it isn't true. All right.

MR. AYER: Well, let me jump in. This is Charles Ayer from Research. Let me just to correct the record a little bit.

The SECY paper we're not risk-informing 50.46.

MEMBER SCHROCK: Yes.

MR. AYER: It was looking at several issues, part of which was the Appendix K model for decay heat. The petition to change the decay heat came along later, and that was just something that's come in very recently, but that was not the driving force --

MEMBER SCHROCK: Okay.

MR. AYER: -- the NEI submitted a petition and the agency jumped up and ran off to limit it to 50.46. It came in subsequent wanted a simple change on 50.46. This other effort to risk-inform, which is also looking at the large break LOCA and loss of power. But that effort had been going on and is going on. At the onset we're looking at the technical basis for the smaller needs that Jack's branch is working on to see if you can incorporate '94 decay heat, to see what other things would have to be incorporated and perhaps be more realistic in the other areas.

But I just wanted to make it clear this wasn't initiated because of a petition from NEI.

MR. LAUBEN: As a matter of fact, Paul, you were at several workshops last year in which this group was starting to deal with it, so you know, so you knew this was augmented to the initiative.

MR. BOEHNERT: Yes, that's correct.

CHAIRMAN WALLIS: Well, I think the message for us is I think we would have said that you could sort of change this decay heat code independent of all the other considerations. And let's do it, it's an obvious thing to do under the ACRS initiative.

We ought to follow that line. And what we are being warned about here is if you do that, you're giving up some conservatism which you really need to cover some of these other things, and therefore you should be more careful about saying, viewing the decay heat code as something completely independent that you can fix and then you can deal the other part separately.

MEMBER SCHROCK: Well, another way of looking at it is that there is a very simplistic rather conservative scheme for licensing put in place in the early '70s that's antiquated, it was grandfathered when the new rule was passed in '88. And now the issue is, does it make sense to reduce conservatism in an antiquated method. That's an overall issue, it seems to me, and it needs to be addressed, and it ought to be addressed by this Committee, too.

To me it makes no sense whatsoever to say we are going to go back and take all the conservatisms out of an antiquated scheme and expect that it's going to be technically sound in the end.

MR. ROSENTHAL: We briefed about two weeks ago.

MR. KELLY: Two weeks ago, yes.

MR. ROSENTHAL: We briefed the PRAs and

members of the subcommittee --

MR. LAUBEN: And this subcommittee, too.

MR. ROSENTHAL: And this subcommittee.

MR. LAUBEN: We briefed three subcommittees.

MR. ROSENTHAL: And now we have some more technical work to do, and we would more than welcome an opportunity bringing the technical work before this Subcommittee. I think it would be very appropriate.

MEMBER KRESS: But I think from the point I've heard in these other reviews that we're basically on the same page you are with respect to that issue. They're not going to just go in and blindly change that Appendix K. They're going to look at what the implication are.

And so I think we're closer to your side of the table than you might think of.

CHAIRMAN WALLIS: We probably have to move on.

MEMBER KRESS: Yes.

CHAIRMAN WALLIS: And we're going to have a whole meeting on 50.46 some day.

MEMBER KRESS: Right.

CHAIRMAN WALLIS: And we can't dig into that in depth today.

MEMBER KRESS: Yes. Okay.

CHAIRMAN WALLIS: But we've been warned, I think, that we've got to worry about some of these things, which has been very useful.

MS. UHLE: The point of slide too is to point out that with respect to your concern about doing the technical work to make sure that this is a viable technical approach is that we will be running and analyzing a great deal of cases with respect to any of these activities. And it was with support that we have given to NRR concerning the effect of the downcomer boiling, especially as being a primary concern that is shaping the technical position that is leading in a direction that I think is very consistent with yours. So we are using these tools for their purposes.

Again, in the future we would also be using them in the SECY paper to look at certainly the effect of redefining large break LOCA size, looking at success criteria evaluation for the PRA runs and the effect of the different restrictions concerning delay diesel generator start time, loss of offsite power and signal failure. But, again, we will -- all of these activities 54 -- or the risk-informing Part 50 are going to be made, you know, using the available tools and, as well as the knowledge and the analyses of the staff.

MEMBER KRESS: My next door neighbor in Oak Ridge has asked me to be sure you pronounce his name correctly. It's Pawel, Dr. Pawel; just as if it were P-A-U-L.

MS. UHLE: Pawel.

MEMBER KRESS: Yes. Not P-A-W-E. It is spelled correctly, but it's not pronounced Powell, it's Pawel.

MS. UHLE: I know how he feels, because nobody pronounces my last name right either.

MEMBER KRESS: I know this is trivial, but it upsets him.

CHAIRMAN WALLIS: Jennifer, are you going to take a long time now?

MS. UHLE: No. I can skip over 61. We're doing the same thing with 50.61. We're running the tools; you had a briefing on that. We've made sure that the calculations are consistent with data that was taken at OSU. So we're looking at the idea of when we use these potent codes how can we prove that -- or at least appease the masses that the answers that we are generating are acceptable. We're not believing everything that comes out of the code, that we're skeptical about it.

With respect to AP1000 design certification, we had an NRR user need request concerning looking at the Westinghouse assertion concerning the scaling of AP1000 is consistent with the AP600 work, and that they're claiming no additional testing is required and minimal code modifications would be required. That's the Westinghouse position.

So NRR requested technical assistance from Research to review these assertions, identify what code versions should be used if phase 3 were to take place.

And we for the small break loss of coolant accident, I know a lot of you are involved in the adequacy assessment of RELAP5 over that 5 to 6 year period. TRAC had not been -- we didn't have a program to do adequacy assessment for small break LOCA on the TRAC code, so the RELAP code will be used for the AP1000 phase 3 for small break. And in phase 3 if it were to come in, the TRAC code would be used for the large break LOCA application.

Now, one thing of note is, and an activity that has stemmed from this initiative is that the AP600 had a lower power density. So PCT values predicted by TRAC were below the limits, the 2200.

AP1000 has an increased power density. We realized that there won't be as much margin there and we're based on calculation run with the reflood models in TRAC. We're expecting that it would be over the limit, not because of the actual physical processes but because we have a lot of conservatism in the TRAC large break model.

To remove some of this conservatism, will we do a preliminary or an interim model development on the reflood model. Bajorek is working on that currently with Weidong Wang of the staff. And it is hoped that or it is the goal to have that in by, say, the spring or the summer and start doing some developmental assessment work on that version for the consolidated code. So by the time the consolidated code is finished we will have, you know, this interim reflood model developmentally assessed and use that for the AP1000 submittal. Because RELAP large break model tends to be nonconservative and TRAC is too conservative.

Now, I don't want to confuse you with the fact that the RBH, the rod bundle heat transfer program. That's focused on developing a mechanistic model for reflood. And we're thinking 2004, 2005 time frame for it to be the model in the code.

What we're doing for the AP1000 work is more of a -- we're simplifying what's currently in the code with something that's more of a -- Joe, do you want to say what you're doing?

I don't want to say it's simple, but it's not the mechanistic model with a droplet diameter and the interfacial area tracking, and what have you. It's going to be easier to follow than what's currently in the code. It will get rid of the conservatisms that are coming from too much entrainment at the punch front. And we're hoping to have that done by the spring/summer time frame.

Do you want to --

MR. KELLY: I'll have several slides in my presentation, so I'll wait for that.

MS. UHLE: Okay. I didn't mean to say what you're doing is simple.

Again, we know that the phase separation model in the RELAP5 code was determined to be inadequate for the phenomena. It turned out that the AP600 had so much water reserve that it didn't make a difference in assessing collapsed liquid level, so the code was determined to be adequate for the AP600 calculations with the fact that they have a higher power density and the inventory to power ratio of the AP1000 is reduced. We realize that the phase separation model for the stratified conditions during ADS 4 time frame is going to be of higher priority, so we're looking at that.

Steve Bajorek will talk about that in more detail.

We have there, too, for the PBMR design certification that we're expecting to come in. I think you know the background on that with the idea that it is now a helium cooled/graphite moderated reactor. It's a little bit different than the light water designs that we currently deal with. It's a pebble bed rather than the force flow parallel to the bundle situation that we currently deal with.

We've drawn the conclusion that we would be upgrading -- or not upgrading, but extending the TRAC code and the MELCOR code to be used in real certification if it were to come in. And we have identified what needs to be changed in the code, and you have a list of them on your slides. I don't need to go into them. I don't think --

CHAIRMAN WALLIS: You're worried about water ingress?

MS. UHLE: Yes, water ingress because of

the reaction with the graphite. Because you have the second -- well, you have the cooling on the -- you have the bring cycle but you've got the compressor in the intercooler.

CHAIRMAN WALLIS: The intercooler is a water cooler?

MS. UHLE: Yes, so you can get water ingress. We can do water ingress and air ingress at the same time and we have it working for the helium. So we'll be able to run the whole gambit of the accident scenarios with respect to the pebble bed.

Now, we do have some code development to do as well as benchmarking, and we'll be doing that in-house as well at Las Alamos National Laboratory.

CHAIRMAN WALLIS: Hydrogen and CO --

MS. UHLE: Yes.

CHAIRMAN WALLIS: -- process, or whatever.

MS. UHLE: Yes. Modifications also had to be made to MELCOR and we've identified those, and those will be done at CND and National Laboratory with staff involvement.

So that's where we're heading. We're going to not get away from this. It was thought that maybe we would use a special code for the pebble bed. Again, we're focused on this idea of having modules that only need to be exercised if they need to be exercised to get this approach with the consolidated code having one code.

MEMBER KRESS: Well, what's the purpose of looking at the water ingress for example. There are no graphite structural ingress in there, are there?

MS. UHLE: There are no what?

MEMBER KRESS: Structural ingress in the graphite? There's only the spheres of graphite isn't there, they're not structural. So that's the --

MS. UHLE: It's a fuel damage issue.

MEMBER KRESS: We're looking to see whether in the break the spheres --

MS. UHLE: Yes, that would be in the severe accident situation.

MEMBER KRESS: -- break or something of that kind?

MS. UHLE: Or would oxidize, getting brittle, break and then get the fission products out because the pebbles are the --

MEMBER KRESS: But you have no data on sphere strength. I don't understand what you will do --

MS. UHLE: That's the last bullet. Data for benchmarking.

Originally in the budget this year there was going to be some money for fuels testing.

MEMBER KRESS: I don't understand --

MS. UHLE: As the submittal comes in, that will be ramped up to meet the data needs. We're not going to use the code unless it's assessed.

MEMBER KRESS: You're going to degrade these spheres, make them go through the separating devise and see if the break --

MR. ROSENTHAL: All right. I'll be fast. Presentations of the pebble bed say it's a very benign system.

MEMBER KRESS: Yes. Absolutely. Okay.

MR. ROSENTHAL: And so we started asking ourselves, okay, what about the accident provisions. And bare in mind that design bases accident goes beyond design base, or even that language is not yet defined for this system.

MEMBER KRESS: Yes.

MR. ROSENTHAL: And we may be talking about a spectrum of accidents, one accident, whatever. Okay. And so we started saying, okay, what kind of issues might we face, and we recognized that we needed to start thinking about well what happens if we put air in there, or water in there instead of helium, and what kind of chemical reactions would take place, or whatever. And because of the time it takes to develop a code, we needed to get a jump start on these issues. And that's really where we are now, you know, we haven't thought it through. We're still defining the research plan for it.

MEMBER KRESS: My question is --

MR. ROSENTHAL: But the concern is --

MEMBER KRESS: Yes. My question is are you concerned about degradation of strength of these spheres or are you worried about the effects on fission products, or both?

MS. UHLE: Both. I mean, you get the oxidation action causing fuel heat up and then you're also getting fuel damage and how that's going to -- essentially if there's no containment, how the fission products would be escaping because of that. So with respect to the why in the TH code are we worrying about that? Well, we're going to tell you how much water comes in and what state it's in. Is it steam, what temperature, what have you and get the oxidation reaction and then, of course, going into the theorizing for the core degradation.

CHAIRMAN WALLIS: So you're identifying all the things that you're TRAC-M modification have to be able to handle, that's really the message you're giving us?

MS. UHLE: Yes.

MEMBER SCHROCK: What doesn't come through clearly to me is why one would choose TRAC-M as a code to analyze this new system.

MS. UHLE: It's probably at this point in time --

MEMBER SCHROCK: I mean almost none of the --

CHAIRMAN WALLIS: It's the only one they have.

MEMBER SCHROCK: Well --

MS. UHLE: No, that's not right.

CHAIRMAN WALLIS: It's the only one they will have.

MS. UHLE: No, that's not the answer. I'll give you the answer.

MEMBER SCHROCK: But they're so different from one another and --

MS. UHLE: Well --

MEMBER SCHROCK: -- all these gory details of what goes on in water reactors has no impact.

MS. UHLE: Again, it's going to be physical models that are going to be different. You have -- I mean, if you look at the code as far as how many hundreds of thousands of lines it may be, the physical model package, I mean it's dinky. It's maybe where the correlations are, maybe 400 lines or more, or less. I mean, it's not -- putting in a different wall drag or a different -- you know, effective conductivity for the fuel. I mean, that's small. What's in the code is the setting up of the matrix, it is the communication of the data between the cells if you have -- like in a 3-D. We have a 3-D vessel here of porus media. The hydraulic model in TRAC is essentially a porus media 3 dimensional model with wall drag, that is assuming the flow is parallel. Well, now the flow is going to be over spheres so we have to replace that wall drag term with something that represents the fact that you're flowing over a pebble bed.

So, looking at all the codes that are out there, TRAC was the one that had the less amount of work done. We already have helium as a working fluid in the code. Again, we have the porus media hydraulic model.

We can do -- on the intercooler side, the secondary -- if you want to call it the secondary side, you know, we have the water loops for the heat transfer. We have a turbine model that we have to modify so that it's a two-phased turbine. But, you know, we have the equation set up and already dispertized; it's a matter of putting in different physical models. But that's the, in some sense, the easy part.

MEMBER KRESS: But you could have a break in the intercooler, and the water is a lot lower pressure than the helium. How do you deal with that in terms of ingress to the water, or you haven't gotten that far yet?

MS. UHLE: Well, I mean we are modeling the -- you mean, the intercooler breaking and not flowing into the helium, because there are two different sections. And so if we had an intercooler break, it'll just be like faster flow out and cooling down; it'll be like a main steam line break in some sense.

MEMBER KRESS: Yes, but I presume if you're given a small leak, you know, have a crack in it.

MS. UHLE: Yes.

MEMBER KRESS: And it gets some water ingress.

MS. UHLE: Into the helium? Sorry?

MEMBER KRESS: How do you get water into the helium is my point?

MS. UHLE: Oh, how do you get the water? Well, for instance, if a steam generator were to rupture, the same kind of situation where it's passing over, if you get the water in -- oh, you're saying the helium's higher pressure. Oh, I see.

MR. ROSENTHAL: Let's not get too far ahead.

MEMBER KRESS: It's a technical issue.

MR. ROSENTHAL: At one time a few weeks ago I asked at this plant if it had MSIVs, and I was told, well MSIVs is the wrong term. There would be an MHIVs. And so I said okay, is this plant going to have MHIV? And I was told we don't know yet.

So let's not get too far out ahead of the planning cycle. What we know is we started. We really need tools to do analyses.

MEMBER KRESS: And that's the main thing.

MS. UHLE: But in the sense that you can have a lower pressure or you can have a break in your helium side, you get loss of forced circulation and you still have hot graphite, you're at a low pressure, water can get in. Because -- okay.

MEMBER KRESS: I'm sure there's some areas that we can -- again --

CHAIRMAN WALLIS: We can't spend an hour on the pebble bed reactor. We have to move on. Yes, they're just giving us an overview, I think.

MS. UHLE: Yes. We will be using the code and, again, the changes in the physical models are --

CHAIRMAN WALLIS: And you're thinking of all the things you need to put in that code, you need to build a model.

MS. UHLE: Yes, and we have done that or in the process of doing that.

CHAIRMAN WALLIS: And when are going to be ready to run?

MS. UHLE: Well, the work scope for next year is putting in the physical models for next year and finding data for benchmarking and doing modeling. So by next time we meet in front of you, we should have a pebble bed.

CHAIRMAN WALLIS: I just hope that you've got models up and running before someone's already made a decision for license on what the design bases accidents are and all those sorts of things.

Do you actually have put in inputs to give so quality decisions are made?

MEMBER KRESS: Well, is one of the models going to be the fusion of water vapor in the graphite spheres and what is the chemical reaction?

MS. UHLE: That would be the MELCOR side.

MR. ROSENTHAL: We may do that in MELCOR fusion and hydrogen. We've got two major efforts. One is TRAC and the other is MELCOR.

At one time we thought that -- just conceptually that many of the pebble bed issues really would be more chemical type issues and that the MELCOR frame would be the place to focus. Then at the experts meeting -- but we still had money in for TRAC. Actually, it was Andy Kadak that kept bringing up issues of reactivity events that might occur with restacking or you lose the pressure, the walls move, or stuff like that. Well, again, we had PARCS again with TRAC. And so PARCS TRAC becomes the natural place for us to want to explore that. But we really are at the level of building the MELCOR models, building the TRAC as tools for what we don't know yet.

MS. UHLE: I just want to point out with looking at the kinetics, since that's been brought up, we're really benefitting from the MOX program. I can't believe I'm bringing that one back up. But the things that are immediate is the soon to be needed for the pebble bed work is similar to what has already been for MOX. And so what has to be done for MOX is a cylindrical co-ordinate system, but that's pretty simple to do. And the fact that the control rods are in the peripheries, we would need a transport in that area, but we have that for MOX already, and that's currently being tested.

So, we're using what we already have.

All right. So I'm going to summarize. I think this was the slide that Professor Wallis has been looking for.

CHAIRMAN WALLIS: So we can get close to the end?

MS. UHLE: Yes. I don't know if it's the end of my life or my career, or at least my presentation.

The branch provides technical support to the offices as needed, and we use the analytical tools and, of course, the analyses capabilities of the branch to meet those needs.

We're currently looking, the applications we're looking at are associated with licensee submittals, such as the power upgrades and the MOX fuel. Generic issues such as the CRDMs, steam generator tube integrity. Risk-informing activities, 50.46 and 50.61. And design certification, AP1000 and pebble bed.

We realize that we will have to make to improvements to these codes as emerging issues arise and, again, we're focusing on doing that more in an in-house fashion looking at perhaps coupling to other codes as needed rather than using separate codes with the same functionality. We're going to get away from that. We're only going to use -- or only use what we need to versus having ten codes in our code suite for just TH.

And, of course, we're doing internal model improvements such as for the AP1000 case.

Jack had talked about this, and I mentioned it in the introduction, is that we have hired -- we are in process of hiring entry level employees as well to round out the technical capabilities of the branch. Because we are actually busier now than we have been in a while.

It's not just going to be for thermal-hydraulics. It's also computation of fluid dynamics as we start to use CFD more as a tool, especially with the pebble bed work CFD will be used in the single phased situations.

The severe accident in the fuel behavior, we'll be ramping up the program and making a strong connection in the branch so that we can work seamlessly across the sections.

MEMBER FORD: Could I just ask a question? Why entry level?

MS. UHLE: Because there's a lot of -- if you look at the Office of Research, there's a lot of experience in the Office of Research. And so with staffing issues, we're not allowed to be top heavy and all 15-10s there's some --

MEMBER FORD: Six to 1 ratio or something?

MS. UHLE: Yes. There's some, you know, there has to be some ratio. And this idea of everyone's going to start to retire, we need to bring in entry level and mentor and, you know, have a more gradual --

MEMBER FORD: I wasn't thinking of the 60 year olds, I was thinking of the experienced 40 year old.

MS. UHLE: Experienced 40 year olds.

MEMBER FORD: Given the fact that you've got a lot of workable --

MS. UHLE: We found some positions for 15s in the branch that we are hiring in the severe accident as well as the fuel behavior. In thermal hydraulics, if you look at who has been hired, they've been at the higher grade levels. So it's not all entry levels.

I say entry levels, that's in some sense we are more active in the entry level hiring because there's more positions available. But the office is looking at, you know, the higher grades as well.

MEMBER KRESS: Are any of those new hires here?

MS. UHLE: Yes, they're all here. You guys want to stand up.

MEMBER KRESS: They're all here.

MS. UHLE: Steve Bajorek. Da, da, da. He's our SL, senior level scientist. He's our experienced -- you're at least a 40 year old. Okay.

Joe Kelly, you know Joe Kelly. He is -- yes he's another -- he's another 29 year old.

MR. BOEHNERT: I think, Jennifer, you're going to have to give your age now.

MS. UHLE: 32 November 23rd. I just turned 32.

MEMBER KRESS: You're not counted in the new hires, are you?

MS. UHLE: What?

MEMBER KRESS: Are you one of the new hires?

MS. UHLE: No. I've been demoted to assistant branch chief. They won't let me touch the code anymore.

CHAIRMAN WALLIS: Jennifer, we're way behind in time. How long are you going to go on with this?

MS. UHLE: Chris Murray. For introductions, it's quick.

Chris Murray's from Penn State University. Tony Ullses from NRR. He's been sparing with Professor Schrock there for quite a bit.

And Joe Staudenmeier from NRR.

Chester Gingrich has been in severe accidents. He was doing some thermal hydraulics work, now he's going to go back to severe accidents.

And then, of course, there's Weidong Wang in the back. Shanlai Lu and Jim Han is doing analysis for us in the back. And Dave Bissette lead on the PTS work.

CHAIRMAN WALLIS: We're severely behind in time. Of course, you have given us more detail in some of these things, since you were going to summarize. Does that mean that we can move faster with some of the later.

MS. UHLE: I think the question is how

many questions get asked.

CHAIRMAN WALLIS: Well, you had a tremendous amount of stuff.

MS. UHLE: Well, when I went over it in my head, it went very fast.

CHAIRMAN WALLIS: We need to be finished-- or you need to be finished by 1:30 because we have another group, a very different group coming in and we can't short change them. So we're going to take a break now and then maybe you can work with your colleagues to get us through on time. You work with your colleagues to get us through on time.

And I'm a little nervous about Joe Kelly, he always runs over. Maybe we could find a way to prevent that happening.

So we'll take a break. Thank you very much. And we'll start again at 20 to 11:00.

(Whereupon, at 10:25 a.m. off the record until 10:40 a.m.)

MR. KELLY: My name is Joe Kelly, and I'll be talking about the TRAC-M code consolidation and development.

Now, the last time I was in front of this Subcommittee I was up here for 6½ hours. And since we're already an hour behind schedule, Professor

Wallis is concern is well taken.

So this presentation really is three presentations in one. I was going to talk about the code consolidation status followed by Jennifer Uhle talking about the SNAP development that's the graphical user interface. Then I was going to talk more about our long term development plans and a movement about how we're going to integrate some of our stand alone programs into the code development.

So what I'm going to do is condense the code consolidation status, you've heard a lot of this, in half, and Jennifer is going to skip this presentation, because you've hard about SNAP before, and then I'll try to give most of what I had planned to give.

When we first started this program back almost 5 years ago, we laid out five areas that we wanted to make improvements in. Modernize the architecture, accomplish the code consolidation to conserve resources, improve the ease of use, accuracy and numerics. And I was going to say something in each of those areas, but I'm going to shorten it because first I what wanted to do is give you an idea of where we are today. And that should somehow avoid --

Take my word for it. The colors on the view graph are much prettier than the colors here. That's really horrendous.

But anyway, this is where we are today. We have accomplished the modernization and the functionality. We have parts and functionality of TRAC-B and RELAP5. We do not have physical models of those codes, nor do we intend to implement all of the physical models of those codes.

What we're working on at the moment is called the component mapping, and that's the way that you take your RELAP5 component through the SNAP graphical interface and translate it to a TRAC-M component. And that's what's going to enable us to take the RELAP5 input deck, read it in and run it with the TRAC-M code. This work is almost complete. This line is supposed to show about where we are. It will be complete shortly after the beginning of the year, at which point we'll start a development assessment.

Now, originally the idea was to start the assessment and let the model deficiencies from TRAC-M show up as a result of the assessment. Then when you identify a deficiency, go look at them. First, go look at the models in the other code and try and make a judgment that, say, interfacial drag in TRAC-B is better than TRAC-P, etcetera, and then bring that model in. And that would then be a cyclical process.

Now, we're still going to do that to some extent, however there are two deficiencies that immediately showed up. The first is rod bundle interfacial drag, and that's what we've alluded to in the Peach Bottom Turbine Trip when Jennifer was talking about that. We simply couldn't predict the action in a void track and operate the BWR accurately enough. So what we're going to do is implement, again, basically the interfacial drag and interfacial heat transfer routines from the TRAC-B code to be used only for BWR channels and probably also the BWR core, but not globally.

Likewise, this is a deficiency that has been identified in the reflood model. I'll talk a little bit more about that. That's what I'm working on.

These will feed in as soon as they're finished through developmental assessment, and we'll have roughly about a six month period where all the models of the code will be frozen and go through the entire assessment matrix and then that leads to releasing the consolidated code at the end of calendar '02.

CHAIRMAN WALLIS: So maybe by the middle of next year or something you can show us some of your development assessment work?

MR. KELLY: Yes.

These are the type of slides I'm not going to belabor. The only thing I want to point out on this one is that we have something called an exterior communication interface, and that was built in to allow us to very easily couple the other codes or special modules into the codes for capabilities that we don't either have in TRAC-M or don't want to build in. It's already been done in an explicit with the REMIX code, the PPS calculations, and also we've done a preliminary coupling with the CONTAIN code.

We're skipping the SNAP presentation, and I'm going to not belabor this also, but we've put a lot of work in the draft communication interface making it easier to use. So if you ask our new group what is their highest priority item, this is it.

Most of what we need is going to be done in early 2002, but the playback capability will be mid-2002 and interactive display with user feedback, that is where you can run it like a simulator mode, is sometime in the future.

Documentation was mentioned earlier.

Documentation is a very important step and it has to be a continuing effort over the life of this project.

CHAIRMAN WALLIS: Is it true that the code has not yet run?

MR. KELLY: Excuse me?

CHAIRMAN WALLIS: The question I get is that it hasn't yet run, because it hasn't yet done these PWR transients or PWR LOCA or anything?

MR. KELLY: No, we did those. No. The code runs and it has been all throughout the process. We did the development that way. And there are several hundred test problems designed with each developmental version.

I shouldn't have skipped probably over this. It's seeing results of TRAC-M coupled to PARCS for the Beach Bottom Turbine Trip as well as a main steam line threat. So that is TRAC-M doing those calculations.

We can do BWR to our transients now, the reason I say early 2002 here is so that we can read in a TRAC-B input deck, and existing one, and run it in TRAC-M. All that capability is there. But the reason it has this date on it is for the upgrade to the interfacial drag package. Which Tony Ullses is trying to quickly put that in to see if it would work and make the improvements that he needed for Peach Bottom, but we want to put it in a more correct way according to what we call a low level modularity. And so that's when this work will be finished.

For the SBLOCA, I don't know that we've actually run any of those. The completion date here, though, is for the component mapping, you know, when that development work will be finished. And that's when the assessment for SBLOCA applications will start.

For large break LOCA, we could do a large break LOCA now but from my standpoint the reflood model was flawed so that this is the date by which we'll have an interim reflood model and we'll start doing the reflood part of the assessment matrix.

MS. UHLE: Joe, can I just clarify one thing on that. I'll only be a second.

This is the last --

MR. KELLY: Just don't get my slides out of order.

MS. UHLE: I know. I am just going to -- this was going to be my presentation on the buoy. Again, here, with respect to RELAP release, we have a RELAP5 version completely finished for the post processing and the model editor where you're dragging and dropping models. We can interact with that display. You want it in a simulator mode already with RELAP and TRAC. The date here being future is that with the idea of having -- we have a three dimensional model and right now when you look at the playback, you're seeing it in 2-D. We want, for the ease of use for the user, extend that so that you can represent the three dimensionality in a more easier way. So that's why under this there's a future, although we do have the ability. We showed you that last year, running of TRAC in an interactive mode while we opened valve and saw it blow down. So that's been in for two years -- I mean, for a year.

And in early 2002 being able to run, taking a RELAP5 input deck and converting it to TRAC and doing the drag and drop through the TRAC model, that's the last bit that we're doing right now. And also the plotting here with the mid-2002 date. Again, that's associated with three dimensionality. We can 2-D plot now already. We want to be able to 3-D plots very easily to get the surface plot of the core boil fraction and the 3-D kinetics.

MR. KELLY: Good. Thank you, Jennifer.

CHAIRMAN WALLIS: I notice the documentation is a continuing effort.

MR. KELLY: Yes.

CHAIRMAN WALLIS: Doesn't the documentation come first or do you write the code and then figure out what you did and write up the documentation?

MR. KELLY: As Jennifer said earlier, we have conformed with a fairly rigid SQA, certainly compared to anything that's ever been done with NRC code.

CHAIRMAN WALLIS: Then this documentation should be in good shape.

MR. KELLY: Yes, but each piece, each new piece has to be folded in in the overall documentation. And, for example, it was mentioned earlier that the TRAC manuals were extraordinary and it's hard to find your way around in some of them. Rewriting all of that from scratch is a major task. And what we're doing at the moment is basically putting in the pieces that we're changing. We do need to go and make all the whole restructure done but that's a huge effort and we've connected randomly to make it work at the moment, but we are going to -- that's why somebody has to start and keep working at it.

CHAIRMAN WALLIS: But it's important. The way you present the documentation is important; that's what's out there, people look at it.

MR. KELLY: Yes.

CHAIRMAN WALLIS: It's got to be credible and not have typos and all the usual stuff.

MR. KELLY: Right.

MR. BOEHNERT: Historically what's happened is the documentation was always put off the end and then somehow it never got done.

MR. KELLY: Right. What we're trying to do is have the people as they develop a model or implement a component do the documentation for that as part of the SQA. But it's still does need to get folded in better to a master document. We're not there on that yet.

CHAIRMAN WALLIS: As I said earlier, we can help in the early reviews of this documentation.

MR. KELLY: That would be very good.

CHAIRMAN WALLIS: We'd like to do so.

MR. KELLY: We also need to, as you know, improve the code accuracy. And really what I wanted to say here is we're beginning now to put the models in the code. And that's a huge effort, but what we focused on for the last few years is putting in capabilities of the functionality consolidating. But we've got a lot to do here and this is just starting. But that's not part of the code consolidation, that's part of us evolving to this actual state-of-the-art thermal hydraulic code.

CHAIRMAN WALLIS: Beginning isn't a good word, though.

MEMBER SCHROCK: One of the problems has been that the codes run part of the way through a problem and then crash, and then people fix it up and run the rest of it. In my mind that leaves a lower level of reliability when that kind of thing happens.

Do you have an objective for this code that that is not going to be allowed or is this going to be a continuing problem?

MR. KELLY: What we have the objective of is to improve the robustness of the code and that is just what you are talking about. It is making the code be able to run to completion and not only run to completion, but run without these periods where it just grinds to near like halt and you go to, you know, 10^{-6} time steps.

So what we're going to do is starting in the assessment when we start running in to those problem, the code either fails or it has significant swim outs, we're going to, in effect, ship that problem off to our numerics guru, otherwise known as Professor John Mahafty, who is going to help us track down the root cause of it.

Sometimes it'll be the numerics. You know, some like the way the water tracking interacts with level tracking of whatever. Sometimes they will just have to be an old condition numerical, an old condition physical model, the physical model that causes, you know, oscillations or causes you to accelerate your condensation as you go to saturation, which makes it hard to put numerics to solve. In which case if it's a physical model, John will kick it back to me and we'll work together to try to make it more robust. But, again, that's going to be a process. It's going to be a process over a lifetime of the code. But it is something we're committed to provide. John?

MR. MAHAFTY: Yes, this is John Mahafty from Penn State.

If I could make one comment on that. You know, I've given guidelines to people at NRC and other places that if the time step gives 10^{-5}, there's something wrong with the code, and I should see it. If it runs for any significant period of time below 10^{-4} there's something wrong with the code and I should see it. So, you know, we're not taking the kinds of shortcuts -- that's a good term -- that were done in the past. And I've seen problems where people have run RELAP5 and it grinds down and runs at 10^{-6} seconds for long periods of time and finally it recovers and goes on. That's not acceptable for us, because it tells you there's something wrong with the code, some kind of numerical problem is potentially masking what physically should be done and audited. It needs to be looked at and it needs to be fixed.

MR. KELLY: And I agree completely.

The numerics can also effect accuracy, and there are a few things here. In the future we'll be looking at higher order differencing in order to resolve things like thermal fronts. As most of you know, the difference in the code at the moment is first order accurate upland differencing so it tends to smear out sharp interfaces. Thank will be future activity. One that we have gotten created is level tracking. And level tracking doesn't just mean, you know, we are a 2 face interface hits and where is this continuity of void fractions. What it means is you find where that is and you go in and modify as part of the time step your mass energy and momentum conservation equations to take account of where that interface is on your computational grid. And I'm going to show you an example in an oscillating manometer problem of why that's important.

In this last, we reimplemented semi-implicit scheming code which turned out to be very revealing in order for that we could do our stability calculations so that you don't get the damping that can develop in implicit scheming.

This is an oscillating manometer test problem. Very simple. Two vertical pipes, they're each 10 nodes one meter long. And this is collapsed liquid level versus time. The two pipes are joined at the bottom, they're open to the atmosphere at the top so it's an air-water simulation. They were initialized half full at the 5 meter level with a velocity such that this should oscillate with an amplitude of 3 meters.

CHAIRMAN WALLIS: No friction?

MS. UHLE: No friction.

MR. KELLY: No friction. Water pressure is turned off. Thank you.

Hence, we can tolerate it, but the orange curve is an analytical solution, and when this was cut and pasted from the frame maker document into PowerPoint the curves got kind of shaky. But this is an analytical solution showing no dissipation.

The black curve was the TRAC-M calculation with a standard curve. And after about two cycles it's totally damped out. And the reason for that has to do with the discretization of the momentum-flux terms across that sharp interface.

CHAIRMAN WALLIS: It's a numerical diffusion, in a way.

MR. KELLY: Yes. It's an artificial --

CHAIRMAN WALLIS: Artificial --

MR. KELLY: -- viscosity that wasn't intended, but because of the way the two phase momentum-flux changes. When you correct that, and this was work done by Birol Aktas of ISL, this is what you get.

Now, the test problem was changed slightly --

CHAIRMAN WALLIS: Are all those points on the curve, those are predictions?

MR. KELLY: Yes.

CHAIRMAN WALLIS: That there on a big Sine wave which is in length about ten times the --

MR. KELLY: Okay. The legend is missing here. The upside down triangle is simply an identifier for the curve. It's not a point.

CHAIRMAN WALLIS: Oh.

MR. KELLY: And likewise, so what you're seeing are two curves sitting right on top of each other. And they're virtually indistinguishable, which is very nice that we can actually reproduce the innerlocal solution. But not only that, we make the test problem a little bit more difficult. It still is two pipes, but it's actually now six individual pipe components so that we could make sure that the level traction in steam could cross boundaries between pipes smoothly without putting any dissipation between that.

So, as far as the level tracking concern, there's no difference now between a no boundary in a pipe and a boundary between pipes. And it's just part of the QA process to make sure the model works.

There have been a number of improvements to the --

CHAIRMAN WALLIS: It seems to me there's a whole slew of these QA models you need to check, not just this one.

MR. KELLY: Right. And the more of that we can do the better.

CHAIRMAN WALLIS: And I think it's been one of the concerns with all these codes that they're okay for nuclear safety, but they can't predict some of these very simple lab experiments.

MR. KELLY: Yes. I don't hold to that theory. I think you have to predict the phenomena that are actually there.

CHAIRMAN WALLIS: I think you should, right. It's got to be honest.

MR. KELLY: And that's going to be a process, and one of the most important parts of this program is going to be the assessment. And that's got to be a continuing activity at a fairly high level for years, and just continue.

CHAIRMAN WALLIS: I hope you keep doing it already. Have been doing it.

MR. KELLY: I'm not going to really talk about the improvements to the kinetic module. That was pretty much gone over in Jennifer's presentation. I simply don't have the moxy to do it.

MEMBER SCHROCK: What did you do to the numerics again to change the picture so drastically?

MR. KELLY: Okay. If you get me off into details, I may have to go to Birol, but I think I can give you the idea.

MS. UHLE: Birol left. John Mahafty is his thesis advisor, he can answer the question.

MR. KELLY: If you have an -- in the

momentum-flux term there's an alpha row of DVDX. How do you discrotize that term across an interface is the problem. And if you look at the way it's typically done in RELAP or TRAC normally, it's really built into the two fluid model an assumption that you have these continuous evolution of weight fraction across the computational mesh. And when you do that, that term is suitably accurate. But if instead you actually have a sharp interface, so let's say you're on convection vapor out as this interface goes, but you're averaging between these cells to get these alpha rows and DVDXz then you introduce a dissipation term.

And I've actually even seen in some codes when it said dissipation, in fact sitation will reduce oscillation. But normally it's dissipating.

So what we've done is say we have this tracking scheme that tells where this level is. Now in our, you know -- we basically pull our back of the envelop and write down what the momentum equation should be if you've got the single phase vapor going across this with this level approaching it. And then put in, adjust the terms in the momentum equation and make them what they really should be.

MEMBER SCHROCK: But I thought there was

already a level tracking in the original track.

MR. KELLY: There was one in TRAC-B.

MEMBER SCHROCK: Yes.

MR. KELLY: And there was -- it works more as an interface sharpener. So what it would do is try to track where the level is and adjust interfacial drag in an interfacial heat transfer model. Level tracking has to, if it's going to work right, has to do a lot of things. And so it basically it turns interfacial drag down. It says, okay, there should be a level here. Let's lessen interfacial drag so we don't pull this liquid up when we shouldn't be. Likewise, it says okay the interfacial area is a pipe instead of treating the vapor as bolts.

But that's just really -- that's the easy part. The tough part, which really gets this to work, is going in and actually fixing the conservation equation for a different physical situation.

CHAIRMAN WALLIS: So this goes back to what I said this morning. Jennifer was talking about a pipe. You can recommend an equation for a pipe, and in fact they were this way, so that it behaves like a pump.

MR. KELLY: True.

CHAIRMAN WALLIS: Under some circumstances because of the way you're averaging the stuff.

MR. KELLY: You have to be very, very careful. And this is something that Birol under John's guidance did a very good job on.

The last stage of the consolidation program is developmental assessment. And what I've done is put together an assessment matrix that we're going to start to do during calendar year 2002. I'm going to give you an example of how that was put together.

I also have a handout, I'll give you what I've proposed test matrix says. I've got that written in, I'll get it to you.

Now, the test matrix is quite extensive, but it is far, far from comprehensive. I mean, there are whole areas that are left out. And those areas are going to have to be plugged by the assessment we do in the future. And that's why Steve Bajorek is going to talk after me.

We're going to a per face developmental assessment for each of the applications that the code is going to be used for.

So what I'm doing here is, remember our success criteria for the consolidation. For the TRAC-M code we will be able to run it against each of the predecessor codes; TRAC-B, TRAC-P and the RELAP5 for the application of interest for each of those codes, and TRAC-M would do at least as well. That's our success criteria.

CHAIRMAN WALLIS: Do you have a matrix like this for simple experiments, like the manometer as well as these --

MR. KELLY: Yes.

CHAIRMAN WALLIS: -- messy experiments where everything's going on and you get AB compensating errors and so on?

MR. KELLY: There's about half of those in the works. And that's something that could be expanded.

This one is for separate tech specs reflood heat transfer. And what I'm going to do is just give you an example of how this got made up.

The first thing I did was for the three predecessor codes; this was TRAC-M the F77 version, which is basically just TRAC-P. There are no models in this version at all. An assessment of that was done relatively recently, and that's the document NUREG/CR-6730, and that was published, I think, about a year ago.

For TRAC-B the last NUREG-B developmental assessment code was, i believe, the 3663, and after that there were two other NUREGs by other contractors that did further assessment of the TRAC-B code. And I also had input from INEL and Penn State.

For RELAP5 this was the last published development assessment of the code, because there was also an assessment of it in this NUREG as well as the assessment we did for as part of the AP600.

So I looked at all of the tests that were done for these, and for this phenomena listed each of the ones according to the code it was used for. And we then in the TRAC-M column, I basically summed them.

Now, if we simulated with one of the other codes, I brought it over and stuck it in here. And so these are the ones we're going to do unless there was some reason not to do so, and that logic is what I'm going to show you now.

All of the codes the flux is at 31504; that's a one inch per second 40 psi base case force reflood test. So, obviously, we're going to do that.

31701 is 6 inches a second, so that's at the other end of the spectrum, so that was done in RELAP5 and we will include that here.

Now, this test 33436 is a gravity reflood test done in FLECHT SEASET, and because of the way the downcomer is and the way the exit coming up the front is there are a lot of uncertainties in the downward positions. So there's no good reason to do a gravity reflood simulation for that facility when we have facilities like CCTF and SCTF. So I'm going to eliminate this test.

Now, when I looked at -- I just mentioned CCTF. As part of the TRAC-B assessment measure there was a CCTF basis run in 14. But what they did is actually a gravity test, but they ran it as a forced reflood test. What that means is they stripped off the downcomer, stripped off the wall clamp and imposed a flooding rate at the bottom of the core. That's artificial. No one knows what that flooding rate is. They inferred it, it was inferred from the experimental data based upon what came out the top of the bundle and the build up of inventory in the bundle.

So running this as a forced test -- I mean if you're not even monitoring what you're putting into the bundle right, how can you do an assessment of it. So I think this is of limited value and as part of the integral effects testing we will be doing a couple of CCTF cases. That's something in the future we'll have to expand. I saw no point in doing this, so I took it out.

FLECHT SEASET there was one test run for TRAC-B, and obviously we're going to keep this. This is a large scale reflood gravity. It's 8 bundles, 2,000 meter rods lined up in a slab, so it models at full scale the distance between the reactor core center line and the core barrel. Very important contributing effects. The Lehigh Rod Bundle, this was done with TRAC. It's a nine live bundle, so it's 3 by 3, which means it's about this big, and there's a heated shroud there's a lot of questions in its regard to things like heat losses, its quality fully instrumented and plus as you know, if you try to do a two phrase, and this is also in one atmosphere, test in something this big, any vapor structure is going to span it and it's going to act not like a broad bundle at all, but more like a tube. So it's not productive, its of limited usefulness, let's not waste our time on it.

FLECHT test 9077 which was done on TRAC-B, is from the original FLECHT series and it's 6 inch per second new core rate capacity. Now that facility does not have delta P cells, and likewise did not measure specifically the steam temperatures. There is a lot in that less experimental information than there is with the more modern codes like FLECHT SEASET. So I'm going to get rid of this test and replace it with 31701.

The GOTA, and I know I don't pronounce that right, but this reflood test is combined top spray cooling and bottom reflooding. Well if we're going to discuss the BWRs you need to have assessment cases for that, so we've got to keep this.

The NEPTUN facility which was done in Switzerland is 33 rods of half height. Now, again, 33 rods as counted as 6 by 6 with the corners taken off, is relatively small.

The two tests that were done, one was at one 1½ centimeters and one was at 15 centimeters a second. So what I've done is instead of doing these two tests, I'm going to substitute the FLECHT SEASET test run. This one is 6 inches a second. I added this test, which is 34006, which is 0.6 inches a second to compensate for the NEPTUN test that I'm going to drop out. So what I'm trying to do is to come up with a test basis that makes sense and covers the range of conditions that we have been testing before.

CHAIRMAN WALLIS: Of course, the advantage of something like NEPTUN in is that it's not -- your conclusions are not test dependent so much on FLECHT. You can say you've got something independent, you're able to predict. And if there's something wrong with the modeling because of the geometry of NEPTUN, maybe that means that something should be in the code anyway. So you might see if you can get a more diversity, perhaps, in the sources of the experiments.

MR. KELLY: What we will be doing -- remember, these are -- we will be doing in the future, okay. We're going to expand the matrix both in the CCTF and SCTF, and this is something that Steve is going to talk about. There are actually forced reflood tests in SCTF, which are of great value because then you know actually what you're showing in. You don't have the complications of, you know, potential oscillation and some down time.

CHAIRMAN WALLIS: You've got to prepare NEPTUN. When you sort of release the code somebody else may, and you might want to do it ahead of time.

MR. KELLY: The problem is there's a lot of data out there and we have to just do the best we can.

MS. UHLE: Our international partners have, especially Switzerland with respect to NEPTUN, are interested in doing assessment for us. And that's what helps us get, you know, broadening our assessment range. And so I think that first to meet the 2002 deadline, we are trying to make one that, you know, take some consideration with the good data that's out there and then in the future, with the fact that we have the PM program, that really broadens out our assessment on this.

MR. BAJOREK: We would welcome other groups coming in, any additional tests that would have matrix. One of our problems has been resources and trying to pick the test step that will give us the most information without letting the matrix get to out of hand.

MR. KELLY: And I'll give you a copy of the proposed matrix as soon as I get off the stage here.

That ended the part the presentation on the status of the code consolidation. And we're going to skip over the presentation on the SNAP, and what I'm going to jump into now is instead the code development effort for the future.

Again, when we first started this project we went out and queried our users, both internal and that would have been NRR and was then a ADOD, as well as PT and RAS. And our external users liked, you know, he said, if we're going to have a state of the art thermal-hydraulics code, what should it have in it? And this is the laundry list they came up with.

In items number 1 was an improved user interface. And that part of the reason why we're putting in the effort on this now, to make the code easier to use.

I'm not going to go through all of these, but I'm going to do instead --

CHAIRMAN WALLIS: Well, where there's a gap, does that mean you're not doing it at all?

MR. KELLY: No. It means that that it hasn't started.

And what I'm going to show you now --

CHAIRMAN WALLIS: You're not using modern numerical method?

MR. KELLY: Well, that means that developmental efforts incorporate, for example, either higher order differencing or a more fully implicit scheme has not started. And for what I'm going to show you, again the colors are abominable on the viewgraph. I don't know what it worked out that way, but --

MS. UHLE: They're extraordinary.

MR. KELLY: To say the least. This is our plan for what we're going to do next year and the in the future.

So, up to here is the conclusion of our current five year plan. From this line on is the future.

Now, I've broken this down into these categories: Consolidation and assessment; physical models; numerics improvements; modeling capabilities; and, then along the bottom I've shown code release dates. And this Rev zero will be the first release of the consolidated code, and that is at the end of 2002. And what we're planning is annually at the end of each calendar year to release another revision to the code.

And now let me explain this a little. If you could see the colors, there's a color code here. This is supposed to be a light blue. You notice these boxes go with this code release. So these activities will be finished and go into this code release.

Likewise, the green boxes feed into this one.

CHAIRMAN WALLIS: You must have had a color consultant or something.

MR. KELLY: Well, apparently I didn't have a very good one. A budget decrease.

And so forth. And then actually this is

revisions 3 through 5. I didn't -- once we get out this far in the future I don't know exactly what we're going to be doing when.

CHAIRMAN WALLIS: Why didn't you use the primary colors?

MEMBER SCHROCK: If you put actual dates in there, whatever they're going to be, 0.0 is October 1, 2002.

MR. KELLY: No, these are the calendar year.

MEMBER SCHROCK: Calendar year.

MR. KELLY: Yes.

MEMBER SCHROCK: So that means January 1, 2002?

MR. KELLY: Exactly. That'll be the first release of the consolidated code.

MEMBER SCHROCK: So that's 13 months away and you don't have a document that describes the code in any complete way today. You intend to have one prior to that and have some feedback as to how good it is?

MS. UHLE: You want me to answer?

MR. KELLY: Please.

MS. UHLE: We have -- I mean, because we started from a TRAC-P code, we have the base TRAC-P theory manual. And in-house we're going through that, Frank Odar, Jim Han and they're pointing out where things are confusing. With the developmental work that has been going on we follow S2A procedures. And when it involves physical modeling, of course, there are sections written by the developers documenting what was done so that those sections will be put into the theory manual. So we have the documentation. It has to be merged and it has to, again, get another read through to make sure that there are --

MEMBER SCHROCK: I guess what I'm asking is are you going to release this whether the documentation has been reviewed or not?

MS. UHLE: No, no, no. We will have the documentation released with in-house review and if you're offering review from the ACRS if that's what you're offering. But, yes, I mean we realize that it's fast approaching. But I don't want you to think that there is no documentation.

If you go up to the consolidation room, there's documentation like up to here. It's a matter of going through, organizing it and putting it into the master document. Now everything is written in the same way, word processor format, so that's going to facilitate things. And then we're starting to begin the merging.

The user manual is up to date. We have to put in modeling approaches on how to model the BWR, but we're going to take out of the TRAC-B and, again, go through read through and add to it as necessary. But the user guide is the one that's in the best shape, and the theory manual is our one that -- especially during the developmental assessment and we start to replace physical models, we'll be adding to that.

The programmer's guide talking about the architecture of the code, we have made revisions to that with the modernization, but that one is the one that's lagging the most, although because we're focusing on making this code more maintainable that is something that we will have to do.

MR. BOEHNERT: Does the master document include those three things you just mentioned?

MS. UHLE: No. There's a theory manual, that is a master document. There's a user guide, that is a master document.

MR. BOEHNERT: So each one -- okay.

MS. UHLE: But, again, if you're interested in looking at what we've generated so far and reviewing it, then we would be, I would think, more than willing. Although I don't know, I'm not the office director.

MR. KELLY: Thank you, Jennifer.

CHAIRMAN WALLIS: Is this thing suitable for any two phase flow problem? I mean, it doesn't have to be nuclear reactors, does it?

MR. KELLY: Well, you can keep the components of a couple of different pieces.

CHAIRMAN WALLIS: I think it would add a tremendous amount of credibility if it was something like the commercial code which is out there and has been proved to work for oil and gas, and chemical plants and all kinds of things. If it works for all these other areas as well, then it must be really good. When it's only been shown for a couple of nuclear applications, then it looks real suspicious.

MS. UHLE: We are getting requests for the code for the oil industry. And also heat exchanger.

CHAIRMAN WALLIS: It would be very nice if you could in some of these presentations, particularly the public presentations show that it's not just been tuned to some nuclear applications. Okay.

MR. KELLY: For the consolidated code, what is going to show up is what we've talked about before, finishing the way to translate RELAP through

SNAP to be able to run it in TRAC-M, the developmental assessment and then there's the two model changes, the bundle interfacial drag, which is an implementation of the TRAC-B and the interim reflood model, which I'm working on. And then we are going to do some work in the beginning of the year on robustness.

And one of the things I tried when I set this up was have development activities in approximately in mid-year so that we would have a frozen type version for, hopefully, as much as six months to go through the testing before you get to the release date. So that on December 31st we're not changing the code model of a code that we're going to release January 1.

Probably one of the most important activities here is the PIRT based assessment which I show across the top. And that's what Steve is going to talk about. And it can be this assessment where you look at the important phenomena and see how well the code does against them that then will drive what we do here.

The only other thing I want to talk about is some of the model development from out experimental programs. The green box here is supposedly subcool boiling, and that refers to the UCLA program on subcool boiling and low pressure. We're going to receive a model approximately mid-year and we'll be implementing it during the end of 2002. But because it's going to come in at the end of year, I don't want it in the release code version because we want in for suitable testing. So it will be part of the Rev 1.0 release.

This box is phase separation and this is to build on the experimental work at OSU. So when we're able to get a model from them that we have confidence in we'll be putting in the code, hopefully, in early to mid-2003 to show up in the Rev 1.0 code.

The other one is -- it really should be more mechanistic but obviously we're not thinking we put first principles, but more mechanistic than certainly what we have today. And that's going to build on the external information from rod bundle heat transfer facility at Penn State. And I have several slides on that later.

These tests are not yet defined, but I have an idea of what's going to go in them. And those actually take the next few slides. I'm not going to go over those in detail, but this is what we anticipate as of today that we're going to have change in the code to make it really do a good job on more and more reactors. Core spray model, boiling transition. For example, the model incurred is normally the OXY correlation, which is basically the annular pore regime in tubes. Obviously that does not give a very good representation of dry auditing of water reacting models, but also putting the place in the code that supports, if you will, where a user in NRR or actually at the request of NLR can incorporate on a temporary basis a proprietary model in order to help them facilitate their review. You have to adjust modern fuel designs and fuel designs. And obviously, as I'll show you, the reflood model needs a lot of work. That applies to more and more reactors as well as to pressurized water reactors. And also we'll have to look a little more at top-down rewet both on the channel box and the fuel lines.

MEMBER SCHROCK: This item on the BWRs incorporated the proprietary model, you have in mind something like what GE says they have for their rod bundle on the first principle.

MR. KELLY: I assume what you mean is where they have a drop of pH and look at the stripping of the drops and the deposition of the drops downstream of the grid. That's not what I meant. What I meant here was the better correlation. Like, for example, I'm looking at pressurized water reactors. Each vendor of each fuel design goes through a testing program and develops and in effect licenses the correlation for that particular type of fuel. And then if you go and do some kind of operational transient where your success criteria is DMBR margin, well if you forget to write thermal-hydraulic conditions versus time, but you want to check the margin, you need to have an actual correlation for the BMBR that suits that fuel geometry. And it even depends upon all the little tabs on the rib spacer, and it's somewhat analogous for boiling-water reactors.

MEMBER SCHROCK: Well, here's you're talking about boiling-water reactors.

MR. KELLY: Right, that's true.

MS. UHLE: Can you do a Drexal correlation?

MR. KELLY: Well, what I'm talking about is more like that.

MEMBER SCHROCK: That's what I'm talking about.

MR. KELLY: Not going to actually trying to predict it by stripping the film off the rods and then depositing the drops downstream; that would be a research project.

CHAIRMAN WALLIS: Joe, you are half way through your slides and taken about the time that was promised.

MR. KELLY: Really? I thought this was going pretty fast. Okay.

MEMBER SCHROCK: One last simple question. Do you envision this option to incorporate the proprietary model as something to be used by industry in their use of the code or something you would do with your code?

MR. KELLY: I envision it as something that we would do in order to facilitate doing our in-house calculations. But it would be something that other people could use to more easily implement.

MEMBER SCHROCK: Yes.

MR. KELLY: And this is, you know, just what we would like to do.

MR. BOEHNERT: Well, I don't know how you get around, though, the thing that these codes are supposed to be publicly available. I mean, that's --

MR. KELLY: That's why we're not going to build it into the code.

MR. BOEHNERT: Yes, I understand.

MR. KELLY: Just a box, you code it

yourself.

MR. BOEHNERT: Yes, a black box you put it in. Yes. Okay.

MR. KELLY: Similarly, I've looked at, you know, based on what we've done in the past, we looked at small break LOCA, what were the problem areas. And we made up a laundry list of where I think once we really start doing the PIRT based PA we're going to have problems. And this is the list, and in the essence of time, I won't go through the list.

I have a similar one for large break LOCA. And that takes me to what we're doing now, which is the current model development activities. And there are two, as I've mentioned. The first is not model development so much as it model implementation, so it's a rod bundle on interfacial drag, boundaries necessary for the Peach Bottom Turbine Trip benchmark. What we're going to do is implement the TRAC-B interfacial drag and interfacial heat transfer models all in route for the CHAN which is a BWR fuel assembly. And we're going to look at applying it to the core region of the 3-D vessel. Because, obviously, the interfacial drag per bundle is better than the correlations we have at the moment, which were mainly focused for 2-D. And it'll just be implementing them at what I call low-level modularization. This is an in-house effort by Joe Staudenmeier and Tony Ullses.

The development activity is to come up with an interim reflood model, and it's necessary for doing realistic auditing calculations for the AP1000. The reason it's necessary is the current model has unacceptably large oscillations and at least for separate FLECHTs tests it's highly conservative. I'll briefly show you the results of one of those.

We have to look at two things; the physical models and also the fine-mesh numerical scheme, and also is an in-house effort with Weidong Wang and myself.

I'm going to skip the fine-mesh rezoning scheme, just in the interest of time, unless there are questions about it.

So I'm going to skip over the next two slides.

MEMBER SCHROCK: You never question the adequacy of flow regime maps in the code.

MR. KELLY: Well, I do. Do you mean the idea of using flow regimes in general or the ones in the code in particular?

MEMBER SCHROCK: Well, I mean the ones in the code in particular.

MR. KELLY: Yes. I mean, certainly something that's based on a one inch diameter air-water atmospheric pressure is not anything close to what, you know, we should be having in reality. And that's something we have to look at. There's lots of areas of physical models --

MEMBER SCHROCK: But it isn't going to be a part of the TRAC-M development?

MR. KELLY: Not part of the development to be released at the end of December 2002. In my master--

MEMBER SCHROCK: So you think it will be eventually?

MR. KELLY: Yes.

MEMBER SCHROCK: Okay.

MR. KELLY: I'm pretty sure. There was an item on here for low pressure interfacial drag that I didn't talk about, and we're pretty sure that once we start doing things like AP1000 and low pressure EKD models that we're going to over predict interfacial drag. And that's a point where we revisit the bundle interfacial drag model and try to establish a database and maybe come up with a new model if we can't find or develop one that is accurate enough.

The whole idea of replacing flow regimes is out here much later in time, and that's the interfacial area transport work.

But speaking of flow regimes, there are a number of idealized points in reflood. And what I'm showing here is clad temperature versus time of 1 inch porous reflood case, and this is the heat transfer coefficient versus time. And so at any one point you do through a progressional regime starting with steam cooling. The steam cooling actually probably stopped here, and this is when the dispersed flow film boiling started. The dispersed flow film boiling started as the most important regime simply because that's the point in which the turnaround in the clad temperature established a peak value. So you always think you need to model this very well. However, there's another regime just a little up stream of it which I've labeled the froth region here. And in the future you'll hear me talk about invert slug, invert annular, those types of things.

This region could be anything from a few inches to a couple of feet, depending on the flooding rate and liquid subcooling. It's very important from the standpoint that with this cooling that brings the clad temperature down to your quench, the temperature at which the rods get wet. So not only does it control the propagation of the quench time, but it's again that the vapor generation in this area and at the quench time provide the source term for vapor flow and entrainment that you have in the dispersement area.

Currently in TRAC-P -- this is from the manual, this is the reflood heat transfer coefficient module. And I don't expect you to be able to read that from this, but it's okay.

This is an imagine the idealized flow regimes when we go from transition boiling, smooth inverted annular, rough wavy, agitated inverted annular, dispersed flow, highly dispersed. In all these different regimes, you go through the code and you use a weighted sum of contribution to each regime. So what you have is one model turning on, ramping off and another model turning on and ramping off and so on, and you add all these pieces together. Well, it's highly confusing, it's also very complicated.

CHAIRMAN WALLIS: That is the problem in using a high pressure syllabus. I couldn't read it, so I thought the flow was coming from the right.

MR. KELLY: That's hysterical --

CHAIRMAN WALLIS: You're going through

bubbly and slug and annular.

MR. KELLY: But worse, you know it's bad that it's so highly complicated. But what's worse is that it's poorly suited for inclusion in the computational model. I'll briefly tell you what I mean. Each of these regimes is characterized by a link and that link is a function of capillary number. So this is based upon the type of break-up you get if you take your garden hose out and turn it upside down and have a jet coming down, when that jet breaks up. So each of these links is a function of the liquid velocity, at the quench front. And any of you that have ever worked at code calculations you know how noisy that is.

So what that says is the length of each of these regimes that's been used oscillates with the liquid with velocity. So, in effect, this type of scheme amplifies any numerical noise whatsoever. And in practice, for a forced reflooding case it leads to very large oscillation that throws most of the liquid out of the bottle.

MEMBER SCHROCK: I mean this view of physics ignores the fact that when you look at such experiments you actually see some large masses of liquid that get thrown up and then they fall back. So at any given instant what's happening at some point above quench front is some liquid going down, some going up and some hitting each other, of course some not and net flows. There are aspects of the physics that are not recognized in this view point.

MR. KELLY: And there are aspects that we will never capture, even if we implemented second liquid fuels so you can have some going up and some down. Because we'll always end up having to treat it in a time average sense. You know, averaging over some suitable period which may be on the order of seconds. But that's --

MEMBER SCHROCK: Well, this is a very fundamental issue with regard to these equations altogether. You have variables which are presumably space and time averaged. No attention given to what that really has to mean in terms of specific parts of the two phased domain, where in fact the time scaled at which you have to be doing the averaging is pretty long. It's a little bit of a stretch to imagine that you really have meaningful time smooth variables that you can work with the same sense that you do, for example, in single turbine and single phased flow.

MR. KELLY: That's a limitation that, you know --

CHAIRMAN WALLIS: This is the present state of the arch you put up there and they're going to improve it.

MR. KELLY: Well, the first thing I want to simplify it and come up with the energy and hopefully, the plan then is to use the theta from the RBHT facility to come up with a more mechanistic model. But to go to the one more detail that Professor Schrock is, that's really out there, especially in a computational framework where you're talking of modeling the power output.

And as a result, this shows an example. This is FLECHT-SEASET 31504 which is the rate excessive force flooding case. Clad temperature versus time, this is just above the core mid-plane. This is the data from three different thermal couples, and this is the current TRAC calculation. And you notice this is more than 300 degrees K, and this would be completely --

MEMBER SCHROCK: Well, it's conservative.

MR. KELLY: It's highly conservative.

MS. UHLE: Extraordinarily.

MR. KELLY: At least for a forced flooding case and that's because there are in effect these vapor explosions, if you will, which throw most of the liquid from the bundle out the top and FLECHT-SEASET, you know, can point to that as a phase separator so the water can't come back down. So a one inch per second case ends up being like a one-tenth of a second case which with that flow rate we have a very hard time turning the temperature around.

CHAIRMAN WALLIS: It's pretty good to be up at 1400 degrees Fahrenheit.

MR. KELLY: So, obviously, we have some work. This is why we're doing the work, while we try to apply this to AP1000. And so, obviously, improvements need to be, we have to reduce the oscillatory behavior and improve the accuracy of the prediction. And I'm going to try to do that with using a simple modeling first, and wherever I can use bundle data, sometimes tube data to come up with a simple way of doing this and one that is less suspectable to oscillation.

I'm now on the last page of my talk and, hopefully, this is practically finished.

CHAIRMAN WALLIS: Well, you've had your hour.

MR. KELLY: Yes, I'm afraid so.

And I'm going to talk about incorporation of experimental results, and hopefully very briefly.

And I've got a little note here, the ACRS role is I think that would be very good. As we do these experimental programs and these are our contractors who are asked to develop models from them, it would definitely help us to come in front of you, present those models and get your opinion. And in effect, for us to have a peer review via you of how good those models are before they get, you know, encapsulated in concrete. So this is certainly an area where I think you could help us. You know, as kind of as unpaid consultants.

We currently have four experimental programs. Low pressure, subcooled boiling at UCLA, phase separation at OSU, which you already know about since it's been out there, the rod bundle heat transfer programs at Penn State, and the interfacial area transport at Purdue and the University of Wisconsin.

This general program will be finishing in the middle of the year. The model will be delivered and they will be implementing the code late 2002.

Phase separation, hopefully that in 2003.

The rod bundle heat transfer, this is one I want to talk a little bit more about. It's designed to provide detailed measurements for model development. It's not simply let's get some more reflood data, because there's a lot of reflood tests out here. But there was a lot of thought in how to try to instrument the bundle and what development information we need. And that's the example that I'm going to use on the incorporation of experimental results.

The reflood tests will be conducted in mid-2002. There will be 15 of them, roughly 12 or 13 will be for model development. There will be no constant flooding rate for test cases to look at one particular regime.

There will also be 2 or 3 variable flooding rate cases which we'll use for code validation.

But we're also going to use steam cooling and drop injection tests, and I'll talk about those in a little bit. And those will be in late 2002. Then the data analysis and model development will be in 2003/2004. And at that point we'll have low mechanistic reflood model in the code to do that.

The interfacial area transport, this should be viewed as a long term exploratory research program and the idea is to try to move the level of the physical models one step closer to something mechanistic where you're now looking at pebble coalescence and breakup instead of the static flow regime model. And so we're due to implement this model in 2005, however the data is being generated now we'll be able to use as part of PIRT assessment program.

And the key thing on this slide is as these programs end, we hope to start other experimental programs to take their place so we keep the level of thermal-hydraulic experiments that we're funding more or less constant in time instead of on/off. But replacements to these experiment programs will come about from code assessment results. We identify a deficiency in the code in an important element, can't find the data in the extent database and get a targeted date, then we'll identify an experimental program and try to get one started.

So this is the example of how to incorporate the experimental results. At least the beginning of that. What I'm going to talk about is the dispersed flow film boiling agent, which is the one that we think of as the most important in terms of the large break LOCA because that's where you turn around the clad temperature.

In this regime the most important heat

transfer mechanism is forced convection from the rods into the vapor. To the superheated vapor. But there are two major unknowns. One is the drop diameter, which is a rather fundamental quantity and the other is two-phase convective enhancement.

The drop diameter is primarily important because of its effect on the vapor superheat. I mean, after all, that's your sink temperature. You're transferring heat via conduction through the steam to a highly superheated steam. So what that temperature is is very important.

However, it also effects drop breakup on the grids, the two-phase convective enhancement, as well as the wall-drop radiation heat transfer.

In reflood the drop formation mechanism is not known, and every paper you read says something different. Is it aerodynamic breakup of liquid slugs, or a breakup of an actual inverted annular column. Sometimes it may be one, sometimes another.

There could also be wave entrainment either from waves on an inverted annular core or if you're in a low flooding rate case where you actually have annular core below the quench front it can be waves on that film.

You can also have wall to drop

interactions. A drop can collide with the wall and bounce off and shatter, or it can collide with the wall and in effect be blown off by rapid boil, and that can shatter the drop.

Which of these mechanisms or how these mechanisms interact to control an average effective drop size is really unknown.

CHAIRMAN WALLIS: These are all speculations or fantasy, you mean?

MR. KELLY: Yes.

CHAIRMAN WALLIS: They're not based on observation?

MR. KELLY: Well, some of them are.

CHAIRMAN WALLIS: They are?

MR. KELLY: Yes. Depending upon which paper you read, various people say different things.

CHAIRMAN WALLIS: Is that because they've actually seen it or they've speculated it?

MR. KELLY: Well, some of it is seen. For example, the annular film and waves on the annular film comes from a British paper reflood in, I believe it was a quartz tube.

The breakup of liquid slugs, I don't really remember.

But, you know, I've been through a lot of references and you see a lot of different things.

Two-phase convective enhancement, what this is, you know, we know that the core's conduction is steam. But if you have a dispersed phase, whether it happened to be solid particles or drops, that will effect the heat transfer rate. And now especially in the case of drops, the act is heat sink, so vapor sources -- preliminary estimates of data say that this should enhance your flows convection heat transfer by 20 to 100 percent. But, again, the controlling phenomena is not known. Is it via turbulent enhancement?

We know from like, you know, grasped particles in air if the particles are very small, in the order of 30 microns or so, they do tend to excite the turbulence and increase the heat transfer. If those particles go up to about 100 microns, they damp the turbulence and decrease the heat transfer.

Our drops tend to be more like 1,000 microns. So how do they interact with the turbulence? But, of course, there's not one drop size anyway. There's a spectrum of drop sizes. Some might enhance the turbulence, some might dampen it. But once you get up to a millimeter and larger drops, now you've got drops with significant weight regions which could generate more turbulence because of that.

Likewise, if you have all these drops distributed in this hot steam, you change the temperature profile of the steam.

CHAIRMAN WALLIS: It's dispersed flow boiling, it's not film boiling. There is no film. It's dispersed flow boiling.

MR. KELLY: That's true. That's, you know, just the way it's been. And what we're trying to say is that the surface is dry.

CHAIRMAN WALLIS: Yes, but what I think what they mean is the surface is dry.

MR. KELLY: Right. That's what the film in that context means. If you will, a vapor film.

So those are two of the most important things or us to look at. And how are we going to do that with the rod bundle heat transfer facility. Let's talk about drop diameter first.

And what I've done is basically put up all of the drop diameter data that I could find in the open literature, and this drop diameter data from a reflood test. And, as you know, there's tons of data for primarily air, water and tube annular mist flow, but even if you go from one of those papers to the other, what the correlations for drop diameter are are different; there are dependencies on physical properties or even the vapor momentum flux are different.

So what I've applied are sauter mean diameter versus pressure reported in a test. ACHILLES and FLECHT-SEASET are actually bundle reflood tests. This is the FLECHT-SEASET data all run at about 40 psi. I spread it out in pressure just so you could see the points, but they're actually all at the same pressure or almost the same pressure.

These are from a -- high speed group from several -- about half a dozen different reflood tests, different flooding rates and so on. It's actually pretty amazing that the sauter mean diameter is as constant as it is, just a little above one millimeter.

CHAIRMAN WALLIS: Six millimeter is a humongous drop.

MR. KELLY: Yes, that's a problem, too. And what you have then is water plugging the tube, and that's why the drop can be carried up. It's the container wall effect otherwise for those cases the vapor velocity would be low enough you couldn't carry the drop up.

CHAIRMAN WALLIS: Even one millimeter

seems pretty big.

MR. KELLY: I agree, especially if you look at a rod bundle with the grid space, and you go how can a poor little drop get through.

The ACHILLES tests, those were actually from two different reflood tests, but the distribution isn't --

CHAIRMAN WALLIS: So mean diameter, that's a mean diameter of 6 millimeter. It must mean some of them are two centimeters. That's crazy.

MR. KELLY: Well, it can't be bigger than the tube. I agree, those are huge.

These tests, these are rod bundles, these are tubes. This Hall & Ardron, this was done at CEGB I think in the early '80s, I don't remember. This was done at University of California Berkeley by Seban et al.

This is reflooding --

CHAIRMAN WALLIS: Before they married to one another.

MR. KELLY: It's hard to know which of these to believe. But it would appear --

MEMBER SCHROCK: This kind of statement bothers me, it's hard to know which of these to believe. These experimentalists are presenting data from different kinds of experiments and why do you think that as a code developer you're going to evaluate which among these that had maybe different objectives even, is right or wrong? I wouldn't begin by assuming some are right and some are wrong. I'd begin by trying to understand why is there this kind of apparent discrepancy that arises out of these different kinds of experiments and how does it relate to the simple or the actual system that I'm trying to model with this code.

MR. KELLY: No. That's a very good point. Both of these were tube tests, but one was a quartz tube, one was a, I don't know if you know the CRE valve. They were both basically the same kind of traditions opposed to directed methods of tube resonance.

MEMBER SCHROCK: If you get into details of the paper, you'll see that the credibility of the meaning of a sauter mean diameter for some experiments may be better than, you know, some other experiments.

MR. KELLY: Depending on the sample size, that's exactly correct. And what I probably misstated, mispoke a little -- what I should say is this isn't solely a function of pressure. And what you may very well be seeing here, rather than one being right and one being wrong, there may be at different values of the vapor momentum flux, and that may explain the large discrepancy. But from everything I've seen so far, is the vapor momentum flux goes up, the drop diameter goes down.

CHAIRMAN WALLIS: I guess the message I get is that you're looking at all these things, you're trying to figure out the reasons for discrepancies and do better at it.

MR. KELLY: Right.

CHAIRMAN WALLIS: At the level we're at today, we can't get into the details.

MR. KELLY: Right. Most of the current models that people tend to use in codes are simple functions of the LaFosse number. So it's a function of pressure only.

CHAIRMAN WALLIS: LaFosse with gravity in it?

MR. KELLY: Sigma over G delta rho squared.

CHAIRMAN WALLIS: Does gravity have anything to do with the phenomena that's happening here?

MR. KELLY: Well, what they're saying is that you can only -- if you're given vapor flow, you can only get up to a certain size drop, and less, then use a critical web number of criteria for what that size will be equated to and you come out with that. And that's roughly how you come up with that. And that will give you the maximum size drop, then you have to make some assumption to get from that to a sauter mean, typically a factor of 3 or so, but you know what exactly it is is hard to define. But it looks like there's a pressure relation here, but that could be some other reason.

CHAIRMAN WALLIS: If you did the experiment in a space shuttle, the drops would have zero diameter is that what you mean?

MR. KELLY: No, because then you would have to have different non-emitional groups because you have a different control element.

CHAIRMAN WALLIS: Okay. I understand.

MR. KELLY: And as I recall, you'd tend to get really large drops.

So anyway, that's the data that's there, but that is certainly not sufficient to develop to a correlation level. And the real reason it isn't is because the data base lacks the information on the flow conditions and we don't know what the vapor velocity is. We don't know the vapor density. So we can't come up --

CHAIRMAN WALLIS: So that's right, this is an example of why you need your RBHT?

MR. KELLY: Exactly.

CHAIRMAN WALLIS: So can we skip to the conclusion, do you think?

MR. KELLY: Sure. And I'll just go ahead and skip the convective enhancement.

CHAIRMAN WALLIS: And the rest, we can read the summary page.

MR. KELLY: Yes. The real point here is with RBHT we've tried to design the instrumentation to give us the information, and Professor Schrock earlier talked about the mechanisms. One of the things to look at will be high speed video, and I'm looking forward to seeing and looking at high speed video over and over again to try to get a better idea of physically what's happening.

For the convective enhancement by the drops, I mentioned earlier there'll be two types of tests run. The steam cooling test, with steady state heat transfer forced convection to steam.

CHAIRMAN WALLIS: And you haven't been skipping. I was asking you to go to the end.

MR. KELLY: Okay. But this is something

unique about the facility.

CHAIRMAN WALLIS: We can't spend a lot of time on all these different items.

MR. KELLY: Right. And there's the summary.

CHAIRMAN WALLIS: We'd probably need two days.

MR. KELLY: Okay. So the code development associated with the consolidation effort will be completed in the year 2002, probably by the end of January.

The developmental assessment will be conducted throughout calendar year 2002.

We're going to update the interfacial drag and the reflood models; those will appear in the consolidated code. The consolidated code will probably be released at the end of 2002.

And then long term code development and experimental programs will be driven either by code deficiencies that arise from the assessment program or by user needs for new capabilities.

CHAIRMAN WALLIS: So you're counting on a lot of input from this work -- your subcontract? Do we need to have presentations from these people during the year so we can see how they're doing?

MR. KELLY: I think that would be a good idea.

CHAIRMAN WALLIS: Should we probably do something like that at sometime in the middle of the year.

MR. BOEHNERT: Sure.

MR. KELLY: I would prefer coming --

CHAIRMAN WALLIS: Again, you don't want to us shoot down, let's say, phase separation models just before you're putting them in the code?

MR. KELLY: Right. And Steve -- Steve's talks is on the status of these program, but I think that's a good idea.

CHAIRMAN WALLIS: There's nothing like speaking to the people who are actually doing the work.

MR. KELLY: Right. Well, hopefully, we're going to be --

CHAIRMAN WALLIS: Maybe at the end of the day, but I don't that we'll have any time. We need to think about how the ACRS can be more central to you folks.

Although I think when I look at a schedule here, I wondered if it wouldn't be better off to -- well, I guess, Steve, you have two presentations.

MR. BAJOREK: I've got to two of them. The first one --

CHAIRMAN WALLIS: Maybe you should make the first one, and then we can have lunch and come back for a second one.

MR. BAJOREK: I think that will do well. Well, if you pass out the one handout, what I am going to do is I'll just bring out a couple of overheads?

MR. BOEHNERT: This one first, Steve?

MR. BAJOREK: That one first, please.

MR. BOEHNERT: Yes.

CHAIRMAN WALLIS: You're in the last lap here, and you've got to make up -- you've got to run at double speed.

MR. BAJOREK: I'm ready to go now.

But in the earlier presentations, one of the things you may have noticed that Jack noted that, in the long run, we're going to be counting on the code and more to make regulatory decisions. The accuracy will be much more important to us now then they had been in the past, because we're going to be relying on TRAC-M, the developers of TRAC-M to come up with these decisions as opposed to information that we had previously been asking from the vendors.

Joel also in his presentation pointed out in the developmental assessment that's going to be done over 2002, most of that is being directed at completing the consolidation showing that TRAC-M can meet the functional requirements of RELAP, TRAC-B and TRAC-P. The matrix that Joe put up using primarily the tests that had been used in the past to try to develop the code and assess its performance. It's not necessarily the best set of experiments to use to try to determine whether we're doing a good job or what we're weak in, or to really characterize the accuracy.

So what I'm going to talk about now is assessment and quantification of the performance of TRAC-M. But in many ways what this really represents in the elements, I think, another five year plan. The consolidation effort is going to go on through most of 2002. Through that effort we're not going to be able to do the total amount of assessment that we would like to have. So we're looking at work further downstream, 2003 and beyond. What I'd like to try to do is layout a better picture of where we think we're going to be able to go with TRAC-M, apart from the development of the potential model development that Joe just talked about.

We see three major elements. One, a continuing model improvement to get information from these test programs, having we things out, they are success oriented. We are assuming that the data that we're going to get from the rod bundle heat transfer programs are high quality, and likewise for the phase separation.

We're talking about those programs that are all the things that we're going to be doing to try to ensure that we are going to get the right information to develop those models.

But I think one of the more important aspects that we're going to have to address in the next one or two years is how do we assess the code accuracy?

We've seen models of the code right now that clearly don't perform as we'd like them to. We see TRAC as being "conservative," and RELAP being nonconservative in the reflood heat transfer.

CHAIRMAN WALLIS: Do you really mean uncertainty? Is that the same thing as accuracy in your mind?

MR. BAJOREK: Pretty close, yes.

CHAIRMAN WALLIS: So for the user, the user needs to come to us to identify some uncertainties in the use of the code, and accuracy may be a part of that.

MR. BAJOREK: The way I would break it down is we're going to be looking at various processes in the code. We're going to have to assess how accurate the code --

CHAIRMAN WALLIS: Well, all codes are probably perfect for one point, if you're on the right point.

MR. BAJOREK: When we get everything in there, the issue, of course, is that errors arise. And one of the things I'm going to point out in one of the next coming overheads is how we are going to try to overcome that.

CHAIRMAN WALLIS: So I say, the second bullet is related to the first. The user uses the code and there's some uncertainties associated with that. And that leads to margins and all kinds of stuff. If the uncertainties are reduced, that could be model improvement. For certain applications you don't need any model improvement. But for other applications you made a lot of model improvement. It's got to be somehow related to the uncertainties which are needed for the purpose of regulation.

MR. BAJOREK: That's why as we go through our model development we'll be relying on separate effects testing to get this biased uncertainty from models. But an element that we will build in early on is how to do those uncertainties propagate a behavior when you apply them to a PWR or a BWR.

CHAIRMAN WALLIS: So you're going to have some sort of mathematic or analytical framework for this? If you know the uncertainties in this current correlation of the model from separate effects tests, and then you can predict the uncertainties in the integral effects tests and so on?

MR. BAJOREK: Yes.

CHAIRMAN WALLIS: And then you can predict the uncertainties associated with some licensing calculations.

MR. BAJOREK: Yes. Many times I've seen in the past we've spent an awful lot of time developing a model for one process or phenomena only to find out that when you arranged it in a PWR calculation, it was only effecting your answer by a few degrees. I mean, that kind of tells you that your model development effort is being misdirected, where as other models --

CHAIRMAN WALLIS: This is risk-informed code development.

MR. BAJOREK: Risk-informed without development. But that final piece, seeing how the uncertainty needed in the light water reactor application is very valuable, because that allows ut to redirect our model development. It also allows us to refine or define new experiments that we need to do. And that's why what we'd like to try and do with the assessment element of this is to start looking at light water reactor applications early on, assess the uncertainties so that we can account for and correct those in the model development efforts.

MEMBER SCHROCK: I don't quite grasp the significance of the separate PIRT-based assessment.

MR. BAJOREK: The difference between what we are calling a PIRT-based assessment matrix and the code consolidation matrix is in the overall scope and how the simulations that were performed in the PIRT-based give a broader coverage of those processes that have been highly ranked in the PIRT.

The code consolidation matrix is largely historical. It picks certain FLECHT tests, some which are antiquated data, they didn't have the best test instrumentation in there by way you could assess some of the code models and correlations. What we would like to do is to get away from some of these tests that had been used on more of a historical basis, broaden that to make use of a broad range of FLECHT

SEASET, but not rely just on FLECHT SEASET, look at the Skewed test, some of the Cosine tests, the ACHILLES test and other reflood tests to avoid coming up with a code where it may work good for FLECHT SEASET, but not do well for other types of experiments

MEMBER SCHROCK: Well, it sounds kind of like you have to limit the amount of assessment you can do, and so here is a way of choosing more important things to perform the assessment on. But that increases the likelihood that there maybe something and it's never been understood these things, it isn't going to be properly addressed in this new code version, and it never will be.

MR. BAJOREK: We'd like to try to expand the matrix so it exercises the code over a much broader range. In some ways there's also some economy in doing that.

A lot of the work in developing these input decks for a certain test facility, in some cases it takes as much work as it does to set up a PWR or BWR deck. But when you're only going to be running one test out of the higher series of tests that can run in that matrix, you're losing a lot of information that you may gain by increasing the number of tests that you look at in that facility. CCTF or SCTF are an example. The consolidated matrix only looked at, I think, one or two tests. What we're proposing is to expand that to look at on the order of 10 or 12 tests so you examine how well the code can perform as you change things like your boundary conditions, your break size, your power distributions both axially and laterally within the core. See how the code uses -- the sensitivities you can get through the code rather than just looking at one point.

CHAIRMAN WALLIS: I would like you to move in the direction of risk based assessment. PIRT is just some expert sitting down and saying "Gee, you've got to do a better with more than condensation." I mean, there's no measure of better job until you come up with things that you're going to use it for. Use it for making regulatory decisions.

So PIRT I never felt was a really good measure of goodness of a code, even if it were used for that purpose.

MR. BAJOREK: Well, the PIRT's kind of done beforehand, and it really only gives you some guidance on what --

CHAIRMAN WALLIS: PIRT is a starting point.

MR. BAJOREK: Right.