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UNITED STATES NUCLEAR REGULATORY COMMISSION'S

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

July 7, 2008

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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

(ACRS)

MEETING OF THE THERMAL HYDRAULIC PHENOMENA

SUBCOMMITTEE

PEER REVIEW OF THE TRACE CODE

MONDAY

JULY 7, 2008

ROCKVILLE, MARYLAND

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The Subcommittee met at the Nuclear Regulatory Commission, Two White Flint North, Room T2B3, 11545 Rockville Pike, at 12:00 p.m., Sanjoy Banerjee, Chairman, presiding.

SUBCOMMITTEE MEMBERS:

SANJOY BANERJEE, Chairman

SAID ABDEL-KHALIK, Member

MICHAEL CORRADINI, Member

WILLIAM J. SHACK, Member

ACRS CONSULTANTS PRESENT:

THOMAS S. KRESS

GRAHAM B. WALLIS

DESIGNATED FEDERAL OFFICIAL:

DAVID E. BESSETTE

T-A-B-L-E O-F C-O-N-T-E-N-T-S

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

11:58 a.m.

CHAIRMAN BANERJEE: This meeting will now come to order. It's going to be a long meeting so we can't waste time right at the beginning.

PARTICIPANT: We'll waste it later.

CHAIRMAN BANERJEE: Yeah, we'll waste it later. This is a meeting of the Advisory Committee on Reactor Safeguards, Thermal Hydraulic Phenomena Subcommittee. I am Sanjoy Banerjee, Chairman of the Subcommittee.

Subcommittee members in attendance Said Abdel-Khalik, ACRS member. Mike Corradini who is delayed apparently by a thunderstorm somewhere in the midwest.

MEMBER SHACK: If you didn't get out of Chicago early this morning, you had problems.

CHAIRMAN BANERJEE: But who promises to be here before the end of the meeting. William Shack, who is also Chairman of the ACRS. We would like to also welcome former ACRS members and consultants Tom Kress and Graham Wallis, both of whom have been Chairman of the ACRS as well.

David Bessette is the designated federal official for this meeting. The purpose of today's meeting is to hear the results of the peer review of the TRACE code that was completed recently. We will also hear how the Office of Research intends to respond to comments they have received. We will hear presentations from the four peer reviewers and from the staff.

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

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. We have received no written comments or request for time to make oral statements from members of the public regarding today's meeting.

The transcript of the meeting is being kept and will be made available as stated in the Federal Register notice. We request that participants in this meeting use one of the available microphones when addressing the Subcommittee. The speakers should first identify themselves and speak with sufficient clarity and volume so that they can be readily heard.

This meeting will go on apparently until 7:00 this evening. The division of time here is what our DFO, which is Dave Bessette, suggested in consultation with the staff. However, if there are issues which the Subcommittee want to pursue in more detail, I'm going to give you the license to do that and we just cut the time down for the other parts which perhaps will be of less interest to the Subcommittee. As long as we finish by 7:00. As I'm jet lagged it has to be by 7:00.

CONSULTANT WALLIS: Sanjoy, I noticed that NRO user experience is limited to 10 minutes. That would seem to me a rather important area to investigate. If a code is not used, then that's telling you something. If it's used a lot and works, that's also telling you something so it would be nice to know.

CHAIRMAN BANERJEE: All right. As required we'll give more time. As far as the agenda is concerned, let's use it as guidance right now but feel free to suggest to the staff that you curtail some thoughts and expand on some thoughts as we go along. This is sort of impressionistic.

With that I would like to introduce Chris Hoxie who is the branch chief in research responsible, I guess, for development.

DR. HOXIE: Development.

CHAIRMAN BANERJEE: Thanks, Chris. It's all yours.

DR. HOXIE: Good afternoon. We have a full agenda today so I will keep my remarks and introductions brief. I am the branch chief of co-development in the Division of Systems Analysis under Dr. Farouk El-Tawila in the Office of Nuclear Regulatory Research. TRACE code development efforts at the NRC are focused in my branch.

We are here today to discuss the TRACE code, peer review, status, regulatory use and selected topics which the subcommittee has expressed an interest in such as the momentum equation. Let me briefly introduce my colleagues that are on the agenda today. Bill Krotiuk, Steve Bajorek, Mirela Gavrilas, and I believe Ralph Landry from NRO is in the background.

TRACE 5.0 is one of the most heavily assessed NRC sponsored computer codes ever. As we near the end of the formal peer review I want to personally thank each of the peer reviewers for their excellent well thought-out critique of TRACE 5.0 and its associated documentation. I look forward to a spirited discussion today as we gather further insights from both the peer reviewers and the

Subcommittee.

The peer reviewers have flown in from around the world to share their views with the Subcommittee so without further ado I would like to turn the presentation over to Bill Krotiuk who will deliver a summary overview of the peer review and then introduce the peer reviewers.

DR. KROTIUK: Everybody has a copy of the first one. I'm Bill Krotiuk. I'm with the research group and what I want to do is present a little overview of the methods and processes that we use for the peer review and how we gave instructions to the peer reviewers.

I'll start out by saying there were specific tasks that we identified for the peer review. Specifically we wanted to have them review the TRACE code and the documentation and produce reports that summarize code deficiencies or code strengths and recommendations.

A primary objective was to identify deficiencies in the code itself that will preclude its use for doing the thermal hydraulic analyses and ultimately present the findings to this group.

CONSULTANT WALLIS: Presumably they could also tell you about great successes of the code besides deficiencies. Maybe it is somehow much better than previous coding. We'd like to know that.

DR. KROTIUK: That's --

CONSULTANT WALLIS: Or is it just like the previous codes?

DR. KROTIUK: Or it would make an assessment of whether it's the same, better.

CONSULTANT WALLIS: My impression is it's just like the previous codes with a few tweakings of the pieces.

DR. KROTIUK: There were four peer reviewers, international experts who had been assigned this task and have contracts to do this; Dominic Bestion, Peter Griffith, Marv Thurgood, and George Yadiogarouglu. The contracts were awarded in August of last year. We had a meeting. The first thing that we had was a meeting to discuss the documentation and the other items that were given to the peer reviewers.

Specifically the documentation included the TRACE theory manual, assessment manual and appendices, user's guide, Volume 1 and Volume 2, and the TRACE Code Version 5.0 including the executable and the source. The intent of the peer review is not to run the code but that the code is executable and sample problems were given to the peer reviewers to use at their discretion if they wanted to look at it for any reason.

CHAIRMAN BANERJEE: Bill, let me ask you a question. When Dr. Elka Wheeler wrote this letter in January '07, the ACRS undertaking to do something, there was a mention of a theory manual supplement in that letter which was to go into great detail into how the equations were derived and their basis. I don't see such a manual and that was due in September '07 if you refer to his letter.

DR. KROTIUK: I'll have to ask.

CHAIRMAN BANERJEE: The theory manual as it stands was promised in his letter somewhat earlier but it was not supposed to contain great details on the equations because the equations were always subject to a lot of controversy. If you recall the RETRAN, RELAP S there's a long history where we've had problems with the equations so the theory manual supplement was supposed to lay this matter to rest. As far as I can see, no such manual supplement exist.

DR. KROTIUK: I would have to ask someone to answer that question.

DR. BAJOREK: This is Steve Bajorek from the Office of Research. Our original intent was to break the theory manual up into two separate volumes.

In the actual production some of the authors decided to put everything together. In retrospect it became easier to put the information that would have gone into the supplement directly into those chapters into the theory manual itself, the Volume 1 part.

One of the things we will talk about today and I think the peer reviewers will point this out, is that in doing it that way it has caused some confusion as to which models are actually in the code right now versus which models are in the theory manual because you want to talk about the history. It will be our intent to take the theory manual as it stands today and split it into two volumes.

One will be a concise and very factual description of the models and correlations which are used in the code and how they coordinate with other models and correlations and what had been called the supplement would become a Volume 2 where we put the history, the details, those items which are interesting and very important to know in applications of the code but allows you to expand on those things without bogging down what's there in Volume 1.

CHAIRMAN BANERJEE: Unless I missed it looking at the current theory manual it certainly does not contain the sort of depth of treatment that we had expected in the supplement so hopefully in the supplement there will be more detail than we have here.

Now, you expect a letter from us in September. Essentially that letter would address what Dr. Wheeler had undertaken to do in January '07 of which the peer review was one part.

DR. KROTIUK: Okay. I'll continue. The peer reviewers were given instructions to review general topics but also specific focus areas. The general review topics were assigned to everybody but considering the length of the manuals itself. It was decided that the focus areas be assigned to specific individuals to enable them to give more in-depth review of those sections.

The general review topics included a review of the capabilitiesand limitations of the code itself, numerical solution methods, fundamental equations, models and correlations, and also the general quality of the documentation. You will see that there are comments in all these areas.

With regards to the specific focus areas that were identified and assigned to individuals to give in-depth reviews included the conservation equations, applications of the conservation equations, the thermal hydraulic closure relations and physical models, numerical solution schemes, nuclear system components and models, and the assessment matrix and results.

The way it was decided to break up the responsibilities were for the conservation equations applications. Marv Thurgood and George Yadigaroglu were assigned to review in-depth those sections. The closure relations and physical models were assigned to Dominic Bestion and George Yadigaroglu. Numerical solution methods to Marv Thurgood. System components, features and models to Peter Griffith. And the assessment matrix and results to Dr. Bestion and Dr. Griffith.

CHAIRMAN BANERJEE: How much time did they have to do all this stuff?

DR. KROTIUK: The contracts were awarded in August of 2007 and basically they had a year to review the documents.

CHAIRMAN BANERJEE: But how many days of work did they do? They could have a year but worked only one day.

PARTICIPANT: Two hundred hours.

CHAIRMAN BANERJEE: Two hundred hours. Okay.

CONSULTANT WALLIS: So when we do our review can we take a year, too?

CHAIRMAN BANERJEE: Two hundreds hours each, not total.

DR. KROTIUK: Not total.

CONSULTANT WALLIS: Five weeks. So that's how long it takes. Each of them did part of it so if I try to do the whole of it, I need something like 20 weeks.

DR. KROTIUK: If you're going to divide it up in that fashion.

CONSULTANT WALLIS: Well, I think it's an indication of the magnitude of the task which is really quite immense.

DR. KROTIUK: The manuals are quite big.

CHAIRMAN BANERJEE: So these 200 hours included how many meetings?

DR. KROTIUK: We had a kick-off meeting in August of 2007 and at that meeting we had presentations by the members of the research staff to try to highlight areas and answer any obvious questions from the peer reviewers.

We asked at that point the peer reviewers to start reviewing their sections and come up with draft reports by January time frame. Then we had a working meeting in February to discuss the draft reports and findings with the staff and answer any other questions that may have arisen.

We got copies of final reports from the peer reviewers in the May time frame, May 2008. Now we're at the stage where we are presenting it before the Subcommittee. My plans are to try to coordinate this and have a final report in the August time frame.

CONSULTANT WALLIS: So the final report will be on the peer review and there will not be a final report on ACRS review, will it?

DR. KROTIUK: That's the report for the peer review, the peer review report. Correct. That will include basically the four review documents that you have already received but cleaned up with editorial review. I was going to basically make some introductory comments in the beginning to try to stand on the procedures and processes that we use for the review and maybe to summarize in general the overview of the findings.

CONSULTANT WALLIS: This would be a NUREG type document?

DR. KROTIUK: That has not been decided yet. I'm not sure I can answer that.

What I'll do now, you know, I did review and read all the reports received from the reviewers and I'm just going to highlight some overall impressions that I got from the review. I will leave it up to the peer reviewers themselves to make more detailed discussions of items. I just wanted to point out some overall impressions and findings that I did find.

General findings. This was quotes from the reviewers. TRACE 5.0, which is the code that was reviewed, is a good system code with extended capabilities to simulate PWRs and BWRs within the assessment range. The other quote was that getting a code as complex as TRACE to provide reasonable answers is an accomplishment.

CHAIRMAN BANERJEE: How does this improve on RELAP 5?

DR. KROTIUK: I think we will probably --

CHAIRMAN BANERJEE: What's your opinion on this?

DR. KROTIUK: My opinion? It's different. It's different than RELAP 5. I've used RELAP 5 in TRAC and TRACE. They are different. Some of the correlations are different and the models are different but the attempt was made to try to make improvements to the TRACE code to handle things better, to put in new correlations where appropriate and we will talk about that. We do have some --

CHAIRMAN BANERJEE: Will you address this issue because we have to address it?

DR. KROTIUK: There will be some discussions of that after I finish my introductory comments.

DR. GAVRILAS: This is Mirela Gavrilas and I'm the Branch Chief in Reactor Applications. I think one of the answers that we get when we ask that question is applicability for large break LOCAs and PWRs and BWRs. RELAP has never been assessed for those. TRACE has been and it is providing good agreement in both those transients.

CONSULTANT WALLIS: I don't know what you mean by reasonable. I guess what you mean is adequate answers for the RC to implement its job of evaluating nuclear safety. Is that what you mean?

DR. KROTIUK: Basically the intent was to be able to do independent assessment.

CONSULTANT WALLIS: Right. And if there are some assumptions in the code which might seem peculiar to the lay person with some basic knowledge, one could argue that it's all a wash because it works for what you need to use it for as long as you assess it and show that it works for suitable against suitable data.

DR. KROTIUK: The important thing is to make the assessment and to say the range at which the code is --

CONSULTANT WALLIS: Right. In that assessment it would not matter if one of the co-efficients, which is called friction, is actually really compensating for something else in the code as long as it works.

CHAIRMAN BANERJEE: I think he's leading you down a garden path.

DR. KROTIUK: Yes, I know. I was afraid to answer that part of the question.

CONSULTANT WALLIS: Essentially it's developed for an application so maybe one should assess it for its application. I'm just trying to think out loud here. Rather than assessing it as some tool which might stand up in a broader context within some thermal hydraulics technical community.

DR. KROTIUK: I think that the first statement here is appropriate when they say this is applicable within the assessment.

CONSULTANT WALLIS: In other words, if you used the NERO network and took all this data and fitted it, would it be the same thing? Just take a lot of assessment data and you find a multi-dimensional code fit.

CONSULTANT KRESS: You would have to have a lot of data for that. I think we don't have enough for NERO network.

DR. KROTIUK: Okay. Let's talk about specific overall findings regarding closure relations and the physical models. There was an overall finding that there were improvements that could be made to some of the physical models and that further review and analysis and assessments would be recommended. There was a recommendation that a validation matrix, which was missing from the documentation be included for the physical models and phenomena.

CONSULTANT KRESS: Would that be included actually with the code or with the manual?

DR. KROTIUK: No, this is in the manual. I'm sorry. In the manual.

CONSULTANT KRESS: It wouldn't be built into the code?

DR. KROTIUK: It would not be built into the code. There was a comment that there is a new interface tracking model in the code which was developed and is innovative and efficient but there is some lacking of user guidance since it is a new model.

Other specific findings were included for the conservation equation applications. The V delta V of the momentum equation was termed to be incorrect.

CONSULTANT WALLIS: Presumably that's what the code calls a V. delta V?

DR. KROTIUK: Yes. A very unhappy term.

CONSULTANT WALLIS: That's very nice. Maybe that's appropriate. Did you do that as a joke or did Bill Gates do that to you?

DR. KROTIUK: There will be more discussions on the momentum equation in the future in subsequent presentations. One of the other comments was to provide user guidance for the use of the nonconservative form of the momentum equation which is in the code itself. There was a comment that the water packing methodology was overly restrictive.

Regarding numerical solution methods the SETS method is used in TRACE code. It was commented that it was innovative and allows the delta-ts to exceed the material Courant limit.

CHAIRMAN BANERJEE: Did anybody check conservation of each phase?

DR. KROTIUK: I'm sorry, conservation of?

CHAIRMAN BANERJEE: Mass and energy for each phase?

DR. KROTIUK: Yes. Conservation of mass is --

CHAIRMAN BANERJEE: I don't really care about momentum. Professor Wallis will deal with that later. I'm just starting with basics.

DR. KROTIUK: Yes.

CHAIRMAN BANERJEE: Did somebody do a check to see massing?

DR. KROTIUK: I've done some checks on that myself.

CHAIRMAN BANERJEE: Usually these pressure velocity coping methods don't conserve individual field masses or energy. They do it overall or something but they don't --

DR. KROTIUK: Yeah, I did it --

CHAIRMAN BANERJEE: I'm just talking about each field. Did you check each field?

DR. KROTIUK: I personally have not checked.

CHAIRMAN BANERJEE: You have the same problem with OLGA in the Pipeline code. KAPIRA gets around this, of course, by using a completely different method guaranteed to conserve everything.

These methods don't generally. I'm just interested in knowing has anybody run an individual phase conservation? I ran it on OLGA once which they all do the same thing. If you have a droplet field it can be off by about 1,000 percent. The droplets vanish depending on sort of a random percent.

DR. MAHAFFEY: This is John Mahaffey, Penn State University. I have run mass conservation checks, energy conservation checks to the extent possible and I believe, yes, it conserves mass for both master and individual phases.

CHAIRMAN BANERJEE: Do we have some results of that available?

DR. MAHAFFEY: We would have to go back and generate it. That's all fairly old but you can, for example, turn off the phase change terms and follow your liquid, follow your gas and the mass is conserved within -- it comes down to the convergence level you set on your semi-implicit numerical method.

CHAIRMAN BANERJEE: Okay. We would be interested to see that.

DR. MAHAFFEY: If you would like to describe specifics of what you would like, I can tune into exactly what you want.

CHAIRMAN BANERJEE: Well, all I'm

interested in seeing is if you have multiple fields that you conserve mass and energy in each field.

DR. MAHAFFEY: I can give you some samples of that.

CHAIRMAN BANERJEE: Not of the mixture field.

CONSULTANT WALLIS: Do you have examples -- maybe we'll get into this later but you can always exceed the Courant limit but then, of course, you begin to get things you don't like like diffusion, numerical diffusion, unreasonable numerical diffusion. Are there examples showing that, for example, in the ESBWR riser chimney that the voids don't artificially diffuse when you use the search method as they do if you just exceed the Courant limit with some of the other methods?

DR. KROTIUK: I haven't looked specifically at that item but I have looked at other problems and I have run cases where I have used the SETS methods and the semi-implicit method.

CONSULTANT WALLIS: We have a very simple thing where you have to avoid perturbation propagating unchanged in the channel and you have to see whether it changes its form or not.

DR. KROTIUK: I've looked at a few of

those cases and I found that it was handling the situation.

CHAIRMAN BANERJEE: But mot of these low-ordered differencing methods if you go up in Courant number you are bound to get diffusion. There is no magic.

CONSULTANT WALLIS: But the question is how bad is that diffusion and what does it do?

DR. MAHAFFEY: This is John Mahaffey again. I was the guy that invented that years ago. I can comment directly on it. It is not designed to do problems where you have continuity waves that you want to track. SETS methods, fully implicit methods, those are items that you want to use when you have gradual evolution from, in effect, one quasi stationary state to another small break LOCA, for example.

We have user guidelines that say if you want to follow density waves in a BWR, you want to activate the semi-implicit option in the code because that is going to give you less numerical diffusion.

MEMBER SHACK: Okay. I guess that was the question that I had as I was reading it. I mean, you needed the numerical diffusion to make the problem well posed. How much numerical diffusion is good and how much is bad and how do you tell the difference.

DR. MAHAFFEY: John Mahaffey again. There is no simple answer to that. You need to go back. There's a classic paper.

MEMBER SHACK: You're just a classic user.

DR. MAHAFFEY: If you're a classic user, all you do is you get as little numerical diffusion as possible if you are following continuity ways. The amount that's necessary to provide stability is below anything that you're going to see. I would refer you back to some of the work by Bruce Stewart from the late 1970s to get some feel quantitatively for what is going on in terms of making sure that it behaves as a well-posed system.

CHAIRMAN BANERJEE: Carry on. We'll excuse your time.

DR. KROTIUK: I'm almost done. Regarding the test assessment matrix and the results there were comments that additional assessments, extensions of some test data assessments would be recommended. We are constantly increasing that assessment base. The other ones is that there were recommendations that the assessment be referenced to the PIRT tables and SET matrix which we'll have to adjust some documentation.

Finally, regarding the nuclear system components, features, and models. It was recommended that the user manual will be rewritten and a test is in the process of being started very shortly. We consider this the most important part of the manual because it is the one that the users will use to make models and they need good guidelines for both doing the modeling and for just general use of the code.

MEMBER ABDEL-KHALIK: Were there any direct answers to the two specific questions you posed in the very beginning insofar as identifying major deficiencies that preclude the use of TRACE for confirmatory thermal hydraulic calculations and identifying deficiencies that introduce significant errors in TRACE predictions? You posed two very specific questions. Has the peer panel returned with specific answers to these specific questions?

DR. KROTIUK: They have returned with specific comments regarding the models that are in the code and recommendations for improvements but I think generally as a whole I don't think there was what I could remember in reviewing the documents I would leave it up to the peer reviewers to elaborate on this. I don't believe there was any finding that precluded its use in any thermal hydraulic analyses that we would be able to perform.

MEMBER ABDEL-KHALIK: I guess we'll find out more.

CHAIRMAN BANERJEE: So the peer review did not function with the chairman and writer's joint report. They wrote reports on each thought was assigned to them in some way.

DR. KROTIUK: That's correct.

CHAIRMAN BANERJEE: I think in -- will they be able to answer Said's question? It seems to require an overall view. I mean your question.

DR. KROTIUK: I've been reviewing all the reviews and I'm going to try to address that when I write the report but my opinion of it is I don't feel there is anything there that would indicate very large deficiencies.

MEMBER ABDEL-KHALIK: I think it would be incumbent on the peer review panel, too, inasmuch as this is a direct charge. You asked them to answer these two questions so it may be a good idea for the panel to sort of combine their individual recommendations or findings and try to address these two specific questions.

CHAIRMAN BANERJEE: In other words, he's saying let the peer reviewer do the job of addressing the questions, not you trying to put their comments together.

MEMBER ABDEL-KHALIK: That is correct.

DR. KROTIUK: That was my original intent and I guess we could talk about that a little bit more with the peer reviewers but that's not the way -- when the process started that's not the way it --

CHAIRMAN BANERJEE: Okay. At least we know where we stand right now. We may make some comments later on.

CONSULTANT WALLIS: So the review panel doesn't have an overview of how useful it is. Maybe they have looked at all the details and said this correlation should be modified or this correlation good and so on but they haven't looked at -- maybe they have. I get the impression they haven't looked at, say, outputs for large-break LOCA and uncertainty studies which show that the result is insensitive or not to some assumption somewhere, that stage of review.

DR. KROTIUK: Well, they had looked at the assessments that were performed. The assessment reports and the appendices are quite extensive.

CONSULTANT WALLIS: They haven't looked at the suitability for the use for assessing something like large-break LOCA?

DR. KROTIUK: I think that was part of the case.

CONSULTANT WALLIS: It was part of the job.

DR. KROTIUK: Yeah.

CONSULTANT WALLIS: So part of that conclusion should be this code is sort of proper for large-break LOCA?

DR. KROTIUK: As I said in the very first on one of the early slides is that it was applicable within its range of assessment and there are three appendices of detailed assessments, cases that have been run.

CHAIRMAN BANERJEE: Now, also Farouk's letter promised that you would have an ESBWR applicability document.

DR. KROTIUK: Yes.

CHAIRMAN BANERJEE: Did they have any of that available to them to look at?

DR. KROTIUK: Applicability document was published in March of 2008 so they did not have access to it because it was too late in the process for them to have access to that document.

CHAIRMAN BANERJEE: I haven't seen the

document but did it include or address the issues of instabilities?

DR. STOUDEMEIER: This is Jim Stoudemeier, NRC staff. The applicability document was specifically for ESBWR LOCA.

CHAIRMAN BANERJEE: Only for LOCA?

DR. STOUDEMEIER: Yes.

CHAIRMAN BANERJEE: Now, TRACE has been coupled to PARTS. Right?

DR. STOUDEMEIER: Correct. The group wants me to add it's because NRR was using a different code for ESBWR stability. They are not using TRACE.

CONSULTANT WALLIS: Was TRACE applied to questions of stratification and noncondensable mixing and containment?

DR. KROTIUK: That is something that I think is being addressed now.

CONSULTANT WALLIS: Being addressed.

DR. KROTIUK: That is not something that --

CONSULTANT WALLIS: We can't just say we'll use TRACE to do it today.

CHAIRMAN BANERJEE: Okay. That was helpful. Let's move on. You orchestrate things as you think appropriate.

DR. KROTIUK: The next presentation is by Marv Thurgood and he will talk specifically about his review regarding the conservation equations and numerical solutions.

CHAIRMAN BANERJEE: Do you have slides?

MEMBER SHACK: They don't want us to lose them so they don't give them out ahead of time.

CHAIRMAN BANERJEE: Marv, you probably have to sit and speak because of that mic, unless we can give you a portable one.

DR. THURGOOD: That's fine. I can sit. That's easier on my feet anyway. I'm going to talk today about my review of the application of the conservation equations and the numerical solution methods in TRACE.

CONSULTANT KRESS: Did you spend 200 hours?

DR. THURGOOD: I believe mine was actually a little less than that. Basically we spent five days in meetings so we had four weeks left to do the review and whatever.

CONSULTANT KRESS: Did you think you had enough time to do a good review?

DR. THURGOOD: There's not adequate time to review this code in detail. Not by quite a bit.

First some general comments. The developers in their manual have indicated -- I don't know if they indicated in their manual but in their presentations to us they indicated an intent at some future time to add a droplet field. I fully support that idea. The current model is inadequate to address the flow phenomena during stratified/dispersed film/dispersed flow and re-flood without --

CONSULTANT WALLIS: How complicated does this make the solution strategy? Does it take a lot more time to run and things like that?

DR. THURGOOD: Each time step takes more time to run. However, my experience and the experience of others who have used the droplet field I think they have found that in general the code will be able to use larger time steps and arrive at a convergence --

CONSULTANT WALLIS: Now, if you put droplets in there, presumably when droplets go around the bend they hit the wall and make a film. Is that all sort of modeled in this droplet field?

DR. THURGOOD: TRACE does not have a droplet field.

CONSULTANT WALLIS: This is the kind of thing which TRACE doesn't model very well, a droplet flow going around a bend and getting centrifuged and turning into a film on the outside wall which has secondary flows and things. All that is sort of swept over in TRACE.

DR. THURGOOD: It's all swept over.

CONSULTANT WALLIS: Is that the kind of thing you put into this droplet model?

DR. THURGOOD: Yes. And you try to get dan Trayman also.

CONSULTANT WALLIS: This might mean there would be some need for some more experiments.

DR. THURGOOD: In the case of existing experiments there was some very good data.

CHAIRMAN BANERJEE: At the moment TRACE does not have a droplet field?

DR. THURGOOD: It does not have a droplet field. Currently TRACE attempts to handle the mixture of droplets and film or stratified flow by solving a mixture equation where they use a correlation to obtain velocities for the film and for the droplet. There is no way that this can give you correct transport times for each phase because there is only a single liquid velocity.

Also, if droplets and film are going in opposite directions as often is the case in counter-current film flow, it can only have a solution either up or down. The solution it has to get to get that more or less right is an oscillatory.

CONSULTANT WALLIS: That's very interesting. When TRACE senses a stagnation in the liquid flow, it may well be it's a situation where the film is going down and the droplets are going up and there is no sedation at all.

DR. THURGOOD: Correct. Generally codes without a droplet field end up with an oscillatory solution.

MEMBER ABDEL-KHALIK: Would you consider that deficiency to rise to the level that would provide a yes answer to either of the two questions that were posed earlier in terms of identifying major deficiencies that preclude the use of TRACE?

DR. THURGOOD: I don't because for the problems they are looking at they have been able to make adjustments to their models to get good comparisons with the experimental data. That does not mean if you looked at the details that the flow would always be correct but the overall result is usually --

CONSULTANT WALLIS: They have dozens or scores of fudge factors we call them, or correlations which can be adjusted to fit data even if the physics is somewhat different from reality.

DR. THURGOOD: That's correct. We need to realize, too, there are all these limitations in how much detail you can model some of these. I'm simply saying I believe this would be a move in the right direction and I strongly support it.

CHAIRMAN BANERJEE: I like your second point. Are you going to talk about that?

DR. THURGOOD: Yes, I am.

CHAIRMAN BANERJEE: Let's move on.

DR. THURGOOD: I think this raises a question is the droplet field adequate or do we need four fields, continuous liquid, continuous gas, dispersed gas, and dispersed liquid. There is some indication that, yes, in fact this is needed. Some may disagree with me. Essentially that is what their stratification model does. Basically it's capable of handling above the liquid flow below the interface and the droplet film flow.

CHAIRMAN BANERJEE: This is exactly what is being done now in the oil gas industry. They are getting rid of flurogens and with enough finalization they can capture slugs. They have a de-entrainment and entrainment correlation so they get rid of everything. They just have one set of correlations for everything.

DR. THURGOOD: That's what I found with the droplet.

CHAIRMAN BANERJEE: But you have the four fields.

DR. THURGOOD: You do when it transitions from droplet to film.

CHAIRMAN BANERJEE: You capture a lot of the 1-D transitions.

DR. THURGOOD: Yes, you do.

CONSULTANT WALLIS: You get columns in the reactor transients or accidents where, you say, a bubbly mixture is coming out of some place and going into a horizontal pipe. With the velocities you can do some hand calculations and say just in a few feet it should undergo transition and just try to find flow but I don't know that TRACE can do that.

CHAIRMAN BANERJEE: A static flurogen.

DR. THURGOOD: You probably can't do that as well just with a single one-directional --

CHAIRMAN BANERJEE: You can do it with a full-field model. You can stop with the disperse flow and it becomes stratified flow.

DR. THURGOOD: I know that you can do that but then enter in turbulence and secondary flows.

Anyway, my recommendation is that the NRC should maybe carefully evaluate the final model that is needed and up front do that rather than stepwise making changes because each time you add another droplet field, or another field, you can completely change the constituency of relations that are required. For example, you change what is required for a flow regime.

Also, I wonder if consideration should be given to solving the conservative from of the momentum equations rather than non-conservative form. For me it's not always clear that momentum is conserved using a non-conservative form. I only make this statement because of statements the developers themselves made about some concerns they had with the non-conservative form.

CONSULTANT WALLIS: Now, this non-conservative form is simply an effective equation of motion.

DR. THURGOOD: It's an equation of motion. It's the momentum equation where you substitute it.

CONSULTANT WALLIS: Until I read the text I had no idea what you meant by that term.

Okay. It is stated in the documentation that the code uncertainty for transients in both current and advanced PWRs and BWRs has not been conducted. That answers, I think, in part your question. We couldn't really evaluate whether it is applicable because no applicability and uncertainty analysis have been performed at the time they did our review.

I don't know if that is completed to date or not. The question is is the code usable by the NRR without this? I find the documentation generally well written and complete with regard to equations, references and nomenclature.

CONSULTANT WALLIS: The nomenclature to me was a problem because it didn't give any units. When it simply says gamma is the rate of vaporization, is it per unit volume, per unit surface area, or what? There were a lot of places like that where it didn't really explain what it meant. Anyway, let's just overlook that.

CHAIRMAN BANERJEE: To me looking at that equation it looked like a very superficial treatment.

DR. THURGOOD: I was going to say with the exception that when you really want to look at details of how a momentum turn, for example, works at a certain condition such as at a T you would find most of that generally and it was difficult to tell what actually is done in the code. You could only see it by going into the code. There is a description of the code's mission, its purpose, objectives and capabilities. Its range of applicability is also discussed.

Based on my review of the documentation, I conclude that there is an adequate description of the code limitations. I believe there needs to be a few more added as I will discuss later. The conservation equations are described, I feel, in complete detail with the exception of specific applications with Ts and 3-D connections.

CONSULTANT WALLIS: I was very interested in your review because you said the models and correlations and numerical methods are well described but you didn't say anything about whether they were valid or not.

DR. THURGOOD: I think I get to that on another --

CONSULTANT WALLIS: You're going to get to that?

DR. THURGOOD: I believe they are valid.

CONSULTANT WALLIS: Did you follow the strange derivation from tensors to rental stresses to pressures that somehow evolve without any explanation whatsoever?

DR. THURGOOD: I understand them because in my code I have all those.

CONSULTANT WALLIS: You do the same thing?

DR. THURGOOD: I don't do the same thing. In my code I do have all the stress tensors. Basically they are simply throwing those out saying they can be replaced by loss coefficient.

CONSULTANT WALLIS: It would have helped me if there had been more explanation about how you go from one to the other and what is left out when you do that or what assumption is made and so on.

DR. THURGOOD: Right. I agree. As far as --

CONSULTANT WALLIS: Let's go back to this. Your job was to look at the consolation but I guess we're going to get to this later. Really the code is based on a nodalization description in terms of control volumes. One might expect sort of an emphasis on how do you write a conversation equation for control volume.

Here we have about 80 equations in vector form which would be very good if you are going to do 3-D CFD but what's that got to do with the control volume? It's a very surprising thing to see at the beginning of a code which is based on control volumes.

DR. THURGOOD: We think it would be best to just start with one.

CONSULTANT WALLIS: I would have done that I think.

DR. THURGOOD: I agree. I think that is probably the best way to go. I understand why it's the way it is because there was a lot of pressure and emphasis given early on by mathematicians who tried to show how the equations in TRACE and TRAC --

CONSULTANT WALLIS: You could say it's a kind of ritual almost of religious significance where you produce all this stuff. It makes everyone feel happy to know this is going well. It's sort of playing homage to the right kind of sources. Then somehow it comes down to something you could almost write down in the first line.

DR. THURGOOD: In my view the equations that are solved are volume-concentration equations. They are kind of where you start when you let things, volumes and DXs and DTs go to zero to form the first of differential equations.

CHAIRMAN BANERJEE: It's also the equation for straight pipes?

DR. THURGOOD: Well --

CHAIRMAN BANERJEE: There is some argument about the line of a pipe or something but I couldn't follow it.

DR. THURGOOD: They are basically 1-B equations.

CONSULTANT WALLIS: But you have to think in terms of the vectors really being not vectors but being aligned. I found that a rather peculiar statement.

DR. THURGOOD: More streamlined.

CONSULTANT WALLIS: If you had written down the momentum equation for a bend you would find that the force from the wall, the normal force from the wall comes end to end but it doesn't come into theirs. We'll get into this at 6:00 or something presumably.

CHAIRMAN BANERJEE: Can you stop to have a pump where you have a bend?

CONSULTANT WALLIS: I haven't yet checked whether TRACE makes a bend into a pump but there are the codes to make a bend into a pump and that is a little disconcerting.

CHAIRMAN BANERJEE: It depends on the force.

CONSULTANT WALLIS: Then you could have a continuous perpetual motion machine. Just keep putting enough bends together and you can pump as much as you'd like.

DR. THURGOOD: I have reviewed the following sections, the field equations; the solution methods; heat conduction equations; Appendix A, which is the quasi steady assumption and averaging operators; Appendix B, finite volume equations.

I have also reviewed the entire section on level tracking, numerical experiments, the off-take model and Form Loss models. I have also reviewed some of the fluid properties, those of the gas mixture especially.

CONSULTANT WALLIS: Well, to get back to my other question, I think it is important to state in these field equations something which is believable to the technical community or to a sophisticated graduate student who looks at them and says, well, okay or not okay. It is important to get something which is believable in the beginning.

I was a bit surprised that none of these reviewers did that. You sort of said either it's the standard thing or it is well described or it looks okay and let's move onto the details. Maybe we could if the reviewers whose job it was to review Section 1 went back and did a more thorough job.

DR. THURGOOD: In terms of deriving --

CONSULTANT WALLIS: Say if I had a graduate student looking at this and he says, "Well, how can they possibly go from equation 115 to 119?" But there is something there and we say, "Here is a reviewer who explains why it's okay."

CHAIRMAN BANERJEE: Graham, do you have a set of routine questions because this issue has come up when RETRAN had it's untimely demise.

CONSULTANT WALLIS: Well, we shut down RETRAN because it tried to do something which this code doesn't try to do which is to take the vector equation of momentum and really develop it for things like bends and Ts and so on.

The problem was when they did and you look at the examples you found that a bend was a pump and you found that a T produced absurd momentum transfers because someone had put in boxes and said, "Let's make these balances." That's not how bends work and how Ts work so these are important things because you can shoot down the whole thing by saying, "Look at this very simple example and it doesn't get the right answer."

CHAIRMAN BANERJEE: We still have all the documentation on that. Is it available to the TRACE group?

CONSULTANT WALLIS: I'm not sure they want to --

CHAIRMAN BANERJEE: Did that actually happen?

CONSULTANT WALLIS: This was the demise of RETRAN although they tried to do something.

CHAIRMAN BANERJEE: It's the same sort of equation.

CONSULTANT WALLIS: It was really better than TRACE. There is a fundamental problem of taking three-dimensional momentum and translating it into a one dimensional model which has to be faced in an honest way and a believable way. We'll get into that later.

DR. THURGOOD: We're into right now.

CONSULTANT WALLIS: I know but I thought it was probably your job. That's why I'm asking you.

DR. THURGOOD: That's fine. It is part of my job. I'm here and I'll attempt to start addressing that.

CONSULTANT WALLIS: You made corrections I noticed to make it work right for certain geometries.

DR. THURGOOD: That's the problem, yes. First up front we have to recognize that we are trying to model just one-dimensional pipes and to really solve for the pressure gradience and momentum we've lost the gradience in various components.

In the piping system you would really have to solve a multi-dimensional flow equation. It's virtually impossible with just very simple equations used in TRACE to arrive at the correct answer. Generally what they do is try to avoid momentum sources.

How do you difference the terms such as you do not get momentum sources and then get the correct overall pressure top by specifying loss coefficients. Currently, I believe, it does not allow negative loss coefficients so any pressure rise that you might get to an area of expansion you have to rely on the equations to give you that pressure rise.

What I did without really saying anything is I gave a couple of sample problems to the co-developers, one where we had flow in from a branch at the top there and going out the run of the T or a similar problem where you had a branch or pipe connecting into the vessel. I asked them to run this in the forward and reverse direction.

CONSULTANT WALLIS: Did you actually have flow out of the branch, too?

DR. THURGOOD: Zero flow on the inlet of the --

CONSULTANT WALLIS: That one is a bit easier. I think Peter Griffith pointed it out that when you have flow out of the branch, when you've got a flow split, it's much more difficult to get the right answer.

DR. THURGOOD: For this test problem, however, I just had a zero flow here. The area of the branch is the same as --

CHAIRMAN BANERJEE: Marv, you have to speak into the mic.

CONSULTANT WALLIS: So it's behaving like a bend.

DR. THURGOOD: It should behave like a bend, yes. We have a branch and the branch had the same flow area as the run of the pipe. It has the same velocity in and the same velocity out and zero velocity coming in to the round of the branch. What we found when we ran that -- what they found when they ran that is they got a net delta-p going through the branch.

CONSULTANT WALLIS: Was it positive or

negative?

DR. THURGOOD: It was a positive rise.

CONSULTANT WALLIS: So it's a pump. Okay.

CHAIRMAN BANERJEE: That's to be expected of course.

DR. THURGOOD: No, I think --

CONSULTANT WALLIS: It's patented.

DR. THURGOOD: Yeah. I think that can be avoided by the way you set the velocities and the VgradV term. They indicated they felt there were errors in the way the T momentum was handled, branch momentum, the 1-D/3-D connection and flows at the bottom and top of your vessel.

CONSULTANT KRESS: If you did that would it dilate the conservation of momentum?

DR. THURGOOD: If you do it correctly, you can get a zero change in pressure which is what you should get because --

CONSULTANT KRESS: If it comes at 90 degrees you get --

DR. THURGOOD: This comes in at 90 degrees and go around the bend and has no area change so the reversible losses you would expect to be zero but they were non-zero. Okay. So the last end of that VgradV term, or the area that is nearest the solid surface, should be set to zero for connections that are at 90 degrees to the solid surface.

CONSULTANT WALLIS: What do you mean by VgradV when you've got a bend like this? I don't know what VgradV means.

CHAIRMAN BANERJEE: It's the momentum flux, I guess.

DR. THURGOOD: It's just the momentum flux term. Let me get the equations on that.

CONSULTANT WALLIS: It's what comes in through the wall so it's a control volume you're using. When you start talking about gradV, I don't know what gradV means in a place where there really is no gradV. You've got a bend.

DR. THURGOOD: A V of zero.

CONSULTANT WALLIS: Well, it isn't because of the velocity change.

DR. THURGOOD: There's the source of the wall. There is a pressurize at the wall.

CHAIRMAN BANERJEE: The V there is a vector.

CONSULTANT WALLIS: Which way is it? Is the V at 45 degrees some average velocity or what velocity are you talking about? I don't know how to define gradV in a bend like this. What you mean is the surface fluxes.

DR. THURGOOD: The surface fluxes and that's it. That's the way.

CONSULTANT WALLIS: That is part of what bothered me about starting off with this VgradV at the beginning because really what they plan to do is integrate the whole volume and get the surface flux which they could have started with at the beginning because that's really what's happening.

Using a sort of Gauss to integrate up is sort of the reverse of what one usually does which is to start big and then make it small and say that's the divergence. These things are not trivial, these basic questions.

DR. THURGOOD: No, they are not.

DR. MAHAFFEY: This is John Mahaffey. I would like to correct one comment before it wanders too far into the record. TRACE when you make a right-angle turn, as you indicated there, does not act like a pump. There is no injection and momentum. What it does is it produces more irrecoverable loss than you would like.

CONSULTANT WALLIS: It does not act like a pump?

DR. MAHAFFEY: It does not act like a

pump.

DR. THURGOOD: Bottom momentum source. It gives a pressure rise.

CONSULTANT WALLIS: It gives a pressure rise? So it gives a pressure rise with no velocity rise? So it's a pump.

DR. THURGOOD: Isn't that correct?

DR. MAHAFFEY: No.

CONSULTANT WALLIS: So it does behave like a pump.

DR. THURGOOD: Let me go back.

CHAIRMAN BANERJEE: Perhaps this needs to be cleared up. I don't know if you have the time to do it.

DR. THURGOOD: I believe I saw in here.

CHAIRMAN BANERJEE: Again, can you have the mic follow him?

DR. THURGOOD: Old habit. Sorry.

CHAIRMAN BANERJEE: Give him a mic.

DR. THURGOOD: For this momentum equation here for the branch the gradV term is zero. Vj equals Vj minus 1. You saw the separate momentum equation here and here the grad term is zero here and B here.

CONSULTANT WALLIS: I'm saying that gradV is all in the primary direction which is not really what it is. What is the primary direction in a bend anyway? Is it the way it comes in or the way it goes out or somewhere halfway in between? We could go on with this forever.

DR. THURGOOD: You see the problem.

CONSULTANT WALLIS: This is what I tend to do is what you have done is to say, "Okay, here's all the stuff. Let's apply it to something I think I understand to see if it works."

DR. THURGOOD: That's correct. I basically gave it to them and I didn't attempt to say how they should do it, just recognize that there is something not quite right at the Ts and 1-D/3-D junctions.

CONSULTANT WALLIS: This is part of TRACE that I think works fine when all the losses are form losses or can be ascribed to sort of loss factors and loss coefficient which is probably true of most of the circuit but there are some odd situations like pressurizers and some transients where you don't want to have the flow pumping into the pressurizer unrealistically because of the way momentum is modeled with the T.

There are some situations where you worry about that. Fortunately I think there are not very many so even though this is a problem it may not have much significance for reactor safety except sometimes. We need to know when it does.

CHAIRMAN BANERJEE: Let's move on to the next slide.

DR. THURGOOD: This slide or the next one?

CHAIRMAN BANERJEE: No, the slide that you had up. You were going to show us something about the conservation with the equations. That was your slide number -- your slides are not numbered. The one just be -- yeah, that one. What were you going to tell us about that slide?

DR. THURGOOD: The response they gave me on this were the changes in the wording in the document in which they said either the velocity or the area is set to zero for the gradV term in the momentum equation.

CONSULTANT WALLIS: What you're saying is the Vgrad would be zero but, in fact, the way that they note it, it comes out to be vj squared over xx? Is that your bottom line there?

DR. THURGOOD: That's what it gets to by the time you get completely around there.

CONSULTANT WALLIS: It produces a nonphysical result.

DR. THURGOOD: It's nonphysical in that you get pressure rise.

CONSULTANT WALLIS: I send a homework following to my students saying use TRACE for a bend and then tell me whether you believe the answer.

DR. THURGOOD: For a bend formed with a T.

CHAIRMAN BANERJEE: I would like to go back to John Mahaffey's comment. Do you agree with this or do you have some disagreement that there is something wrong here?

DR. MAHAFFEY: What's wrong is that it will produce more irrecoverable pressure loss, particularly in the version that they were looking at than you might prefer to have in a calculation. I'll go back and you can set up all kinds of situations. I've looked at this over the years. It does not pump flow artificially.

CHAIRMAN BANERJEE: But if there is a pressure rise here, why is it that you recover the pressure loss?

DR. MAHAFFEY: We would have to look at the exact details of the problem that he was discussing.

MEMBER ABDEL-KHALIK: Were the findings of this review panel made available to you prior to this presentation?

DR. MAHAFFEY: Yeah, and I don't recall seeing anything that told me there was a pump in there.

DR. THURGOOD: I restate what I said there. He asked me if it was a pump and I stated that it resulted in a net pressure increase across the branch junction. It is not a momentum source where we are adding momentum to the system.

CONSULTANT WALLIS: You're adding pressure which is like adding momentum in the momentum equation.

CHAIRMAN BANERJEE: In the RLAP version, Bernoulli's.

DR. THURGOOD: Yeah. I think what John and I are talking about in terms of the source of momentum is that it doesn't keep adding back in and keep accelerating the flow over and over.

CONSULTANT WALLIS: There is also a problem in the nomenclature section that says P is the total pressure. Well, total pressure is often used to mean static plus dynamic. I think by P they mean the static pressure.

DR. THURGOOD: That's what I think.

CONSULTANT WALLIS: Okay. We're clear on

that point.

DR. THURGOOD: If that's in the nomenclature, I missed seeing that. The pressure used in the equations is the static pressure.

CONSULTANT WALLIS: I think what they mean is it's the same pressure in both phases. Therefore, it's the total pressure but that's not what total pressure sometimes means.

DR. THURGOOD: Okay. The VgradV term is essentially this. The problem is you don't know velocities when you are solving the momentum equation which is solved at a cell phase you don't know the velocities at the cell centers so you have to do some kind of averaging.

If Vj plus 1 in this equation were zero, then the VgradV term would give you one half V squared, or maybe one half V squared. If Vj were zero, then it would give you a positive one half V squared.

MEMBER ABDEL-KHALIK: I wish you would always add the dot between the V and the grad V so we would all know it's a vector rather than a third order tenant.

CONSULTANT WALLIS: Maybe it's a fourth order.

CHAIRMAN BANERJEE: Anything is possible.

DR. GRIFFITH: One question I had and this is a new slide that I put in this presentation that I haven't been able to talk to John about is while this is the definition of this term, ADA substitution of constant volumetric flow, if you substitute these equations into this equation to obtain this equation.

Rather than now being a function of Vj plus 1 and Vj, it comes out to be just a function of Vj plus the half. It appears to me that if all these areas are constant, then you get a one half Vj plus one half times the gradient term.

However, if Aj plus a half, Aj plus 1 -- Aj plus 1 and Aj plus half equal Aj minus half then Vj minus half would be zero in this case. The gradient term would be V squared j plus a half over delta x. Whereas, if you made that same substitution in the previous equation, in this equation you would get one and a half V squared.

CONSULTANT WALLIS: In reality there is a separation bubble at the corner.

DR. THURGOOD: Pardon?

CONSULTANT WALLIS: In reality there's a separation bubble in this corner and recirculation?

DR. THURGOOD: You've got separation and, as they state, they handle that by --

CONSULTANT WALLIS: You can go on forever about how badly we've modeled this bend and we should probably move on.

DR. THURGOOD: They state simply -- the bottom line is this. They simply state that they can't treat all of the pressure losses at Ts and 1D and 3D connections from first principles because it is just a 1D momentum equation. Therefore, they use loss coefficients to give them the correct pressure loss.

CONSULTANT WALLIS: I guess you --

DR. THURGOOD: The concern is sometimes given the way they formulated the equations they can get an excessively large reversible loss and they can be written differently such that you get no reverse data loss or more of a Bernoulli reversible loss rather than a rho-V squared loss.

Water packing. Water packing occurs in several problems. Look at some of their problems as well as some of my own experience with the code I find that water packing sometimes catches pressure spikes and sometimes it doesn't. There are several exceptions when you see a large pressure change that are taken in the code and I think sometimes those get water packing fix when it should not. One thing they have done is they have added the level tracking model and with that essentially eliminated water packing

CONSULTANT WALLIS: Level tracking means a transition from all water to all steam?

DR. THURGOOD: Or tracking on interface and not necessarily --

CONSULTANT WALLIS: Do you have a bubbly flow going to a drop flow or something like that. Can it attract that kind of an interface?

CHAIRMAN BANERJEE: They don't have four Qs.

CONSULTANT WALLIS: You have bubbles coming up and then there is an interface and you get a spray above that.

DR. THURGOOD: They do have a level tracking model which treats it in that sort of way. Below the interface they say the flow is bubbly or two phase. Above the interface it's mostly gas --

CHAIRMAN BANERJEE: So that must be arbitrary, void fraction of .5 or something.

DR. THURGOOD: What they do is they track the level as it goes through a cell.

CHAIRMAN BANERJEE: When is the level identified?

DR. THURGOOD: They make assumptions about what the void fraction is below the interface based on what it is in the cell below that. They are saying that the part that is below the interface has simply that same void fraction or they have an algorithm for determining the void fraction of that. Then they look at the cell above to see how much the liquid flows in the cell above to determine the void fraction.

CONSULTANT WALLIS: Do the bubbles have to burst? I mean, if you had soap in there you get a foam and then the bubbles would keep going forever without bursting.

DR. THURGOOD: Really all it's doing is looking at a two-phase level that is moving up through a mesh and they don't --

CONSULTANT WALLIS: A jump in the two-phase void fraction.

DR. THURGOOD: They do allow bubbles to cross the interface and join the others.

CONSULTANT WALLIS: It's like the head on a glass of beer?

DR. THURGOOD: Yeah.

CONSULTANT WALLIS: And they can track that.

DR. THURGOOD: They tried. I think it's kind of an innovative model and I think it works very well in specific cases. What is not clear to me is if it can be generalized to work --

CONSULTANT WALLIS: We have exact solutions to bubbly flow through interfaces and forming a foam and all that. Check it there and see whether it works.

DR. THURGOOD: They have started with some simpler type problems just looking in the channel filling with water and an oscillating manometer. It works very well both in terms of eliminating water packing as well as eliminating numerical diffusion. Again, there could be lots more sophistication put into that but I think it's a step in the right direction.

CONSULTANT WALLIS: One of the problems sometimes is whether the code knows when to create a level. It's like the shockwave and converse and diverse. Once you know the shockwave is there you can analyze the problem. If you are doing a supersonic code, it has to somehow build up the shockwave from interactions of other waves and then know there is a wave there, know there is a level there before it goes further. I wonder if the code always does that or if you have to somehow put the level in there.

CHAIRMAN BANERJEE: I haven't followed

this in detail but is it a 1-D version of either a BOF or level-set method? Is that what they are doing or is it something different?

DR. THURGOOD: I'll have to ask John to answer that one.

DR. MAHAFFEY: This is John Mahaffey. BOF is probably the simplest analog.

CHAIRMAN BANERJEE: 1-D analog?

DR. MAHAFFEY: Yeah, that's a way of looking at it. You are just trying to resolve on a subgrid level distribution of void. We don't claim perfection. To partially answer Graham's question, it does a very good job of finding a level that is there based on void fractions and, to a much lesser extent, velocities.

If you want to have a concern, it's like any kind of numerical subgrid model where you are trying to look for discontinuities. Once it latches onto a discontinuity it tends not to want to let go. You talked about froths and we have seen examples where it may not be as good as it should be.

Going from a situation where you've got a clear interface between bubbly flow and droplet flow to something where you get this real froth that builds up where the void fraction is relatively high but it's the head on your beer. It's gone up and filled all the way to the top of whatever region you're in. That is something that needs to be researched further.

CONSULTANT WALLIS: In other applications like gastronomics you've got situations where the shock or the input to the jump sometimes diffuses away and it splits up into little waves and sometimes strengthens and you have to model that process.

I wonder if you need to do that but you probably don't do that now. You are dealing with a situation where there is a jump in something like void fraction within a control volume so averaging doesn't do a good job. Then there is the more sophisticated question does that jump strengthen with time or does it diffuse away and spread out --

CHAIRMAN BANERJEE: I think --

CONSULTANT WALLIS: -- spread out artificially.

CHAIRMAN BANERJEE: If it's like rock it's only the kinematics, just a --

CONSULTANT WALLIS: Once you've got it there it's hard to make it go away. Okay.

DR. THURGOOD: The manual states that exaggerated momentum transfer can occur in TRACE 5.0 when a steam/water droplet mixture flows down towards the surface of a liquid pool due to the use of the non-conservative momentum equations rather than using the fully conservative momentum equations. It is recommended that the solution to this problem is to engage the TRACE interface tracking model when practical.

CONSULTANT WALLIS: Those are good comments. It means that the user has to be sophisticated enough to understand this.

DR. THURGOOD: Exactly. The interface tracking model is activated only when the user specifies for it to be used and only when the criteria specified for the interface recognition are met. The questions I had does the user know when he should activate it and what are the chances that the user will invalidate the code assessment by applying it inappropriately. I think there needs to be more discussion on how and when to use the interface tracking model as well as the limitations.

CONSULTANT WALLIS: I guess the user will activate it when his manager says to try something to bring down the PCT.

DR. THURGOOD: That may be and I guess my recommendation would be to try to search for a global model that would not have to be activated by the user but there would be logic in the code that would tell it when it should and should not be used and not have that --

CONSULTANT WALLIS: Well, he has to have some kind of appreciation for the likely physics when he's doing this.

CHAIRMAN BANERJEE: Is this in Version 5 of the code?

DR. THURGOOD: It is but they say it's still being tested and I don't think they said it's ready to be generally applied now.

CHAIRMAN BANERJEE: Okay. Go ahead.

DR. THURGOOD: Non-condensable gases. First is the observation I made for the developers and that is you can solving n-noncondensable gas equations without increasing the number of equations that are in the Jacobean when you do the matrix conversation so that can save some time if you want to have more than one non-condensable gas.

Non-condensable gas species specific heats should be temperature dependent. Currently they are constant. The specific heat of the gas/vapor mixture are calculated incorrectly.

CONSULTANT WALLIS: This is particularly true when one of the phases is steam.

DR. THURGOOD: Yes.

CONSULTANT WALLIS: I think if it's gas it's not too bad but when you put in steam --

DR. THURGOOD: Bipolar molecular.

CHAIRMAN BANERJEE: The third point of interest I didn't realize it should be based on mass fractions rather than mole fractions. Then this would be the mole specific.

DR. THURGOOD: The gas mixture properties, fiscosity and thermal conductivity, should be based on accepted methods for calculating gas mixtures properties rather than using pressure ratios to define the mixture properties. Again, they are alluding to steam, bipolar molecular.

When will the new method for handling the effects of non-condensable gases be available. The current method is wrong and requires that the interface be at the temperature corresponding to the bulk steam partial pressure. I believe George is -- Dominic is going to address this in his presentation.

This is simply the equation form of what I've been saying. This shows you how you get the mass fraction for the mixture specific heat.

CHAIRMAN BANERJEE: Capital C is --

DR. THURGOOD: Specific.

CHAIRMAN BANERJEE: But it's a mass specific heat, not a mole specific.

DR. THURGOOD: BTUs per pound. Same with the viscosity. There are equations for getting a mixture of viscosity and mixture of thermal conductivity of bioavailable gasses.

CONSULTANT WALLIS: Is it as simple as this or not?

DR. THURGOOD: It gets more complex for steam and I haven't shown what it should be here for steam but it can be more complex when bipolar mole is present.

MEMBER ABDEL-KHALIK: So if we try to use TRACE to find out whether a large gas bubble in a gravity-driven system like in a ESBWR would be able to block the gravity flow, we could not trust these results at this time.

DR. THURGOOD: The properties, the viscosity of the gas mixture and the thermal conductivity of the gas mixture would not be directly correct if steam were present which it always will be in a bubble in water.

CONSULTANT WALLIS: Do they ever use the viscosity? I thought they threw out the stresses.

DR. THURGOOD: They use it in all the

interfacial drag for a license and, of course, wall shear when there's wall and they use it in all the interfacial heat transfer and the wall heat transfer.

CONSULTANT WALLIS: Used in the correlations for Reynolds number and that sort of thing.

DR. THURGOOD: It is a round number of conductivity.

Finally, the saturated steam internal energy has an inflection between 1e to the 5 and 2e to the 6. The derivative of the internal energy with respect to pressure and temperature actually changes sign in this region and it turns out that this has been the primary range of interest for small and large breaks. They need to correct that with vapor internal energy equations.

That's all I have.

CHAIRMAN BANERJEE: I have a question. Early in the theory manual they make a blanket statement about well posedness, imposedness, and refer to some paper or something. We know that in reality if you refine the mesh, as we have done in oil/gas problems that if it's not well posed and just unstable because you don't have enough numerical diffusion, acceptance stratified flow or whatever are actually imposed.

The French take the precaution to make the equations hyperbolic in those cases so that they can nodalize as finely as they wish. With the big nodes it won't make any difference if they are very large nodes. Of course, if you refine it down to very fine nodes, one civil completely. What do you think of that statement?

DR. THURGOOD: Well, I think the limitation of TRAC is that it is based on using only large nodes.

CHAIRMAN BANERJEE: TRACE or TRAC?

DR. THURGOOD: TRACE.

CHAIRMAN BANERJEE: TRACE seems very TRAC derived.

DR. THURGOOD: Basically it is. I use the numerical schemes and equations from TRAC.

CONSULTANT WALLIS: Did you look at the numerical methods at all or not?

DR. THURGOOD: I did. I didn't try to re-derive them all. I understand what they're doing. Because I understand them I can see how they will allow them to exceed both Courant as well as --

CONSULTANT WALLIS: We've had some criticism from other sources of the numerical methods.

It's not something I've dug into myself but I just wondered if they were resolved satisfactorily by this peer review panel and whether someone is going to say that the numerical methods are A okay.

DR. THURGOOD: You have to understand what the numerical methods are and they are what they are because of the complexity of the equations being solved. I don't think anyone today knows how to solve them better. That is the methods used are simply first order methods. There is no attempt to obtain higher order differencing methods which they are more common in CFD. I don't know what more I can say about that other than that is just --

CONSULTANT WALLIS: Is there a different way in which the 3-D version is treated numerically from the 1-D version?

DR. THURGOOD: Well, the momentum flex terms because you have the 3-D they do not handle the stress tensors.

CONSULTANT WALLIS: Oh, they couldn't do, for instance, the turbulent velocity profile in a pipe?

DR. THURGOOD: TRACE should not be used -- you should not bring the control volume down to be smaller than that which would contain structure because they do not have the viscous and turbulent shear stress tensors. You always have to have some --

CONSULTANT WALLIS: It's a funny kind of 3-D. It's 3-D that applies to a complex geometry like a core but it doesn't apply to a simple geometry like a pipe.

DR. THURGOOD: That's true because the lack of the stress tensors. You don't have the flood, flood shear. All the control volumes in TRACE should have -- one control volume should have several fuel rods. It should have structure because the stress tensor is completely treated by specifying --

CONSULTANT WALLIS: Isn't that a reasonable assumption to throw out the interaction between the decent nodes this way?

DR. THURGOOD: There are situations where I found that is an issue. Way back in the '70s I was kind of criticized for one of those. What I found using these types of equations in the downcomer, for example, is the code really kind of preferred to have liquid go down one column of nacelles in the downcomer while allowing vapor to go up the adjacent one.

I was very happy with that solution because there was no interaction between this gas and this liquid. Any gas that tried to go in through this channel met a lot of resistance so it just said, "Let's go over here." In my code I did an interfacial drag at the cell boundary to try to accommodate that problem.

If you look at Jerry's derivation of the two foot equations, for example, he actually has a term which contains the interfacial area at the boundary. But then as he completes his derivation he has all others to throw that term out because generally they are thinking more in terms of bubble flow and we are talking about the very small fraction of interfacial area that may happen to touch a boundary and they say we can ignore that but there are cases clearly when you cannot ignore that.

CONSULTANT WALLIS: Suppose we have this problem of a core which the whole base is covered with debris except for one little hole and the flow comes in one hole. The only way it spreads out through the whole core is by some kind of circulation and mixing between all these different channels and a PWR. It can't do it in a BWR. I just wonder if TRACE is going to model that sort of internal mixing in the core properly trying out some of the mixing terms.

DR. THURGOOD: It's difficult to -- well, it's difficult in its pure form to even model void migration without adding models to give you void migration data.

CHAIRMAN BANERJEE: Would you seriously advocate using this for a real treaty situation like the one Graham was describing which is, of course, of great concern to us at the moment?

DR. THURGOOD: It cannot be used anytime the friction factors due to the structure become nondominant. Now it's gradient terms which cause the fluid motion.

CHAIRMAN BANERJEE: You probably can't get the cross flow terms right. I mean, all its got is loss proficiency of some sort. Right?

DR. THURGOOD: That's correct.

CHAIRMAN BANERJEE: There is no real cross momentum flux terms due to the stresses.

DR. THURGOOD: Not due to the turbulent stresses. There is no flood footage here and no lead turbulent interchange within the flood. You cannot model mixing.

CONSULTANT WALLIS: So in something like ESBWR when you have steam and water coming out of the core, all the way across the core going into some kind of a large chimney rosenberry is in there modeling the entrance region where there's mixing probably is going to be very coarse and crude and won't show you if --

DR. THURGOOD: I haven't looked that they're noding for that but I would say any large open region wouldn't be modeled rather poorly.

CHAIRMAN BANERJEE: Okay. Thanks, Marv.

MEMBER ABDEL-KHALIK: Just one question about the inadequacy of the equation for saturated vapor between one and 10 atmospheres. Is this something unique to the TRACE? I mean, is this a new empirical fit?

DR. THURGOOD: I think you made your own fit somewhere way back. It's an empirical fit I believe.

DR. MAHAFFEY: This is John Mahaffey. One thing that is missing in Marv's comment there is which equation to state. I understand that TRACE has two options. You can use an old empirical fit. A set of correlations were developed for the TRAC code. Or you can use tables that started from the RELAP 5 tables.

I don't think they are quite the same thing but they are ASME steam tables. There are reasons to go either way. My guess is you were using the TRAC per fits. If there was a flaw in there, it wouldn't surprise me. You've got to look on an application by application basis and look at the user guidelines to decide where to go there.

CHAIRMAN BANERJEE: Thank you, Marv.

Now we will have Dominic. Maybe you can tell us why they don't make the equation hyperbolic, Dominic. That's okay. I know with these big coarse nodes it doesn't matter. If you went fine nodes it would matter a lot.

DR. BESTION: You need very, very fine.

CHAIRMAN BANERJEE: I know. You have to have very fine.

DR. BESTION: Okay. The scope of my reviews. I base it on the documentation and I focused on filtration but not focused so on pressure model and on the assessment. For the assessment I considered SET and IETs devoted to PWR.

In my report for each closure model I tried to evaluate the importance with regard to safety, to evaluate the correctness and adequacy with regard to the knowledge, the consistency with the limitation of the model, the degree of empiricism with regard to the physical understand of the corresponding flow process, and also the validation of each model in a SET way, and the adequacy of the section of the theory manual where it is described.

At last, sometimes I give recommendations.

Additional R&D work for improving the model or additional validation or for improving the documentation.

I will start by giving all the main conclusions and then I will be more specific on a few points. TRACE appears to be a good system code with extended capabilities for simulations of LOCAs, PWRs and BWRs. About equations and --

CHAIRMAN BANERJEE: Let me interrupt you for a moment. Were you ever asked to also consider instabilities or just LOCAs?

DR. BESTION: It was said at the beginning that it was validated for small break and large break LOCA for PWR and BWR. I don't remember exactly what was stated about instabilities. I don't think there was a specific charge.

DR. BAJOREK: We weren't asked to look at those.

CHAIRMAN BANERJEE: You were not asked?

DR. BAJOREK: No, not that I recall.

CHAIRMAN BANERJEE: It can be BWR. I mean, this has now been coupled to marks and look at real problems and even atlas instabilities which is a big concern for us.

DR. BESTION: The assessment on the LOCA.

I found that impressive work was done to revisit all closure models and improve some old correlations and these produced a coherent set of models. Coherent means that all these models work together.

The most easy to do would be to take the best model of this compare and the best model of this from other places because one must take care of having models that work together well and this was well done. Probably the fact there was one people who did the control of everything during the period may be a good condition to obtain a good set of motives.

CONSULTANT WALLIS: So you seem to have focused again on the closure models. You did have some comments which I agree with about the transition from equations 115 to 119 is not well justified. You talked about the single pressure P. I mean, there's interfacial pressure and there's all that stuff which is just sort of glossed over in the derivation.

I haven't spent much time on this because I just looked at this but I'm told I have to think in terms of a coordinate system in which I follow the center line the vectors coming in and going out are somehow in the same direction even though they're not. I don't quite understand what that means. Did you understand what that meant?

CHAIRMAN BANERJEE: There is a specific statement which also struck me so I was wondering did it strike anybody else.

CONSULTANT WALLIS: We spent time on this. I just looked at it and said I need to understand what they're doing. Did you understand what they're doing when they say any though the vectors have different directions we are supposed to think about them as if they don't?

DR. BESTION: In the derivation of equation I think there are not enough -- all the steps are not well described in sufficient detail. This is one of the --

CHAIRMAN BANERJEE: You are very kind when you say that. I would have said it's superficial.

DR. BESTION: I will have some comments at the end but particularly for the 3-D modular I never saw, for example, the positive factor coming from the -- of course you can eliminate them at the end if you make some assumptions that they are uniform and so on. At least one should recognize that they exist and then they can be simplified. There are several steps which are not explained.

CONSULTANT WALLIS: That's good. I think it will be nice to spend more time telling them what they should do because we keep telling them.

CHAIRMAN BANERJEE: Nobody listens to us.

CONSULTANT WALLIS: This documentation keeps changing and then every time it changes we have --

CHAIRMAN BANERJEE: The core problems stay the same.

DR. BESTION: I have some comments about that at the end. Most models seem adequate and reflect the present state of the art. The degree of empiricism of most models is consistent with the available understanding.

Mechanistic models were selected when it was possible and most of the time the good mechanistic models were established in some ideal condition and when you go to more that is where the condition is -- sometimes some chilling is necessary and it was used when it was necessary. There were also some pure empirical models which were used only when no other approach could do a better job, for example, for the particulate flux tables.

There were a few models which have an unnecessary degree of sophistication. I can mention the convect which flux to liquid or the nucleate boiling. By unnecessary I mean you will not see a difference in LOCA with more simple model and also you cannot prove that it is more precise because there are no experiments in the validation which show that you have better connections.

It's not a big problem but just to mention. A few models may require further analysis and I will give a few examples later. Again, I did not find any big flaw in the equation or in the models which might lead to a wrong prediction or to wrong conclusion on safety issues.

CONSULTANT WALLIS: Isn't there a problem with CCFL where the random equations are predicting something different from the correlations and the correlations aren't very good anyway?

DR. BESTION: I made a comment on this because it is not described in the documentation. In the validation there is a specific correlation which is implemented and there is a flaw so it should be at least analyzed.

CONSULTANT WALLIS: It is a flaw and it might in some situations make a difference to whether or not the water gets into the --

DR. BESTION: I'm not sure it's a big flaw because it -- and at one time it escapes. I guess it should be analyzed why it is like this and when it could be even worse. I don't know. I cannot prove it is a big flaw. It's a deficiency.

CHAIRMAN BANERJEE: Isn't the CCFL model backed out from essentially a plotting correlation? I mean, my understanding all these vertical type of flurogens is, you know, many of the interfacial friction correlations are simply backed out from under a flux model or CCFL correlation. I don't know. Maybe people who develop this correlation can comment on it. That is normally the way it's done because in the fluid model it's not easy to specify.

CONSULTANT WALLIS: I don't think they do. I think your point was they sort of impose a correlation quite apart from the momentum equation. They don't synthesize the two.

DR. BESTION: I don't know exactly how it is written.

CHAIRMAN BANERJEE: Probably just the explicit correlation is put in.

CONSULTANT WALLIS: I think it's sort of imposed. The momentum equation says something and CCFL says something else so you go with CCFL.

CHAIRMAN BANERJEE: Can somebody here tell us what you do?

DR. STOUDEMEIER: It's imposed independent of the momentum equation. It's designed for situations where the hardware is more complicated than what the interfacial drag can really predict. You have specific measured correlations for CCFL and that hardware code goes through and goes through checks of whether CCFL criteria are exceeded or not. If they are, then the equations get modified.

CONSULTANT WALLIS: There is a separate criterion from the momentum equations themselves. It's not like what some vendors try to do which is to synthesize the two so that the momentum equations give you the correlation. You don't do that?

DR. STOUDEMEIER: Well, I think what they do is a little more sophisticated than that. I think they turn the CCFL criteria and embed them into their momentum equations better so that there is a smooth transition from CCFL into their interfacial drag.

CONSULTANT WALLIS: If it's a stability problem it's not quite fair to do that. If it's an actual limit of momentum equation that is one way to get CCFL. You can also get CCFL from an instability which isn't modeled. I don't want to go on too long.

DR. BESTION: One has to understand this specific option in the system because there are some things similar for pressure losses. Normally the friction terms, the classical friction terms predict the pressure loss.

When there is a single geometry you need something to add to some loss efficient LOCA. The second also are the same -- normally interfacial friction predict the -- but in some complex geometry like the -- you need something more specific and you implement specific correlation.

MEMBER ABDEL-KHALIK: The statement that you have in there about no big flaws were identified except one, is this statement true even for passive safety systems like ESPWR where the driving delta-Ps are very small and noncondensable gasses may be present?

DR. BESTION: We didn't see a need corresponding to these reactors. We had a reassessment on second generation reactors.

MEMBER ABDEL-KHALIK: So shouldn't you constrain the statement someway rather than making it so sweeping?

DR. BESTION: Okay. Yes.

CHAIRMAN BANERJEE: You didn't have access to the ESBWR applicability document.

MEMBER ABDEL-KHALIK: The statement in its present form implies that we can do ESBWRs correctly and I'm not sure that is true.

DR. KROTIUK: Let me -- this is Bill Krotiuk. Let me say again as I said early on that the report on the applicability report for the ESBWR was not available at the time that the peer review started to review. It was published in March of this year so they did not include that in their review.

CHAIRMAN BANERJEE: I guess you just say constrain it.

MEMBER ABDEL-KHALIK: Right. I mean, if you present a conclusion without sort of indicating the limits of its applicability, that would be misleading.

DR. KROTIUK: It was out of scope.

CONSULTANT WALLIS: So we have the example we had with the previous speaker. We have this T which is behaving like a pump which is a flaw but presumably it doesn't affect safety because you conclude it doesn't affect safety issues. As far as you know for the applications you are familiar with it doesn't matter.

CHAIRMAN BANERJEE: Not for LOCA.

DR. BESTION: Also for the numerics, only set of numerics I did evaluate but level tracking method got pumped very well because I have two tests

-- by simple water and the oscillation in the U-tube and they are much bigger than any other so that's okay. I don't understand if there was a problem in the bend, it should not be -- in the U-tube oscillation and the U-tube oscillation is perfect. I have no worry about the momentum equation.

CHAIRMAN BANERJEE: Probably there is no T junction there.

DR. BAJOREK: That case would work all right because of the junction with the T. There was a pipe couple on it that was used.

DR. BESTION: Okay. Now, about the assessment. SETs and IETs validate many models and covers many physical situations. Some validation calculations are not sufficiently analyzed. It seems to me that sometimes the people didn't have enough time to analyze in depth the result of the calculations.

Additional assessment is required for more exhaustive coverage of all models and of all important phenomena. No big flaw was revealed by assessment calculations. It doesn't mean there is no flaw but none was reviewed by this existing calculation so some checks in some models and some additional assessment are necessary to finally demonstrate that there is no flaw. No flaw was identified in the documentation of the model. No flaw was identified from the existing calculations but, of course, it is not the final demonstration.

CONSULTANT WALLIS: What do you mean by a big flaw because you did a very thorough job and I was very impressed with your review but you made many recommendations for improvement. How big does the flaw have to be before it's a big flaw?

DR. BESTION: It has a big affect.

CONSULTANT WALLIS: Something which really affects an accident 100 degrees. Okay.

DR. BESTION: Okay. The documentation of the physical modeling in the theory manual gives not only the selected models but also justification of choices and this is very efficient. The documentation of the validation and verification starts by a PIRT table and the result of each SET or IET simulation.

I have a general recommendation about the assessment. The analysis of some calculations should be improved. Each assessment work should be related to the PIRT table. The PIRT table identified important phenomena but when you calculate some tests you should say, "Okay, this test addresses --

CONSULTANT WALLIS: Can I go back to this question about the big flaw. We had an example where TRACE calculated the peak clad temperature for a certain accident and the reactor then also did a calculation for clad temperatures and these differed by 400 degrees. The explanation was TRACE did not model radiation, heat transfer from the hot rod.

The vendor went back and said, "Okay, we'll run a calculation in which we suppress radiation." Their peak clad temperature went down by 400 degrees. The conclusion was if TRACE had put in radiation probably it would have got this 400 degree change. That seems to me a big flaw. If radiation isn't in there or is improperly modeled and it can account for hundreds of degrees of difference, then it's a big flaw.

DR. BESTION: It appears very strong. 400 degrees for radiation for me is too much. I would --

CONSULTANT WALLIS: This was fahrenheit.

DR. BESTION: Oh, fahrenheit. Okay. I would say the affects may be 50 Kelvins, the PCT, the radiation, not more than 100 Kelvins probably. Of course, when you look at the calculations which are produced in the assessment some radiation is taken into account and the PCTs are plus or minus 50 K, not more. I have no big problem with the physical behavior during flooding.

DR. STOUDEMEIER: I would like to make one comment on TRACE. This is Joe Stoudemeier, Research. TRACE does have the ability to model radiation and if it wasn't turned on, that was the user that didn't have it in his model. It's not lack of capability in the code.

CONSULTANT WALLIS: Does it account for 400 degrees?

DR. STOUDEMEIER: I don't know. It would depend on the problem. For instance, in a BWR there is more cold structures to radiate to in a LOCA. I think it would have a bigger affect there than it will on a PWR.

CONSULTANT WALLIS: I think we might visit this later when Peter Griffith gets up because the question is when you actually evaluate heat transfer co-efficients in these experiments, how do you account for radiation may affect whether or not you include some radiation in the heat transfer and then ascribe it to heat transfer coefficients instead of to radiation. You need to be sure what it is you're correlating. We'll revisit that later.

DR. BAJOREK: What analysis are you referring to with the 400 degrees?

CHAIRMAN BANERJEE: It was a submittal.

CONSULTANT WALLIS: It's in the record.

CHAIRMAN BANERJEE: We had to have a reconciliation between the vendor and the calculations.

CONSULTANT WALLIS: I guess it's all in the public record.

DR. BAJOREK: Is there a specific plant we can look at?

CHAIRMAN BANERJEE: Yes, it's a specific plant.

DR. BAJOREK: Which one was it is the question he's asking.

CHAIRMAN BANERJEE: I don't remember.

DR. BAJOREK: I'm trying to remember.

CONSULTANT WALLIS: Susquehanna.

MEMBER SHACK: I thought it was an Appendix K calculation versus a realistic calculation. I think it was Susquehanna.

CHAIRMAN BANERJEE: Maybe it was Susquehanna.

MEMBER SHACK: I think it was Susquehanna but I recall the vendor did Appendix K and the staff did --

CONSULTANT WALLIS: As I recall the

vendor --

MEMBER SHACK: They did it half realistic. They didn't quite -- they couldn't make their code do a full conservative model but they tried to get part of the way there and I thought --

CONSULTANT WALLIS: When they switched off radiation they got something like that. I remember that.

MEMBER SHACK: I'm pretty sure it was Susquehanna.

CHAIRMAN BANERJEE: Look up Susquehanna.

DR. BESTION: Okay. Recommendations. A cross reference matrix with the models against the SET matrix should be added. The range of parameters in which each closure law is validated in a separate effect way should be identified. Some recommendations to users based on assessment work. For example, recommendations on mesh size and the time step should be added.

Now I go to some recommendations about models. First, the stratification criterion. There are three criterion. The first is KH instability which tells you when the stratified flow becomes unstable. It is necessary. It tells you when a bubbly flow will be able to go to a stratified flow when the turbulence is not too big because turbulence tends to originize the bubbles.

It is also very useful because in reactors the horizontal pipes or vertical pipes so it is generally starting with a bubbly flow. There is a subcriterion which is based on CCFL flooding limit based on UPTF tests. This for me we can accept to something based on a CCFL limit but it is very geometry specific.

This one is based on UPTF tests which are specific to the design of hot legs where there is a specific device called the HUTSA which directs the SSC flow to the special vessel and this affects the flooding limit so it should not be the standard option. It can be subcriterion but it should be at the discretion of the user and not both from UPTF.

For the direct contact condensation I analyzed an experiment a long time ago where I found that at the place where the ECCS there is a strong condensation. For example, in this test I found that 80 percent of the total condensation -- because of the high turbulence mixing in the zone. This mixing is due to the jet-induced turbulence.

This is not modeled in TRACE and probably this should be modeled because you will not be able, for example, to predict that there will be a condensation driven instability because condensation is very isolated but that creates a -- and so on. You may miss some phenomena if you do not model this.

CONSULTANT WALLIS: I think if you have enough velocity coming in the side the liquid actually goes up around the walls.

DR. BESTION: No, this does not do that.

CONSULTANT WALLIS: Not in this case? But there are some reactors where the injection lost is enough but the liquid comes down and goes up around.

DR. BESTION: I did some simple experiments and as soon as you have some liquid level, that does not appear anymore. When it is fully empty, okay, it can go this way but as soon as there is some -- it doesn't go.

MEMBER ABDEL-KHALIK: Don't the vendor models include this affect?

MEMBER ABDEL-KHALIK: Well, from what I remember of the vendor models, they would usually take a simplistic view of a jet and look at your facial heat and mass transfer around the jet but they didn't necessarily give this 80 percent condensation rate in the zone very much close to the jet.

A lot of times the model would be biased to give you a low condensation because that would give you instabilities in your transient and you would get more of your condensation in the stratified regions downstream of that injection but it's not physically correct with what happens to tests like one-third scale mixing or COSI.

DR. BESTION: Okay. Also, there is not the facility to initiate overheated synchronization. For example, you have hot vapor possibly with no condensable gas flowing down here in the presence of a cooling wall and it may happen that locally the steam temperature reaches saturation and can stop some condensation.

In TRACE you need first to break all the steam saturation before stopping condensation. In fact, it is the exact opposite indication of simple boiling. You use these direct wall to interface to create vaporization and here you should use the same direct word to interface to condensate some steam when the LOCA temperature reaches saturation so this should be added.

In the presence of noncondensable gases also there is an additional term which is directed first from gas to liquid. Normally when you do mass energy balance of wherever you need to show interface you obtain this equation between the liquid interface and vapor interface. Here you do not verify Qli. You multiply Qvi by the pressure of steam divided by total pressure and you add this directed flux multiplied by P, noncondensable pressure divided by total p.

This, in fact, is not established. It is not justified. One can understand that one cannot treat the situation with only these because there are situations when the pressure of the steam goes to zero and we should stop condensation because there is no more steam to condense.

And there are also situations when you discharge nitrogen from -- where you can have negative temperature below the --. In this case also it may be necessary to have a direct from gas to liquid without --.

As it is done now this term will -- situation where it should not. Even the situation where there is no condensation but vaporization it will change the physics of the -- where it should not end so I recommend to limit this treatment to some extreme situations like this one.

So from the wall heat transfer selection logic one should allow wall to liquid heat exchange for alpha greater than this. Probably another 9 would be better because with -- is flaw. Liquid can also vaporize. And the selection of the film condensation model should be based on the criterion on film thickness rather than on the void fraction because in a large diameter pipe this would represent very large and very thick films.

The CCFL. The reason for the misprediction of the Wallis type flooding curve in small diameter pipes should be clarified. If necessary the CCFL model implementation in the equations should be revised if there is something wrong in the implementation.

Then some other recommendations about models. The flow regime map, one could add stratified mist flow regime for relatively low void fraction. It only exist for low void fraction and could normally exist also for void fraction below .5.

Interfacial friction in pipes was focused on boiling situation where normally when you have a boiling situation where you are in the core, and if you are in the core you are not in the pipe so normally you should better focus the modeling on the situation which actually encountered in the reactor and you can encounter bubbling and slug flow in the pipes but not in boiling situations.

Momentum equations. I was not completely sure that the momentum equation was capable of calculating the void fraction radiant in horizontal pipes when there was a rather complex situation. I will give you an example. This is a benchmark that I did a long time ago that was cut out. There is a pipe with a restriction and -- and then a bend. If you close the entry but inject some liquid very slowly you should check that your interface stays horizontal even in the convergent. When it reaches this you should stop filling this part.

You should obtain horizontal interface in this case and then starting to create a second level here. You should keep horizontal also in the bend. If you do the same with the weight, we use additional term. One is proportional to gradient and the other to the area change. I didn't see how it was clearly done in TRACE so I would recommend to do these tests.

For core interfacial friction it is one of my correlation which is used and I know that it is doing a rather good job in boiling water reactor. This correlation over-predicted in PWR so it should be corrected because it's not a very good thing to over-predict function in pressurized reactor core because it gives more favorable description of small break

LOCA. If you have interfacial friction you will predict the levels well in case of problems so it is favorable.

The flashing. The flashing delay is not modeled so it should be at least a qli model should be at least to allow some sensitivity testing to take into account in the uncertainty analysis. The film condensation of gases there are two models. One is said to be not very good. There is a good model which takes into account the mass diffusion which calculates the temperature.

This model is not yet the stronger one because it is not run enough. I remember when we implemented the same in the -- we also had problems of our business and we could obtain the same results by just simplification of the mass diffusion equations and we could obtain some elimination of the temperatures which would simplify the calculation.

CONSULTANT WALLIS: Can I ask you about this flashing delay model? This has to do with nucleation?

DR. BESTION: Yes.

CONSULTANT WALLIS: It's very sensitive. As you know, you can de-gas water and you can treat it very carefully and then it does delay flashing considerably. If you already have some bubbles in there, then you can get flashing to happen much quicker.

DR. BESTION: It is a very difficult problem.

CONSULTANT WALLIS: Right.

DR. BESTION: At least adding -- and it can be taken into account --

CONSULTANT WALLIS: Then the question is how big a range of delay should you use? It's up to the user?

DR. BESTION: Normally in industrial situations the flashing delay is not --

CONSULTANT WALLIS: In an industrial situation it's not. It might be in a reactor. If you go through a transient it snuffs out all the nuclei. It might be hard to get it going again.

CHAIRMAN BANERJEE: But there have been a lot of experiments done on this. The original Edwards pipe break experiment showed a pressure undershoots significantly.

DR. BESTION: Yes, in -- you have some pressure undershoot.

CHAIRMAN BANERJEE: I thought that Leonard and Allan Gear or somebody had developed sort of a correlation for this undershoot delay. Lackmay did some work as well.

DR. BESTION: I would say that none of the correlation to my knowledge is okay for all situations.

CHAIRMAN BANERJEE: Does it make a big difference? I mean, it may make a big difference when it comes to forces on structures. At one point I remember a problem where for BWR people were looking at forces and structures due to breaking the feedwater pipe through the spotter and stuff like that and there it does but for most of these LOCA type problems I don't think it makes a difference.

DR. BESTION: I don't expect big affects on LOCA.

CONSULTANT WALLIS: We can easily do a flashing delay model with a soda bottle.

CHAIRMAN BANERJEE: And some whiskey.

CONSULTANT WALLIS: Depending on how much you shake it up.

CHAIRMAN BANERJEE: Okay. Keep going, Dominic.

DR. BESTION: Okay. For longer term recommendation the flashing model itself, which is the liquid to interface with the flashing in metal --

liquid is not physically based at present. In fact, it's not a big problem but if you want to be able to calculate a physical flow in one simulation with more meshes and so on, it would allow you to avoid the chocking model and it could be another -- to the TRACE code. For this we need a physically based flashing model.

Momentum equations, I may recommend to implement the added mass force particularly if you want to model flashing flows in convergent and so on. In the end you may also to do a well posed model. This is not to be quoted as I said to have a ill-posed problem but in the future people will tend to decrease the mesh size and so maybe there will be some problems and people will not be able to do some sensitivity tests, some convergence tests to the meshing if it is not a real problem.

Also for this 3-D model the implementation of the turbulent dispersion force may give it other than a life force which is mentioned in some document. About the validation, condensation at ECCS injections should be validated. At present there is nothing. Non-condensable gasses should be addressed.

Film boiling in blow down and in reflooding is one of the most -- coefficient which has an effect on all these LOCAs. At present there is an original model which I love because it is rather simple. I was a little surprised not to see any effect of the mass flux in the coefficient nor of the subcooling. I think that maybe should be revisited.

The validation hunt should be extended to lower steam quality and higher -- to better check that there is actually no effective mass flux and no effects are necessary on this. I think that the variation range of this important model is not large enough.

Downcomer refill. There should be a policy with respect to the affect of non-condensable gasses, nitrogen, for example. We don't know if we should model the nitrogen or not so there is no recommendation on this. Since it may have an effect, it should be said that you need to model or not.
A reference 3-D nodalization for the PV should be defined for the core and the downcomer and applied to both the validation and to the reactor application.

For the reflood the same. The assessment calculation used non-converged 3-D nodalization and it is not possible to converge nodalization in 3-D. It would go to very large CPU time which I'm not particular. Using non-converged 3-D nodalization there should be a policy. There should be a reference to nodalization so that people do the same compensating the -- and when they apply to reactors so this is not clarified.

The sensitivity to the time step should also be investigated. I know from experience that the reflood calculation particularly when the oscillations are very sensitive to the maximum times that you use them so there should be a recommendation on this.

Also the oscillation during reflood are part of the PIRT as an important phenomenon of large break LOCA but they are not addressed by the assessment. LOFT is not representative because of scale distortion. SCTF and CCTF not representative since oscillations were avoided by using LP injection. They were rated by injecting water in lower plenum instead of public.

You should add the LOFT test in Dead Sea, in Precale, in Aquitis, maybe in LOBI where you can assess the capability and try to predict oscillation during the beginning of the reflood. Also, for the de-entrainment in upper plenum the UPTF test was designed to investigate this phenomenon and it should be added to the validation.

The hot wall heat transfer in downcomer

during reflood where there can be some boiling and reduce associated with reduction of the pressure head so this could be also addressed and there are some tests, some Japanese tests from the JAERI. The reason for the deviation should be analyzed. There may be some model corrections.

Also sensitivity to the meshing and recommendation to users should be given. It should be checked that the friction does not impose anymore limitations than the CCFL model. Also the validation can be extended to some generator in the header and the inlet of --

CHAIRMAN BANERJEE: In the validation did you see any comparisons to predict reflux condensation and especially liquid pulled up?

DR. BESTION: No.

CHAIRMAN BANERJEE: They didn't? We are quite concerned about that for some of the new reactor concepts.

DR. BESTION: Normally if you --

CHAIRMAN BANERJEE: While you are using the steam generators heat sink to pull down the pressure.

DR. BESTION: In this case we can keep the water and for this that's why I recommend to add validation of the CCFL option at the inlet of steam to check that your model is able to control the CCFL limit.

CHAIRMAN BANERJEE: Not only that --

DR. BESTION: The existing correlation you just should put them in your deck and check that it is actually --

CHAIRMAN BANERJEE: So the accumulation of the water in the steam generated tube is a function of the CCFL but as the water can't fall some of it keeps accumulating and building up a head.

DR. BESTION: Yes. That's an important phenomenon. Normally if you have the CCFL option and if it works correctly you should be able to predict this behavior.

CHAIRMAN BANERJEE: There's no magic. It's relatively easy to predict if you do it by hand.

DR. BESTION: Even with the codes if the CCFL option works correctly, it should be able to do it with no problem.

DR. BAJOREK: Sanjoy, a couple of times we've talked about some of these phenomena that are important to the advanced impassive plants. One way to think of the assessment that has been done and the peer reviewers have gone over is generic assessment that should apply and be used by everybody. When we deal with things like noncondensable gasses in the PCCS heat exchanger or reflux condensation, we see elements of that in some of our assessments here but not to the extent that is necessary for ESBWR for an EPR. When you do get those applicability documents --

CHAIRMAN BANERJEE: Are you doing applicability for EPR as well?

DR. BAJOREK: Yes.

CHAIRMAN BANERJEE: I would be interested in that.

DR. BAJOREK: In that one you would find reflux condensation against the flex CSET experiments, some work we've done at APEX, and some additional separate affects type tests that were done using the ROSA facility.

DR. BESTION: There were some in LOBI and I think also in TKL. I mean, it goes way back.

DR. BAJOREK: But those were the three that you should find in the EPR because we thought that those tests were instrumented well enough and could be used for the assessment perhaps better than some of the other ones. We don't have the resources to do everything but we thought those would be good choices.

CHAIRMAN BANERJEE: Let's see what it looks like.

DR. BESTION: This is another list of some assessment methodology. I will not comment on all of them just to show you that I found some lack of analysis in many of the calculations. I can comment maybe one. TPTF horizontal flow tests are tests in horizontal pipe to validate the temperature in stratified load.

I remember that some of them are critical and others are supercritical. If you are critical you impose function and then the validation of the fraction indeed gives you information on the correctness of the interpretation. If you are subcritical, besides the reflection all along the pipe is done by recondition.

In this case you do not validate anything. This is not even explained in the analysis that some of these tests are not validated in the friction so they should be identified with tests which are supercritical. There must be some more analysis of some of this.

CHAIRMAN BANERJEE: Subcritical or supercritical is the outlet?

DR. BESTION: Sometimes you get a crude

number with respect to the propagation.

CONSULTANT WALLIS: Sometimes you get a hydraulic jump which changes everything.

CHAIRMAN BANERJEE: We must say that we have the missing member here.

CONSULTANT WALLIS: Do we have to bring him up to date?

PARTICIPANT: No.

CHAIRMAN BANERJEE: Coming through a thunderstorm.

DR. BESTION: About the recommendation.

CHAIRMAN BANERJEE: Excuse me. I want to go back to that point because if you correctly formulate your hydraulic -- I mean, your fluid model you should be able to capture the hydraulic jump when you come in.

DR. BESTION: You can capture the hydraulic jump.

CHAIRMAN BANERJEE: So it should not be a problem. Do they formulate the equations correctly? Even in a one-dimensional set you should capture it.

DR. BESTION: I wanted to see, for example, how they did this simple test with the original stratified flow to be sure that it is correct. I guess it is correct but I have no --

CHAIRMAN BANERJEE: You haven't looked at the equations to see if they are correct.

DR. BESTION: The additional term which is in TRACE --

CHAIRMAN BANERJEE: Did you have the right factor?

DR. BESTION: It is not the right factor. It is the liquidate which is calculated and there is a term which tends to homogenize and liquidate so this normally should be part three but I just would like to see the proof.

CHAIRMAN BANERJEE: The proof against experiments is good but one needs to take a look at the equations to see if they are correct.

DR. BESTION: It was not in the part of the documentation which was given and we were explained at the March meeting that, in fact, there is something which takes into account these effects and normally it should be correct.

CONSULTANT WALLIS: Does TRACE predict a critical flow at the outlet? It may not even do that.

DR. BESTION: It should be equal to the gravity wave velocity.

CONSULTANT WALLIS: Two-dimensional affects of the lip. I wonder if they even do that.

DR. BESTION: Okay. I go now to recommendation --

CHAIRMAN BANERJEE: That is a very -- that's a good point because it is very simple to test actually and should be done.

DR. BESTION: The recommendation, I would prefer to separate the derivation of balance equation for the 1-D and for 3-D because, in fact, they are simple steps in derivation and some of them are not the same.

In 1-D you are right about the procession. In 3-D you have a volume which contains some metallic switchers, some solid switchers. Then particularly for the 3-D special razor one should do one. I say one because it is not only the case for -- and I didn't find it neither for RELAP 3-D model explanation of all the simplification which are used for the --

All the steps are not clearly described and all the simplifications are not identified. Some of them are identified but not all of them. Also, the scale of space averaging should be clearly specified for each subcomponent of the PV.

When we are in the downcomer we should say that this case should contain the whole thickness of the downcomer because if you want to mesh within the thickness of the downcomer, you wouldn't need turbulence modeling. There is no turbulence modeling so it should be clearly said that downcomer calculations can only be done with only one mesh and it is not clearly said.

Also, the volume averaging of 3-D equations should be presented showing how the porosity appears and what simplifying assumptions allow to eliminate it. I didn't see a single time porosity appearing in the equation. It's better to start from the clear equation and then to identify all the steps which go to the simplified reason.

Also, the absence of turbulent diffusion is justified but the absence of dispersion terms is not justified. This is not mentioned at all. I will just show you an example. Also, a clear policy with respect to the use of non-converged 3-D nodalization should be defined. There are two possibilities.

First you can use reference nodalization for both the assessment and the application to reactor. Since you have data for downcomer organization, the UPTF test, this is possible. You have the SCTF test. You have the same distance from the center to the site of the --.

Your practical data which can be

calculated with the standardization in the assessment and in the application. Now we must evaluate the non-convergence from assessment calculation and to use it in the reactor application. You need to know something more. I will give you an idea of how this is the reactor official list which is very complex geometry and this is simplified like this with a classical nodalization. There are generally 20 meshes or something like this in this direction, five in the direction from the axis to the pressure wall and eight in the other direction so this is a very cross validation.

I wanted to show you just what is the averaging volume. If you first use a time averaging values and then you apply the volume averaging so there is a value and a difference. When applied to this convection equation, the FPF is the calculistic function of the grid which is equal to one when you are in the grid and zero if you are in the solid. When you apply this averaging you can find two terms.

One of the convection which is in the final equation and this one is moment to dispersion related to the object of the differences. This is neglected. It is different from the regular system because it was already the answer of the first averaging. This one is probably more important than the turbulence test. The term for the convection term is microscopic convention and enthalpy dispersion.

If I look to the main effects during -- my feeling is that the turbulent diffusion terms which are not modeled are lower than the dispersion terms which are not modeled which are done in the interfacial and -- which are addressed by different models so these may explain one probably can neglect the first approximation plus diffusion inspection there.

Now, if I tried to give my feeling about the important PCT of simplifying assumption, these are figures coming from just my feeling that they should be established in more objective way. I would say that the absence of -- diffusion has particularly no effort. Let's say 2K of dispersion or so a little more but not big, 5K.

The non-linear fusion when we use for -- This is known to have a very argumental diffusion and probably it is larger than the physical diffusion and dispersion. Let's say 10 divisions. This is more of the numerical error we make on the closure terms right inside of the equation on the interfacial and wall transfers. These are a function of variables and when we calculate the function of the other quantities we make -- should calculate the other function of the quantities.

This may be significantly larger than the numerical diffusion. The numerical diffusion, in fact, is the -- convection terms and this is the discharacterization on the pressure attempts in the --

Then we compare this to the physical uncertainty on these terms --. Probably it is something like plus or minus 18 degrees. If we consider all the sources of uncertainties, it means all the models if we were in the circuit and uncertainty also due to the inlet conditions, bundling condition, matter of properties so we rate something like plus or minus 150K or maybe 250K. There was an international exercise based on -- predicted something like this.

So when you compare these features you can understand that it is not that urgent to model diffusion. It is not urgent to model dispersion. It is not urgent to change a numerical scheme and to have a second order convection term because you will change this small feature.

Also, if you reduce the mesh size you will just decrease this one and this may be applicable but, of course, dimensions of uncertainty. There is uncertainty in the physical model. I get figures which are not granted but in the documentation there should be more objective estimation of this simplifying assumption so that people when they use this model know what they are doing.

My conclusion from this is that it is good using this model even with nodalization, even with simplified numerical scheme. Even with absence of dispersion is okay because we accept a rather high uncertainty and all these are minimum with respect to the other ones.

If you want to use this same model, for example, mixing problem or mixing cold water like in -- this case seems very different. You have no more of bigger -- due to temperature here. The numerical is a problem. The absence of -- diffusion is a problem and the use of large nodalization is also a big problem. I guess that more -- on the documentation of this special vessel model would be necessary for helping the users to have a better idea of what are these models.

I feel that they are good models for LOCA but there should be more defined. They are subject to a lot of --. Many people decide that we use so many meshes so simple discharacterization people think is nonsense not to have turbulence and so on. I just demonstrate that it is not a problem in some applications but it may become a problem in other situations.

So to conclude, in the documentation there are also a few comments. It is said somewhere that the chocking occurs when -- is reached. That is not true. This probably was taken from an old version of the chock flow which was taken from an old code and probably was written some 20 or 30 years ago. Now we know that chocking does not appear --.

The speed of sound in -- depends on the frequency and the characteristics which gives you the speed of sound only for high frequency. For low frequency the velocity is different. The propagation velocity is different. I would verify many times that when I calculate another flow I decrease the pressure and I obtain the flow blockage. I look at the characteristic of the speed and is not zero.

Probably there is a place where we have reached the speed of sound but the speed of sound caused only to low frequencies, not high frequencies. High frequency waves do not play any role in this because they are done so this should be abated. This is an example of an improvement which may be implemented in the documentation.

And now to conclude. No big flaw was identified. TRACE is already a good system code for LOCAS of PWRs and BWRs. A few models require further analysis and improvements. Assessment should be extended and improved in some cases. Then for long term additional recommendations.

Add a third field, the droplet field. Improve the modeling of PV by allowing local mesh refinements. For example, having a -- which has more finalization in keeping the same nodalization of the --. This could be a good thing. And also implement dynamic modeling of turbulence and Aj. That's it.

CHAIRMAN BANERJEE: Do you have any comments at all on the desirability of paralyzation of these codes?

DR. BESTION: Normally when the code has all the bugs removed the CPU time is not a big problem. I have the reference of -- which now can calculate in six or eight hours even with the 3-D model in the pressure vessel so you can run 100 calculations or so if you want to make analysis without having big problems for the CPU time.

It may become a problem if you really want to decrease the mesh size for the model but it's difficult. You cannot decrease it because you do not have enough information to model the determinance so you cannot decrease the mesh size. If you want to decrease the mesh size there is a time when you will need to also model the dispersion time and so on. Before going to more final paralyzation you must also implement some additional modeling.

CHAIRMAN BANERJEE: You don't think that paralyzation is essential?

DR. BESTION: For me that's not urgent. We have used paralyzation in -- but only for the application to real time translations. In this case people want to have the real time for some small break LOCA so that people can operate -- for operation.

CONSULTANT WALLIS: Isn't it possible to model a small break LOCA in a much simpler way and get a code which will run in minutes instead of hours? Do we really have to do all this stuff in order to get a good enough answer?

DR. BESTION: I believe --

CONSULTANT WALLIS: Small break LOCA is just a pot boiling from a hole. There's nothing much happening.

DR. BESTION: I'm not sure you can do it by hand. If you look at what is actually calculated by this code during the small break LOCA it is not so complex but there are many things interacting and so you need a code. I'm sure you cannot do it by hand.

CHAIRMAN BANERJEE: Graham might be able to.

DR. BESTION: One or two people in the world could do it.

MEMBER ABDEL-KHALIK: You gave some sort of intuitive estimates of the impact on the peak clad temperature of various effects. Do you have an intuition as to how much ignoring a direct contact condensation enhancement due to the turbulence would have on PCT?

DR. BESTION: I would say it may create -- in the plus or minus 150 degrees there might be probably plus or minus 30 which are due to condensation in the reactor mainly during the refill phase of the LOCA. This is one of the most difficult situations to model in system codes. This -- on large break LOCA is always very difficult to --

MEMBER ABDEL-KHALIK: But vendors do take credit for that, albeit in an empirical fashion. This is a very important effect for them. If you were to ask them to take this out just simply because it's relatively small and only has 30 degrees, I'm sure you would get a lot of objections.

DR. BESTION: You mean some people think it has a larger affect than that?

MEMBER ABDEL-KHALIK: Right.

DR. BESTION: Well, I remember sensitivity tests to this and I don't think that is affected the PCT by much more than what I said.

CHAIRMAN BANERJEE: I think we will have to take a break, Dominic. Thank you very much. It was very helpful. Since we are slightly behind schedule, we'll take a break for 10 minutes. Come back at 5 of 3:00.

(Whereupon, at 2:43 p.m. off the record until 2:57 p.m.)

CHAIRMAN BANERJEE: So can we resume, please?

George Yadigaroglu will give us his comments about this very rapid review that you had to do, 200 hours. Tell us all about it.

MR. YADIGAROGLU: Thank you, Chairman.

So my scope of the work was on the models, mainly. And I looked at the theory manual. I looked at the assessment manual whenever I had some questions about the applicability of the models, but--

CHAIRMAN BANERJEE: Do we have the same slides?

MR. YADIGAROGLU: My presentation here is very, very slightly improved.

MEMBER CORRADINI: Meaning no one has the same dates and everything? Ah, clever.

MR. YADIGAROGLU: It has a couple of more words and a couple typos corrected.

CHAIRMAN CORRADINI: Okay. Okay.

MR. YADIGAROGLU: But you're not missing anything. This one is the major difference.

Like, wait a minute. What I'm showing here is --

MEMBER CORRADINI: Not the same.

MR. YADIGAROGLU: -- not the same.

CONSULTANT WALLIS: That's right.

MR. YADIGAROGLU: I don't --

CHAIRMAN BANERJEE: It doesn't seem like -- it looks like a more interesting presentation.

MR. YADIGAROGLU: No, it is not my presentation.

CONSULTANT WALLIS: It's from a different --

MEMBER CORRADINI: I think that's what it is, George. I think --

CONSULTANT WALLIS: Somebody else's presentation.

MEMBER CORRADINI: Did you change the date? Is that what you just did?

MR. YADIGAROGLU: I did. Oh, yes, you -- we loaded a very old presentation by mistake.

MR. KROTIUK: You want the new one?

MR. YADIGAROGLU: Let's do it again.

CHAIRMAN BANERJEE: Now you can have your side conversations if you want to.

Can we go off the record?

(Whereupon, at 2:59 p.m. a recess until 3:00 p.m.)

CHAIRMAN BANERJEE: We're back on the record.

MR. YADIGAROGLU: So I looked at the readability of the theory manual and its completeness.

The overall modeling approach, overall decision making approach. And then in particular the chapters on drag, interfacial heat transfer, wall heat transfer. And as I was saying earlier, the related assessment cases. And I also had a quick look at the appendix to the theory manual quasi-steady assumption.

Now some of these topics there were to two panel members to review things were already mentioned by others. I'm not going to repeat them.

And then I will concentrate on deficiencies rather than making praises about the code. Because this was done already and I think what we are trying to do here is improve the code, not necessarily say that it's perfect. So I will concentrate on negative findings. And I take the positive findings as granted.

CHAIRMAN BANERJEE: But at some point we do need something from the Committee as to addressing the level questions that were posed.

MR. YADIGAROGLU: Well, I think as far as -- I can say that immediately. As far as the top level questions that were asked is this code adequate essentially for assessment. I don't think my point of view, the other members may disagree, I don't think that it's possible within this document that that we were provided to say definitely yes or no. I think more work is needed, more information exchange in needed. And if I were to do a case of a ESBWR or the AP-600, I would certainly take the code and make sure that it's good for this case and make a number of test cases before I learned.

So I don't think I can give you a clear

yes or no.

CONSULTANT WALLIS: So in 200 hours you couldn't do it. How is an ACRS consultant going to do it in 20 hours?

MR. YADIGAROGLU: That was even, and I questioned that. And if you take out of these 200 hours three meetings at a couple of days a meeting, it's about a week or ten days work. And I spent more time than that, obviously, but it was not humanly physically to do that. It's a huge job, it's a huge code, it has many, many features. And it was too ambitious to do that.

MEMBER CORRADINI: Could I ask you then, George, from that regard is there something in the assessment manual since I didn't look at it in detail, is there something in it that talks about a progression, too? Because if what you said is perhaps it's good enough for Ps and Bs, but you'd have to look at specifically AP-1000 or ESBWR and compare against something. But against Ps and Bs is the assessment manual, are the problems, experiments and code comparisons acceptable at this point?

MR. YADIGAROGLU: Mike, the assessment manuals are huge. If you tried to look in one assessment case and if you try really to look at it in detail, it will take a week. There are maybe 50 figures and you have to analyze them. And if you know mainly what you're looking for. And one of the very first things I asked is a matrix, and I'll come to that.

MEMBER CORRADINI: All right. Okay.

MR. YADIGAROGLU: The matrix of where this phenomenon was assessed. Unfortunately, we didn't get that. But I will come to this.

MEMBER CORRADINI: Kind of like a roadmap?

CONSULTANT KRESS: That would be the PERT matrix?

MR. YADIGAROGLU: No, no. We'll come to that very soon.

MEMBER ABDEL-KHALIK: Let me go back to something you just indicated as to the impossibility of the original task that was stated in essentially the job definition. Was that because of inadequacy of time or inadequacy of the information provided?

MR. YADIGAROGLU: No. Time was one year's calendar time. So time was okay, let' say. But the limits that we had, the contract limits were very much tighter than that. It was two or three weeks per person. And within this amount of time it was impossible to do it.

MEMBER ABDEL-KHALIK: But would the information provided -- if time, the personal time was not a constraint, would the information provided allow you to provide a definitive answer to those questions?

MR. YADIGAROGLU: I regret the non-interactivity of the process. And as you'll see in the next slides I made, for example, maybe 100 comments or questions but I didn't get answers. So these are left as questions. Now some of them have very simple answers, but I don't know.

MEMBER ABDEL-KHALIK: Thank you.

MR. YADIGAROGLU: There is a question if something was properly coded. I mean, there's no way in any reasonable amount of time you can get into the code and start looking at the FORTRAN. But other people can do that because they are much more familiar with it.

CHAIRMAN BANERJEE: Is that list of questions in your peer review?

MR. YADIGAROGLU: Yes. The review report has item-by-item, page-by-page questions and comments and recommendations. And here I'm going just to pick up just a few. But there are maybe 100 of them.

So we have very useful meetings with the developers, and this is where a lot of information was exchanged. However, after that we provided comments, the first version of our reports. And there we didn't get replies. In my maybe 100 comments, I got the reply that this is a mid-priority issue and we're going to look at it in the next two years. It stopped there. If I had gotten some answers, the dialogue would have been much more beneficial. So it was kind of left hanging there.

Also in several places in the theory manual it says this is an intermediate solution. We're going to have a new model, a new development that's going to be incorporated and this is certainly the good direction, but it's not there yet. So we've been reviewing a partly intermediate version of the code, so we cannot say more about what's coming in the future.

Certainly these are highly recommended changes to be made, but it makes the review somewhat tentative. That's another reason why I cannot give you a yes or no answer is it adequate or not.

CHAIRMAN BANERJEE: So let me ask you a question. You're probably not so familiar with RELAP5, but if you looked at RELAP5 and TRACE, I mean do you see significant enough improvements to warrant its use?

MR. YADIGAROGLU: I think you have to make, again, it's a subtle answer, it's not a yes or no. RELAP5 has been around for, I don't know, 20 years, something like that. It has been assessed internationally by hundreds of people, maybe dozens of organizations. We're part of that, we did lots of work on it. My collaborators at PSI, you have had it. We found bugs, we fixed them, we did assessment cases and so on. We had a well established set of models which you can understand and so on. So it's we are comparing something that's very well established, well very worked out, bugs have been taken out, it has been tested very extensively to something that is much less tested.

And one difficulty was that I understand that some models were imported from RELAP5, but you're not exactly sure what and if they were imported as they were, if they were modified or not.

CHAIRMAN BANERJEE: But the assessment matrix for, at least within the NRC, TRACE is very large compared to, say, the amount that was done on RELAP5. At least --

MR. YADIGAROGLU: By far, there were with that very first request.

CHAIRMAN BANERJEE: Yes.

MR. YADIGAROGLU: And let's have a full assessment matrix not only for the final assessment of the code validation but even the developmental assessment so that I know that this particular correlation for friction factor was tested. This I don't know.

Now, the manual is very well written. It explains things nicely, but I think it has a flaw in the sense that it doesn't have a first chapter saying this is what we're going to do, these are the different regimes we're going to model and in this regime you'll find the details in section so on and so on and so on. It's the other way around. We have to go into the particular sections, like drag, heat transfer coefficient and so on. And then the regime is redefined in that section and says for that regime we do this and that.

Let's say you do this for drag, interfacial drag. And then you go in the chapter interfacial heat transfer and you find again the regime redefined again, and this and that, but you're not sure if it's exactly consistent.

So there's not top level definition of regimes and strategy of modeling of the code. And that makes it more difficult.

CHAIRMAN BANERJEE: But is this just a matter of actually recollecting --

MR. YADIGAROGLU: I don't think so.

CHAIRMAN BANERJEE: -- and putting it into some order in the early stage or is there actual inconsistency --

MR. YADIGAROGLU: I don't know. I don't know. Because it would take an enormous amount of time for me to and check line-by-line if it's consistent. Much easier for the developers to do it. But I cannot. It looks consistent, but I'm not sure it is. If it was a top level definition saying this is the regime, it is consistent and go there and there and there to find --

CHAIRMAN BANERJEE: But let's say stratified flaw, stratified may be flawed. You go there to find the friction factor --

MR. YADIGAROGLU: Exactly.

CHAIRMAN BANERJEE: -- you go there to find the heat transfer coefficient?

MR. YADIGAROGLU: Exactly.

So there is no top level definition of flow regimes. They are defined in the particular chapters. And it's not sure that these definitions are consistent between hydraulics and heat transfer.

I assume they are, but it's not absolutely clear. It doesn't say anywhere that they are consistent.

And then something that to me was a difficulty, there is not in one place where you can find a chart of the logic of the code. How does the code accept correlations, how it picks up regimes and in what regime what correlation and what happens what if. I mean, there is no single unique flow chart of the code and how it does it.

It is here and there there are some partial charts, but there's no single chart. I know what I'd like to see. Maybe it's difficult to produce, but should be doable: One chart saying this regime, drag is here and go to section 5.2.5.3 and you'll find the details. And if the void fraction is less than that, you go there. If it's bigger than that, you go there. So this is written in the code. All the detail is there, but it's not in one place. It makes the overview difficult. So this is a question of transparency I think is represented.

MEMBER CORRADINI: So it's probably there? It just has to be reorganized?

MR. YADIGAROGLU: It has to be worked out, yes.

MEMBER CORRADINI: Does that stand for all the pieces or is there one particular thing--

MR. YADIGAROGLU: All the pieces.

This was said earlier also by Dominic, there are certain things that are modeled in extreme detail, like Dominic said, the single phase heat transfer. You're wondering why. Why there's so much care about laminar flow, entrance length effects and so on which usually you don't worry too much about where other more difficult things are more taken care of in a much simpler way. So it's uneven kind of. The easy things are done very, very carefully. The difficult things are more difficult to do.

And then obviously we have to rely on correlations that are old and were not written for a two-fluid code. So those have to be translated, correlation extracted for them to make a heat transfer correlation for interfacial heat transfer and so on. And the process is not always very clear how that was done.

And there is also in the two-fluid model, we have typically the wall to the fluid to the interface to the liquid kind of heat transfer logic. And this logic should be there. In principle it's there. There's a diagram that shows it's there, but when you go to the implementation you're not sure that it was done like that.

MEMBER CORRADINI: Given that it's two fields, it has to be back calculated, doesn't it? You had said somewhere in your writing that you --

MR. YADIGAROGLU: The mixture?

MEMBER CORRADINI: Yes.

MR. YADIGAROGLU: It's like, you know, you take data for inverted annular flue boiling or dispersed for fuel boiling, you have to rework the data out to get interfacial areas in the pressure heat transfer coefficient and so on. How this was done is not always very clear.

Now something that I personally don't like is that in many places there is mixing; mixing in the sense that we take this component from this model and that component from that model and we put them together because this gives the best results. And I think this is dangerous because take for example the Chen correlation, okay, boiling heat transfer. Chen correlation is two kinds of boiling. And one is kind of a nucleate boiling, the other one is post-convection authorization and then Chen blended them together.

Now, I think this is consistent if you use it as a Chen correlation. But I don't think you should take only one component and combine it with something else. I'm not saying that this is done in particular, it's just an example. But this kind of a thing is done quite often; taking one component from something and mixing it with another one with the justification that it works better.

It works better could be good explanation, but then it needs assessment. It needs very detailed assessment. And this assessment is not always present. Maybe it was done, but it was not present.

So very many places you'll find explanations that this coefficient is .5 in the correlation, but we had to change it to .3 because it fits better the data. It keeps you wondering why and whether another set of data would not have made it.08. So --

CHAIRMAN BANERJEE: So are these in the list of a 100 questions that you sent?

MR. YADIGAROGLU: Yes.

CHAIRMAN BANERJEE: Okay. So they could be answered? I mean there could be rationale --

MR. YADIGAROGLU: Yes, they could be answered. As I said, the models have been mixed by picking pieces of here and there.

So, the answers to all of this is really -

- I mean, the test of the pudding is in the eating. It's about code validation. So the very first thing I asked in October was please give us a matrix showing the phenomena of the particular models that we have to have in the code and where the assessment was done. That's not a simple thing to do, and I'm sure there has been lots -- there was model assessment during writing of the code and after that. But we didn't get that. And without that, I cannot say that the friction factor was tested. I mean, I don't know where it was tested. And there's no way that I could into the assessment cases and find out the effect of the friction factor. It's impossible.

I mean, I'm person that works in fundamentals. I would like to see if the friction factor is correct. So I wouldn't look at a the LOCA case for the friction factor, I'll take pipe flow. And this was probably done, but I don't know where.

And this model development tests without an elementary level. I'm sure they were done, but there's absolutely no information on those.

CHAIRMAN BANERJEE: So the assessment matrices that you were presented had a number of, undoubtedly, separate effects tests, some integral tests and so on. And was there no rationale or associated with each test saying that this was the reason --

MR. YADIGAROGLU: No. There is a PIRT table, important phenomena, but then there is no link between this PIRT table and the assessment cases.

CHAIRMAN BANERJEE: So it may exist, but what you don't have is --

MR. YADIGAROGLU: We don't have the link. I don't know where to look to find them.

CHAIRMAN BANERJEE: Okay.

MEMBER CORRADINI: So can I ask somebody from the NRC does that exist?

MR. YADIGAROGLU: I'm sorry.

MEMBER CORRADINI: I'm going to interrupt you and ask from the staff does that exist?

MR. BAJOREK: At the start of the assessment manual we do have several tables that link the assessment tests to highly ranked phenomena in the PIRT.

MEMBER CORRADINI: In the PIRT.

MR. BAJOREK: They were linked in that regards.

We do not have tables that would link individual models and correlations to this particular test, which I think is what you're really looking for.

You want to try to break this down to where, for example, the Chen correlation was assessed. That has been done in developmental assessment, but because that was a particular correlation it's not represented in those PIRT tables which really look at model packages, the Chen plus other correlations to get a desired effect. But that's the part that's missing.

MR. YADIGAROGLU: Now in the manual there is a limited cases of assessment in the manual itself, but they are very limited with limited sets of data.

CHAIRMAN BANERJEE: There's like examples, correct?

MR. YADIGAROGLU: There are examples, yes.

CHAIRMAN BANERJEE: Yes. Because --

MR. YADIGAROGLU: Not systematic. They're examples.

CHAIRMAN BANERJEE: But there is some database where all this is linked? Or maybe Steve can answer it where you've got a comprehensive database of the assessment comparisons and things like that?

MR. BAJOREK: It's there, but the way the documents are set up a lot of what you're looking for would have shown up in this what we call the developmental assessment. And if it didn't wind up into the theory manual, it's still sitting in someone's notes somewhere.

I think what you're asking for and what needs to be done is to take those almost bench top type comparisons of the correlation to the experimental data and build that into the assessment report, probably in that section that looks at the more fundamental things. It's there, it's been done but it hasn't been documented to that level of satisfaction.

MR. YADIGAROGLU: Like when you talk, because he assured us that he has lots of Excel sheets or so, whatever, and he's done that but I don't have it. So I cannot say.

Now this is more about the readability and contents of the manual, the theory manual I mean. And as I was saying earlier, the top-level modeling strategy should have been defined someplace; what regimes you are talking about, what we want to model in those regimes.

So a chapter defining way before we start the drag with interfacial heat and mass transfer and so on, a chapter defining the flow regimes. Because these are defined later and repetitiously many times. Would have been much more efficient to define them and then say now you're going to find details in section so on and so on and so on..

CONSULTANT WALLIS: Don't you have to define more than just the flow regime? You have define the geometries --

MR. YADIGAROGLU: Yes.

CONSULTANT WALLIS: -- which you're considering. I mean, you're considering various things in the core --

MR. YADIGAROGLU: Yes.

CONSULTANT WALLIS: -- and various things in the steam generators?

MR. YADIGAROGLU: Yes.

CONSULTANT WALLIS: Various interactions between the plenums and piping and all that has to be modeled and solved?

MR. YADIGAROGLU: All this in section with flow regime and flow regime --

CONSULTANT WALLIS: Right. To what does this code apply?

MR. YADIGAROGLU: Horizontal, vertical, incline, yes.

CONSULTANT WALLIS: Right. Right.

MR. YADIGAROGLU: There is a very nice historical presentation that helps the reader that was mentioned earlier, but it also detracts because you keep reading two pages about all the history and you think that was implemented in the code. And then at the very end you find out no, that's only history. Now the code is different.

So there should be a way of either formatting it, putting it into the footnotes or a different font, or whatever, to make sure that you can read what's in the code quickly without having to read the whole history. But that was addressed already in the morning.

The manual has no numbers in the chapters, sections and so on, which makes it kind of difficult to read. You can't refer to chapter, section 3.4; it doesn't exist. You have to put the whole title of the section and then you have to chase for it.

And then we are saying a graphical presentation between flow diagram, logic of the code and that's where I find things in. So I can look in one page and say this regime I'll find my drag here, my interfacial heat transfer here and so on so I can put it easily together. And that would have eliminated a repetitions. Because there are other repetitions. Flow regimes are repeated in several chapters because they are not defined in only one place.

The final one is maybe difficult, but there is a lot of reference to the work of Ishii and co-workers about the correlation. And so if really you want to look at the original work, you have to find those documents which are not very easily accessible. So it would have been nice to have them in a place from where you can download them and read them if necessary.

As I was saying, the time was limited. If you really wanted to say my boiling heat transfer coefficient is applicable to the whole range of pressures, diameters and you wanted to do an absolutely perfect job, you have to go back and read the original paper. Make sure that this correlation is really applicable. And this is really extremely time consuming and not doable.

CONSULTANT WALLIS: Well one strategy when you're faced with this kind of task is to say we can't do it all, so I will do some spot checks. I'll pick something I really know myself --

MR. YADIGAROGLU: Yes.

CONSULTANT WALLIS: -- and I'll see how well they did on it. Did you do something like that?

MR. YADIGAROGLU: Yes, obviously. Yes. Yes. And there are some examples. Some questions where I'm saying this correlation was developed for a whole range of atmospheric pressure, now you're using it at boiling water reactor pressure. Is it still applicable?

And obviously if you want really to do severe -- a serious casual relation work, that would take decades. That would take hundreds, or many years of work for many teams to do that. So it's maybe premature for a relatively new code to reach this level of maturity. Now these were the general issues.

Now I have picked up some problems, some particular remarks, the most important ones, out of this maybe 100 or so and I'm focusing on these.

One, this first one has to do interfacial shear. And, obviously, you cannot measure interfacial shear so you use a clever way of extracting it from essentially the void fraction data. And if you take these two equations, which are steady, steady nonmass transfer equations for the liquid and for the gas, the momentum equations have the wall shear terms, the interfacial shear terms and the gravity terms and the pressure gradient here. And if you multiple the first one by the void fraction, the second one with the liquid fraction, you add them up, you eliminate the terms that you don't want to and you get this equation.

Now, in this equation you have the interfacial shear here. You have the void fraction which effects gravity, the gravity term. But you also have the wall shear to the liquid and the wall shear to the gas.

Well, in that section of the manual it simply says -- starts that section by saying, by equating the interfacial shear to the gravity, we get the interfacial shear.

CONSULTANT WALLIS: It's only true for some regimes. It's only true for some regimes.

MR. YADIGAROGLU: That's correct.

CONSULTANT WALLIS: In some regimes the wall shear is very important.

MR. YADIGAROGLU: Exactly. This is not addressed at all.

So these two terms are dropped. And, in fact, the data that's used to calculate the wall shear is vessel void fraction data where these two terms don't exist.

So I understand that these may be weak terms, may not have a great effect. But it should be justified for the assessment of the interfacial shear is based on data, bubbly flow data in vessels, not in pipes.

And this is for vertical flow. So what happens in horizontal flow when this term is zero? And I understand you use the same the correlation. But, again, it's not fully justified and documented. As I said, the validation cases are for some -- the Wilson bubbly --

CONSULTANT WALLIS: It's the Wilson --

MR. YADIGAROGLU: Yes, the old Wilson correlation.

CONSULTANT WALLIS: So why isn't interfacial shear based on interfacial velocity rather than liquid velocity and gas velocity? There's got to be some sort of an interfacial velocity --

MR. YADIGAROGLU: My next one. In fact, the interfacial shear should be based on the average of Vgas minus the liquid and not the differences of the averages. And there's a whole section in the report based on Ishii and Mishima's work where there's a correction to it. And this correction is applied.

Now we recently happened to -- thinking again about that because we got some PIV data where we could actually measure the value --

CONSULTANT WALLIS: Well, you can do exact solution for laminar to laminar flow, annular flow and the interfacial shear is the result of all these velocity profiles. It's not such a simple thing as this, is it?

MR. YADIGAROGLU: Exactly.

Now if you have a single bubble rising in a vessel, the relative velocity is well defined. Okay.

If you have a cloud of bubbles in liquid and the liquid is moving, what is the -- which averages are you talking about? And how are the data analyzed under such situations to get these averages? So I think it's a more complex question.

In the beginning I thought I had the answer because you can compute this term. You don't have -- Ishii and Mishima makes some approximations and they get the result in terms of Czero and Vgas-j; the two parameters. I think you can compute this term exactly. You don't have to make approximations. But the basic question is what's the real physics of the problem and what are the data that were used to derive those correlations, viewed in this way or viewed in a different way. How do you measure in those experiments the average liquid velocity? Because they could have been stagnant liquid with rising bubbles, it could be --

CONSULTANT WALLIS: Well the simple answer, George, is you're looking at a term in an equation because you're going to put in a code, you look at whatever you need to put in that equation to make the code reflect the data, whatever the physics are.

MR. YADIGAROGLU: But you have to be consistent.

CONSULTANT WALLIS: Yes, you have to be consistent. But you don't have to represent the physics.

MR. YADIGAROGLU: But I was saying this is an interesting issue. I don't know if it's -- maybe should be revisited.

CONSULTANT WALLIS: Yes.

MR. YADIGAROGLU: Regarding interfacial heat transfer in the two-fluid model we go from the wall to the fluids, either liquid or gas, and from the fluids to the interface and from the interface to the second fluid. This is apparently what is done, but there's no general statement in the manual this is really done. So there's some exceptions to that.

For example, in IAFB we take the heat flux from the wall and you put it directly into the liquid.

You don't go through the vapor, you don't go wall/vapor/vapor/liquid. The reason for that is a practical one. I understand that if the heat capacity of the vapor is so small that if you do this intermediate step you get instabilities and it doesn't work very well. Fine. But it should be clearly identified and explained and made consistent.

Now, there is a unique film flow model that is used for annular flow, annular flow, normal boiling flow and for condensation. And I think there are condensation that say if you take the case which is of interest, the ESBWR which vertical condensing tube, the liquid film flowing downwards. Yes, this is liquid film flow but I think it has little resemblance to annular flow of 70 bar flowing in a BWR. So having the same model I think is not a very good idea. But a new model for the ESBWR type of condensation is planned, I understand, so that should have been taken care of. But in the present version there is one model for both situations, which are extremely different ones.

MEMBER CORRADINI: Just to clarify, I guess I want to ask the staff on this one. So there was a March 2008 report on ESBWR or TRACE used ESBWR. Is that where one would look for improvements such as that or is that still on the chart to be done?

MR. KELLY: This is Joe Kelly from Research.

Actually, George's last comment shows I probably didn't write the manual quite clearly enough.

There are two models for condensation in the code, a default model and then a newer advanced model which the user has to turn on. And that's the one that has the effects of mass diffusion and for noncondensable gases in it. But that model was expressly developed for the PCCS tubes of the ESBWR design. And --

MEMBER CORRADINI: And it's in what we've got here and I missed it or it's in the March 2008 report? I guess that's part of the question.

MR. KELLY: It's in the theory manual.

MEMBER CORRADINI: Okay.

MR. KELLY: Yes. And the only difference really -- actually it was developed for co-current downflow, which is the ESBWR design. And the first thing I had to do was change the interfacial drag term. The default model in the code turned out to, I believe, over predict the interfacial drag for co-current downflow. So if it's co-current downflow, you use a different interfacial drag model. But then the assessment of the model which I guess is not in the assessment manuals you got but would be in the ESBWR applicability in March will show the cases for the co-current downflow into condensation.

MEMBER CORRADINI: Okay. Thank you.

MR. YADIGAROGLU: But early in the manual this is presented as a unique model for both cases.

Now on the contrary, there is an interfacial heat transfer for stratified flow and interfacial heat transfer for condensation. We're wondering why there are two different models. Same situation.

CHAIRMAN BANERJEE: But in one case the wall, the turbulence of the wall interferes also with the interfacial turbulence generated by shear, whereas if you a deep stratified layer it's primarily the interfacial turbulence generated by shear which effects it. So the film behavior is different from --

MR. YADIGAROGLU: I don't think the differences in the code on based on what you say.

CHAIRMAN BANERJEE: There's physics is the reason for it.

MR. YADIGAROGLU: But the differences in the code are not based on that.

CONSULTANT WALLIS: Isn't the current

model stratified counter-current flow?

CHAIRMAN BANERJEE: It's very difficult to get liquid side control condensation if it was a pure steam because at a free interface the turbulence is completely different. So there's no Reynolds analogy for it. So you wouldn't expect them to be able to get that right.

MR. YADIGAROGLU: There's a fairly long chapter about condensation, laminar versus turbulent films. There is some manipulation by adding essentially the laminar and the turbulent heat transfer coefficients.

There is the work of Kuhn and coworkers which is from Berkeley, University of California at Berkeley versus the older work of Bankoff.

And there's a lot of mixing of pieces of--

CONSULTANT WALLIS: So it's not just laminar versus turbulent? It's also the waviness --

MR. YADIGAROGLU: Yes.

CONSULTANT WALLIS: -- the waviness on the film can have a big effect, can't it?

MR. YADIGAROGLU: In my report I --

CHAIRMAN BANERJEE: Especially if there's a way to break.

MR. YADIGAROGLU: So I was wondering

whether this whole chapter should not be really revisited and kind of cleaned up. Because I don't think it is that you have to start with Kuhn and go to Bankoff. You have to pick one or the other, but not really mix the two.

Critical heat flux has an introductory section showing all the difficulties about critical heat flux correlations and so on. And I think the most important one that here we have CHF under transient conditions. This is never mentioned.

And then I couldn't really understand the logic of how the CHF condition is treated. Because they talk about a temperature difference. To quote "compute a T CHF." A delta-t CHF instead of a critical heat flux. So I couldn't figure out whether this logic is correct or not.

And in one place it says that it is only delta-t CHF but then in another place it says there's always some critical quality. So, again, there is the lack of top-level strategy in --

CONSULTANT WALLIS: No. The critical heat flux it's just a matter of using a correlation, isn't it? There's no model for it.

MR. YADIGAROGLU: Yes, a correlation. But from that correlation --

CONSULTANT WALLIS: Don't you just use whatever is in the correlation?

MR. YADIGAROGLU: Yes, you're absolutely right. But you extract from this correlation a delta-T CHF and then you base your logic after that on a delta-T CHF rather than critical heat flux. So I don't understand.

CHAIRMAN BANERJEE: Maybe somebody explain that. Because most of these correlations are based on either some sort of critical quality or critical heat flux.

MR. YADIGAROGLU:

CHAIRMAN BANERJEE: So is there a reason to do this or is this correct, what he's saying?

MR. KELLY: Okay. Well, the CHF model in TRACE is the AECL lookup table. And it does give you a value of the critical flux. And what's done in the code is basically you use the nuclear boiling correlation to see what would be the wall temperature corresponding to that heat flux. And if the wall temperature is above that, you say you're in post-CHF. And that's pretty much the way all of the transient system analyses codes like RELAP5 and TRACE are written. And it just makes it a little bit easier to do your boiling map.

Now TRACE because it has some background in TRACK, there were other CHF models put in for boiling water reactors that worked on critical quality. And they were put in as options. And so if the user selects that option, then there is a secondary if test that looks to see if you've exceeded the critical quality corresponding to that particular correlation.

MEMBER CORRADINI: But in the lookup tables what's the independent variable you're using then? Not quality, Joe?

MR. KELLY: Pressure, mass flux and quality. And then you have to --

MEMBER CORRADINI: Local conditions?

MR. KELLY: Yes.

MEMBER CORRADINI: Okay.

MR. KELLY: It's a local condition. There's no history in it.

MEMBER CORRADINI: Okay. That's no problem. I got it now.

MR. YADIGAROGLU: But, you know, I have some difficulty in basing all the subsequent criteria on CHF on a delta-T CHF. Because it should be the critical heat flux that controls, not the delta-t.

CONSULTANT WALLIS: So it's based on

pressure, mass flux and quality? So even if there's no heating of the wall, you can get critical heat flux? It doesn't make sense.

MR. KELLY: The lookup tables to tell you what the value of the critical heat flux is are functions of mass flux, pressure and quality.

CONSULTANT WALLIS: But then you end up with a heat flux? Okay. Thank you.

MR. KELLY: And then we pick a wall temperature that corresponds to that. It's because it's a very simplified model of a boiling curve. And I wanted two linchpoints. A CHF point and a minimum film boiling point because if you go through the literature, there's not very good correlations for transition boiling heat transfer. They're all over the place. And I wanted something that would be consistent going from nuclear boiling to film boiling. So I used those two linchpoints and simply do an extrapolation really between those two points.

MEMBER CORRADINI: So it's directional? If you're going up -- if you're coming from nucleate to somewhere, you use that. If you're coming down, you use a different for minimum film? If you're coming down, as you're cooling down?

CHAIRMAN BANERJEE: If there's a

hysteresis effect?

MEMBER CORRADINI: No. But the way he just described it he skips the hysteresis. If he's coming up, you go that way and use this lookup table. If you're coming down, you look at the minimum form boiling point, or in a similar correlation, and cross over that way again.

MR. KELLY: Well if you're in excess of T_{min} --

MEMBER CORRADINI: Right.

MR. KELLY: Okay. You're in post -- you know, to boiling. But if you're less than T_{min}, say if you're between T_{min} and T-CHF, then you use the same curve. So there's no hysteresis built into to the transition boiling.

MEMBER CORRADINI: Okay.

MR. YADIGAROGLU: Now all these --

CHAIRMAN BANERJEE: It's almost like a temperature control system rather than a heat flux control system. Follow the boiling curve?

MR. KELLY: Yes.

MR. YADIGAROGLU: Now all the CHF correlations were developed for co-current flow, right? There's no counter-current flow CHF correlations. But in the code in the reactor there are counter-current flow situations with possibility of CHF.

MR. KELLY: That's exactly correct. And more critical than that for low flow rate CHF is very poorly defined. And one of the problems with using quality in the code is what is it. Because you're always talking about flow quality. But if you go to a pool boiling type situation, you got a lot of water around but the quality is basically one because the only flow is the steam flow. So when you go to low flow rate conditions using quality gets very chancy. And so I corresponded with Dr. Greunewald a few times and he basically said "Ah, low flow rate. Who knows?"

And so kind of what you end up doing is using what's called a static quality which is based on a void fraction, which at least doesn't bounce between zero to one depending upon whether or not the liquid level is going up or down a little bit. So you'll see in a few places in the code -- I mean, excuse me in the manuals where I say, you know, for low flow rate conditions we use this other definition of the quality just to eliminate oscillations that we've seen in the past.

MR. YADIGAROGLU: I figured a very good way and a very physically sound way if you converted the quality which is nonexistent in counter-current flow into a void fraction which is always existent and based on that, which is a good idea. But to what extent these correlations apply is a different question.

MR. KELLY: Yes.

CHAIRMAN BANERJEE: You have to come to a mike and you have to identify yourself, Peter.

MR. GRIFFITH: Griffith, Peter Griffith.

I want to mention that we took -- we did experiments with reverse flow going backwards, you know, and down again and so forth. And the correlation that we came up used void fraction in the region when the mass velocity was not too big and the void fraction was a much better descriptor of the local conditions than the quality or mass velocity. You just switched to a void fraction.

And that's apparently what Kelly is saying; that that scheme is put in. And it works. Not bad. It's fine and it works.

MR. KELLY: Yes. This is Joe Kelly again.

And that's exactly the approach that Professor Gruenewald took for the low mass flux regions of the table was taking a void fraction and converting it to a quality. But that's exactly how that was done. And then taking basically one minus alpha times the pool boiling value.

CONSULTANT WALLIS: And that works?

MR. GRIFFITH: Yes.

MR. KELLY: Yes, surprisingly well.

CHAIRMAN BANERJEE: George, let's move on. We're getting tight on time.

MR. YADIGAROGLU: Yes. I have only three slides left.

Post-CHF heat transfer, I mentioned it earlier. The wall to steam, steam to liquid heat transfer in the inverted annular film boiling regime is at least is ignored. And there is no super heating of the steam in this case. To what extent it's important, I don't know.

MR. KELLY: Yes. This is a case where if I had a chance to get back to your question, the amount of heat for a laminar film is taken out. So you do go wall to gas to interface -- excuse me, for the pure conduction part of it. By number of two. That much does go to the steam. So you end up with the steam temperature approximately half way between the wall temperature and the interface. But any amount of heat in excess of that I go ahead and put directly to the interface to cause vapor generation exactly for the reason you said, because the vapor films are so thin you can have numerical stability problems.

MR. YADIGAROGLU: This was what we discussed in the last meeting or was it another communication?

MR. KELLY: When I talked about the model initially. But, you know, there was so much information in those meetings.

MR. YADIGAROGLU: I could not -- there is a very simple rudimentary logic that I got about post-CHF heat transfer which I couldn't follow. Simply couldn't follow it.

And then in one place T_{CHF}, this delta-t CHF we're talking about is based as this CHF criteria, but later there is a critical quality introduced. So which one is active? When it's not clear.

Rewetting and reflooding, what Joe Kelly was saying, it's based on teaming T_{CHF} basis, which makes sense. But the -- I'm not sure that the progression of the quench front as the reworking mechanism is taken care of. It's not clear.

And then the whole package on post-CHF heat transfer is really new, it's simple. Maybe it's good. I don't know. It is based on some validation, which doesn't give good results. And it's based on some things that are really not validated. And there's been, you know, in the last 20/30 years an enormous amount of work on this. I was wondering why it was chosen to start fresh and to make something simple. But maybe Joe has good reasons for that, but they're not obvious.

So these new models don't seem to be very successful. They've only been very well tested. Why the previous work was ignored.

And in DFFB there is an enhancement of convective heat transfer which is absolutely necessary, but it's based on turbulence. And I'm not sure this is a question of turbulence. For me it's more a question of presence of droplets. The droplets are there. They do heat the steam. It's not a question of turbulence in the steam. It's a question of you need to put liquid droplets when the steam is hot to de-super heat it.

So about this Appendix A, I think we discussed it at our last meeting, it gives all kinds of reasons for the quasi-steady-state assumption but I don't think it's relevant, and I would suggest to eliminate it.

So in summary, I would say there is a lot of good work that's been done. The testing correlations, so unfortunately it's not always visible. It was not linked clearly to assessment cases. So my personal assessment it's inconclusive in the sense that I see lots of changes, lots of things where I cannot definitely say this is better than before or less good than before.

CHAIRMAN BANERJEE: Now all this CAMP work on RELAP5 is there any sort of centralized documentation of the assessments that have been done by these many users or is it sort of scattered all over the map, or what is the situation?

MR. STOUDEMEIER: This is Joe Stoudemeier, NRC.

CAMP assessment reports are documented in NUREG IAs. We have sent over a few 100 -- I think it's over 200 now. So they're not combined into one manual.

I think there have been some manuals which are a review of them periodically. But there isn't anything that's been done in the last five years or so doing that. But we have a list of all the CAMP assessments.

MEMBER CORRADINI: Can you say it again? I didn't completely follow.

MR. YADIGAROGLU: There are lots of

reports. But I'm not sure there's a synthesis report of all this work; that's essentially what he said.

MEMBER CORRADINI: So let me turn back to you, George. So are we holding TRACE up to a standard that RELAP doesn't even follow? That is, you're asking TRACE to do this. Is there any logic that I could look to for RELAP to actually unravel to make sure they've even done the assessment like this phenomena, this is the test, this is the check?

I mean, it sounds perfectly logical, but I'm curious if I want to look for this for RELAP, does it exist?

MR. STOUDEMEIER: Yes. Well, I think you would find out if you compared assessment-to-assessment between TRACE and RELAP5, even counting in CAMP assessments that TRACE stands up very well to the RELAP5 assessment.

MEMBER CORRADINI: Okay.

MR. STOUDEMEIER: And does as well or better than RELAP5 in almost every case that there has been assessment for.

MR. BAJOREK: When there are new versions of RELAP put out that do make corrections, at least for the NRC version of RELAP, they run it through an assessment matrix of 43 individual calculations;

integral tests. separate effects tests, a couple of fundamental problems. And we look at our version as a rigidly frozen code that we run through 550. And it's sort of our --

CHAIRMAN BANERJEE: TRACE, 550?

MR. BAJOREK: TRACE uses about 550 for the work that was in this generic assessment matrix. That's apart from the additional cases that we do for ESBWR, EPR, AP-1000. The frozen version of RELAP when it comes out is assessed against 43 cases.

MEMBER CORRADINI: Okay. Thank you.

CONSULTANT WALLIS: Is part of what George finds difficult due to an insufficient separation between code development and code validation? Because if you have the same person doing both, you get into difficulties. The person who developed it knows why he did it, but it needs to be explained to somebody else. And that validation is not quite the same task as development.

Development, the developer tends to like what he's developed. You need somebody else really to validate it.

You managed to do that or do you not have enough people to split the task?

MR. STOUDEMEIER: As I say, in the past

there have been the concept of independent assessment or an outside organization takes the code and assesses it. I don't think that's really been done in a long time for an NRC code.

I mean, it's not just the development team that's performing the assessment. It's also -- well, it was performed by a large group of people; contractors that aren't really code developers, they're analysts for different things that the NRC contracts out for. It's staff in house that aren't code developers but are performing thermal hydraulic analysis. And I'd say, yes, the majority of the assessments were actually people not part of the code development team that performed them.

MR. BAJOREK: I think there's good communication between the people who are doing the assessments and the code developers themselves. Because I think we found that you can't treat the code as a black box. You have to have an understanding on what it's attempting to do models and correlation wise in setting up those models. So there's, I think, good communication between the various groups.

But as Joe mentioned, because of just the large number of assessment cases it has been split up between some of the people doing the development and other people within the agency, and in some cases contractors outside of the agency.

CHAIRMAN BANERJEE: Okay. I think we'll move on to Pete. Thanks, George, very much. And we are only running half an hour behind.

Pete, I guess you're on next.

MR. GRIFFITH: What I'd like to do is first tell you what I set out to do. I wanted to review the component models and check the constitutive models. I hardly got to the constitutive models as a result of time shortage.

The review of the component models was restricted to the components which were separated in the models report.

I'd like to start by recalling a little bit what happened on the CSAU project. That's a code, the scalability, accuracy and uncertainty. Then make some comments on specific models and then finally go to the conclusions.

One of the consequences of the CSAU study was we found that the most significant uncertainty was the post-CHF heat transfer correlation. And that was by far the biggest.

And what we did is look at the uncertainty for each of the terms which was in the code, then look at the range that we had for that and then do a large number, several hundred, random calculations checking the uncertainties and pulling them altogether into one code, one evaluation.

We found subsequently that the details in the heat transfer package had very little effect on the answer. That is the heat transfer correlations and friction factors didn't have much effect. In retrospect, it appeared that the important part of the code that gave large errors that was concerned with the fluid mechanics, whether fluid got in there or didn't get in there and whether it was going down or up, that sort of thing, major things the heat transfer correlations didn't have much effect. Because in most cases the heat transfer, the heat transfer coefficient is one of several resistances that are in series. And very often it's not the biggest. So that was a finding from that.

I worked with another person on the CSAU for several months on the pump model. And we found first that the pump model wasn't very good, and mostly in that time it was 70 scale pump model. But the second thing that we found that it had almost no effect on the outcome. That is, the pump model had almost no effect on the peak clad temperature. And that was the primary measure of what we were concerned about.

We also found that the worst results were most often due to the code user errors. We thought we were doing this and we were doing that, or they thought they were doing this and they were doing that. And as this is the case, and I think it will be the case in the general, it's very important that the text that goes along with the manual is clear to the point and hard to avoid.

One of the things that I was concerned about with calculating the peak clad temperature, and this is just a brief outline of how I expect the peak clad temperature would be calculated using what's in the TRACE manual.

The subroutine that has the details is the heat structure PWR core design. That's HSTR-PWR core design.

If you look at the nodalization diagram that's in the manual, you find that the nodes are about three tenths of a meter on a side. That's about a foot on a side. So they're big nodes. And the big nodes mean that there are a number of fuel rods in the model. That is, you have something this big, then you probably go, I don't know, 40 or 50, maybe more than that fuel rods in it.

TRACE is used for the fluid mechanics. And I'm not sure how it's used. Is there one velocity for each node? I'm not sure. But in any case, the fluid mechanics doesn't have the detail which is reflected in a number of individual rods.

The heat transfer is calculated for forced convection, CHF and post-CHF using various, I would say, sources. CHF is a lookup, as we've described. And the post-CHF is also a lookup table, but there are a number of other alternatives which are given. When you use, which you use, when you use it is not clear at all.

There is an implication, though no straight statement of it, that radiation should be put in and the RADENC subroutine is used for that. And that's a routine which is part of the list. The peak clad temperature then is calculated from that kind of information.

My concern on this one concerns what somebody would do with the information which was in the manual.

The post-CHF lookup table is constructed, it's the Gruenewald table and I think it's by far the best source of post-CHF heat transfer. It's not even an issue with me. But the information that is given is -- that is given in its function only fluid condition. And if you look at post-CHF heat transfer data, there are surface effects which are very important. So that's out of it.

The surface effects, fortunately, lead to conservatism as far as the predictions are concerned. But they can be pretty big. They can really be pretty big. I think that's a place that more work is needed on.

MR. STOUDEMEIER: I'd like to interrupt for a little bit.

For post-CHF heat transfer we don't use lookup tables. There's correlations depending on flow conditions in the node. The Gruenewald tables are used to look at the point of CHF of transition from pre-CHF to post-CHF. But we don't use lookup tables for heat transfer coefficients.

MR. GRIFFITH: We don't -- no, no, not for heat transfer. It's the -- I'm not sure I understand exactly how you use it. What do you use for the post-CHF heat transfer? Now you've gone beyond CHF. How do you --

MR. STOUDEMEIER: All right. Then you look at fluid conditions, surface temperature conditions, have correlations that calculate heat transfer --

MR. GRIFFITH: Well, this is an example where I need better text.

MR. STOUDEMEIER: Yes.

MR. KELLY: Maybe we should define what we mean by post-CHF. If post CHF is transition boiling, then your comment is a 100 percent correct. Because the CHF point comes from the Gruenewald lookup table and that fixes the heat flux for the transition boiling region. But once you go past T-min or into film boiling, albeit inverted annular or just first flow depending upon the flow conditions, those come from correlations and so those are different.

But you're also correct in that there is no surface effect in the code. The T-min correlation actually is Gruenewald Stewart, I believe if memory serves. And that gives you T-min as a function of pressure. But that is only for Inconel, which has almost no oxidation.

CONSULTANT WALLIS: Aren't there surface effects in film boiling?

MR. KELLY: Well --

MR. GRIFFITH: It's not film boiling, Graham. Surface effects is if it quenches --

MR. STOUDEMEIER: It's transition boiling.

MR. KELLY: Exactly. And so because at the time we weren't able to spend the time to come up with something that was better for things like Zurk oxide, we went ahead and took the conservative approach. And I'm pretty sure I noted that.

MR. GRIFFITH: Okay.

MR. KELLY: But you're right, that's something that we should look at in the future because it is a built-in conservatism in the code.

MR. GRIFFITH: All right. And one of my concerns was that radiation may be put in twice. And assuming that we're working with the Gruenewald correlation to get the heat transfer, if you will, the RADENC subroutine can be used to calculate the radiation heat transfer. But if you think about the application, I think that's a dubious process.

You have a control volume that's that big. You've got lots of rods in it. The hot rod is almost sure to be surrounded by the hottest rod, almost the hottest rods. So the radiant heat transfer to the surrounding rods is going to be negligible. It's just, it's going to be almost no heat transfer.

It's not clear from the instructions in the manual that a user wouldn't do that; that you can't take the hot rod and assume its surrounded by particularly cold rods or that they have a very good view of the cooler regions. And I think that has to be put in the manual one way or another.

The data which is behind the Gruenewald correlations is taken in tubes from 2 millimeters up to about 24 or something millimeters in diameter. And it includes some radiant heat transfer to the colder fluid which is passing down the tube. But it doesn't, you might say, allow heat transfer to a cooler region outside. But I think that's appropriate as far as the hot rod is concerned.

So I think that that particular part of the manual needs to have some very explicit instructions as to how the recommendations then are to be used.

There are also a lot of alternatives for different conditions and geometries and stuff like that which are there with no -- for heat transfer coefficients with no instructions as to how they should be used. If I got confused, other people are getting confused, too.

The report wasn't that easy to read. And it's going to be used by people who probably know more than I do about a lot of it. But some of them they don't.

And the out-of-date models I think are confusing. I think they should all be taken out of the list of component models and put in an appendix for people who are using old versions of the code or something like that.

The component models and the modeling guidelines should be, I think, consolidated. There's no mention in the component models of the modeling guidelines, which are a 100 pages further down the pike. All right. And so you wouldn't know they existed if you opened up the manual and decided I'm going to use this as a handbook to calculate things. All right.

And then the -- this has been mentioned in several ways. But one of the things that made the CSAU process easy to use was they had a figure of merit, the peak temperature and they had a scatterplot given in several ways with a peak clad temperature calculated and measured. And you could look at it and say "Ah, I'm a long way from the limit," or "I've got a problem." All right? And I think that's needed.

It's kind of a stupid way to summarize a tremendous amount of work. But a reader wants to know whether he's getting close to the limit or not, and it'll give him some guidance on that.

The separator text, which I've mentioned here, one of the components which is mentioned is the separator. And the text is almost irrelevant. All right? I have no problem with the answer. The correlations apparently work quite well. They're based on data. But the physics which a separator has is pretty much missed in the text that goes with it. And that, I think, has to be completely rewritten. I'm not saying you should change the answer, but you ought to change the text.

The problem basically is a separator doesn't fail by not separating. It separates as you go up in velocity or fluid low rate or something like that, it continues to separate the liquid but it re-entrains it later on. And you entrain so much that the drains which go from the first stage of the separation down into the steam generator or something like that, those drains flood and the liquid just can't go down with the limited head that you have. And it's the drains rather than the separator which caused the separator to fail.

Something else that is a horse I've been riding for a long time, it doesn't really fit here but I've got a rapped audience. All right?

One of the things we did, a great waste of time, many years ago, a tremendous waste of time was plotting something like a temperature versus time or an inventory versus time or something like that. Time's not a very good variable. You can change time by a factor of two, but the thing that really counts is the inventory which is in the system. And if you plotted versus inventory rather than versus time, and you calculate inventory for the whole system and you could do that -- I mean, this is almost trivial. You have to calculate for the system experiments that you've got. You have to calculate what the inventory is. And the calculation could also -- whatever you did now, you can do again, all right, and get that.

And if you plot this way, I think you'll see that the inventory is really the only important variable shortly after you get into the loss of coolant accident. And the infinite amount of time that we spent trying to improve the blowdown models from data which was taken on systems which weren't very well defined in terms of the break size and what happened, that particular wasted time would be eliminated.

One of the things, too, that struck me was the misplaced precision. The implication was that break flow was calculated with the code, and apparently that's not the case. But if even if it isn't, you have to do something about the break flow in the data that you're correlating. And I think break flows ought to be legislated by the NRC. Don't try to model some damn hole in an apparatus, all right. Legislate the flow and build your apparatus so that you've got a predictable break in it.

And the break that I found is predictable is a two 20 l over d long, make the diameter what you need to get the flow that you want, round the entrance to the tube and you get the stuff out the other end in a predictable way.

The details of nucleation and the details of upstream history are washed out by the 20 l over d. And it's a good way to run things.

CONSULTANT WALLIS: I thought your argument in your write-up was that you look at spectrum of areas. You look at a spectrum of areas. If you look at a spectrum of areas, then it doesn't really matter how you do the break flow --

MR. GRIFFITH: That's right.

CONSULTANT WALLIS: -- because it's break flow times area that matters.

MR. GRIFFITH: That's right.

CONSULTANT WALLIS: And if you're off a bit in break flow, well you get another area, you get the same answer.

MR. GRIFFITH: That's right.

CONSULTANT WALLIS: But the only thing there is if you're worried about the double-ended guillotine break and if that's the worst case, you may want to know what's the worst break flow if that's a believable scenario if that's the worst case. That's the only case I think where you might want to worry about getting the worst break flow right. Otherwise, it's all part of what you assume about areas and breaks.

MR. GRIFFITH: That's right.

CONSULTANT WALLIS: It's all a spectrum and you just cover it anyway.

MR. GRIFFITH: I agree with that. Okay.

So what I'd like to sort of end up with is for this particular section on components, I'd like to see a sort of standard presentation. And it would start with a schematic that would be pretty representative that would slow all the flows in and out. And that would be minor flows, too. Because I think the minor flows in small break LOCAs are important. Okay.

This should be followed by a nodalization diagram. And somebody would have to have a reason to change that, all right? And then I think you want to identify which constitutive relations you use for each application. Use this for wall -- for friction, use that for slip or something like that. But be more specific so that the user doesn't make a stupid mistake because he doesn't know enough about it.

CONSULTANT WALLIS: Peter, and to permanent models you said something in your write-up about TEEs, I think, that when you have a flow splitting device?

MR. GRIFFITH: Yes.

CONSULTANT WALLIS: You really have to base the results on some sort of experiment because it's hard to predict --

MR. GRIFFITH: I absolutely agree.

CONSULTANT WALLIS: So there has to be some empiricism. You can't just draw little control valve with some nodes and say we can predict the flow split.

MR. GRIFFITH: I don't think I put it in my slides, but I think--

CONSULTANT WALLIS: No, you didn't, but I think it's in your text.

MR. GRIFFITH: Yes. What I think is needed in that list of component models is a flow split component which I think you'd have a default which would be basically to say what goes to each of the branches is what comes in is that's not very good. But there is a tremendous amount of data available in the *Handbook of Fluid Mechanics, Volume 3* on flow splits in various geometries. And you could make as long a list as you want on the flow splits of the recommendations based on that data.

It's a very complicated problem. Very, very complicated problem. And as I think about it, I'm not sure how important it is. Momentarily it can be very important. Maybe you aren't getting any flow where you think you are. But ultimately the amount of flow that goes out the break is almost independent of the details of how the flow splits in the various places where it might occur during LOCA. And I think the inventory is the key variable. And if you get that right, which means getting the break flow right, I think you've captured the major physics of the problem.

CHAIRMAN BANERJEE: Well, Pete, I remember that in the AP-600 scaling thing we tried to focus on inventory in the core rather than, you know, the overall inventory. Because it does matter what's in the core.

MR. GRIFFITH: Oh, yes.

CHAIRMAN BANERJEE: Rather than where else it is.

CONSULTANT WALLIS: The pressurizer or somewhere else.

CHAIRMAN BANERJEE: Yes. If it's sitting in the pressurizer or sitting in the steam generator it's not doing you any good. So that really seems--

MR. GRIFFITH: But that's only true for the large break, and the very largest break.

CHAIRMAN BANERJEE: No. It even happened in AP-600 because a lot of stuff got held up in the pressurizers. You know, they would periodically dump, if you remember.

MR. GRIFFITH: Yes.

CHAIRMAN BANERJEE: They would go back up and dump. And there was this oscillatory behavior, you know, strange stuff. And the CMTs and all sorts of places.

MR. GRIFFITH: I know. It was a mess.

CHAIRMAN BANERJEE: Yes.

MR. GRIFFITH: It was a real mess.

CHAIRMAN BANERJEE: So, I mean, we are

going to have to do these things for new reactors, too.

MR. GRIFFITH: That's what I'm thinking.

CHAIRMAN BANERJEE: And, you know, if it's held up in the steam generator in a reflux condensation mode, it's not doing you any good in the steam generator.

MR. GRIFFITH: Okay.

CHAIRMAN BANERJEE: You know, I mean the core -- I mean, if you said that you should plot against you calculated inventory in the core, I would 100 percent agree with you. That's going to be important.

MR. GRIFFITH: Well, obviously, that would be ideal.

CHAIRMAN BANERJEE: Yes. But they're doing a calculation anyway.

MR. GRIFFITH: But we don't know that. We really don't know that. We can calculate it, but it's --

CHAIRMAN BANERJEE: But they're calculating it anyway.

MR. GRIFFITH: Yes, I know.

CHAIRMAN BANERJEE: It's a way to collapse the data basically.

MR. GRIFFITH: Well, that's what I wanted to do.

CHAIRMAN BANERJEE: I had a question, though. If you remember the CSAU stuff, we were always concerned about nodalization and we sort of froze this at some point based on experiments. I don't recall the history, it was so long ago. Because nodalization probably mattered as much as most other things on the PCT. So it was sort of adjusted to fit certain things and then almost frozen.

MR. GRIFFITH: Well, basically that's one reason I'm suggesting we freeze it. Okay. Freeze the nodalization. And you change it for cause and justify your change.

The comparisons with data would be done with what might be called the frozen nodalization. I think it's a better way to go.

CHAIRMAN BANERJEE: But how do you freeze it for -- you know, you've got different integral tests, different reactors, different -- you know, it's no longer just the PWR, you know it's the same sort of thing.

MEMBER CORRADINI: But as needed. Isn't what Pete really is saying for guidance you would have a normalized standardized set of nodalization--

MR. GRIFFITH: Exactly.

MEMBER CORRADINI: -- and that's what you would give the user. And if the user wants to deviate from that, good luck, God speed.

CHAIRMAN BANERJEE: For every different concept.

MEMBER CORRADINI: Yes. But I mean, if I take the extreme, I mean map now the industry for severe accidents there's a standard map PWR deck and standard map BWR deck. And anybody can do whatever they want, but to start off at least there's been some, I want to call it fudging but that's not the right word, tuning to some set of data that essentially starts off with a normalization. That's what I kind of heard you -- that's what I interpreted to what I heard.

MR. GRIFFITH: That's right.

CHAIRMAN BANERJEE: Nodalization is the greatest variable. You freeze it, you've taken out a lot.

MEMBER ABDEL-KHALIK: But with something like power uprates where you try to flatten the radial power distribution in the core, then you can get whatever results you want by varying the number of assemblies that you would include in the hot channel.

CHAIRMAN BANERJEE: Well, certainly you can.

MEMBER ABDEL-KHALIK: So trying to affix the nodalization may not really be appropriate.

MR. GRIFFITH: Well, let's say it depends what you're varying. Okay. If you want to see what the effect of this on that is, all right, you may have to renodalize the thing that you're most concerned with, but leave the rest the same.

MEMBER ABDEL-KHALIK: But the implication, at least that was my interpretation of what you said, was you're going to use a given nodalization during this verification process. And you're going to ask the user to use the same nodalization?

MR. GRIFFITH: That's right.

MEMBER ABDEL-KHALIK: And that may not be appropriate for all core designs.

MR. GRIFFITH: Okay.

CHAIRMAN BANERJEE: I think you're right, but it leads to such a lot of variation in -- you know, so much uncertainty that when we did these PWRs and did the comparisons with various things like LOFT or semi-scale, or whatever, it was all sort of frozen in a certain way. Because if you change them, your results change enormously.

CONSULTANT WALLIS: But isn't that then putting you subject to criticism from the CFD type people who always say you got to look at sensitivity to nodalization? If you freeze nodalization, you're artificially restricting your calculation. And you really ought to say well if I double or half the nodes, does it make a big different.

CHAIRMAN BANERJEE: Well, I think you're right, but let's say if you took the chimney off the ESBWR and you nodalized it finely.

MEMBER CORRADINI: You always pick on that. Take the 8100 for a while.

CHAIRMAN BANERJEE: Because I like chimneys. The results should not change too much. And in fact, you know, that's what they find that the results are not terribly sensitive to it. But if you take the rest of the circular and you suddenly nodalize some part very finely or some other, you start to get all sorts of weird results.

MEMBER CORRADINI: But I guess what Said said I think is a fair criticism, but you're changing the figure of merit. In his case Pete's talking about PCT. If I want to a power uprate in a BWR, your figure of merit might be an instability parameter and I wouldn't necessarily nodalize it the same way to look at that as I would for PCT.

MEMBER ABDEL-KHALIK: I would get a different PCT value depending on how I would nodalize this thing regularly. I mean depending on how many channels you include within what you consider to be the hot channel versus the arid channel in the core.

MEMBER CORRADINI: I guess I haven't seen enough of these for BWRs. But in Ps I would expect I'd get very much of a difference.

CHAIRMAN BANERJEE: Well BWRs, PCT is not a problem, so who cares. I mean we are worried about other stuff there.

CONSULTANT WALLIS: Well, you might get a different -- oxidation because if everything is close to PCT, you may get the same PCT. But if you've got a really flat distribution of everything, you might make a lot of hydrogen.

CHAIRMAN BANERJEE: I think Said's point is important.

CONSULTANT KRESS: That's not his concern.

CONSULTANT WALLIS: Not his concern.

CONSULTANT KRESS: Not hydrogen. The concern is the brittleness of the clad.

CONSULTANT WALLIS: Well, okay. Well,

that's the same.

CHAIRMAN BANERJEE: But we're driving with more --

CONSULTANT KRESS: It's got more clad, it's the same brittle level.

CHAIRMAN BANERJEE: -- much closer to -- you know, much more fuel close to the critical heat flux limits.

CONSULTANT WALLIS: Right. Right.

CHAIRMAN BANERJEE: I think that's really the concern. And then if you do an anticipated transient with it, with a code like this, then how much is nodalization effected? You know, how far away are we from or what is the effect of the minimum critical heat flux.

CONSULTANT KRESS: Well, it seems to me that there needs to be a process for selecting nodalization that would be based on more technical issues than just it looks like the one we did before and the one we did this test, it looks like the node in this test. There needs to be some criteria for selecting the nodalization. And that's really the missing part in all these codes to me.

Now I don't know what that criteria is right now. But that to me is the biggest flaw in the whole business is how you select the nodes.

MR. BAJOREK: It's one of the things that -- well, we're going to pick up on this one when I talk after the peer review. But it's a point well taken. And one of the things that we are taking upon ourselves is to write very clear specific user guidelines on how they should model things component-by-component.

For example, even the core. And I think you can't get everything. You know, the radial nodalization is a good point. But if we look at most cores, they're 12 feet, maybe 14 feet, they have seven or eight grids basically a lot of places where you have loss coefficients or things that perturb the flow.

When we did our assessment we were very careful to model things like FLECHT, CCTF, SCTF with the same nodalization strategy in that you have two axial levels between each grid, which gave almost every core or the test that we were modeling 14 axial cells.

We haven't written it down well, but we did also do nodalization sensitivities doubling the nodes, cutting them in half, to show that it's impact on the peak cladding temperature was fairly small. I forget exactly what it is.

This gave us a guideline for people who are setting up plant models that as long as you model the core like this with this number of axial levels, your nodalization independent and you also can point to your model back to all the assessment that was done and say this is how the model should behave when you run your full scale plant.

I think in many cases we --

CHAIRMAN BANERJEE: The point, however, is that there has to be some sort of a guide. You know, that's really what I think everybody is saying.

MR. GRIFFITH: Yes.

CONSULTANT KRESS: You once talked about the scaling process where you looked at the nondimensional variables. And you talked about a proper range to put those in to make sure, say, a small scale test properly models. Is there a way to use that concept to decide on this nodalization?

MR. BAJOREK: That as meant more for scaling a facility back.

CONSULTANT KRESS: I know it was scaling between tests and thing. But I would assume there might be some merit for that for just choosing nodes. Noding is a scaling issue to me.

MR. BAJOREK: Right. It may well work out in the loop piping where you may want to look at, say, a number and see what range you would be varying it with your nodalization to cut down on those sensitivities.

CONSULTANT KRESS: Yes. Yes. Anyway, to me that's one of the biggest issues.

CHAIRMAN BANERJEE: All right. Go on, Pete.

MR. GRIFFITH: Not much more.

CHAIRMAN BANERJEE: You can see how kind we are to you, which normally ACRS wouldn't let its own members get away with. Because in the last slide "constituative" was -- it's been spelled the same way twice.

MR. GRIFFITH: Okay. Well, these are the components I think you want. And I'm adding a flow splitting module, you can see there. And some of these are sort of standard. I mean the feedwater heaters don't vary that much from plant-to-plant. And I think you can simplify life for the working class and get answers that are more easily interpreted.

So that's my suggestion. Thank you.

CHAIRMAN BANERJEE: Okay. Thank you, Pete, very enlightening again, like all the peer reviews.

Before we go on to hearing from the staff, we may as well take a short break.

CONSULTANT WALLIS: On time pretty well.

CHAIRMAN BANERJEE: Yes, almost on time. Okay. We'll give you 15 minutes, not do a Corradini on you.

Let's make it 4:45.

(Whereupon, at 4:31 p.m. a recess until 4:44 p.m.

CHAIRMAN BANERJEE: We're going to go on the record now.

Just give us a moment, Steve, while we get settled down.

Well, you've got a lot of slides here. Are you sure you can get through all of them.

MR. BAJOREK: I will speed up no matter how long it takes me.

CHAIRMAN BANERJEE: Okay. You will speed up.

I think we can start without Mike -- simply playing hooky again. So let's go for it.

MR. BAJOREK: Okay. What I'm going to do next is go through and kind of summarize what the staff has learned from the peer review.

We've taken the initial reports, we've gone through the presentations and what we want to try to do is describe what's going to be our plan of attack here to deal with all of these.

CONSULTANT WALLIS: I don't think your email address is correct. It's the old fashioned LAN--

MR. BAJOREK: Oh, that still works.

CONSULTANT WALLIS: It still works?

MR. BAJOREK: That still works.

CONSULTANT WALLIS: It still works? Okay.

MR. KROTIUK: Until December.

CONSULTANT WALLIS: Okay.

MR. KROTIUK: And then it stops working.

MR. BAJOREK: Okay. So I have until December to learn my new one, whatever that might be.

One of things that those of us who have been involved with the peer review and in evaluation of TRACE over the past few years, we found this peer review to be very valuable. We've got a lot of very good constructive comments.

We've noted a lot of issues, problems and things ourselves. But getting this from an outside group really re-enforces the message that we'd like to make to some of our management in that this is where we need to spend some additional resources and this is where we need to spend some of our additional times.

CHAIRMAN BANERJEE: So where is the management?

MR. BAJOREK: They probably all ran.

We feel, I think there's a general consensus that, you know, the development process is enhanced by having this external review group. And I think the consensus is that we would benefit by having an external review panel, not a one time only basis, but on more of a permanent basis in the future.

CONSULTANT WALLIS: Well I think, isn't it a true statement, that you got the peer review panel because the ACRS recommended it?

MR. BAJOREK: I believe that's accurate. I think we generally got the question when will you start the peer review. I'll assume that that's where it started.

The peer review isn't the only thing that's been going on in the last year or year or a half. I believe the last time that we talked to this Committee was February '07. That might have been the full ACRS. So it's been a year, a year and a half.

In addition to the peer review we've been putting a lot of our time and effort into developing TRACE input models for the plants. Mirela is going to speak after my presentation. She'll give you, I guess, a list of the plants and talk about some of the work they're doing. But we've identified a broad variety of plants to cover pressurized water reactor, boiling water reactor, different types, B&W plants, CE plants in order to initially give us a good cross section of the plants that are of interest to NRR and to give us enough plants so we can start doing sensitivities and looking at the code and how it performs over the various ranges and conditions.

We have been using TRACE for advanced plants. As has been brought up a couple of times, there is an applicability report that's in its late stages of development for ESBWR. We've been doing ESBWR calculations with TRACE for quite some time now.

CHAIRMAN BANERJEE: But you haven't had any actual confirmatory work done for Browns Ferry, the uprates with TRACE, or has there been any?

MS. GAVRILAS: Well actually, as mentioned in the SER, this all developing research. That will be part of my presentation. But it was -- TRACE is mentioned in the SER for the Browns Ferry EPU.

CHAIRMAN BANERJEE: Good. What did they do with it?

MS. GAVRILAS: I'll refer to Len Ward on

that during my presentation if you want details.

MR. BAJOREK: I think I talk about this later on.

But one of our big challenges is to get the input decks out of their track B format, out of RELAP format, get then into TRACE. TRACE, that input format and run some sample calculations.

We realize that anyone who wants to use these decks or do audit calculations from NRR faces kind of a tough job. They're used to run some other code. They're used to the input in a different type of format and they have some pretty tight deadlines that they have to make. For them to try to do a code, to do an input deck conversion, learn how to use TRACE if they aren't familiar with it, they're going to miss their deadline.

So we've been working with NRR and we've taken it upon ourselves to develop these input decks, run sample cases so that if someone does need to look at a plant uprating or a steam generator replacement, whatever might be the regulatory problem of interest, they have a good starting point from that point in which to begin.

TRACE, as I mentioned, been used for advanced plants. We are doing the calculations for

ESBWR. We've converted a RELAP deck for EPR. And we've been doing at least large break calculations for that. I think we may have also started the small break calculations.

We've had an AP-1000 deck. We're working with that. We're trying to get that ready because, as you may be aware, the vendor is making some changes to the plant and they're going to have to make some revisions to their DCD. So we're trying to be prepared both in terms of the assessment and have the model ready for that. APWR will probably be the next one in line which we would be developing and running TRACE calculations.

We've talked about it a couple of times. The assessment is ongoing on, although most of the work right now is looking at these advanced plants.

CHAIRMAN BANERJEE: Now you show APEX, which is a reduced height facility. But are you doing any comparisons with any new experiments and things like reflux condensation and things like ROSA, which are full height?

MR. BAJOREK: Yes. The assessment basis for the reflux condensation, first we've like ROSA tests that we ran as part of our generic assessment matrix. You can look at the comparisons between level within the steam generator tubes and the predictions to what they were in those ROSA tests. And in general, I think they really weren't all that bad. And there's other things that --

CHAIRMAN BANERJEE: You know, we've heard, the full Committee heard from some tests that were being planned, a new set -- this was the French and the Swiss. I'm trying to remember. Dana had them over.

MR. BAJOREK: There's another --

CHAIRMAN BANERJEE: They're planning some new international tests in ROSA. And this was last month we heard from them. I'm trying to remember who they were now, but I've forgotten.

MR. BAJOREK: There's an international program where a number of countries have been using the ROSA facility to do some tests. The first phase one looked at some tests. They looked at top-break, kind of a Davis-Besse type scenario, some bottom breaks. There were a couple of other tests.

CHAIRMAN BANERJEE: Are we participating in those?

MR. BAJOREK: Yes, we are. In fact, we're doing some assessment against those tests right now. Not ready for publication or anything yet, but we're working with that looking at those.

Phase two there are some tests proposed that would look at this rapid cool down of the steam generator secondary side in order to pressurize the plant and to enhance the reflux condensation. We've recommended to our management that we participate in those tests. I don't think a formal decision has been made yet. But that would be very useful for TRACE assessment as --

CHAIRMAN BANERJEE: That's in ROSA two?

MR. BAJOREK: That's in ROSA phase two.

There's also some tests that have been proposed for PKL that also look at this rapid depressurization.

Tests that we have done -- or excuse me, assessments that we have done --

CHAIRMAN BANERJEE: Isn't it easier just to put a high pressure injection system in?

MR. BAJOREK: If I were a designer I think I would say yes. If --

CHAIRMAN BANERJEE: Mike is looking very snarly since it's not his responsibility.

MR. BAJOREK: Okay. But for the reflux condensation we've augmented the stuff that we've done in the generic assessment with separate effects texts they had done in ROSA some time ago when they looked at the behavior in the long versus the medium versus the short tubes.

We have used the APEX facility, be it at one quarter height, to run some additional experiments. Those may prove to be more useful in making use of the noncondensible gas tests. But those have given us some additional information.

CHAIRMAN BANERJEE: I mean we've always been concerned because these are all gravity driven systems or, you know, different variations. And with such a reduced height facilities you might get pretty -- I remember with AP-600 we got some very different results than we got from ROSA. It was hard to then try to map this and scale them. And it was a mess.

MR. BAJOREK: Which is why we've put most of our emphasis into the flex C set reflux condensation. Those were very close to full height. And the ROSA steam generator tests, which were also full height.

We try to do that in all of our assessment. If in doubt we try to bias our assessment which are larger in scale, which is why we've made greater use of ROSA and Vexy in our assessment than doing additional semiscale tests or APEX tests, you know unless we need those for AP-1000, AP-1000.

So additional assessment has been going on in support of the advanced plant.

In doing the calculations with the plant decks, the Browns Ferry, the Westinghouse 412, as you would expect from a code that's essentially new we have noticed and found a number of problems that you might relate to robustness. It requires too much intervention by the code user to stop, backup and make a change, change the time step, run it out, deal with the next problem that comes along.

Many of these are due to code errors where the calculation finds itself in a situation where the code reaches a singularity or a problem and needs to be dealt with by changing the coding or code errors by the user. The input's incorrect or it's inconsistent with something. We take that to heart in that that means we do have to improve our documentation. We start seeing a number of users make that error. That means we either need to put a diagnostic in the code or improve the --

CONSULTANT WALLIS: By "robustness," you mean that the code doesn't run or it stops or it gives absurd results or what?

MR. BAJOREK: Well, it could be any of those. You know, all of the above is one of the answers. But I think the ore frustrating thing is when the code continues to run, gives an answer which is correct. You don't always know that for a plant calculation, not enough data to compare it to. But when it cranks on down to a minimum time step size and takes ten hours to go a minute in transient time; even if the answers are right, you can't live with that type of an environment. Those are the types of things that we have to correct because --

CHAIRMAN BANERJEE: I mean does that still happen?

MR. BAJOREK: It happens more frequently than we want it to. We keep in the back of our minds that ultimately we're going to need to use this code with an uncertainty methodology. That's where the industry is headed. That's probably the best way to gauge the accuracy of the code calculations. It's not a particular transients, it's somewhere between here and another value. Reality is probably somewhere in between. But you're going to need to run dozens, if not hundreds of calculations. And if you start to have every calculation go down to some silly running time --

CHAIRMAN BANERJEE: Well, Dominic told us that with a mature code like JAERI it takes six to eight hours. I don't know on what processors these are. So how long could it take with TRACE?

MR. BAJOREK: I'd have to ask some of the people who run the calculations. Browns Ferry, my understanding, will run in a couple of hours.

MS. GAVRILAS: Browns Ferry, a small break LOCA I think it's 20 minutes, a large break LOCA 40 minutes. That's a BWR.

PWR, I don't have a very good answer for you yet.

MR. BAJOREK: I think the people I've talked to on the Westinghouse 412, it's a couple to a few hours. Probably within the six to eight CP hours time. We would like to try to get that a little bit shorter.

What's next? So we've gotten a lot of information from the peer reviewers. We've gotten comments from our users. We've gotten some comments back from NRR and NRO who are trying to use the code now for some regulatory decisions.

MEMBER CORRADINI: Can I ask you about that, Steve? So you said you got it from NRR and NRO Are any of their comments at odds with or interesting supplemental to what we heard from the peer group? Or is it written anywhere?

MR. BAJOREK: We get them mainly verbally.

MEMBER CORRADINI: So a phone call and you can hold it like this? But it's usually verbal comments?

MR. BAJOREK: Yes. I think one thing that's important to know is we really are looking at two different worlds. The peer reviewers and a lot of the code developers look at the code in terms of the models, the correlation state-of-the-art modeling techniques and comparisons to data; how accurate is it. Code users want to be able to take that for granted. Okay. They're busy with other things. The most important thing is being able to set up the model rapidly, make changes and get the code to give you reasonable results. And reasonable is often in comparison to what maybe the vendor is predicting or what I saw in that last FSAR, final safety analysis report. But I haven't changed my safety system and I've increased the power by ten percent, sort of expect the peak cladding temperature to go up somewhat. And you want to get reasonable answers with respect to that.

So I think people who are using the code for plant calculations are looking at it maybe a little bit differently than people who are developing the code and evaluating the code in terms of its models and correlations. So we're getting very complimentary information, but in a way we wouldn't expect the users to come back and say "Gee, I really question this model or correlation until we can pin it down to a break model or something that stops the code from running."

MEMBER ABDEL-KHALIK: Why would modeling a large break PWR LOCA take more than ten times the CPU that a large BWR LOCA takes?

CHAIRMAN BANERJEE: CP time?

MR. STOUDEMEIER: Joe Stoudemeier, NRC.

The nodalization is a lot more detailed in a PWR large break LOCA. Like generally our vessel components have on the order of a nodes in them, 30-D vessel cells. And for the BWRs we generally run with 2-D vessels and a lot of the core nodalization is 1-D Chen components. So that's a lot more 1-D computations in BWRs than in the PWRs.

MEMBER CORRADINI: And you need that or you need that three dimensionality in the P why?

MR. STOUDEMEIER: In large break LOCAs there's more three dimensionality. Like the downcomer refilling after -- when the ECCS comes on after depressurization there's three dimensionality in there. The broken loop compared to the intact loops causes normality dimensional behavior.

MEMBER CORRADINI: ECC bypass in the large break and with respect to small break, depending on your break size that can determine how many of the intact loops or how many of the loops vents continually to the break. So there's sort of a built in asymmetry that we don't see in many of the BWR plants.

MR. STOUDEMEIER: Yes. And the multi-dimensional behavior in the BWRs is mostly radial behavior in the upper plenum where water breakdown, CCFL breakdown occurs and the water drains down and then comes back up from the bottom of the channels.

And actually to some extent there hasn't been a lot of testing of three dimensional calculations and effects in BWRs to see whether adding increased nodalization is, like that really has an impact on the calculations. I think GE and INEL when they were developing track BWR they just came up and did these two dimensional nodalizations in the vessel. And I don't know if anybody ever looked at three dimensional nodalizations, to be honest, to see how big an impact it is.

There's reasons why I know where those

things would be minimized just because of the way the hardware is in a BWR. But it's probably something that we should examine a little more in the future.

MR. BAJOREK: I mean when we do some of the other calculations, there's some people are using TRACE coupled with PARKS to look at BWR stability. There their BWR models get very complex. I think they even model every channel in some cases. And now to run, you know, a very short transient you do wind up with a much higher CP time.

CHAIRMAN BANERJEE: I think we're seeing the effect of TRACE most with the BWR uprates. But quite significant. And I think when we get to MELLA Plus, as people will be pushing that, we are going to be looking at plant specific calculations for ATWS instabilities. And I think TRACE is going to have to be able to do those.

MR. BAJOREK: Yes. But for what we've gathered from both sets of information, the peer review comments and what we've received from the users, is that we feel that our most important next steps focus on first of all error resolution. When we find these robustness problems that are stopping the code from running in a reasonable time, we need to fix those. We need to fix the problems that have been identified in the code as outright errors. Deficiencies in the momentum equation, errors in the specific heat, the specific energy. Because clearly it doesn't make a whole lot of sense to start fixing models and correlations, okay, to match data if we know there are other errors out there that have to be corrected first.

Second, is to address the peer review comments.

CONSULTANT KRESS: Let me ask you about that. What do you think you might do about comments like you have some models that are overly complicated and too detailed, whereas others are not. Well, what do you intend to do about things like that?

I would have thought it doesn't hurt to have an overly complicated and detailed model.

MR. BAJOREK: I think that unless somebody points to the model and says its really that that's causing a problem --

CONSULTANT KRESS: Or what's important is running time, isn't it?

MR. BAJOREK: Or its complexity is hurting the running time, we'll probably leave it alone.

CONSULTANT KRESS: Yes, that's what I would recommend.

MR. BAJOREK: It would not be real high on the list of priorities.

I think it's very important to look at the models where there may be some mixing and matching, okay, as keeping the nucleation part of one boiling correlation and using the convective enhancement from the other. Eventually we're going to have to try to make those more uniform or justify that delta. But if it's not slowing the code down and is not clearly inaccurate, we probably won't treat that as anywhere near as important as to fixing the things which are clearly errors --

CHAIRMAN BANERJEE: But basically time step control that's giving you the problems in run time? Driving you down to a very small time step and holding you there?

MR. BAJOREK: Sometimes that's it.

CHAIRMAN BANERJEE: Is that to do with the interface being around or why is that happening?

MR. BAJOREK: I think there could be a different problems with that.

CHAIRMAN BANERJEE: Somebody should tell us. Is there a problem with this and why is that happening?

MR. BAJOREK: I think one of the more

recent things that we've put into SNAP, the tool that helps people with the input, is to try to avoid situations where people are putting very small nodes in conjunction with very large ones. Kind of the thimble tube adjacent to the swimming pool. Because a very small oscillation in the swimming pool, the large node, can overwhelm what's going on in the other one. You get high velocities, rapid changes in some physical parameter, the time step size decreases to try to catch those types of things.

CHAIRMAN BANERJEE: Now why doesn't that happen in RELAP5?

MR. KROTIUK: It does happen in RELAP5.

CHAIRMAN BANERJEE: It does?

MR. KROTIUK: Yes.

CHAIRMAN BANERJEE: So it's actually handled in what way there?

MR. KROTIUK: The way you handle that is you have to make your model correct. You have to optimize your model.

The limitation with the small nodes, some bodies next to large bodies, is the same and it's irrespective of whether it's RELAP or TRACE. It's optimization of your modeling.

CHAIRMAN BANERJEE: So that puts even more force on Tom Kress' comment, right? It gives the user --

CONSULTANT WALLIS: It's also a problem in CFD. You don't want to have such huge nodes here and little tiny ones attached to it.

MR. BAJOREK: But that's an example. But the idea of robustness can come from simply how it's been modeled to how the models and correlations are treating those changes. And in some cases they need to be changed in order to increase that running time.

MEMBER SHACK: Decrease the running time.

MR. BAJOREK: Decrease the running time. I'm sorry.

MEMBER SHACK: Unless you're selling computers, more is better.

MR. BAJOREK: One of the areas that we are putting a very high priority on is documentation. Just to kind of brief everyone. Our feeling is that as we change the documentation at this point we want to try to continue to release it and maintain it as a set. I think the problem that we heard a number of years ago, both for TRACE and some other codes as well, is that changes are continually made to the code. They don't get it into the theory manual. The code never gets reassessed.

We have tried to make sure that as we make changes they get into the theory manual right away. We know there's ways we can improve how descriptive we are there. We will continue to rerun those cases and update the assessment report. Now it may not be every month or every code release, but we don't think it's going to be ten or 15 years, as I think it had been in the past.

As we make a number of these changes the idea is to rerun all of those 550 cases and update the assessment report.

As I've mentioned before, we've tried to take advantage of different techniques to handle the input decks and regenerate the reports. So even though it's a fairly large number of cases, we think we've developed things in such a way that repeating all of these is not going to be near as onerous as it had been in the past. It's not going to relieve you of the engineering of looking at the results and making some decisions, but the physical process has been speed up. And perhaps most important is making sure that when we do have changes those are quickly and accurately reflected in the user manuals.

In terms of what we've learned from the peer review, and I think we've heard it from each of the reviewers, that they all had difficulties with the documentation. One that is of particular note and interest to us right now is that lack of specific user guidance for plant input deck development. We saw just in terms of looking at the documentation. But I also think Dr. Bestion, somebody else said, in looking at uncertainties there's a few things which may give you a few degrees K, but one of those larger ones was the user impact, the user effect. And unless we make our input manual specific enough so that we can give one to this person, one to that person, somebody over here and come up with models that look pretty close to one another, we're just going to be inviting a large user effect in the future.

The other problem in the documentation had to deal with identifying which specific models were actually used in TRACE. We mixed this idea of being specific, identifying the model with maintaining and preserving the history. We want to do that. We want to keep the history, but in the long run we think that is going to have to be accomplished by splitting this up into a very factual theory manual and a second volume that preserves the history and gives you all of the details that you need to have or might want to have in using some of the models.

So with respect to what we're proposing and what we're thinking about as a solution then is twofold.

First would be a development of I say two reports. It's probably two sections that would go into the user manual. And what these would do is they would go through a plant, a PWR and a BWR, component-by-component and layout what you might all "cookbook" on how you set up that plant. Here is how you should model the core. Here is how you model the upper plenum, the pressurizer, steam generator. And laying out specific guidelines.

First, on which component to use. The code's very flexible and gives you actually several choices in some cases. But we need to decide upon that, let the user know exactly what to do. Give them a recommended nodalization and that will point back to the assessment cases that we've done and the nodalization sensitivities that will help the user define his nodalization. And we say as long as you stay within these bounds, we think the code will give you some accurate results and you won't run into some of these robustness issues.

If you have good reason to go outside of that, well then that's something that you're going to have to think about a little bit more, bring to the attention of the code developers and others who are more acquainted with the code to make sure you aren't doing something that's going to invalidate something.

MEMBER ABDEL-KHALIK: When you say "reports," do you mean user manuals?

MR. BAJOREK: User manuals.

MEMBER ABDEL-KHALIK: The theory will remain unified?

MR. BAJOREK: Theory manuals stay there, but when we say model a pressurizer, okay, we'll be looking at the assessment reports and so you should model with five nodes. It should be using a pipe component. The surge line would be part of that. Here's where you should have the area changes.

The reason we think we can do that is if you go from plant-to-plant in those allotted changes, pressurizers look pretty much like a pressurizers, cores different fuel assemblies but they're generally 12 to 14 feet in height, they have grid sets, almost uniform elevations. (1) Because the vendors like to be able to put their fuel in someone else's plant. So they don't want to try to invite more problems than they're trying to solve in doing that. So we can take advantage of that and really lay out a set of guidelines for the users in setting up their models.

As I mentioned: (1) We want to make that easy for the user to use the code but (2) We want to try to rule out and get rid of the user effect as much as possible.

CHAIRMAN BANERJEE: So what PWRs are coming up for uprates after Millstone?

MR. STOUDEMEIER: Millstone's a P.

CHAIRMAN BANERJEE: P. I'm sorry. I said a PWR.

MR. BAJOREK: Millstone. I think I heard Calvert Cliffs at some point.

CHAIRMAN BANERJEE: So you're setting up at Calvert Cliffs?

MR. BAJOREK: I've got a list in my office I can show you.

CHAIRMAN BANERJEE: No. I'm just looking at the decks that we're setting up.

MS. GAVRILAS: Mirela Gavrilas from Research.

I have a table that summarizes the decks that we'll working on over the next two years.

CHAIRMAN BANERJEE: And they are in line with the uprates or steam generator changeouts, or whatever they're doing.

MS. GAVRILAS: They're definitely involved and informed by NRR and their pending needs.

MR. BAJOREK: Most of the initial decks are BWRs. I saw some of those. There are some on the list, NRR has identified those, which ones are coming up I don't know.

And development of the decks, it's looking at not only in the plant uprates and what NRR's needs are, but also to make sure that we have a good variety of plants so that as other regulatory issues come up, such as: What happens if debris blocks the bottom of the typical Westinghouse plant, what might be the impacts? We'd like to have plants that we can look at these different types of phenomena of interest.

CHAIRMAN BANERJEE: You have to keep the bypass flow and stuff there.

MR. BAJOREK: Well, whether it's upflow or down flow, yes, there's different varieties out there.

CHAIRMAN BANERJEE: Yes.

MR. BAJOREK: We've gotten started on this for the user manuals. We've been able to retain Brent Boyak as a technical editor. And as soon that contract gets through our bureaucracy, we'll be able to start making changes to it and make revisions at the same time where we will be deciding some of these modeling decisions so that those can get factored into the user manual.

MEMBER CORRADINI: So Boyak will become kind of a lead author to regularize all of this?

MR. BAJOREK: Yes. Yes. He'll --

MEMBER CORRADINI: He'll be the first intelligent user to try to summarize what you think, what he thinks you guys are saying?

MR. BAJOREK: He will help put it in intelligible language so that it is clear to a user that might be familiar with the plant, familiar with the code but may not have a lot of experience in setting up those models. So they'd be able to take the input guide, but able to apply it for some facility that maybe no one's modeled before, but also have those guidelines on there that if he is given charge of modeling a specific plant, they could go through and do that as well.

MEMBER CORRADINI: The only reason I asked that is I was late, so all I heard is George's and Pete's comments. But to me this is very important because, well I think Pete or somebody said it, that the biggest potential fallacy is that somebody uses it incorrectly, doesn't even know they're using it incorrectly and so the guidance is key particularly a set of guidance where they understand where they're close to a cliff.

MR. BAJOREK: And we also look at it as a way of helping our own development. If you have people starting to use five or six different options to do the same thing, you may be trying to fix five or six different parts of the code to fix robustness problems. At least if you initially say "Do it this way," you can start by making that work and then you can gradually expand on the flexibility of the code as time goes on.

CHAIRMAN BANERJEE: Steve, you're going to keep moving on.

MR. BAJOREK: Oh, okay. All right.

We talked about the theory manual. We want to split that into two different volumes.

We would like to incorporate many, if not all of the suggestions that we've heard for the documentation. Adding a chapter to the theory manual to look at the modeling strategy. A chapter to look at the flow and heat transfer regimes.

Taking use of modern technologies to link various chapters. Those are the question, you know, you're not shuffling through the thousand pages to find where it describes particular input. But go back to the links. And also be able to take all of the references and link those into the documents so you weren't chasing around for some reference that is only somebody's desk drawer somewhere. That's always a problem.

We agree with the idea that we would like to try to relate the assessments, first of all back to the PERTs. We think we've done that. But also get a better cross reference to which tests are testing individual models and correlations.

Ranges are very important. We think we have a very broad range when it comes to looking at reflood rates, pressures, subcoolings but we need to do a better job of summarizing where that's at and making it so that a user would know if they're starting to go outside of that valid range.

Okay. I have to have my arm behind my back and say any revisions and work is contingent on the resources. I mean, this is our intent but we do have to balance that with the need for setting up plant decks, continuing assessment, you know and doing other --

CHAIRMAN BANERJEE: So moving this model development tests to the assessment, this is what pertains to the development or choosing of individual correlations and things and the support and the way that you've assessed them, that sort of stuff?

You know, George was talking about I think how to relate a correlation to the assessment that has been done --

MR. BAJOREK: Right.

CHAIRMAN BANERJEE: -- already. And that wasn't very well written up. I mean, there was parts of it, but I think that needed to be expanded on. So there needs to be some new writing; it's not just moving, you know.

MR. BAJOREK: We intend to do that, but notice that we're starting with the user guide and the user manual.

CHAIRMAN BANERJEE: Right.

MR. BAJOREK: We think that's the greatest need in order to make the code useable for the staff and anyone else that needs to use it. We're going to start with that. And then as additional resources become available, we'll move into the theory manual and the assessment --

CHAIRMAN BANERJEE: Are there flags anywhere in the code if some very important correlation is being used completely outside its range?

MR. BAJOREK: There are a number of warnings that show up if you start to go out of some ranges. But I don't believe it's in there to the extent that would prevent a user from using something where it hasn't been assessed over the right range of pressure or subcooling, or heat flux, anything like that. It's usually when the correlation starts to get into a numerical problem that you get that warning.

CHAIRMAN BANERJEE: You don't know then which correlation, it just bombs, right? Does it tell you?

MR. BAJOREK: In some cases. But that would make things too easy, so --

MEMBER CORRADINI: Actually, if I may?

CHAIRMAN BANERJEE: Yes. Go ahead.

MEMBER CORRADINI: So I'm curious about that just from a calculation -- so in this assessment nobody ran it except none of the four of these learned gentlemen got in there and tried to do something to kill it. So are you letting people outside of the staff -- or not letting. Are you encouraging people outside of the staff to use it and then get feedback onto it?

Because, for example, trace back of failure is very important, is a very important way of learning how it functions. So what are you doing there?

MR. BAJOREK: The more people that use the code, the better. We have our own staff running it. CAMP. There are a number of people running the calculations using TRACE. We got contractors running it.

So the user community is gradually expanding.

MEMBER CORRADINI: Okay.

MR. BAJOREK: It's not just the developers and the people who are immediately down the hall from them. So it is going out.

We tell our CAMP members that the more people that do get involved with the assessment and give us feedback, the better.

MEMBER SHACK: You have Tracezilla for your bug tracking.

MR. BAJOREK: What's that? You have seen that? What is it?

MEMBER SHACK: Tracezilla. That's the usual bug tracking is "zilla" something.

MR. BAJOREK: Okay. We've had some comments on assessment. We think that we have a fairly comprehensive assessment matrix, but we don't see that as the end of the line. We're continuing to expand on that. We would keep those 550 some cases, expand that as need be for new and advanced plants. And also augment that with things that have either fallen through the cracks because we didn't think of those the first time around or we couldn't address them because we had either questions on how the code would perform for those or the lack of experimental data.

CONSULTANT WALLIS: How about containment? I mean, in the ESBWR all the phenomena that happen are in the containment or they're in the large volume like the wetwell. And whether or not you get stratification and the flow patterns develop and how you predict the heat transfers to the walls, and all that. They're all important. I don't see much of that in your assessment.

MR. BAJOREK: For conventional Ps and Bs you would generally rely upon mass and energy release being generated by some code fed into a containment code; CONTAIN, GOTHIC, LODIC --

CONSULTANT WALLIS: Something else.

MR. BAJOREK: -- to give you the containment pressure and then that becomes a boundary conditions.

CONSULTANT WALLIS: Well, TRACE is going to do the CONTAIN as well, isn't it?

MR. BAJOREK: We can link TRACE with CONTAIN.

CONSULTANT WALLIS: But you have CONTAN component?

MR. BAJOREK: We have a CONTAN. We're working to try to activate that and give it the functionality like CONTAIN.

MEMBER CORRADINI: Before we launch off into a whole new development, I guess I'd be -- I mean maybe I'm just too old fashioned. I mean, you've spent a lot of money on other things that supposedly do containment correctly. I would be more interested in how you properly interface between what is truly a primary system model code and a containment code rather than adding more components to this. I mean, that's just my -- everybody's got their own slice of this.

I mean, typically for a pressurized water reactor if you bias the containment pressure low, you'll get more conservative answers. So typically you would take that mass and energy release, feed it into a qualified containment code, get that pressure history. Maybe you bias it down another psi or so.

And then use that as your boundary condition.

CHAIRMAN BANERJEE: I guess the concern with the ESBWR is that the containment and the primary system is very closely coupled.

MEMBER CORRADINI: Yes.

CHAIRMAN BANERJEE: I mean, there's a lot of stuff happening which is very closely coupled.

Now if I interpret it right, is Mike's question he's asking where are these boundaries and interfaces and how are they going to be coupled to other codes? It's not obvious, you know, because everything is so highly coupled. How we do that?

MEMBER CORRADINI: What we saw an audit calculation -- just to finish. I mean Sanjoy's got it right. What we saw in audit calculations is they fed at the break location for the main steamline break, right, to a MELCOR calculation. And I don't even remember what was the -- I think it was a TRACG calculation, right?

CHAIRMAN BANERJEE: Right.

MEMBER CORRADINI: From the licensee -- from the applicant.

So I'm just kind of curious that you could take the path as you suggested, but it strikes me as a development path where you've already spent all the time and effort somewhere else that if you properly think through how you pass information?

CHAIRMAN BANERJEE: If you can find the location --

MEMBER CORRADINI: Yes.

CHAIRMAN BANERJEE: -- then you can actually work like TRACE and --

MEMBER CORRADINI: Right.

CHAIRMAN BANERJEE: If we could couple them together. Those probably a little bit easier to couple, those than these.

MR. BAJOREK: Yes. We've done coupling with TRACE and CONTAIN and that works.

Now ESBWR, that was a different problem.

CHAIRMAN BANERJEE: Because that's so tightly coupled.

MR. BAJOREK: Because it's very tightly coupled.

CHAIRMAN BANERJEE: Right.

MR. BAJOREK: If you look in the ESBWR applicability report, that containment the drywell's been modeled with pipes in the vessel component. We've also done that in tests like PUMA and PANDA as an attempt to basically try to benchmark how you would model that in those tests which supposedly mimic ESBWR and use much of that in the actual plant calculation. So it's sort of been it's own case.

MEMBER CORRADINI: And that's also in the March 2008 the PUMMA?

MR. BAJOREK: Yes.

MEMBER CORRADINI: I mean the PANDA?

MR. BAJOREK: The PUMA, PANDA, Giraffe, all those critters.

MEMBER SHACK: But already benchmarked MELCOR against those same experiments, right? I mean, so you're kind of marching off on parallel paths here.

MR. BAJOREK: Different missions, though. MELCOR is looking at, I guess, the long term pressurization.

MEMBER SHACK: Right.

MR. BAJOREK: Whereas, TRACE wants the pressure early in time to get good feedback and boundary conditions for looking at the inner vessel mixture level. There's an overlap.

CHAIRMAN BANERJEE: This is a question. Is GE doing the long term with TRACG as well?

MEMBER CORRADINI: They were, yes.

CHAIRMAN BANERJEE: They're doing everything with TRACG.

MEMBER CORRADINI: The answer is in my

memory yes, but I want to make sure I understand what you say long term/short term. Can you give me a time what you mean by that?

MR. BAJOREK: I mean long term --

MEMBER CORRADINI: We're talking the first hour when you say "short term?" Because --

MR. BAJOREK: Short term is well within the first hour, Joe? What's the minimum? It's several hundred seconds, I think.

MR. STOUDEMEIER: Yes. The minimum is usually within ten minutes.

MR. BAJOREK: Peak pressure, I think, is tens of thousands of seconds.

MR. STOUDEMEIER: Well, I think right now it keeps climbing and climbing.

MEMBER CORRADINI: It's at 72 hours.

CONSULTANT WALLIS: Like 72 hours.

MR. BAJOREK: Seventy-two hours?

CHAIRMAN BANERJEE: That's due to the hydrolyses, right?

MEMBER CORRADINI: But I want to understand if you could just repeat? Now that I understand what short term is, can you repeat the reasoning of why that's important there? I'm sorry.

MR. BAJOREK: Okay. There has been two

different approaches. The MELCOR approach has been used to look at that look term containment pressure.

MEMBER CORRADINI: Correct.

MR. BAJOREK: So it's more interested in that long term behavior.

MEMBER CORRADINI: Right.

MR. BAJOREK: For TRACE, however, since we're looking at vessel inventory all we need is pressure to give us a reasonable back pressure for the vessel for the assessment and work that has been done there has been focused to look at that early time pressure transient, not the phenomena that we dictate what happens 72 hours into the event.

MEMBER CORRADINI: So let me push that one more time. So what is not in MELCOR that needs to be there for that first hour? I don't think of anything that's missing.

CONSULTANT WALLIS: Doesn't MELCOR have a one node containment?

MEMBER CORRADINI: It can have --

MEMBER SHACK: No. MELCOR has a very simplified reactor model.

MR. STOUDEMEIER: Yes. That's, I think, the big difference is we do a lot more detailed reactor vessel --

MEMBER CORRADINI: Okay. So I'm not going to let go of this until I'm clear. But now we're pointing back at MELCOR where we know it's not perfect, but it balances mass energy which is the vessel. So we're back to coupling.

So the MELCOR containment modeling would be adequate if we had a proper coupling between what TRACE was feeding it in that first hour.

MR. BAJOREK: I believe that's --

MR. KROTIUK: No.

MR. BAJOREK: No?

MR. KROTIUK: I have one extensive coupling between TRACE and CONTAIN trying to model ESBWR in the PUMA experiments. They are so tightly coupled that the coupling between the two codes does not adequately work. There is numerical and calculational problems in doing that. Because there could be slight differences in property tables between CONTAIN and TRACE --

MEMBER CORRADINI: We switched to CONTAIN all of a sudden.

MR. KROTIUK: Yes. Because that's what I did.

MR. BAJOREK: That's already done.

MEMBER CORRADINI: No, it's not. It's not the same thing. What's in MELCOR is Hector. What's in CONTAIN is a different formulation of the basic balance equations; that I know, unless something changed 20 years ago.

MR. KROTIUK: Yes. But all I'm saying is is that you have to have the property tables tightly matched.

MEMBER CORRADINI: Okay.

MR. KROTIUK: And if they're not matched, then you're going to have problems.

MEMBER CORRADINI: Okay. But I -- okay. I'll stop now.

CHAIRMAN BANERJEE: I guess coupling --

MEMBER CORRADINI: But I'm not going to forget this one.

CHAIRMAN BANERJEE: -- coupling, TRACE and MELCOR is an option. I mean, tied to coupling but you have to make sure that it can be done.

MEMBER CORRADINI: But that helped. Thank you very much.

MR. KROTIUK: Okay.

CONSULTANT KRESS: How will you know when you've done enough assessment? You'll only know that when you actually do an uncertainty calculation and the uncertainty is acceptable?

MR. BAJOREK: I think that's the right answer, yes. And once we do uncertainty and if our difference between our 95/95 PCT and the 50th the nominal PCT is 100 degrees, we're happy, we're probably done. If it's 700 degrees --

CONSULTANT KRESS: So you'd be using that as your figure of merit for you've done enough assessment?

MR. BAJOREK: I think the expectation is that best estimate nominal p-cladding temperatures have a lot of margin. And if you start getting a 95/95 p-cladding temperature that challenges the regulatory limit or exceeds it, we've got to work to do on at least one or more of those models. I think that's probably the answer.

CONSULTANT KRESS: Good answer.

MR. BAJOREK: Now for the assessment that we think is more near term and important and has been pointed out by the peer review, we would agree that looking at condensation, direct contact condensation, condensation near the ECC jets as you would see in a COSI experiment or a Westinghouse EPRI one-third scale, as Dr. Bestion pointed out, ranking up there as being one of the more important things that we did not really get to and we should have.

NRU and Achilles were a couple of tests we would like to assessment. We originally did want to those, but it was difficult to get the experimental data. Either it wasn't available to us, the tapes were corrupt. It was going to wind up taking a lot longer to do those.

CHAIRMAN BANERJEE: Isn't it mainly a gap conductions problem? What the fuel rod model there?

MR. BAJOREK: NRU and LOFT are the only experimental tests that made use of nuclear rods. NRU is the only one that was pressurized. So in those material tests that they ran, they ran those up and burst the rods. Okay. And did measurements of the cladding temperature and the gap pressure when burst occurred. So it gives us --

CHAIRMAN BANERJEE: And put it into CHF?

MR. BAJOREK: They ran a reflood test. They basically put a test facility inside the Chalk River Reactor, drained the water out of it, let the rods go up, burst and then went through a reflood transient. Very useful in assessing your fuel rod models, which is something that we don't get a whole lot of opportunity to do in all those other 550 cases.

If you look at uncertainties and contributors to them, the Bemuse work that's being done right now in NEA, I guess, shows that hey, fuel rod models are very important in the overall uncertainty. But if you look at everybody's assessment, ours and what the vendors are doing, there's not a whole lot of opportunity to do that. So that's why NRU is high on that.

CHAIRMAN BANERJEE: I'm trying to understand what this model does. Does it effect PCT, Appendix K calculations, best estimate; which one?

MR. BAJOREK: It can effect your p-cladding temperature because depending on when and where the rod bursts, that will expose cladding to a double sided metal water reaction. And if you're looking at cladding temperatures in excess of 1800/1900 degree Fahrenheit, the metal water reaction will drive you to even higher temperatures very rapidly. So --

CHAIRMAN BANERJEE: But if you stay within the Appendix K limits, is there any chance the rods will burst?

MR. BAJOREK: Oh, they'll burst.

CONSULTANT WALLIS: Steve, we're waiting to see how you respond to the peer review. And you've gone into all this stuff, which is what you're doing even without the peer review you do this stuff. Are you going to get on to how you respond to the large number of specific comments from the peers?

MR. BAJOREK: To an extent. What I wanted to do here is first deal with the assessment. The stuff that's highlighted here in green are items that have been specifically brought up as part of the peer review. So I just wanted to show that, yes, there are parts of the peer review that absolutely we agree with it --

CHAIRMAN BANERJEE: On the fuel rod models, Steve, you're going to have to convince me a little more about their importance here. I still don't really get it. If you do the best estimate or an Appendix K calculation and you're well within the limits, how relevant are these tests to that?

MR. BAJOREK: In almost any LOCA transient you will burst a fair number of rods. Certainly the rods within your hot assemblies. Rods will burst with normal pre-pressurization, 2 to 400 psi.

CHAIRMAN BANERJEE: At what temperatures will they burst?

MR. BAJOREK: They will burst at about 1400 to 1500 degrees Fahrenheit. Depending on your power, you can burst those rods in the hot assembly during blowdown. That's if you're lucky. Because when you burst them there, you'll burst them fairly low -- you'll burst them closer to the peak power location which could be lower in the core.

If you don't do it then, you will probably burst the rods in the hot assembly during late refill or reflood because you'll have a much large delta p between the pressure inside the rod than what you have out in the system, which now could be hundreds of psi as that rod pressurizes and the rod heats up softening the clad, and then it will also burst more at 1400 degree Fahrenheit, but for some different mechanisms.

In either case, you're going to wind up with the rod burst. Now if it bursts higher in the bundle near your peak cladding location, maybe because it's away from the reflood front, now you have the dual penalty of having poor heat transfer and having metal water reaction near that.

MEMBER CORRADINI: But I think the way you're explaining it it's more a matter of the oxidized fraction question than the peak clad temperature question. Because the two -- you aren't going to do a peak clad temperature calculation to answer that. It's going to be assessing that, and that was some sort of multiplier or some sort of additional analyses to determine, right? If I remember correctly. Because I'm a little bit out of my element about there's a multiplier already in terms of the reg guides of what you would assume to be how much it would be double sided.

I mean, you're explanation is mechanistically, but in essence you've transferred from a PCT issue to a fraction of the cladding oxidized issue, right, in terms of if you're past the limit?

MR. BAJOREK: You're really dealing with both. Because you may be dealing with an oxide --

MEMBER CORRADINI: Right. I understand.

MR. BAJOREK: -- limit, but because of the metal water reaction you're putting much more energy into the clad.

MEMBER CORRADINI: Okay.

MR. BAJOREK: And that's going to drive your temperatures up even faster there.

CHAIRMAN BANERJEE: You have it double sided, that's --

MR. BAJOREK: Yes.

CHAIRMAN BANERJEE: And what did NRU show?

MR. BAJOREK: Let's see --

CHAIRMAN BANERJEE: Did they burst at 1400 or 1500?

MR. BAJOREK: At about that range.

CHAIRMAN BANERJEE: Did they? And then you got oxidation on both sides?

MR. BAJOREK: Yes.

MEMBER CORRADINI: Fourteen hundred/1500 Fahrenheit?

MR. BAJOREK: Fahrenheit.

CHAIRMAN BANERJEE: Interesting tests.

MR. BAJOREK: I mean it's --

MEMBER SHACK: We only maintain cool with geometry. There's never any guarantee that you wouldn't burst fuel rods.

CHAIRMAN BANERJEE: Oh, yes. That's all right.

MR. BAJOREK: But anyway, that's on our list.

CHAIRMAN BANERJEE: Okay.

MR. BAJOREK: We'll get that out --

CHAIRMAN BANERJEE: So that sort of justifies doing it. I didn't realize they burst at such a low temperature.

MR. BAJOREK: Yes, they do.

CHAIRMAN BANERJEE: Yes. We're going to have to move on.

MR. BAJOREK: Okay. Okay. Other

assessment, and I'll just go over this very quickly.

So there's yes items that have identify looking at loop seal clearance, CCFL. Yes, we're looking at tests and doing assessment to deal with those.

There's some other green ones down here, blowdown film boiling, downcomer and some of the non-LOCA tests. I haven't filled in a test over here because non-LOCA that's various sources. Blowdown and downcomer, actually I'd like to talk a little bit more about which tests may really fulfill that assessment purposes. Some tests are better than others.

Okay.

CHAIRMAN BANERJEE: Now where are we? Are we finished with your prioritization of peer review issues?

MR. BAJOREK: No.

Switching to the colored slide.

I was going to go to look at some assessment results.

CHAIRMAN BANERJEE: Ah.

MR. BAJOREK: Okay.

CHAIRMAN BANERJEE: Now, is that going to cut into Mirela's time or what?

MR. BAJOREK: I can if you like, but--

CHAIRMAN BANERJEE: How much time are you going to take on this?

MR. BAJOREK: This would probably take about 20 minutes.

CHAIRMAN BANERJEE: And we don't have a spot on the agenda for this?

CONSULTANT WALLIS: Could we come back to other stuff.

MR. BAJOREK: Yes, we could come to that.

CHAIRMAN BANERJEE: Then you're going to go to plans to resolve peer review and user issues, and then you have momentum equations?

MR. BAJOREK: I'll tell you what might work out best. I really meant for this one as primarily background information.

CHAIRMAN BANERJEE: Right.

MR. BAJOREK: Because I heard some many want to see some sample results for our assessment. I can do that, and I do in this presentation primarily with the intent of showing some results. You just can't go through that entire document in a short period of time. But at the end of this summarizing the things that we've identified as being key deficiencies in TRACE that would be intended to be long term development projects.

CONSULTANT WALLIS: I thought we were here to discuss the peer review, not -- this is a whole new subject here, isn't it?

CHAIRMAN BANERJEE: Well, I'm trying to wrestle with what to do because -- why don't we combine your two coming presentations with this one and try to keep Mirela and Ralph where they are right now. Because I think everybody's interested in talking about the user thing.

So let them finish and then you pick up and keep going after that. Can you do that, or is it essential you do this before Mirela's presentation?

MR. BAJOREK: No. This is -- maybe if you'd let me get to the next three or four slides.

CHAIRMAN BANERJEE: Okay. Go through -- let's do this: Give you until 6:00 to finish this off.

MR. BAJOREK: Okay.

CHAIRMAN BANERJEE: All right?

MR. BAJOREK: Okay. In terms of the physical models and correlations and conservation equations, we're putting our highest priority right now on fixing the momentum equation. Okay. We've talked about that earlier. I won't go into that any more because we have a separate presentation to really go into some of the --

CONSULTANT WALLIS: You're assuming it can be fixed.

MR. BAJOREK: Assuming it can be fixed or shown not to have a dominant impact.

CONSULTANT WALLIS: Or at least you can explain the assumptions you made and why they're justifiable?

MR. BAJOREK: Yes. So we'll talk about that. But there's some other things that the peer reviewers of identified --

MEMBER SHACK: Now, are you including adding the extra fuel as part of this work, or that's a separate topic?

MR. BAJOREK: That's the next presentation.

MEMBER SHACK: That's the next presentation.

MR. BAJOREK: That's coming on later.

Okay. This is dealing with things which have been identified as errors.

Longer term priority, we'll be going back and looking at the comments that we've received on the physical models. Some of them are clarification and we'll deal with those as we fix up the documentation.

Perhaps the most important three are the comments we've received on how we deal with the direct contact condensation, post-CHF heat transfer and the CCFL model; how well it meshes with the momentum equation.

CONSULTANT WALLIS: Well, this report that you're going to put together on the peer review, many of the peers had lists of recommendations. Are you going to respond to each recommendation in some kind of formal way?

MR. BAJOREK: Probably not for this report. I don't think there's enough time to do that.

CONSULTANT WALLIS: Not for this report?

MR. BAJOREK: But we'll factor those into the continuing --

CONSULTANT WALLIS: So I think it's important that your response somehow matches up with points that they raised.

CHAIRMAN BANERJEE: You don't have to address them, but you could say we've decided not to address this comment, or whatever.

CONSULTANT WALLIS: Or something.

MR. KROTIUK: The way I envisioned that report is that, you know, there are obviously some items, as Steve mentioned, that have been or are being addressed. So those will be definitely identified. And then I was grappling with how to treat the items that we know we will address in the short term, medium term and long term --

CHAIRMAN BANERJEE: Well, some you may not address.

MR. KROTIUK: Some we may not address, right.

MR. BAJOREK: The ones that are listed here, the condensation and the post-CHF if we look at those comments and the presentation I didn't talk about where we look at the key deficiencies, those are hitting those deficiencies right on. They're problems that we see in condensation from ECCS, the ECCS bypass tests which are causing us not to get some of those experiments right. They are the same things which are causing us not to get the peak cladding temperatures correct in some of the reflood and CCTF tests. And we see those things as being comments that are very directly related towards deficiencies that we see in the assessment. We'll move those up in terms of priority and deal with those. Because we think that's going to make the code more accurate.

Others, you know, this model's too detailed, that's probably going to have to wait. We may do that eventually, but --

CHAIRMAN BANERJEE: Well, you've got 20 minutes to tell us later on how you want to address the peer review and user issues, right?

MR. BAJOREK: Yes.

CHAIRMAN BANERJEE: And then you've got another half an hour on the momentum equation.

Now if you can sort of take that colored presentation and work it so that you can make all three presentations, that would be -- can you do that?

MR. BAJOREK: I can talk real fast.

CHAIRMAN BANERJEE: No. Use less slides. Don't talk too fast. We can't follow you otherwise. Okay.

Are you done now or do you have another slide?

MR. BAJOREK: I think basically we've kind of covered everything.

CHAIRMAN BANERJEE: Good.

MR. BAJOREK: The high priority items, momentum, user guidelines, more longer term, the physical models and continuation of assessment.

CHAIRMAN BANERJEE: But what I suggested to you was when you come back to start talking again that you sort of weave or you can make the other presentation only show the more important slides. You can choose the slides that you want to show.

MR. BAJOREK: Yes. I can get through -- there's actually about four slides in there that--

MEMBER SHACK: At that point it's just us and him, so we can all --

CHAIRMAN BANERJEE: All right. Let's thank Steve and let's move on and let Mirela and Ralph give us -- boy, seeing you two guys together, that's great.

MS. GAVRILAS: Nobody's ever --

MR. LANDRY: This way we can strangle each other easier.

MS. GAVRILAS: We've already -- actually your questions already touched quite a bit of this presentation. So I think we're going to save some time.

I'm the Branch Chief in the Branch that actually developing the model that the NRO and the NRR uses, and using in their work.

So what I want to do is I remember in the letter that you wrote one of the two recommendations that you made was accelerate the introduction of TRACE in the regulatory process. So what I'm going to tell you is what we've done since you wrote that letter and what we plan on doing in the immediate future.

Since March 2007 we have contributed TRACE confirmatory calculations for ESBWR and we've generated the applicability report. That has been mentioned several times this afternoon.

We have used TRACE calculations to assist NRR/NRO in formulating RAIs regarding the large break LOCA topical review. The TRACE small break and large break LOCA decks have been used by NRR in the EPU for Browns Ferry, in the SER for the EPU.

And we've TRACE to support NRR and NRO in GSI-101 scoping analyses.

CONSULTANT WALLIS: Did these activities result in success.

MS. GAVRILAS: I've tried to put here things that have a precise regulatory output. For example, they're used in the SER. The formulation of RAIs informed by the TRACE. GSI specific scoping analyses, that also resulted in RAIs.

CONSULTANT WALLIS: They were found to be useful these activities?

MR. LANDRY: Yes. Yes, they were.

MS. GAVRILAS: These are all things that they --

CONSULTANT WALLIS: Can you give you an example of usefulness of these activities?

MR. LANDRY: Graham, when I go through some of mine I'll talk about some usefulness of it.

CHAIRMAN BANERJEE: Didn't we also use TRACE for Susquehanna?

CONSULTANT WALLIS: You did, yes.

MR. LANDRY: It was a RELAP calc, I think.

CHAIRMAN BANERJEE: No, it was --

CONSULTANT WALLIS: Well, maybe it was NRR who did that.

MS. GAVRILAS: I don't think so. If you want any clarification --

CHAIRMAN BANERJEE: No, no, no.

MS. GAVRILAS: Then how pressed you are for time, would you like Len to tell you specifically what he's used in the SER for Browns Ferry, for example?

CHAIRMAN BANERJEE: That would be interesting. We always have time for that.

MS. GAVRILAS: Dr. Ward? Dr. Ward, can we have two minutes of your time telling us what you actually ended up using in the SER?

CHAIRMAN BANERJEE: Len, you have to come and identify yourself and speak into a mike.

DR. WARD: Okay. Yes. Len Ward, NRR.

I was looking at the Browns Ferry EPU, and in particular was concerned with small break LOCA because small break LOCA was limiting for that plant. And one of the vendors was calculating a pretty low temperature, and I wanted to try to understand that. And they were about 400 or 500 degrees below the RELAP result and the TRACE result.

The TRACE result and the RELAP results were consistent also with the results calculated by GE for their fuel. But one vendor was 400 or 500 degrees lower and I wanted to try to find out what the reason for that was. And it turned out the CCF model, the correlations and some of the modeling techniques weren't conservative and TRACE and RELAP confirmed an area where it was nonconservative.

The EPU was approved, though, based on the conservative calculations that I did with TRACE and RELAP in view of the fact that I didn't agree with one of the vendor's models. But the purpose of the review was not to get in and figure out exactly what was wrong with the models.

PARTICIPANT: (Off microphone).

DR. WARD: Well, I'm getting ahead of myself. The calculations showed that we would support the EPU even though we had questions with one of the models based on the analysis we did with TRACE and

RELAP.

CHAIRMAN BANERJEE: Did you do any Appendix R calculations?

DR. WARD: No. No. Time to just look into small break LOCA. And I looked at about seven or eight breaks, just generated the whole small break LOCA spectrum, 05, 06, 07, 08.1 in that neighborhood. That's where the limiting break would be a Browns Ferry type plant with that power uprate.

CHAIRMAN BANERJEE: And did you use a fairly close nodalization?

DR. WARD: No. I made changes to the code. I put in 24 axial cell, put in a -- a hot rod. I changed CCF correlations to be more appropriate for a flooding for hot bundles based on test data.

What else did I do? I think I made some other changes also.

CHAIRMAN BANERJEE: And how easy was it for you to make these changes?

DR. WARD: Well, some of them were easy and some of them weren't. Well, it's like putting in a fire shape? You know, that was a little difficult. I couldn't used SNAP because for some reason it crashed my computer. So having a tight schedule I had to -- I just modified the input directly. But it was a little -- some of the things were a little difficult to do. But, you know, I managed.

You know, the code ran. I didn't have-- I think it only bound -- it only failed once, but that was probably an input error or some error I had a made, which is normal course of running a code like that. But I was pleasantly surprised that it did quite well. I mean, it ran.

And I checked the two face level swell for the limiting break and took the mass and drift reflux model and did a hand calculation and swelled it, and we got pretty close to what TRACE and RELAP was predicting. So, you know, I was pretty happy with that.

I haven't had a chance to do a lot of review of all the other models, but I was happy with the way the CCF model was working because the calculations that I was doing with RELAP and TRACE was precluding all down flow of liquid into the hot bundle. The vendor calculation was allowing a lot of down flow and that's why they were getting a low temperature. So, that was one of the reasons.

CHAIRMAN BANERJEE: Which vendor is this?

DR. WARD: Well, we don't -- we don't need to go there. But anyway, the code it provided some good support to give the vendor some feedback as to what he might expect the next time we see another power uprate.

CHAIRMAN BANERJEE: Okay. Thanks, Len.

MEMBER ABDEL-KHALIK: Having modified the code to such an extent would you consider yourself an average user of TRACE?

CHAIRMAN BANERJEE: Len, you're not off the hook.

DR. WARD: No. I'm just scratching the surface with it. I've been -- when you're working on three or four power uprates at a time and a couple of other issues, you just don't have a lot of time to sit around and run codes. But my next plan is to take a PWR, like a Millstone 2 which has a pretty high p-clad temperature Appendix K space and run the entire spectrum with TRACE and compare it to RELAP. And then look at models like condensation, counter-current flow in the steam generator's leak seal behavior, which is very important to predicting a large break -- a small break LOCA. You know, level swell, particularly two phase level swell, I want to look at it in more detail. And the heat transfer package.

MEMBER ABDEL-KHALIK: I think you misinterpreted my question.

CHAIRMAN BANERJEE: He's asking a different question.

DR. WARD: Okay. I'm sorry. What?

MEMBER ABDEL-KHALIK: My question is you made a lot of changes to the code to be able to use it.

DR. WARD: Oh, sure.

MEMBER ABDEL-KHALIK: And do you think an average user would have been able to do the same?

DR. WARD: Well, an average user? I would hope that they would, yes. An average user should be able to do that. I mean, that code's a little bit more difficult to use than, say, a RELAP code or a RETRAN code or the old -- the older codes, the flash series, the RELAP4 series; those are pretty easy to use.

But, you know, I think that if they understand the phenomenon and they have run other codes, they should be able to do it.

MEMBER ABDEL-KHALIK: Thank you.

MS. GAVRILAS: Can I ask for a clarification?

Are you asking about changes to the input model or to the code itself? Because as far as I know, Len, you didn't make changes to the code, did you?

DR. WARD: No, not to the code. Just the input model. I mean, I wasn't modifying any of the physics in the code. Just the input.

MEMBER ABDEL-KHALIK: Thank you.

DR. WARD: Okay.

MS. GAVRILAS: A couple of weeks ago we received -- we filed a response to an NRR user need covering the balance of this fiscal year and going into 2010 actually, and we have nine decks that we're preparing for them to support their EPU reviews. Including the decks that we've already prepared, they will cover BWR/3, 4 and 5, Westinghouse 2-3-and 4-loop, CE and B&W lowered loop design.

There is an NRO pending user need. We received a draft of that user need, and in it we're asked to provide support for the confirmatory calculations of the ESBWR. Under that we're also being asked to extend to go beyond LOCA, go to AOO and upper plenum instability.

We're going to support the EPR topical report reviews and DCD confirmatory calculations.

We're preparing LOCA audit calculations and transients for them. And we're developing an applicability report.

US APWR, same thing. We're going to

support DCD confirmatory calculations for LOCAs and transients. We're developing the applicability report and we're supporting the advanced accumulator modeling features.

CONSULTANT WALLIS: So it looks as if NRR is at least beginning to use TRACE and ask you to provide the wherewithal to make it useful.

MS. GAVRILAS: That's what it looks like from where I'm sitting.

CONSULTANT WALLIS: This may be in response to a ACRS recommendation --

MS. GAVRILAS: I believe it is.

CONSULTANT WALLIS: -- that TRACE should become the working code for the NRC.

MS. GAVRILAS: We're climbing up that hill.

Same thing --

CHAIRMAN BANERJEE: Is it a hard hill to climb?

MS. GAVRILAS: For AP-1000.

Sorry?

CHAIRMAN BANERJEE: Is it a hard hill to climb?

MS. GAVRILAS: It's a steep learning curve, sure. Yes. I mean, this is an ambitious program for a couple of years. So many -- types, so many applications. Yes.

CONSULTANT KRESS: A vendor comes in and says his calculation is a 95/95 best estimate as opposed to Appendix K. Can you do that with TRACE?

MS. GAVRILAS: No.

CONSULTANT KRESS: But eventually you expect to?

MS. GAVRILAS: We're working on that. That's one of the top development priorities to actually put in the features that will allow us to do uncertainty calculations --

CHAIRMAN BANERJEE: How long will that be?

CONSULTANT KRESS: So for these user needs you've got to check and see whether they're coming in with Appendix K or --

MS. GAVRILAS: For these user needs we're developing -- right now what we understand under confirmatory calculations largely is a best estimate calculation. And if you have that 300 margin, good enough. If we don't, then we'll have to start --

CONSULTANT KRESS: By "best estimate," you mean to be just --

MS. GAVRILAS: Just a nominal value of best estimate. Yes.

So this is the table --

CHAIRMAN BANERJEE: Let me get clear.

Tom I thought asked you whether you could do best estimate with uncertainty right now.

CONSULTANT KRESS: But she was referring to a different best estimate. She's talking about a firm estimate using the best choice as they can for the parameters.

CHAIRMAN BANERJEE: Oh, okay. Okay.

CONSULTANT WALLIS: But you could do it now, couldn't you?

MS. GAVRILAS: We actually --

CONSULTANT WALLIS: With a lot of tedious handwork, you could vary things --

MR. LANDRY: No. What Mirela is talking about is the code today is a best estimate or a realistic goal. The degree of uncertainty has not been determined yet.

CONSULTANT WALLIS: Oh, it has not been determined.

MR. LANDRY: When we see a code calculation coming from a vendor that is termed "realistic analysis," they have to determine the realistic analysis and they have to assess the uncertainty.

CONSULTANT WALLIS: Right.

MR. LANDRY: So what they report is a 95th percentile p-cladding temperature as determined from their uncertainty analysis of best estimate code. We have not done that with TRACE to date.

CONSULTANT WALLIS: Yes. So you have to take something like a correlation of the CHF and put the uncertainties on that, that is what is being done?

MR. LANDRY: Well, determining the uncertainty is a long drawn out process. Because you have to determine uncertainty in particular models in correlations, you have to determine biases in the code, you have to determine the overall uncertainty that is inherent in the code. It takes a long time, it takes a lot of analyses to do that.

CONSULTANT WALLIS: Isn't this done by taking the uncertainty in each element that goes into the code and then --

MR. LANDRY: That's part of it. You determine a number of uncertainties and you determine overall uncertainty.

A couple of this have been talking about this. Steve Bajorek and I have been talking about how to approach doing an uncertainty analysis of the TRACE code. And with all the other work that the two of us have to do, that keeps getting pushed off. One of these days we're going to have to actually do it.

CONSULTANT KRESS: Are you talking Monte Carlo or are you talking --

MR. LANDRY: There are a number of different methodologies. And I don't know if this is the right forum to go through all the different statistical approaches that can be taken. But there are a number --

CONSULTANT KRESS: Are there any statistics you --

MR. LANDRY: There are a number of uncertainty approaches that can be taken and have been taken by the industry. There's one that we're dealing closely with, IRSN, the French regulators on which is methodology that they have developed. And they've published a number of papers in the open literature on the methodology. It's very, very good. It's a very powerful method. And we're looking very closely at what they're doing and we're working cooperatively with IRSN right now on uncertainty methodology.

CONSULTANT KRESS: That would be computationally efficient for you guys?

MR. LANDRY: I'm sorry, Tom?

CONSULTANT KRESS: That would be

computationally efficient for you guys? You can't really rely on Monte Carlo.

MR. LANDRY: Most of these techniques use Monte Carlo at some point. But this is a method that in its total finalization ends up taking about 500 plus calculations to do the total uncertainty analysis.

CONSULTANT KRESS: Well, that would be all right.

MR. LANDRY: That's a long way from the concept of 59. But it's a very, very powerful tool. And we're communicating very regularly with IRSN and the French regulators on this methodology and its use.

CONSULTANT KRESS: It's just a stratified Monte Carlo?

MR. LANDRY: Well, Monte Carlo's a piece of it. It goes far beyond what has been traditionally known as the Wilkes' method or the nonparametric order-statistic methodology. It's a methodology that uses a technique called bootstrapping and a number of advanced statistical methodologies to do the analysis. And you end up with a very broad spectrum of analyses, a lot of data points and you reduce the uncertainty in the overall analysis considerably. It results in about the uncertainty that the Wilkes' method will result in.

CONSULTANT KRESS: I think the ACRS would be interested in hearing about that.

MR. LANDRY: That would be a long presentation. I'd rather not get into that right now because a very lengthy --

CONSULTANT KRESS: It's not part of this particular discussion.

MR. LANDRY: It's a very lengthy, very mathematical explanation.

CONSULTANT WALLIS: One of the specifications for TRACE should be that they can evaluate uncertainty. But you can't really do a best estimate code without doing uncertainty. So it's got to be a requirement in TRACE that it can.

MS. GAVRILAS: And right now we don't even have the right nobs to the put the multipliers to carry out the Wilkes' method type of uncertainty.

CONSULTANT WALLIS: So you're way behind some of the vendors, aren't you?

MR. LANDRY: There's a difference. The regulation says that if you come in with a LOCA analysis, you have to either come in with a realistic analysis with a determined uncertainty or you must come in compliant with Appendix K.

CONSULTANT WALLIS: Yes.

MR. LANDRY: There's no regulation that requires the NRC staff's confirmatory tool to have assessed --

CONSULTANT WALLIS: So in other words you're outdone by the industry?

MR. LANDRY: No. There's a different purpose, Graham. The industry is licensing their nuclear reactors --

CONSULTANT WALLIS: They're doing something that you can't do, right?

MR. LANDRY: They are licensing a reactor on the basis of this analysis. We are performing an analysis to confirm what they have done.

CHAIRMAN BANERJEE: But can you confirm their uncertainties?

CONSULTANT WALLIS: Yes?

MR. LANDRY: Well, to date no.

CHAIRMAN BANERJEE: I hear that tomorrow we're going to see a best estimate of uncertainty.

MEMBER SHACK: Well, we'd better move on.

CHAIRMAN BANERJEE: Anyway, carry on.

MS. GAVRILAS: This is the table that I was talking about. And as you see, we have just about every plant on it. There's a separate review table that has proprietary information. I'm sure you have access to that.

One of the questions you were asking of Len and we hinted at it, our execution times are still issues. We clear them up in some ones, but we establish targets for ourself when we develop a small break LOCA or a large break LOCA, that these are the target execution times for us.

CONSULTANT WALLIS: They're dimensions?

MS. GAVRILAS: Yes. They're case execution time over problem time.

CONSULTANT WALLIS: Okay.

CONSULTANT KRESS: Oh, I see. That's the problem area.

CONSULTANT WALLIS: What are the units here?

MR. LANDRY: The dimensions.

MS. GAVRILAS: Time over time.

MR. LANDRY: Will you say again.

MS. GAVRILAS: Units of second per second.

MR. LANDRY: One is real time. Application time over real time.

CONSULTANT WALLIS: What's this mean? Over real time?

MS. GAVRILAS: Yes, over transient time.

So if the transient is ten seconds long and we say that it's going to execute in one, that means it's going to execute in ten seconds on a typical agency computer, which is not a very --

CONSULTANT WALLIS: So to do the ESBWR 72 hours. You're accepting taking 72 hours or longer to do it?

MR. LANDRY: Pretty much. Pretty much.

MS. GAVRILAS: There's nothing we can do about that. I'm just saying that's our target.

MEMBER CORRADINI: Let's be careful about what you're saying, Graham. You're comparing it to an ESBWR calculation where that was a fed in set of boundary conditions to a containment calculation. I'm sure that's much faster than real time.

CONSULTANT WALLIS: I would think it would be much faster than real time. Yes. I would hope so.

CHAIRMAN BANERJEE: We've really got to move on.

MS. GAVRILAS: And, of course, as you get down to shallow portions of the transient, you can accelerate it.

Within the next couple of years the staff will be developing decks that represent every family of currently operating plants.

CONSULTANT WALLIS: Why is this a figure of merit? I would think a figure of merit would be the number of runs that the agency needs to do and how much time it has to do it rather than what it's got to do with real time.

MR. LANDRY: It's a figure of merit because it's a figure of merit that back when I was still in NRR we established. We were concerned that the code was taking an exceptional length of time to run, and that was one of the reasons nobody wanted to run TRACE. They wanted to run codes quickly. So we said okay, we want to see the code have a capability to run in a time comparable to what we expect from RELAP if it's set up like RELAP.

So that was an agreement that back in the NRR days that we worked out with RES to set a figure of merit for a fast running model on TRACE to be comparable to that model run on RELAP. Now there's not one-to-one because the codes have very different capabilities. But we wanted to be on that order of magnitude.

We did not want a code or a problem that would run on RELAP in an hour to take ten days to run on TRACE, because we weren't going to use TRACE.

CONSULTANT WALLIS: I understand. So this is TRACE versus RELAP? I thought we --

MR. LANDRY: No. No.

MS. GAVRILAS: No. It's actually in fact--

MR. LANDRY: We've used a merit that we termed those that would make TRACE a useable tool.

CONSULTANT WALLIS: But don't you have to look at what the user needs? I mean, some managers got some new question about some phenomenon, you know how sensitive p-clad temperature to X. He wants to go home and run his PC and get sort of 50 calculations about sensitivity and get an answer. That's what he wants to do. He doesn't want to compare with how long it takes the accident.

CHAIRMAN BANERJEE: But I guess then these are targets which are set to try to answer the type of question you're asking. So if they get it in sort of real time --

CONSULTANT WALLIS: But the real question, though, for this to be really useable by NRR how fast does it have to be?

CHAIRMAN BANERJEE: So then you can that directly.

CONSULTANT WALLIS: That's the real question.

CHAIRMAN BANERJEE: That this is coming-- this is developed in discussion with NRR, right, or NRO?

MS. GAVRILAS: This is developed jointly, yes.

CONSULTANT WALLIS: Okay. It's a problem time? I thought problem time was real time of the accident. Did I misunderstand.

MS. GAVRILAS: That's what it is.

CONSULTANT WALLIS: It doesn't seem to me that's -- that's not a good figure of merit. The figure of merit should be what's the expectation of NRR about how quickly they can turn around an answer.

MS. GAVRILAS: That's --

CHAIRMAN BANERJEE: That's factored in here.

CONSULTANT WALLIS: Right.

MS. GAVRILAS: We'll edit that.

I think that, as I mentioned earlier, we have a BWR sample deck which means that we are now preparing other BWRs. We have something that we can give to a modeler to use as an example. We're still working on getting the PWR sample deck, one that's sufficiently robust.

And several of you have mentioned it, the peer review have mentioned it, this is something that we're dealing with everyday, it's clear to us that at this point working on the user guidance is as important as developing the code further.

MEMBER CORRADINI: And just to repeat what you said earlier, just so I remember right, Mirela, the still working on PWR is because the size of the problem is bigger or because when you're doing large break LOCA you have more phenomena being called upon within the code, or some combination of that? I'm still trying to understand that. I'm sorry.

MS. GAVRILAS: I think it's more the latter. It's more challenging to the code. It's also bigger so you can have more problems in more places. But I think it's more the latter. Just more challenging for the code.

CHAIRMAN BANERJEE: So you don't have a sample deck currently for PWR decks operable?

MS. GAVRILAS: We have one, but it's sort of -- it's a Westinghouse 412, and we've used it to play with it. It's not the real plan. I will tell you we're trying to make the EPR the same deck for it; that's what we're after. Right now we're working to make the EPR the sample deck for PWR.

CHAIRMAN BANERJEE: Well, what happens

with all the power uprates that you're having coming in?

MR. LANDRY: RELAP.

CHAIRMAN BANERJEE: You use RELAP?

MS. GAVRILAS: No, no. No.

MR. LANDRY: No. If you look back at the table, one of the tables that Mirela has that shows all the decks, now that table shows you decks that are available if you look on the far right corner.

CHAIRMAN BANERJEE: Right.

MR. LANDRY: Those decks that are available are decks that when we sent out through what was at that point the Thermal Hydraulic Technical Advisory Group, which is NRO, RES and all the different offices working together, we laid out what we wanted to see in the way of TRACE decks set up that we could have to use for an analyses if we needed them. And we realized that we could not have a deck for every plant. Not in this lifetime. So we set up and said okay what are the most important decks to have first. And we said well Browns Ferry looks like the more common BWR type. Calvert Cliffs we wanted a CE plant. We wanted Oconee because we wanted a B&W plant. We wanted a 4-loop Westinghouse plant, we wanted a 3-loop Westinghouse plant. And those were the decks that we prioritized as first priority.

CONSULTANT WALLIS: And those are available?

MR. LANDRY: Yet they weren't then.

CONSULTANT WALLIS: Yes, but they are now?

MR. LANDRY: They are now. This is going back a period of time to when we met and agreed to what decks we wanted first as a user office.

After that, we then started seeing EPUs come in and said wait a minute, we need other decks available. We don't need a different deck now. So we redid some prioritization and you see those that are being developed in '08. Those are decks that we put at a higher priority; that we wanted those decks next. So this was a process --

CHAIRMAN BANERJEE: When you say Robinson is available, that means that you can use it for assessments --

MS. GAVRILAS: What that means in this context, it means that we have a deck right now. But it's not the deck that Research would hand over to Ralph to run. It's not sufficiently robust.

CHAIRMAN BANERJEE: Okay.

MS. GAVRILAS: So we still need to work on it until --

MR. LANDRY: And this is a process.

CHAIRMAN BANERJEE: Sure. At least it was clarified.

MR. LANDRY: This is an actuation of how we're getting to the point of having the ability to use this tool.

MEMBER CORRADINI: That's fine.

MR. LANDRY: One of our complaints have been we can't TRACE because we don't have any decks.

CHAIRMAN BANERJEE: Right.

MR. LANDRY: So we sat down with Research and said okay, what do we have to do to get some decks so we can use this tool.

CHAIRMAN BANERJEE: So this is a very substantive program --

MR. LANDRY: We tried to put a logic into what decks would go first because the answer of all of them is not an answer.

MEMBER CORRADINI: Yes, I got it.

CHAIRMAN BANERJEE: Okay. So, Mirela, are you done?

MS. GAVRILAS: I'm done.

CHAIRMAN BANERJEE: Let Ralph speak.

CONSULTANT WALLIS: Well, it looks very ambitious and very useful. I just hope you have enough resources to make it happen.

MS. GAVRILAS: We haven't had problems yet with resources. I think everybody's taking it as serious.

CHAIRMAN BANERJEE: I think we need to write that in our letter that you don't need any more resources?

MS. GAVRILAS: No. What have I done? Where have I gone wrong?

MEMBER CORRADINI: They're just in a feisty mood. It's 6:00, that's all.

MS. GAVRILAS: All right. Right.

MEMBER SHACK: IT's 6:30.

CHAIRMAN BANERJEE: Now why did you abandon Ralph? Why did she abandon you?

MR. LANDRY: Because she wants to throw me to the lions.

Okay. I'm Ralph Landry. I'm a Senior Level Advisor in the Office of New Reactors.

We just went through a little bit of the overview of the user needs that have been evolved from NRR and NRO for deck development and some of the logic behind the development of those decks. Rather than talk about those decks in particular, I'd like to cover a couple of points that come up in workings with the code. And one point addresses an issue that is raised by the peer review group.

If I can put a positive spin on the error in the code, which is not something people like to do very often --

MEMBER CORRADINI: But you're going to jump right in?

MR. LANDRY: I'm going to jump into the lion's den and try to put a positive spin on the error in the code.

The momentum formulation we've discussed quite a bit. Marv made a presentation and Steve and John are going to both make presentations dealing with the momentum formulation.

This has provided us with a tool to go into the codes of the vendors. Now, this formulation of momentum doesn't originate with TRACE. This problem originated with TRAC. It originated with TRACP and with TRACB. Well TRACB is the basis for the General Electric TRACG code. So we reasoned if this error is in TRACE, it's in TRAC, it must be TRACG also.

So we've worked with the Office of Research. And Research put together a little test problem. It's a very simple problems, it runs in a matter of seconds and in fact can be calculated by hand. We talked with our friends down in Lynchburg -- or Wilmington -- in Southern Virginia. These down in Southern Virginia and we told them that we were sending them this test problem and we wanted to run it.

We haven't gotten the results back yet. But we want to talk with the General Electric people and see the magnitude of this error in a licensed code.

Now we suspect that the error isn't going to be large in magnitude, but nonetheless if it's approved code for licensing purposes, you have to correct the errors. So we have provided this sample problem to General Electric.

We have also talked with Westinghouse and we're going to provide this sample problem to them because they use WCOBRA TRAC, but they use the code because they've linked with COBRA. They use it in a different fashion, and this problem will probably not appear. However, we let them run it away. We want to be sure.

And we're going to send the problem to the people in the other part of Southern Virginia, down in Lynchburg even though they use a derivative of RELAP, the S-RELAP code. We want to be perfectly fair to the whole industry. They all have the opportunity to run the problem.

CONSULTANT WALLIS: But all codes have a problem of taking a vector equation and turning it into a scalar equation.

MR. LANDRY: But these are the approved code. And the approved codes are all going to get the opportunity to run this sample problem. It's a very short problem, runs very quickly. But we want to see if they have an error because of --

MEMBER ABDEL-KHALIK: How was the sample problem selected?

MR. LANDRY: How -- I'm sorry?

MEMBER ABDEL-KHALIK: How was the sample problem selected?

MR. LANDRY: It was a problem that was created by the Office of Research.

Steve?

MR. BAJOREK: After we started looking at the momentum equation and its issues last year, we came up with a series of test problems to look TEE flow splits and also some to look a 3-D vessel components. We picked these problems so that they had textbook solutions. We could go to Idle Check or some handbook and say this is what the pressure drop should be for this flow.

We've been using those to test out the TRACE code. One in particular gave us a problem and looked very much like a vessel geometry, so we focused on that one. We were convinced there was an error associated with TRACe, and that's when we talked with NRR and NRO about how prevalent is this. Because we were convinced that the error predated TRACE itself and went back to TRAC and potentially the other industry codes.

MEMBER SHACK: How does that problem --

CHAIRMAN BANERJEE: This is not the T problem? This wasn't the T problem?

MR. BAJOREK: It was not the T problem. No, this was --

CHAIRMAN BANERJEE: What was the problem, Steve?

MR. BAJOREK: This was the problem looking at a 180 degree turn of the flow as it comes down a downcomer and goes up through the lower plenum into the core.

The sample problem is one of a concentric tube where there is some handbook solutions to that. We set up a geometry to get us very close to that picking dimensions and velocities which aren't too dissimilar from what you'd see in a plant. We found that TRACE was grossly overprotecting the pressure drop that you should get for that type of geometry. So we passed that along to our friends everywhere.

CHAIRMAN BANERJEE: Now this T problem is a sort of an interesting problem, isn't it?

MR. BAJOREK: Yes.

CHAIRMAN BANERJEE: Maybe you should think of that, too.

CONSULTANT WALLIS: Especially when the flow is going out of tube legs.

MR. LANDRY: Well, right now --

CHAIRMAN BANERJEE: At the injection point for emergency cooling systems.

MR. LANDRY: At this point we have this simple problem. It's been done on TRACE. We've run it on TRACB. And we're waiting to talk with the vendors to determine how the problem runs on their individual codes also. But this is a way in which the tool TRACE has provided a positive feedback to us.

We found an error here, and we reasoned its existence elsewhere. So it has had the positive effect on the regulation that it's given insight into a wave related code.

MEMBER ABDEL-KHALIK: If your logic is correct, then there is no reason to expect that the vendor codes would do any better than TRACe.

MR. LANDRY: Right.

MEMBER ABDEL-KHALIK: And if that is the case, what would be the next step?

MR. LANDRY: They'd correct their codes.

CHAIRMAN BANERJEE: Can they be corrected?

MR. LANDRY: We will determine that.

CHAIRMAN BANERJEE: I mean, can you correct TRACE?

MR. LANDRY: Well, we'll get to hear-- if I ever get done.

CHAIRMAN BANERJEE: Okay.

CONSULTANT KRESS: Would you specify in the thing about the use of these codes? For example, if you have an analytic solution to this problem, they probably have one, too.

MR. LANDRY: Right.

CONSULTANT KRESS: And if I weren't constrained, when you figure out a way to make my code give me a --

CONSULTANT WALLIS: That's cheating.

CONSULTANT KRESS: Well, did they constrain you --

CHAIRMAN BANERJEE: The problem is they'd find out.

MR. LANDRY: The problem is it's a noding diagram, it's very defined. Even though you don't like to put a positive spin on a code error, this is a way in which there has been a positive result from using this tool.

MEMBER CORRADINI: We say that in universities it's a learning experience.

MR. LANDRY: Yes.

CHAIRMAN BANERJEE: Now give us a negative result.

MEMBER ABDEL-KHALIK: Would an acceptable response by the vendor be to restrict the use of the code to geometries of this type?

MR. LANDRY: We are not trying to preclude what their resolution is going to be.

We provided the sample problem for them to run and we want to see what their results are. And then hear what their proposed resolution is.

MEMBER ABDEL-KHALIK: But if that is an acceptable solution, then the whole process seems sort of fallacious. Because you have to identify each and every geometry that the code would fail.

MR. LANDRY: Well, they have to. We don't have to. All I'm saying of the code is this is a place where we found a problem and we have found a way to use this problem in the regulatory arena.

CONSULTANT KRESS: What did you specify in this problem? Just go through a set of different flows and calculate the pressure drop?

MR. LANDRY: Well, the problem gives them a geometry, a loading. It gives them a flow in pressures. As Steve said, you can hand calculate this.

CONSULTANT KRESS: Giving a flow and an inlet pressure?

MR. LANDRY: Yes.

CONSULTANT KRESS: Several of them?

CONSULTANT WALLIS: You can calculate the two phrase flow or no?

MR. LANDRY: No.

CONSULTANT WALLIS: Single phase.

CONSULTANT KRESS: Oh, this is just single phase.

CONSULTANT WALLIS: It's just single phase.

MR. BAJOREK: I'll show you the geometry, you know, when we have the other presentation. But it's something that the geometry is very simple. No two phase flow. Try to make just as simple as possible in code. Should be able to do the hand calculation. That's what we've imposed on ourselves.

CHAIRMAN BANERJEE: I think we should move on, Ralph.

MR. LANDRY: Okay. The other point I'd like to discuss is the use of TRACE for the GSI-191 issues.

CHAIRMAN BANERJEE: Right. I think most of the Committee is familiar with that.

MR. LANDRY: Most of the Committee members have been involved in the recent --

CONSULTANT WALLIS: In fact, you were one of the presenters.

MR. LANDRY: The wounds have healed.

CHAIRMAN BANERJEE: I'm glad to hear that.

MR. LANDRY: I know you'll rectify that situation.

CONSULTANT WALLIS: And love it in that position?

MR. LANDRY: Well, I'm still involved in that. I promised I would stick it out.

When we presented the results of analyses which we had been doing trying to resolve the GSI-191 issue the concern was that we were using a model for the Westinghouse plant and that instead of having the full floor area into the bottom of the core, we took a 95 percent reduction in the floor area. And the floor area was just a small sector out of the bottom of the core.

And the concern was well is this going to artificially accelerate the flow through that small sector and allow the flow to move through the core and provide artificial additional cooling throughout the core.

So we went back and asked Bill Krotiuk who did the first analysis for us in Research to instead of having one five percent opening for the entire core area, have distributed five percent openings for every bundle so that each bundle had only a five percent floor area introduced it.

Well, Bill did that. And then he and Steve Bajorek put their heads together and said well why don't we take advantage of some other features of TRACE and so some other modeling with TRACE and have a porous medium as the introductory plane to the core instead of a plane where you have solid plate with an opening in it, have the entire core entrance as a porous medium. And we consider the pressure drop that you would have through a porous medium so that you'd get a pressure drop equal to only five percent that you would have normally.

So they did that analysis also. That analysis took a considerable length of time. This weekend project had finally gotten done with. That was --

CHAIRMAN BANERJEE: And what did you find?

MR. LANDRY: That was about six months ago they started.

But they found that when you used the core with a single five percent opening, a core with distributed five percent openings and a porous medium you're going to get almost no change in the p-cladding temperature between those three cases.

CONSULTANT WALLIS: It doesn't matter how the water gets in?

MR. LANDRY: With any of those three you'll get the same temperature PCT-wise or the five percent flow. So whether you have one slot opening, distributed openings or porous medium you get approximately the same PCT.

And none of the PCTs would exceed what we have set as the limit for the second heat up of the core.

CHAIRMAN BANERJEE: Except if you made

it.5 percent or .05 percent?

MR. LANDRY: The details of this we'll discuss with you when we come back with this 191 resolution proposal. But at that point we'll go through the report that Research has prepared and discuss the results with you.

But when they did this report they also did a hand calculation to demonstrate the flow through the porous medium. So all that material will be provided to the ACRS.

So conclusion from the perspective of a user office, we've seen the codes used in support of operating reactors, power uprate issues. We've seen the code in support of new reactor design reviews --

CONSULTANT WALLIS: The first one is interesting because it means that we need more code errors?

MR. LANDRY: We don't propose to find a lot of code errors. We don't propose that the developers --

CONSULTANT WALLIS: They're useful for us. They're useful.

MR. LANDRY: We don't propose that the developers put errors in the code. But we found the code error that did have a positive result in that it gave us insight into the regulated codes.

The code flexibility that we've seen with TRACE has allowed us to model phenomena such as the porous medium flow resistance as an inlet to the core.

Throughout all this work in support of NRR and NRO we've seen very responsive status and stature from the Office of Research. As a user office we've been very pleased with the relationship with the Office of Research and with their responsiveness to our needs.

CHAIRMAN BANERJEE: So now why did you say she was going to throttle you or you were going to throttle her?

MR. LANDRY: Because she didn't know exactly what I was going to say at the end.

MS. GAVRILAS: And, you know, old habits die hard.

MR. LANDRY: We've known each other too many years to trust each other that much.

MEMBER CORRADINI: IS that for the record?

CONSULTANT KRESS: Are those finger marks on your neck?

MR. LANDRY: That's from the rope that was around it earlier.

CONSULTANT WALLIS: Well, the last bullet is always true, isn't it?

MR. LANDRY: Yes.

CONSULTANT WALLIS: The difficulty sometimes is getting the user office to ask for the right thing.

MR. LANDRY: We've had a very close relationship with Research, especially in the usage of TRACE. We've been working very closely with each other, hearing what our needs are and then come back and then we change our needs and they respond very quickly. They've been creative in here's a need for GSI-191. They get creative and say well why don't we do this, too. So from our perspective the relationship has been very good.

CONSULTANT WALLIS: I think that's good from an ACRS perspective. Because what we were hearing a few years ago was less positive. This sounds very good.

MS. GAVRILAS: It's an iterative process because we are on that steep curve. So what they want, we can't always do. Unfortunately, they know enough to adjust their expectation.

CHAIRMAN BANERJEE: All right. Thank you, guys.

Let's have the next -- is Steve back on

now?

MR. BAJOREK: I'm back on.

CHAIRMAN BANERJEE: Okay. Thanks.

MS. GAVRILAS: We're off.

CHAIRMAN BANERJEE: Okay. Steve, you've got half an hour to finish everything.

MR. BAJOREK: I have a half an hour. Okay. That's easy to do. Harder is to find it.

CHAIRMAN BANERJEE: Now just before this, before everybody vanishes, we have to write a letter in September. And in some way we need to decide, you know, today for the Committee members as to what sort of guidance we should give the full Committee, what the full Committee might want to hear. Keep that in mind while we're talking.

CONSULTANT WALLIS: And you're going to have a full Committee meeting in September of which choice is presented, or just the peer reviewers presented?

MEMBER SHACK: Will we have Bill Krotiuk's report by then?

MR. KROTIUK: The report is finished. It's a question of when we will officially release it.

CHAIRMAN BANERJEE: This is a point maybe to discuss. Could we address just the peer review or should we address basically Farouk's letter and our last letter? Because our last letter made some recommendations and they've responded to those. What should be the limitations on what we write this time? Because there were four points under Farouk's letter that we wrote also. One of them was the peer review. The other was to go forward with all these, you know, things to get into the regulatory process. The third was the documentation. I think the fourth point was, if I remember now, well it was more related to the current status of TRACE, it had to be frozen and documented and stuff like that.

So now do we deal with all those issues in the full letter or do we just deal with the peer review?

MEMBER CORRADINI: Peer review.

CHAIRMAN BANERJEE: I mean, it's a lot easier to do just the peer review. It depends on they inform us about.

MEMBER SHACK: Well, I mean today we've heard about the interaction with the users also. I think that's fair to talk about. Certainly the peer review is fair to talk about. And at least from my point of view the documentation is a whole lot better than the last set of TRACE documentation I ever saw.

CHAIRMAN BANERJEE: If you received it?

MEMBER SHACK: Yes, we've received and it can be improved, but compared to what there was before --

CHAIRMAN BANERJEE: At least one order of magnitude.

MEMBER SHACK: Yes. I mean the last one we had was a sort of hand-me-down documentation from a previous life.

CHAIRMAN BANERJEE: So I guess the guidance we should give them about the presentation--

MEMBER SHACK: I think you should probably focus on the peer review and the interactions. I think -- you know, they're clearly going to be doing lots of work on the documentation. Kind of a universal agreement that this is not where they're at. But I think the peer review and the interaction are probably the things of most concern to us I think at the moment.

CHAIRMAN BANERJEE: And the most concern to us is that it gets into the regulatory process.

MEMBER SHACK: Well, that's it's getting the right answers is handy, too.

CHAIRMAN BANERJEE: Right. I believe that it should get the right answers. But then maybe nobody's getting the right answers.

Okay. Steve, now you've only got 20 minutes.

MR. BAJOREK: Okay.

CONSULTANT WALLIS: Are we supposed to hear from John Mahaffey as well? Because if we get into that, that may take the whole hour if we get into that.

CHAIRMAN BANERJEE: It's not on the agenda, is it?

CONSULTANT WALLIS: It's not on the agenda at all.

MR. BAJOREK: The last addition for 6/10 was to deal with the momentum equation.

CHAIRMAN BANERJEE: Right.

MR. BAJOREK: Now if you would like what I would do, since there was a request to look at some of the assessment, I will go through this assessment summary and as Dr. Yadigaroglu did, I'm not going to focus on the good. I want to go to the things which are really very bad.

So in all of the assessment if we go through and take a look at it, we do many of the --

CHAIRMAN BANERJEE: Can you make it bigger?

MEMBER SHACK: Yes. Hit your F5 key.

MR. BAJOREK: Oh, I'm sorry.

CONSULTANT WALLIS: This is the blue presentation, is it?

MR. BAJOREK: Yes, the blue presentation.

The assessment ranges from cases which come out very, very well. We use modeling as close as we can to what we would adapt for the full scale plant.

I'm going to skip some of this to get to the stuff where we've deficiencies. As we go through some of this, many of the separate effects tests we do a reasonable job on getting the peak cladding temperature and getting the heat transfer coefficients, getting a number of the other parameters in there.

If we go to some of the integral effects tests, and this is where some of the deficiencies start to get shown, when we look at the peak cladding temperature by itself, which would be shown in here, this is the highest temperature, the data's in red, TRACE is in black, yes we get the peak cladding temperature here. But if we look at the temperatures higher in the bundle we over predict those by a fair amount.

The reason for that is seen in the upper plenum whereas TRACE tends to over predict entrainment, throw the water up into the upper plenum building its level, whereas the data shows it would take a much longer period of time to entrain that much water.

If we look at the quench profile, it does a reasonable job near the bottom but you can see that the predicted quench time is much in excess of the experimental data partly due to the over entrainment. You don't have that water to keep down below the quench front, but also because we do not have top down quench models that would allow the quench front to proceed from the upper core plate down.

MEMBER ABDEL-KHALIK: But the p-clad temperature is predicted to happen at least in the data around 200 seconds or so.

MR. BAJOREK: Yes.

MEMBER ABDEL-KHALIK: And up to that point or well beyond that point the quench front promulgation is quite similar in both model and data?

MR. BAJOREK: Correct.

MEMBER ABDEL-KHALIK: So this can't be the reason.

MR. BAJOREK: I'm sorry?

MEMBER ABDEL-KHALIK: This can't be the reason for the over prediction.

MR. BAJOREK: The over prediction of the p-cladding temperature --

MEMBER ABDEL-KHALIK: Correct.

MR. BAJOREK: -- is because we've entrained so much water it delays the time when the quench front gets closer to that particular elevation because of the rise between where the quench front is and that elevation, it's not getting the cooling it should be cooling.

Retraining the water, it's not de-superheating the vapor and we're retarding the quench front. I mean really in time it doesn't make a whole lot of difference, but over several hundred seconds that delay penalizes you at the top of the bundle.

CHAIRMAN BANERJEE: But now the CCTF tests, were at they controlled at inlet or were they gravity?

MR. BAJOREK: Gravity. It's gravity. Gravity driven. This one is a base case. We look at the other ones. In fact, we even look at SCTF, gravity or forced reflood. We start to see many of those same deficiencies.

CHAIRMAN BANERJEE: You see same sort of

features?

MR. BAJOREK: Right. So what we've done is we've tried to go through all of the tests and we find that the deficiencies are an over predicament of an entrainment -- of the entrainment quench front, lack of a top down quench. And the other, you know, is related to condensation and CCFL, much as we've heard from the peer review. Okay.

So I think we're in agreement because we've seen many of the deficiencies that have been pointed out in the peer review from our own evaluation of the assessment.

Okay. So in going through that, that allows us now to focus on what we think are the larger issues, that's looking at the deficiencies, those comments from the peer review and the problem related to the momentum equation. And in the interest of getting through all of this, we're heard about the input decks and some of the problems we've had associated with those. And I'll start to get more into the momentum equation right here and I'll jump to the -- which will be the final presentation that John will help with.

CHAIRMAN BANERJEE: The momentum equation issue? Now which -- are these green slides?

MR. BAJOREK: They're the green slides, slide 4

CHAIRMAN BANERJEE: Got it.

MR. BAJOREK: Okay.

MEMBER SHACK: Different green slides.

CHAIRMAN BANERJEE: I have different green slides. But there is a momentum question here as well.

MR. BAJOREK: There's another one that starts off momentum equation issue. That's on the front page.

CHAIRMAN BANERJEE: Yes, that's the one.

MR. BAJOREK: Okay.

CHAIRMAN BANERJEE: When you change the colors on us, you really beat us. All slides should be colored the same way.

MEMBER SHACK: Just a simple number, we can find it. I'll settle for the same words. You can leave the color out.

CHAIRMAN BANERJEE: And I can't find those words anywhere.

CONSULTANT WALLIS: I'd settle for another day to discuss the momentum equation.

CONSULTANT KRESS: I'd settle for the same slides. I don't have it.

MR. BAJOREK: There are going to be two presentations. If we take a look at the one that says "Trace Issues and Long Term Development," that one first. And let's go to the very last slide. Because some of this is going to be in the next presentation when it comes to dealing with the momentum equation. Some of the other stuff we've covered in some way shape or form. So there's no sense going through with that.

MR. BAJOREK: Let's just go to long term development, which is really the only important part we haven't covered on this. And our plans for TRACE in its long term development, we agree with the peer review comments that we will probably be better off by activating it, at least the third field to represent the droplets as we get away from the strict two field formulation.

Part of the reason why we are over predicting those temperatures at the top of the bundle is as we entrain the liquid near the quench front, it does not interact with the spacer grids. Breaking those droplets, breaking that field up into finer droplets where it can de-superheat the vapor. So we think that the only good way of doing that is to activate this third field for use with the droplets and complete the development of spacer grid models that would account for local conductive heat transfer enhancement, break up of the droplets and also some rewet of the spacer grid itself which also contributes to the interfacial area.

MEMBER CORRADINI: Can I just ask something about that?

MR. BAJOREK: Yes.

MEMBER CORRADINI: So that means that instead of having a film model which is particular to annular flow, you would have a film model plus droplets?

MR. BAJOREK: We have a film model plus droplets, yes.

MEMBER CORRADINI: That would then taken you away from just essentially a liquid droplet field -- or no. I'm sorry. Excuse me.

MR. BAJOREK: It would allow us to simultaneously model liquid films and droplets.

MEMBER CORRADINI: In a node?

MR. BAJOREK: In a node.

The biggest benefit for that is likely going to come when we look at two loop upper plenum injection plants where you have CCFL breakdown at the upper core plate, liquid films dropping down while you're entraining droplets lower in the core--

MEMBER CORRADINI: So let me not necessarily accept that you need to improve this. So what computer program now -- what have you done in the past to get around this relative to current model? I mean you just told me an application where you expect it to be there. So what did you do to essentially engineer around the need for that or --

CHAIRMAN BANERJEE: How did you handle in the past?

MEMBER CORRADINI: Yes, how did you handle it in the past?

MR. BAJOREK: To my knowledge, I don't think the staff has done those types of calculations in the past.

MEMBER CORRADINI: But you had to assess whether or not there was a conservatism. So that means you did something.

MR. BAJOREK: I think it had been done based on the review of the vendors' codes and their application. Not on the staff's audit --

CHAIRMAN BANERJEE: Data from the vendor codes, which ones have a droplet field and which ones don't?

MR. BAJOREK: Cobra TF, Cobra TRAC has

that.

MEMBER ABDEL-KHALIK: And the code will have the physics for droplet fragmentation, coalescence --

MR. BAJOREK: The tough part on that is putting in the closure models for entrainment and de-entrainment in order to get the exchange between the liquid film and the droplet.

CONSULTANT WALLIS: It's all right for a long pipe, but if you're going to do something like a TEE, you're going to have an awful lot of trouble with a three field model.

MR. BAJOREK: It's been done.

CONSULTANT WALLIS: Well, you have to rely on experiment, I think.

CHAIRMAN BANERJEE: Well, there are experiments on de-entrainment of droplets.

MEMBER CORRADINI: So back to my original question: This buys you margin in less than a handful of plants? Everything else is interesting, academically thrilling, but not necessary?

MR. BAJOREK: You could do it but we would have to start tuning the models. Okay. To meet the two field formulation.

CHAIRMAN BANERJEE: But they're already

getting problems, as you can see, with the upper regions.

MEMBER CORRADINI: But I'm still struggling with historically how you handle it. So RELAP doesn't do this. I'm talking staff, not vendors.

MR. BAJOREK: Yes.

MEMBER CORRADINI: RELAP doesn't do it. TRACB, TRACP does not.

MR. BAJOREK: No.

MEMBER CORRADINI: Okay. So there was a work around relative to calculations except for upper plenum injection. And with upper plenum injection all I'm doing is essentially getting myself to calculate margin. So I'm still trying to understand the value added, other than it's interesting.

I mean I'm just still not there yet. I'm trying to understand what I'm getting for it.

MR. BAJOREK: I think it's getting the accuracy of the upper part of the fuel bundle where we're over predicting the temperatures right now and we feel that the reason for that is our inability to get entrainment correct and the inability to get those droplets which are entrained to break up at the spacer grids and de-superheat the vapor. That's what we see when we look at our assessments right now.

CHAIRMAN BANERJEE: For best estimate code you are over predicting things right now?

MR. BAJOREK: Yes. Now does that allow us to do a broad new class of plants? Well, we'd be able to do the two new plants --

CHAIRMAN BANERJEE: Well, what I might say is imagine somebody came in with a calculation that showed a much lower peak clad temperature than you did, a vendor? And you did that calculation which showed the higher. Then you'd have to sit and reconcile why you're getting it. And you could give the physical reasons for it.

MR. BAJOREK: Yes.

CHAIRMAN BANERJEE: But in fact your confirmatory calculations would not be matching apples with apples. It would be quite a different thing. And you might have a difficult time.

I can see the vendor is going to come in with best estimate calculations on the uprates. If they can push the plant powers up by 20 percent instead of ten percent, why not?

MEMBER CORRADINI: But the upper plenum plants are the key, though.

CHAIRMAN BANERJEE: No, it's not just

upper plenum. Any plant, they're going to push them now.

MR. BAJOREK: And any plant where you can have phased separation in the upper plenum as well. Because steam binding and how much fallback you're going to get can also have an impact on the p-cladding temperature.

MR. KELLY: And if you go back to Steve's slide on CCTF, the p-clad temperature was the core midplane because it had a chopped cosine power profile. But if you have a plant a top skew that's limiting, then the upper elevations become your PCT point.

MEMBER CORRADINI: So and then therefore you have an experimental database that would give you a clear indication that what you just put in is correct?

MR. KELLY: Yes.

CHAIRMAN BANERJEE: Or what you have in right now is incorrect?

MR. KELLY: RBHT test series were designed just for that.

MEMBER CORRADINI: I'm sorry?

MR. KELLY: The RBHT test at Penn State were designed just for that.

MEMBER CORRADINI: Okay.

CONSULTANT WALLIS: Yes, they've been designed for it but have they worked out to give you results for that?

CHAIRMAN BANERJEE: I think they have. They have.

MR. BAJOREK: WE've gotten a lot of information. Now we have to take that information and use that to develop models and correlations and put them into TRACE. But we're happy with the experimental --

CONSULTANT WALLIS: And there is now a report on all that very detailed experimentation which is useful?

MR. BAJOREK: The gentleman in the back is smiling because I think he just put together five reports to put into ADAMS. Just two? Okay. But we have three or four more on the way.

CHAIRMAN BANERJEE: Are these reports available to the vendors, database?

MR. BAJOREK: They will be eventually. I don't think they're publicly available yet.

CHAIRMAN BANERJEE: They'd be able to compare their codes? What could happen is if it is a local imagined plant and they come in with best estimate plus uncertainty, they can get substantial relief and uprate those plants by quite a bit more than they are right now. Maybe. It depends if they're CHF limited --

MR. BAJOREK: Or whether it's a secondary --

MEMBER CORRADINI: So just to review, the RBHT tests are the database by which you could at least attempt to add this and then get in the constitute relations or the coefficient modeling, the interfacial modeling?

MR. BAJOREK: There is some existing data based primarily on flex C set that allow you to get information on spacer grids. However, those are for egg grate type grids, not mixing vein grids. Virtually everything else associated with the grids, especially detailed information, is proprietary. So we feel that we can use the RBHT data to help benchmark these spacer grid models and also look at reflood in situations where you would expect to have a disperse flow film boiling field near the top of the bundle, which we have for reflood.

MEMBER CORRADINI: Thank you very much.

MEMBER ABDEL-KHALIK: Now presumably these long term plans for version six would start after you've corrected all the errors that you found in version five?

MR. BAJOREK: The top priority is to make sure version 5 with its corrections is suitable for doing advanced plants and for the plants that NRR has asked us to do. We've got to make sure that that code runs efficiently and robustly for those. So that's where our resources are going, in addition to developing the plant decks.

As that effort wraps up, we would start to divert more of our attention to this TRACE 6.0. There's a little bit of work going on now, more feasibility in nature than anything else. But when it comes to more model development using the RBHT data, we won't really hit the accelerator until this initial phase is done --

CONSULTANT WALLIS: So there's no plan to use interfacial area?

MR. BAJOREK: There's a plan right now. We are looking at another version of TRACE that was put together a couple of years ago where it does have three fields, but there are two bubble fields. One for a small bubble and one for a continuous vapor field in order to investigate --

CONSULTANT WALLIS: I'm talking about a

conservation law for interfacial area. So added to the other conservation laws, that the sort of thing that you have been doing?

MR. BAJOREK: Yes. Yes.

CONSULTANT WALLIS: So do we have a plan to use that stuff?

MR. BAJOREK: Yes. The long term plan would be to incorporate that. Now, to do that we would probably have to use the fourth field in TRACE. Right now TRACe, there is an update that allows you to go to n fields. But in looking at using droplets for spacer grid and de-superheating and incorporation of interfacial area, we probably need to go to the four fields --

CHAIRMAN BANERJEE: But if you have four fields and one of them is a drop, one is a bubble and one is continuous gas, continuous liquid, you don't need interfacial area because you automatically have that. I'll explain it to you later. But you don't really need that interfacial area equation. Because we have to have a droplet size and a bubble size.

MR. BAJOREK: Right.

CHAIRMAN BANERJEE: And you've the continuous interface.

MR. BAJOREK: But you're still going to

need to the coalescence and the break up of the bubbles.

CHAIRMAN BANERJEE: Well, but that's a bubble thing.

MEMBER CORRADINI: It's that little bubble thing that worries me, though. I'm still back to the coefficients you got to put in here to make this all tune out. I mean --

CHAIRMAN BANERJEE: Yes. Let's leave this for now. I think we need to move on.

MR. BAJOREK: But that's the long term plan.

CHAIRMAN BANERJEE: Right.

MR. BAJOREK: I think that we've heard many of these suggestions from the peer reviewers as well.

CHAIRMAN BANERJEE: Right. I don't think you're asking the ACRS to comment on the long term plan. That would have to be a separate discussion at some point. So the two things that we'll address would be the peer review and the incorporation of the code into the regulatory process.

CONSULTANT WALLIS: Well, I would hope that the modern reactors, the future reactors are designed so you don't have to have four fields. These phenomena never occur.

CHAIRMAN BANERJEE: Graham, the only thing you know are the vendors are going to push it to whatever the best estimate code --

CONSULTANT WALLIS: But they're going to have so many dials, they can get all kinds of results.

MEMBER CORRADINI: I have an example for that, but I won't share it.

CHAIRMAN BANERJEE: Whatever it takes.

MR. BAJOREK: It's 6:05. Central time.

CONSULTANT WALLIS: We promised to stop at 7:00 I understand.

MR. BAJOREK: We can still do that.

CONSULTANT WALLIS: No, we have to set the clock back. It's 7:00 Eastern daylight savings.

CHAIRMAN BANERJEE: Actually we are on--

MEMBER CORRADINI: We're on Pacific time, don't worry.

CHAIRMAN BANERJEE: That was Pacific Standard Time, 7:00.

MR. BAJOREK: Okay. There's been a lot of discussion concerned with the momentum equation. We've identified that as one of our top priorities for resolution. Peer review pointed out some problems and issues with this. So what we wanted to do now was to go through and describe to the Committee what we have been doing with regards to the momentum equation over about the last year.

Just in the way of background, we talked about this in the December 2006 meeting. If I look at the issues and concerns that have been passed around, I think it really comes down to three questions:

Why is the formulation in TRACE with its averaging and approximation an appropriate representation of the vector nature of the phenomena?

Secondly, when you put this into a finite difference form and you start to put it into the numerical solution, are you introducing systematic errors?

CONSULTANT KRESS: I would have rephrased that first subbullet. I would have said why are the formulations in TRACE an appropriate representation of control volumes given the node system that you're in.

MR. BAJOREK: Okay. That's a better way of saying it.

CONSULTANT KRESS: Yes.

MR. BAJOREK: I mean, you're forcing this equation on nodes of a certain size.

And then finally, is that formulation adaptable to places where we've already talked about where we had problems, TEE junctions where the flow bends?

We've acknowledge those concerns really weren't new. Those initiated with the RETRAN code back in, I guess, the late '90s.

CHAIRMAN BANERJEE: And it killed the RETRAN code.

MR. BAJOREK: What we'd like to do --

CHAIRMAN BANERJEE: So the same thing could happen to TRACE.

MR. BAJOREK: No, we'll leave that with RETRAN. But what we will do is not ignore the problem and try to address what the problem is, how we can at least the characterize what the problem is so we can at least put some bounds on it.

So what we want to talk about are the test problems that we've developed in order to investigate the problem. We've talked a little about the review findings. We'll try to get through that as quick as we can.

We've also tried to estimate what's the effect of these deficiencies. We don't know whether we're looking at two degrees, 50 or 500 degrees. And we're going to try to put some numbers on that.

CONSULTANT WALLIS: The thing that bothers me is that if I look at your TEEs, I don't know that you can set problems to students and ask them solve those using a momentum equation. It requires some creativity. It's not something that follows immediately from fundamentals from using an integral momentum equation.

So you may be trying to do something that really isn't feasible. You could fix up the code so that it represents these things reasonably well. Maybe that's what you can do.

MR. BAJOREK: To solve it for all situations --

CONSULTANT WALLIS: I don't think you can from fundamentals prove that you've got some exact answer or something like that.

MR. BAJOREK: No, we can't do that. But our goal is for those geometries and situations that we typically encounter in a plant, a TEE as it's used to accept safety injection, those places where 1-D components have to come into a downcomer at the lower plenum; there those we think we can least limit the number of possibilities and look at those and then find whether the approximations are reasonably correct or whether it's something all along we have to go to the fully implicit solution.

So what we did not too long after that December 2006 was to develop a set of test problems where we stepped up the complexity looking for single phase flow in a straight pipe. No problems there.

To abrupt expansions, contractions where we could take textbook solutions with the loss coefficients that would be recommended and make sure that the code was predicting those analytical types of solutions. You know, flow through an orifice.

Where we did wind up identifying problems are shown on this slide, on slide 5. Situations where we would have converging or diverging flows in TEEs. That was one place where we took a look at what TRACE was predicting and compared that to the flow splits that we expect to get from a handbook, we weren't getting those.

The other ones dealt with the vessel, the 3-D component. And we set up problems in a very simple Cartesian geometry shown over on the left hand side where the flow would come down to a simple 90 degree bend to the one that we talked about earlier that Ralph Landry was alluding to where we looked at what we called -- what we used a radial vessel geometry. We don't worry about the core or its complications, but we did look at flow coming down a concentric annulus, turning 180 degrees and going back up through this central tube.

We could go back to handbook solutions and based on the hydraulic diameters of the inner and outer channels, which we'll refer to as the core and the downcomer just as it looks. But by defining those hydraulic diameters, those flow areas in the height of that bottom most axial cell you should be able to get a certain pressure drop for the known flow.

So we took each of these geometries, and I'm jumping ahead. We also took a look at plant calculations that we were able to get now that we've developed several available plant decks. And we stepped all the way around the loops looking at delta ps that we would get in the hot leg, the steam generator through the core. Compared what we've got from TRAC from two sources. Information that we were able to get from final safety analysis reports that would give some of that.

And the last column is kind of from my memory because I spent my first several years doing hydraulic forces analyses. And some of those numbers just kind of stick with you like a bad tattoo. You kind of remember that the core pressure drop, about 30 psi. What you see in the lower plenum, a couple 3 psi. But when we started to look at these steady states some things that stood out. When we looked at where the flow was coming from the cold leg into the vessel, that turn at the downcomer, getting a much higher pressure drop than we should have expected.

At the lower plenum getting something on the order of 20 psi when two or three would have been acceptable.

CONSULTANT WALLIS: You didn't get any positive pressure changes?

MR. BAJOREK: No. In fact, everywhere you went around the loop where it was a one dimensional flow where you could represent it with a pipe with a loss coefficient, the code was doing a pretty good job. But it was where the bends showed up, that's where we started to identify problems.

CHAIRMAN BANERJEE: But the -- the little problem that you showed us there was a pressure gain, right?

MR. BAJOREK: Pressure gain in the TEE component. Yes, the pressure increased there. But when we looked at the vessel component --

CHAIRMAN BANERJEE: But let's say you blocked off one end of that TEE? Then that's what you did?

MR. BAJOREK: That was the problem that--

CHAIRMAN BANERJEE: Yes, and you got a pressure gain?

MR. BAJOREK: Yes.

CHAIRMAN BANERJEE: So, I mean, when the stuff comes into the downcomer leg that's like a blocked off TEE, isn't it?

MR. BAJOREK: Right.

CHAIRMAN BANERJEE: So why don't you get a pressure gain when he got a pressure gain? You are repeating what John Mahaffey said that there's always a bigger loss. There was a simple that he showed where you got a pressure gain, not a bigger loss.

MR. BAJOREK: That was a little bit different in the geometry where I'm looking at that pressure loss, it's after it's already gone around the bend as well, so the loss may have come in that next node down.

CONSULTANT WALLIS: So something is really weird here, Steve. I mean, you're looking at an error of a factor of ten and all you've got to play with is rho-v or a half rho-v squared times something. And you can't be off by a factor of ten just by having an error in momentum, it seems to me, or you've got something else going on. Because are you going to say that you somehow magnified the momentum flux by a factor of ten by turning it around a bend? I mean, the most you can do is lose all of it. You're going to lose ten times of that by turning it around a bend?

MEMBER CORRADINI: I'm not sure what we're looking at. I mean, am I supposed to be focusing on the things with the double lines.

CONSULTANT WALLIS: With the arrows.

MEMBER CORRADINI: Yes. Those are the arrows.

MR. BAJOREK: At the double lines.

MEMBER CORRADINI: Am I supposed to believe the SMB memory or is that just a rule of thumb?

MR. BAJOREK: Those are some approximate values.

MEMBER CORRADINI: Okay. So I take that as your memory versus a calculation?

MR. BAJOREK: Right.

MEMBER CORRADINI: Okay. So what should I look at as a calculation compared to a calculation or somebody else's calculation compared to TRACE?

MR. BAJOREK: Look at the very bottom.

MEMBER CORRADINI: Right. And that expected FSAR psi is from where?

MR. BAJOREK: That is from this plant's final safety analysis report where the vendor expected 47.6 psi pressure drop going from just outside the cold leg nozzle to just outside the hot leg nozzle.

MEMBER CORRADINI: Ah.

CHAIRMAN BANERJEE: And you get that hand, I take it?

MR. BAJOREK: I'm sorry?

MEMBER CORRADINI: This is what the vendor reported, or this is what the utility reported in the --

CHAIRMAN BANERJEE: And it's in this plant?

MR. BAJOREK: Yes.

CHAIRMAN BANERJEE: Okay.

MR. BAJOREK: Okay. TRACE was giving a much higher pressure drop. If we do our best job to try to look at where those components of the pressure drop were too high, we identified the vessel, the inlet nozzle and at the bottom of the lower plenum.

MEMBER CORRADINI: Ah. Got it.

MR. BAJOREK: Okay. So then at the very bottom of this slide 6, that's our number versus somebody else's number.

MEMBER CORRADINI: Okay.

MR. BAJOREK: Okay. The other three, yes, you're relying on my memory there.

MEMBER CORRADINI: Okay.

MR. BAJOREK: But I have also kept that--

CONSULTANT WALLIS: So it looks as if the FSAR psi is to zero pressure for the lower plenum because the other pressure drops that up to 47.6. So there's zero pressure drop for the lower plenum they're predicting.

CHAIRMAN BANERJEE: So you don't know exactly what --

CONSULTANT WALLIS: No, 47.6 is the sum of 40.1, 3.3 -- so it's a zero for that, isn't it, or am I screwing up?

CHAIRMAN BANERJEE: You're doing it right, but the steam generator does the --

MR. BAJOREK: You don't always have the components here.

CONSULTANT WALLIS: Okay.

MR. BAJOREK: Okay. So the FSAR expected 47.6 going from this -- where I'm pointing out right now, the vessel inlet nozzle delta p through the downcomer and lower plenum, lower core plate core, upper core plate that should total up to about 47.6

MEMBER CORRADINI: So that's around the

whole loop or that's from the cold leg to the outlet?

MR. BAJOREK: That's the cold leg to -- the inlet to the vessel to the outlet of the vessel.

MEMBER CORRADINI: Okay.

MR. BAJOREK: Okay.

CONSULTANT WALLIS: It looks like a bigger problem than a momentum equation, though.

CHAIRMAN BANERJEE: It is through the core?

MR. BAJOREK: Yes, it goes through the core.

CHAIRMAN BANERJEE: The biggest pressure drop should be through the core, right?

MR. BAJOREK: Usually is. It's usually about 30 psi. Okay. That should be -- where'd my mouse go?

That TRACE was predicting 29.6. It's typically around 30. It depends on your grids.

CHAIRMAN BANERJEE: Okay.

MEMBER CORRADINI: But I guess what Graham's asking and what I'm confused about is he's adding up all the numbers in the second column to get 47.6.

MR. BAJOREK: No.

MEMBER CORRADINI: But that's not correct?

MR. BAJOREK: No.

CHAIRMAN BANERJEE: No. The 40.1 is probably mainly through the core.

CONSULTANT WALLIS: That's all right.

MR. BAJOREK: The 47.6 through the vessel. Okay. The rest of the loop, okay, you add up these numbers. Most of it's with the steam generator. Okay. That's about the balance you should get.

MEMBER CORRADINI: Okay.

MR. BAJOREK: But the rest up in the top half of this, the numbers that we were getting; you know, 46 for TRACE versus 40 not too far off. Probably within the uncertainty of whoever put the loss coefficient in the model together. Okay. But within the vessel where we saw those turns, those were clearly showing up as locations of excessive pressure drop, not only in the plant models but also in those simple test cases that we were putting together.

This is where we received comments from Marv Thurgood. John Mahaffey took a look at the formulation of the momentum equation and found two separate problems. One that was related to the transfer of momentum at these TEE junctions, the other in how the momentum was put into the pressure turn at bends and locations within the vessel where the flow would encounter a solid surface.

John worked to put together code updates to TRACE version 5.0 in order to try to resolve that.

I'm going to jump ahead --

CONSULTANT WALLIS: But the pressure drop around a bend like this is an empirical thing and you have a loss coefficient.

MR. BAJOREK: Right.

CONSULTANT WALLIS: And that's what it is. You do it from experiment.

MR. BAJOREK: Right.

CONSULTANT WALLIS: You don't try to predict it.

MR. BAJOREK: That's where the --

CONSULTANT WALLIS: So how can you be off by a factor of ten unless you put in a wrong loss coefficient?

MR. BAJOREK: No. There's no loss coefficient. The code was mishandling the momentum flux.

CONSULTANT WALLIS: It can't. It can't. It's just a huge pressure drop.

CHAIRMAN BANERJEE: How can it be more than rho-v squared?

MR. BAJOREK: Okay.

CHAIRMAN BANERJEE: There's some puzzling, Steve, you have to get to the bottom of it. Because --

CONSULTANT WALLIS: It'll take us a day to get to the bottom of it.

CHAIRMAN BANERJEE: Yes. The delta p 25.1, that looks enormously larger than rho-v squared. It's not one momentum --

MEMBER CORRADINI: What's the typical velocities in these region? I don't know.

MR. BAJOREK: Yes. In the test problem we picked it was like ten meters per second.

MEMBER ABDEL-KHALIK: Help me for a moment, okay. If I start from the differential form of the equations have change, I can integrate those to get the macroscopic balances, right? So by the same token if I start from the finite difference equations, I should be able to get a macroscopic momentum balance. If I were to do a macroscopic momentum balance for this entire component taking into account the forces exerted by the fluid on the solid, I should be able to get that integrated form of the macroscopic balance by simply adding the equations that are written for the finite difference formulation. Where did that go wrong? Did anybody sort of do that check by doing an overall macroscopic balance?

CHAIRMAN BANERJEE: On the momentum equation?

MEMBER ABDEL-KHALIK: On the momentum equation?

MR. BAJOREK: I'm not aware of one.

John, do you know if that's been done?

MR. MAHAFFEY: Well, you've got to understand that the momentum equation --

CHAIRMAN BANERJEE: Come to a microphone. Identify yourself.

This is John Mahaffey of Penn State.

What you need to understand is the momentum equations were inherited. Okay. And past history, I don't know what it was. We regarded other things as higher priority issues for corrections within TRACE for a long time. And this problem was identified. We've gone in and understood why it does what it does. It is purely -- you know, it has to do with the fact that you're doing finite volumes and within the limits of the dervitization, although you can star at the root of the dervitization and say oh that's fine in sort of approximation to this term in a momentum equation. When you really look at what happens it goes bad on you.

So we've gone in and we've reformulated the dervitization within our ability to work with a first order finite approximation which allows us a certain amount of slop in our averaging and we've come up with something that will in effect preserve Bermouli flow as you go around one of these corners. That's what's happened.

But should somebody have caught this earlier? Oh, sure. But it's lost in the mists of time.

CHAIRMAN BANERJEE: I think what's puzzling still though is that the -- if it was ten meters per second, at most you can lose half a bar.

CONSULTANT WALLIS: Well, no. I'm looking at this slide 10 for flow around a sharp corner, that one.

Rho V squared is about 15 psi for ten meters a second.

MR. MAHAFFEY: Right. About.

CONSULTANT WALLIS: I would expect a bend like that to lose about a velocity -- at least. Because there's good separation around the corner. There's a lot of losses around a bend like that. So I don't understand how you get it to be so low as 1.6 psi. Is this based on just some guess or is it based on an experiment or what?

CHAIRMAN BANERJEE: Whether it's a sharp elbow or a smooth elbow makes a difference.

MR. MAHAFFEY: Sure.

CONSULTANT WALLIS: CFD would predict a much bigger loss. I suspect we're not going to get anywhere with this --

CHAIRMAN BANERJEE: A smooth angle without separation you might --

MEMBER CORRADINI: So just to back up so we're understanding. So the one you have in front of us, the delta p hand calculation from Idle Check?

MR. BAJOREK: Yes.

MEMBER CORRADINI: You basically took a handbook approach on how you handled a right end -- an elbow?

MR. BAJOREK: Yes.

MEMBER CORRADINI: Okay.

CONSULTANT WALLIS: A sharp angled bend like that?

MR. BAJOREK: A sharp angled bend.

CONSULTANT WALLIS: That seems awfully low in terms of a K factor.

MR. BAJOREK: Well, I can pull it out for you.

CONSULTANT WALLIS: What's your K factor for that bend?

MR. BAJOREK: I'll find it for you.

CONSULTANT WALLIS: It looks as if it's about .1. I don't believe it. Or maybe it's .2.

MEMBER CORRADINI: I guess a takeaway for me would be we'll stick with that slide and get the background how you got 25, 1.6 and 3.5 and send it to us and move on. Otherwise, we're going to--

CONSULTANT WALLIS: We'll be here forever.

MEMBER CORRADINI: -- stay needlessly here.

CHAIRMAN BANERJEE: I think Mike's idea is a good. Why don't you just explain that slide, how you got the 25.1, how you got the 1.6. It'll be just one more slide, just send us that slide.

MR. BAJOREK: We can go into the details. But the point that we're seeing from the test problems, we were getting too high a pressure drop. When the fixes are implemented now we're getting much lower pressure drops, much better agreement with the hand calculations.

CHAIRMAN BANERJEE: Now, does that handle--

CONSULTANT WALLIS: So the pressure for

this geometry, doesn't it?

MEMBER CORRADINI: No. His was going up--

MR. MAHAFFEY: Well, for K I did.

CONSULTANT WALLIS: But it's just coming in. His was coming in.

MR. BAJOREK: Yes. This is the case that we're looking at.

CONSULTANT WALLIS: This is very interesting because we're sort of at the level of undergraduate flow mechanics here.

MEMBER CORRADINI: And we're struggling.

CONSULTANT WALLIS: We're struggling.

CHAIRMAN BANERJEE: That's the case he was talking about, correct?

MR. BAJOREK: Yes. This is a zero velocity fill over here. So this is essentially a stagnant part of the TEE.

CHAIRMAN BANERJEE: Correct.

MR. BAJOREK: Flow is coming in the side pipe, turning and going out through this break component.

CHAIRMAN BANERJEE: Right.

MR. BAJOREK: Now we're actually looking down on this so there's no gravity heads or anything else associated with this. Steady state, single phase liquid, uniform flow areas, hydraulic diameters.

CHAIRMAN BANERJEE: Now if you go to pressure rise.

MR. BAJOREK: Okay. This one you got the end calculation. It should basically have a small pressure drop.

MR. KROTIUK: On this one if you look at the blue lines, it says V5. -- well, let's do the hand calculation first.

If you look at the green it's the hand calculation. And basically what I've plotted here is the pressure and this side is the side leg flowing in. So if you look at the green, it's the pressure drop from this volume down to here. So the pressure goes from here to here, and there's your pressure increase at that TEE volume.

CHAIRMAN BANERJEE: Where is that shown? Can you show it?

MR. KROTIUK: Okay. This pressure here is this pressure here.

CHAIRMAN BANERJEE: All right.

CONSULTANT WALLIS: Was the stagnation pressure or something achieved there --

MR. KROTIUK: No, that not a stagnation.

That's a flowing --

CHAIRMAN BANERJEE: It was rho-v squared in it?

MR. KROTIUK: Well there's this flow coming in here, there's actually a boundary condition flow coming through here, it turns and then there's a pressure boundary here.

CONSULTANT WALLIS: Yes.

MR. KROTIUK: So this is just calculating flow from here and out to a pressure boundary.

CHAIRMAN BANERJEE: Okay.

MR. BAJOREK: No area change?

MR. KROTIUK: No area change, right. There's no area change.

CHAIRMAN BANERJEE: So now what is the pressure showing?

MR. KROTIUK: Okay. So the pressure at this volume here, is this pressure right here.

CHAIRMAN BANERJEE: Okay.

MR. KROTIUK: And similarly going down the pressure here is the pressure just before entering the TEE. The TEE then goes up in pressure.

CONSULTANT WALLIS: Up?

MR. KROTIUK: Right. So that's this volume right here. And then as you continuing down in this leg here, the pressure here is this pressure here. And the pressure at the final volume here, just at the end, is this pressure --

CONSULTANT WALLIS: There's no pressure loss at all?

MR. KROTIUK: It's actually a small pressure.

CONSULTANT WALLIS: Very small? I mean, obviously that spike is something like rho-v squared --

MR. KROTIUK: That's right.

CONSULTANT WALLIS: So you're saying almost no loss at all?

MR. KROTIUK: Right.

CONSULTANT WALLIS: Which is very unphysical. I mean really the separation around the bend, and there's a lot of loss around that.

MR. MAHAFFEY: Yes, but this is just calculated from the momentum flux term.

CONSULTANT WALLIS: But you can't do that.

CHAIRMAN BANERJEE: What is the solid green line on top?

MEMBER SHACK: Their approach is to get this minimum ones that were sort of lossless and then they add the coefficient.

CONSULTANT WALLIS: Well, how is it then that Marv gets this half rho-v squared or --

MR. THURGOOD: Well, they don't get that. That's with the version 5.0 --

MR. KROTIUK: Okay. Now let's go to the version to blue, which is the version 5.0. Okay. Again, this point right here is a pressure boundary condition. And all I have a velocity coming in here.

So going along, what TRACE is now calculating, the 5.0 version, it's calculating a pressure up here. This pressure here is equivalent to that one there. The pressure at the TEE is there, right here. Then the code is calculating a large pressure drop. This is essentially two velocity heads. And it's coming --

CONSULTANT WALLIS: Just getting a rho-v squared just by momentum balance. It's stopping the stuff at the wall.

MR. KROTIUK: They had an uprate accelerate.

CONSULTANT WALLIS: All right. Then it never recovers anything.

MR. KROTIUK: Yes. And so this pressure here is now -- this pressure bunch is here. The stagnant end is up here, though. The stagnant pressure which is equivalent to the pressure in the--

CONSULTANT WALLIS: Stagnation pressure? But then you can't do that. Because you can't equate an upstream static pressure with a downstream stagnation pressure.

MEMBER CORRADINI: I think that's the point.

MR. KROTIUK: That's what we're saying. That's what the code is calculating.

CONSULTANT WALLIS: Yes.

MR. KROTIUK: Okay. This is what the code is calculating. The unmodified version.

CONSULTANT WALLIS: Yes. So there's something wrong with the code.

MR. KROTIUK: Right.

CONSULTANT WALLIS: We know that.

CHAIRMAN BANERJEE: Now what about the solid green line?

MR. KROTIUK: The solid green, this is the hand calculation and this is the pressure in the stagnant --

CONSULTANT WALLIS: Well, this is just the kind of stuff you teach undergraduates that when you make balances like this, this is the sort of thing you get if you're naive about what really happens.

CHAIRMAN BANERJEE: All right. All right. I think we've spent enough time on this. Let's move on.

MR. BAJOREK: Okay. Then this --

CONSULTANT WALLIS: But we can't move on and we're --

CHAIRMAN BANERJEE: Well, we have to move on right now.

MEMBER SHACK: The red line is the new version?

MR. KROTIUK: The red line is the new version. And all we did is trying to show here what the code was calculating before and after the modification.

MEMBER SHACK: Yes, we got it.

CHAIRMAN BANERJEE: We got it? Okay.

Now somewhere this stuff is going to be written up in a little more detail. Okay. Is that in that write up that you're going to give us from Mahaffey?

CONSULTANT WALLIS: The Mahaffey thing is completely different. It opens a lot more of Pandora's boxes.

CHAIRMAN BANERJEE: Oh, my God. Don't tell me that. Can we never put this issue to bed?

Probably done.

MR. BAJOREK: One of the things that we wanted to try to estimate now that we've identified where the problem shows up and what we think the potential of correction to reduce is, is what's the impact on some calculation.

We picked LOFT L2-5 because it has experimental data to compare to. It's a large break integral test. It's got early pump trip simulating a double ended guillotine.

We have three cases that I'll show you on the next slide. One just the base case with version 5.0. Another one, since we identified the lower plenum as being the place where it was over predicting the pressure drop, let's make it worse. We took the loss coefficient and that one we increased at a factor of five, 500 percent increase in the pressure drop down there. Then we removed that and we ran the base case with a version with these latest correction, which actually reduced the pressure drop by 50 percent.

This is the p-cladding temperature as a function of time that we get from those cases.

CHAIRMAN BANERJEE: And none of it agrees with the data, right?

MR. BAJOREK: None of it agrees. Thank you.

MEMBER CORRADINI: That makes me feel better.

MR. BAJOREK: Thank you. Well, no because this is one of the points. As we take the base case, which is shown in red, and make the pressure drop at that bend much, much worse, it drags it a little away from the data. Okay. It gives a higher p-cladding temperature. When we take all of those bends and correct those, okay, make them closer to what we think it should be, it has a very small impact on the base case. It brings it a little bit closer to the data, almost --

CONSULTANT WALLIS: That's very true for LOFT. And you're going around a loop and this is not an important loss. But in some of these passive coolant system if you're off by a factor of ten on your pressure drop, you may not get the water core. So you've got a very small driving head.

CHAIRMAN BANERJEE: Because that's the point we've been making for a long time that you're in a very different situation with the passive systems.

MR. BAJOREK: But if our first goal here, and I think the question we got before, is industry's been doing this, the staff has been doing this for years. Okay. Is that a big effect for conventional plants?

CONSULTANT WALLIS: I think we know that, that this isn't a very big effect. It can't have been because all these codes have worked successfully in the past and there aren't very many bends and there aren't many pressure drops that are really significant around the bends in conventional plants.

MR. BAJOREK: In conventional plants. So our point by this with the experimental data that was pointed out as being quite a bit different is that the deficiencies that we see in the momentum equation are likely small in comparison to deficiencies and uncertainties that we have in the closure relations. Okay. We think it's a small effect in conventional PWRs and BWRs. Okay.

Now we've done it for LOFT because we had experiments. We kind of expect to see the same thing in a regular plant calculation. But we realize that, yes, the effect on other transients, pump coastdown, BWR stability where that error can effect the velocity in the core, and those changes in those velocities can have a large impact like --

CONSULTANT WALLIS: Well if when you go

back, though, Steve you're saying it doesn't matter because it doesn't have an effect --

MR. BAJOREK: No, it doesn't have a particular --

CONSULTANT WALLIS: Somebody who is reading the code manual and comes up against something which is plainly an error is going to lose a little confidence in the code. So it would be good to fix it. I think especially when it's in Chapter 1. And if someone gets to Chapter 1 and says I see enough errors in Chapter 1, I'm not going to read any further. You've got to get Chapter 1 right.

MR. BAJOREK: I guess we need to move that to Chapter 10 then so it takes longer.

Over the last year we've set up problems, we've tried to identify the problem and try to correct it. It's ongoing. We recognize that there are situations and transients that these corrections may still have -- even with the corrections there may still be too large of impact. But as where the code stands today, we don't think the deficiency is such that it would render it excessively deficient for conventional plant large break and small breaks. That's the point we want to make with that.

Now where we're going in the future with this is we're going to continue to use these targeted assessment test problems. We're going to build these into our assessment and put those with the fundamental calculations so that if there are any additional tinkering or modifications to conservation equations. And the momentum equation, we're going to find it. It's not going to go on for how many years that this problem may have existed in the code. Okay. That's where we'll fully document it. If you don't like the problem, you don't like the hand calculation, it'll be very clear, scrutable.

We're looking at these revisions. We still have to take a look at what they do to the passive plants, low velocity type situations. We're going to continue to work on those revisions. If they prove to be acceptable and have small effect, we'll probably go with that. If not, then the next step is probably to go to a fully conservative form of the momentum equation.

CONSULTANT WALLIS: That's going to solve everything? The conservative equation still has the problem of being a vector equation you're trying to make a one dimensional equation.

CHAIRMAN BANERJEE: It will all be around bends and things that you have to do something more like a mechanical energy equation.

CONSULTANT WALLIS: Right. Right.

CHAIRMAN BANERJEE: I mean, if you look at Bud Steward and Lightfoot, I mean that's a good starting point.

MEMBER CORRADINI: That's the only thing I remember, so it sounds like a good place to start.

CHAIRMAN BANERJEE: Yes. And they do little macroscopic balances around bends and junctions. We should be able to reproduce those things.

MEMBER SHACK: Isn't that what they do? I mean, they just do a Bernouli equation around the bend and fix it up that way?

CONSULTANT WALLIS: That's right.

MEMBER SHACK: Well, that's your patch.

CHAIRMAN BANERJEE: That's the mechanical energy equation.

MEMBER SHACK: Right.

CONSULTANT WALLIS: And that's what Joe Kelly tried to do at one stage, which seems to have all disappeared.

MEMBER SHACK: Well, it's coming back as far as I can tell.

CONSULTANT WALLIS: Well, I think we need another day on this stuff. I mean we're not going to solve it --

CHAIRMAN BANERJEE: At least, maybe another meeting.

CONSULTANT WALLIS: Especially at 8:00 tonight.

MR. GRIFFITH: Is this any different from a --

CHAIRMAN BANERJEE: Please come to a microphone.

MR. GRIFFITH: Peter Griffith. Question. Is this any different from Randolph error?

CHAIRMAN BANERJEE: It depends on the problem.

MR. GRIFFITH: Okay. Then I think we have to live with that.

MR. BAJOREK: It probably is more along those lines. But for passive plants --

MR. GRIFFITH: I don't think you can take an ideal problem and make the assumptions that we have to make to get an answer in this business, and then say "gee, it's not perfect." I think that we're trying to wring orange juice out of a stone.

CHAIRMAN BANERJEE: As long as the stone doesn't sink.

CHAIRMAN BANERJEE: I think, Steve, we need to probably end and so I'm going to thank you. I think we've got the picture of what you're trying to do.

MR. BAJOREK: Yes.

CHAIRMAN BANERJEE: We understand you've identified an issue and you're going to deal with it. You're going to have to do a little more assessment. And really right now what I'd like to do is to thank everybody and ask the Committee members to --

MEMBER SHACK: We have to go through this?

CHAIRMAN BANERJEE: Huh? Well, can we defer that? I don't think we want that to be discussed in front of the --

CONSULTANT WALLIS: So you want the consultant report on it?

CHAIRMAN BANERJEE: And we want a consultant report from both of you. You can deal with the Mahaffey document in the consultant report as well.

The main thing, though, is the full Committee meeting. So I'd like to get ideas from the members of the Subcommittee as to how we should deal with this matter in the full Committee to give you our views so that you can come to that in the full

Committee meeting.

How much time have we got scheduled for the full Committee meeting?

MEMBER SHACK: It's too far away. Two hours.

CHAIRMAN BANERJEE: Two hours. We expect a letter?

MR. BAJOREK: That's the idea.

CHAIRMAN BANERJEE: And you expect a letter out. Let me ask the Committee Chair, is this letter going to then deal with the peer review and the incorporation of the code into the regulatory process, the progress made on that?

MEMBER SHACK: It seems to me that's suitable, yes.

CHAIRMAN BANERJEE: All right. Are there any other topics?

MEMBER SHACK: No, I think that's enough.

CHAIRMAN BANERJEE: All right. So I think that's the guidance that we can give the staff. So the guidance would be to focus on those three issues, you know, the peer review and the way this is getting incorporated into the regulatory process. You know, I think that would be sufficient.

Really, what you should use in my view,

and we can ask each of the Committee members their views, is the letter that we wrote and the document that Farouk wrote on the 31st of January. And basically what progress has been made against those. Because there were some very clear goals set out in those, both those documents. And the two major goals, I mean you've made progress towards two major goals. One is the sort of the diffusion into the regulatory process, which is what we wanted to see, and the second is the peer review. And then you've also done the documentation, which was another.

Okay. So any other specific comments that we need to have?

MEMBER ABDEL-KHALIK: I'd like to comment about the peer review process.

You started out with a fairly well defined high level questions that you posed to the peer review panel, and specifically the questions about identifying deficiencies. And yet in the end you ended up essentially with four different reviews, not an integrated peer review that provides an answer to what I consider to be very important high level questions. How are you going to address that?

MR. KROTIUK: First, let me say that the decision to come up with the four reports as opposed to one was at the request of the peer reviewers. That's the way they wanted to handle it.

MEMBER ABDEL-KHALIK: And yet in some cases, okay, George expressed sort of disappointment or he lamented the fact that there was no give and take; where questions were posed by at least himself, they were not responded to. In a sense that it is not just that you have four different reports that have not been integrated into a peer review panel report, but there is something wrong with the process itself.

You may have gained a lot of, you know, from the inside provided by the individual --

MR. BAJOREK: It may be that the process is new again to the staff. As I understand back in the '70s and '80s there used to be a code review group that would provide feedback on a continuum basis with the codes that were being developed at the time. And that seemed to have gone away for ten or 15 years. And now with this peer review, it's being resurrected again. And I think because of that newness, the staff hasn't gotten used to dealing with this outside group and making it part of our mission to respond to them as part of the review.

MEMBER ABDEL-KHALIK: Let me just put my question in a direct as fashion as I can.

If I were to try to find the answer to the two questions that you posed, where would I find it?y

MR. KROTIUK: In the individual reports.

CHAIRMAN BANERJEE: They don't specifically address those issues.

MEMBER CORRADINI: I mean, to put it differently, I thought you were going to answer differently, which is in each of the cases they had a different answer, or maybe they can speak for themselves. But I heard it by reading their four things ahead of time.

By the way, I want to thank the four of them since I didn't have to read the whole CD. I only went back to the CD when they made some sort of comment and I go okay, I got to go back and look at that.

But I kind of read it that the written material was of a form that needed to be upgraded to the point that they couldn't answer the two top box questions. That's kind of what I took away from all four of the reports.

MEMBER SHACK: I agree.

CONSULTANT WALLIS: I think I took the same thing away. Yes.

MEMBER CORRADINI: If there was a better

documentation, a little bit more different assessment, they could have --

CONSULTANT WALLIS: And more time.

MEMBER CORRADINI: And more time, there was that time issue, they could have come through with the answer to those two top box questions. But I didn't sense that they were going to go out on a limb and answer them given the situation they went through. Yes. Are we off base in reading it that way?

MS. GAVRILAS: Mirela Gavrilas from Research.

Our feeling was that what was meant by more time was an unrealistic amount of more time, at least in our discussions with them. That in order to actually answer those questions categorically, they would have to go to a different level of review.

MEMBER ABDEL-KHALIK: But isn't that what we want to find out?

CONSULTANT WALLIS: Well, maybe it's premature to ask for them at this time? Maybe we will find out sometime.

CHAIRMAN BANERJEE: Well, there's a practical aspect to this, which is that is the code good enough to be able to help us with confirmatory analysis or not on certain problems. And we are in desperate need of that type of help.

I think this Committee certainly appreciates whenever a calculation is a confirmatory calculation is done by the staff instead of just accepting what the vendor is serving up.

So, I mean that is the issue really that we need to deal with. Is it good enough to do certain types of calculations right now? If it's not, then I think we should say so.

MS. GAVRILAS: I would like to add one more thing. That our view is that the peer review compliments our assessment. So therefore, we look at the code as it is today as the sum of their opinions, their views of the areas that they reviewed in depth in addition to the traditional means of gaining confidence in a calculation, which the assessment phase.

MEMBER CORRADINI: So let me just try it a different way and so you can see where we're all coming from.

I guess I wouldn't feel bad that the peer review -- I guess I wouldn't feel unhappy with the comments from the four individuals that they couldn't answer your top box questions, but there was a path forward to answer those top box questions you got to do A, and you got to do B and you got to C, and you got to do D. And I think you kind of said that. You haven't formulated it that way, but one might consider if you were to formulate it that way, that actually addresses where they hesitated and then also leads you forward, a path forward to actually do what Sanjoy's after, which is pragmatically you're going to want to use this, you need to use this and it has places to be used, but that's how you marry all these things and such stuff together.

CHAIRMAN BANERJEE: And we need to know its limitations.

MEMBER CORRADINI: Yes. Yes.

CHAIRMAN BANERJEE: Where it can be used and where it can't be used.

MEMBER CORRADINI: Yes. That's just a way to look at it, I guess.

CHAIRMAN BANERJEE: Where are its weaknesses, you know. And it's not just large break LOCAs and small break LOCAs. I mean, this is going to be -- if this works, then we want to use it for a lot of stuff.

MR. BAJOREK: Transients, stability.

CHAIRMAN BANERJEE: Yes. AOOs. I mean, minimum CPRs. All sorts of stuff. We want to couple it to PARKS. And so it's coupled.

So, I mean in a way we need a serious answer to this as to where are the deficiencies, what can you use it, what shouldn't you use it for. You know, a little bit less sort of -- you know, this has been pretty vague. I think what we've got back from the peer review is that they seem generally supportive of the code, and most of the stuff in it. But they haven't been able to answer where it's deficient and where it shouldn't be used and where it should be used. I mean, they had too little time. They had to focus only on small break and large break LOCAs, whereas we're using the code for a lot of different things right now.

So, it's a good beginning, but it's certainly not the end, I would think. We need to continue this process would be my feeling.

CONSULTANT WALLIS: Can we close on that statement? I think that's a very good statement.

So I'd like to thank everybody. I thought the meeting was very good and very informative. And I don't know if anybody has anything to add, but I hope that you don't. In that case, I'm going to adjourn the meeting.

(Whereupon, at 7:51 p.m. the meeting was adjourned.)

CERTIFICATE

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

Name of Proceeding: Advisory Committee on Reactor Safeguards; Thermal Hydraulic Phenomena Subcommittee

Docket Number: n/a

Location: Rockville, MD

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

Toby Walter
Official Reporter
Neal R. Gross & Co., Inc.

TRACE Version 5.0 Assessment; Summary of Results

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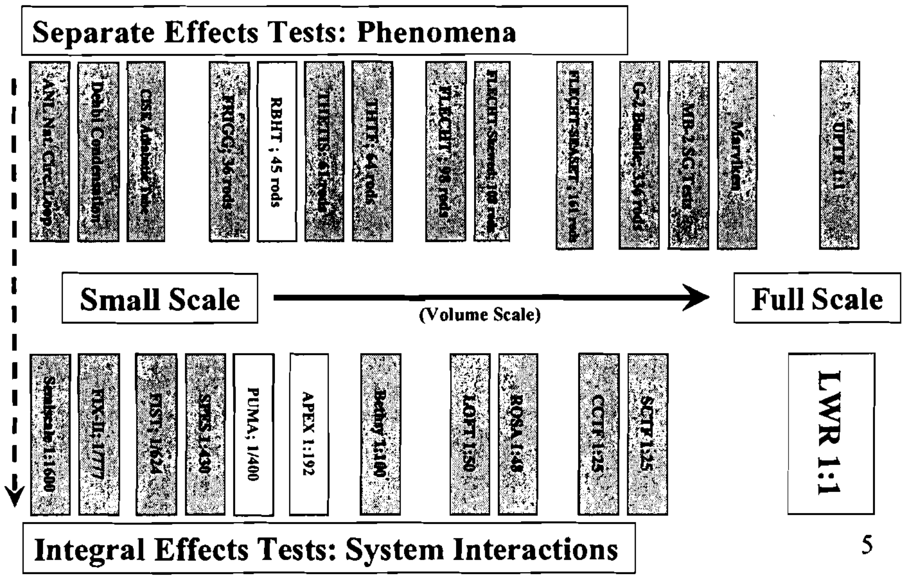
Presentation to the
Advisory Committee on Reactor Safeguards
Thermal-Hydraulic Phenomena Subcommittee
July 7, 2008

Outline & Objectives

- Provide a brief summary of status:
  - TRACE Version 5.0 assessment matrix & basis
  - Results
  - Deficiencies
- Summarize additional assessment being done in support of new & advanced plants:
  - ESBWR
  - EPR
  - AP1000
  - APWR

T/H Assessment Tests



5

TRACE Assessment & Documentation

- "Generic" Assessment Documented:
  - Main Body (Description and Summary of Results)
  - Fundamental Problems (Appendix A)
  - Separate Effects Tests (Appendix B)
  - Integral Effects Tests (Appendix C)
- New & Advanced Plant Support
  - ESBWR (ESBWR Code Applicability Report includes assessment using PUMA, PANDA, GIRAFFE, PCCS SETs)
  - EPR (EPR Code Applicability Report includes assessment using FLECHT-SEASET Reflux Condensations tests, ROSA-IV SG tests, APEX Reflux Cond.)
  - AP1000 (AP1000 Code Applicability Report to include assessment using APEX-AP1000 and ROSA-AP600 IETs, CMT tests and ADS tests)
  - APWR (APWR Code Applicability Report to include assessment using MHI advanced accumulator tests)

6

Separate Effects Tests

Highly Ranked Phenomena

- Break Flow:
  - Marviken (6)
  - Moby Dick (10)
  - Super Moby Dick (8)
- Condensation:
  - UCB (20)
  - Dehbi (21)
  - U. Wisc. (6)
- ECC Bypass:
  - Uniform, Subcooled (1)
  - Uniform, Saturated (7)
  - Non-uniform, Saturated (8)
- Flooding & CCFL:
  - Bankoff (1)

Assessment Report; Appendix B

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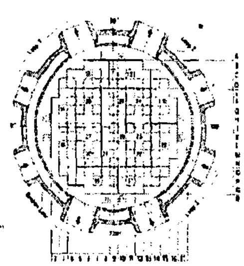
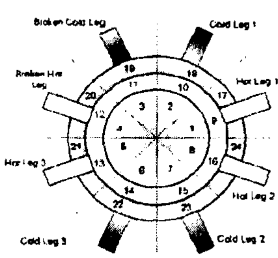
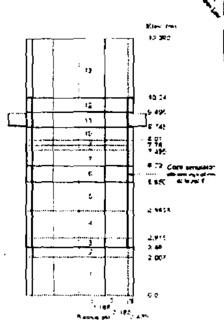
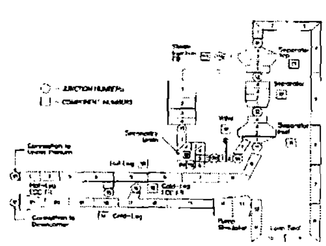
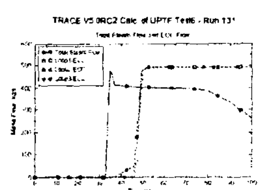
ECC Bypass Assessment

UPTF Tests 5, 6, 7, and 21

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Separate Effects Tests

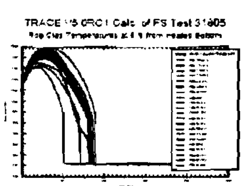
Core / Vessel Thermal-Hydraulics

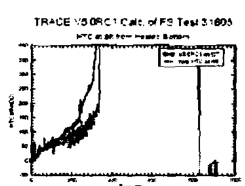
- Blowdown Heat Transfer:
  - THTF Steady-state (4)
  - THTF Transient (3)
- Reflood Heat Transfer:
  - FLECHT-SEASET (8)
  - RBHT Steam Cooling (7)
  - RBHT Reflood (4)
  - GOTA Reflood (1)
  - GOTA Radiation (1)
- Mixture Level Swell:
  - FRIGG (29)
  - RBHT Steady-State (73)
  - RBHT Trans. (1)
  - GE Vessel Blowdown (2)

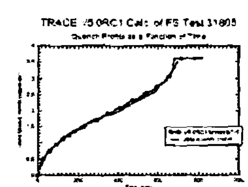
Assessment Report; Appendix B 13

Reflood Test Results

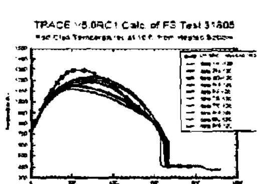
FLECHT-SEASET Test 31805



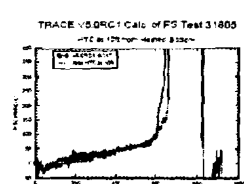




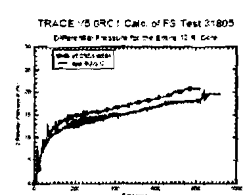
Quench Profile



Cladding Temperatures



Heat Transfer Coefficients



Bundle ΔP 14

Integral Effects Tests

PWR LBLOCA Tests

- LOFT LBLOCA (3)
- CCTF (7)
- SCTF (7)

BWR LOCA Tests

- SSTF (2)
- FIST (2)
- TLTA (2)

PWR SBLOCA Tests

- LOFT SBLOCA (3)
- ROSA-IV (6)
- Semiscale (4)
- BETHSY (2)

Assessment Report; Appendix C

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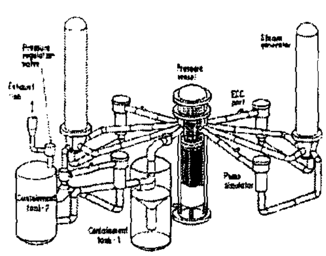
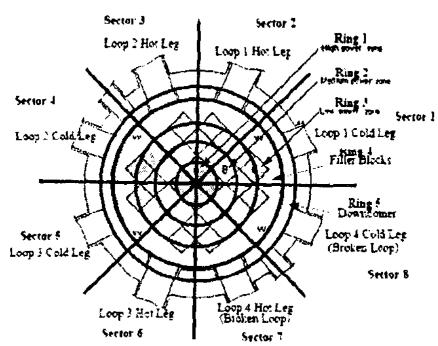
CCTF Run 62 Results

TRACE Version 4.271





TRACE Model for CCTF:

-3-ring core, 8-sector vessel

-Axial noding consistent with SETs

-Explicit loop representation

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Deficiencies & Issues

- RBHT Reflood Test Modeling / Agreement
  - IVA Flow Film Boiling Model
  - Entrainment
- Break Modeling (ROSA-IV)
  - Offtake model and unique geometry
- Momentum Equation Issues
  - Induced Motion
  - Excessive ΔP at Flow Direction Changes
- "Robustness"
  - Excessive CPU time for some cases
  - Code maintenance / error correction

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Summary & Conclusions

- Assessment of TRACE Version 5.0 was accomplished with an assessment matrix of over 550 cases.
- In general, TRACE Version 5.0 shows reasonable agreement with most data and can be used for NRC audit calculations.
- Deficiencies to be addressed in future development activities.
- Advanced reactor designs are subject to additional code development and assessment because of their unique features.

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TRACE

TRACE ISSUES and LONG TERM DEVELOPMENT

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Presentation to the
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July 7, 2008

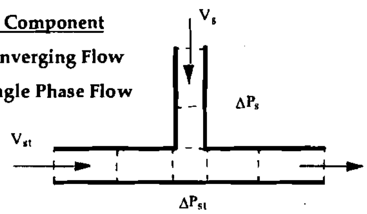
TRACE

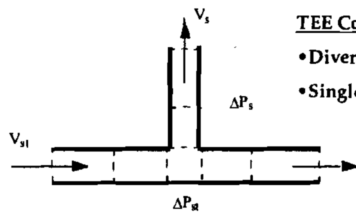
Peer Review & Code User Experience

- THE PEER REVIEW HAS BEEN EXTREMELY VALUABLE. THE MANY CONSTRUCTIVE COMMENTS WILL HELP US TO IMPROVE THE CODE, ITS ASSESSMENT BASE, AND THE DOCUMENTATION.
- THE STAFF HAS ALSO BEEN ACTIVE SINCE RELEASE OF TRACE VERSION 5.0 IN DEVELOPMENT OF PLANT MODELS AND WITH ADDITIONAL TESTING OF THE CODE.
- MAJOR ISSUES AT THIS TIME ARE:
  - **Resolution of Peer Review and User Identified Errors**
  - **Momentum Equation Formulation**
  - **Runtime and Code Efficiency Problems**

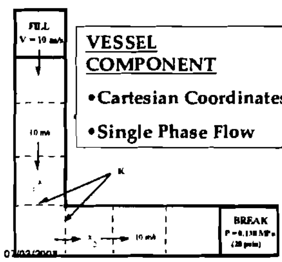
TRACE Example Test Problems

TEE Component
- Converging Flow
- Single Phase Flow

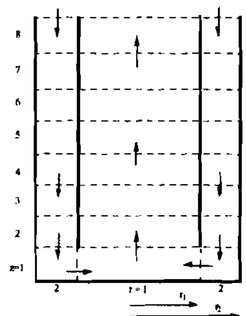




TEE Component
- Diverging Flow
- Single Phase Flow



VESSEL COMPONENT
- Cartesian Coordinates
- Single Phase Flow



VESSEL COMPONENT
- Radial Coordinates
- Single Phase Flow

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TRACE Summary

- WE ARE IN THE PROCESS OF EXAMINING THE PEER REVIEW GROUP REPORTS AND BALANCING COMMENTS VERUS AVAILABLE RESOURCES.
- HIGH PRIORITY ITEMS INCLUDE:
  - **CORRECTION OF THE MOMENTUM EQUATION AND OTHER KNOWN ERRORS.**
  - **DEVELOPMENT OF USER GUIDELINES AND REVISION OF THE USER MANUAL.**
  - **IMPROVEMENT OF PHYSICAL MODELS WITH EMPHASIS ON THOSE WHICH MAY BE RESULTING IN DEFICIENCIES**
  - **CONTINUATION OF ASSESSMENT WITH EMPHASIS ON THOSE PHENOMENA AND PROCESSES NOT WELL COVERED IN WORK TO DATE**
- THE PEER REVIEW WAS A VALUABLE EXERCISE AND THE COMMENTS WILL HELP THE STAFF TO IMPROVE THE CODE AND ITS APPLICATIONS.

07/03/2008
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TRACE

STAFF COMMENTARY ON PEER REVIEW OF THE TRACE THERMAL-HYDRAULIC CODE

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Advisory Committee on Reactor Safeguards
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TRACE **Peer Review**

- THE PEER REVIEW HAS BEEN EXTREMELY VALUABLE. THE MANY CONSTRUCTIVE COMMENTS WILL HELP US TO IMPROVE THE CODE, ITS ASSESSMENT BASE, AND THE DOCUMENTATION.
- THE STAFF COMPLIMENTS THE PEER REVIEWER GROUP FOR WHAT WE CONSIDER AN INDEPENDENT & COMPREHENSIVE EVALUATION OF TRACE VERSION 5.000.
- LONG-TERM DEVELOPMENT AND RESOLUTION OF ISSUES MAY BENEFIT BY HAVING ACCESS TO A PERMANENT EXTERNAL REVIEW PANEL.

TRACE Documentation

- CURRENT DOCUMENTATION WAS ISSUED AS A SET (Aug. 2007) AND CONSISTS OF:
  - THEORY MANUAL
  - ASSESSMENT REPORT
  - USER MANUAL (VOLUMES 1 and 2)
- EACH OF THE PEER REVIEWERS HAD DIFFICULTIES WITH THE DOCUMENTATION. OF PARTICULAR NOTE WERE
  - LACK OF SPECIFIC USER GUIDANCE FOR PLANT INPUT DECK DEVELOPMENT
  - DIFFICULTY IN IDENTIFYING THE SPECIFIC MODEL(S) ACTUALLY USED BY TRACE

TRACE Documentation

- SOLUTION:
  - **DEVELOPMENT OF TWO REPORTS (PWR & BWR) WITH PLANT MODELING RECOMMENDATIONS. THE RECOMMENDATIONS ARE INTENDED TO PROVIDE A COMPONENT BY COMPONENT "COOKBOOK" ON HOW A USER IS TO SET UP A PLANT.**
    - → **Each region (core, UP, HL, SG, PZR, etc.) of the plant will have specific guidelines on which Component to use, a recommended nodalization, mandatory parameter settings if a model/correlation selection is necessary, and identify the basis for the recommendations.**
    - → **Goal is to address the Peer Review comments, and also to help minimize the "User Effect" in plant calculations.**
    - → **A Technical Editor has been obtained to facilitate revision of the User Manual.**
  - **THE THEORY MANUAL WILL BE DIVIDED INTO TWO VOLUMES; VOLUME 1 WILL BE A CONCISE DESCRIPTION OF MODELS & CORRELATIONS. VOLUME 2 WILL PROVIDE DETAILS ON HISTORY & HOW PARTICULAR MODELS WERE DEVELOPED.**

TRACE Assessment

TEST	PHENOMENA
UPTF and/or IVO Loop Seal	SBLOCA Loop Seal Clearance
Inlet Elbow Flooding (various)	CCFL
MIST	IET (B&W plants)
PKL	IET for LBLOCA
	Blowdown Film Boiling
	Downcomer Hot Wall
	Non-LOCA Tests

- A test series is being planned (2Q/2009) using the RBHT facility to examine the effect of oscillatory reflood flows. Objective is to provide data for reflood in which the magnitude and frequency of oscillations are controlled.

TRACE Assessment Results

Switch to Presentation

"TRACE Assessment Summary"

TRACE Physical Models and Conservation Equations

- MEDIUM PRIORITY ASSIGNED TO IMPROVEMENT OF PHYSICAL MODELS AND RESOLUTION OF DEFICIENCIES:
  - Condensation Processes
    - → **Direct contact condensation at ECCS jet**
    - → **Film condensation with and without NC gas**
  - Post-CHF Heat Transfer
    - → **Modeling of IAFB and DFFB**
    - → **Lack of top-down quench**
    - → **CHF Models**
  - CCFL Model
  - Interfacial Friction

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TRACE Summary

- WE ARE IN THE PROCESS OF EXAMINING THE PEER REVIEW GROUP REPORTS AND BALANCING COMMENTS VERUS AVAILABLE RESOURCES.

- HIGH PRIORITY ITEMS INCLUDE:
  - **CORRECTION OF THE MOMENTUM EQUATION AND OTHER KNOWN ERRORS**
  - **DEVELOPMENT OF USER GUIDELINES AND REVISION OF THE USER MANUAL**
  - **IMPROVEMENT OF PHYSICAL MODELS WITH EMPHASIS ON THOSE WHICH MAY BE RESULTING IN DEFICIENCIES**
  - **CONTINUATION OF ASSESSMENT WITH EMPHASIS ON THOSE AREA NOT WELL COVERED IN WORK TO DATE**

- THE PEER REVIEW WAS A VALUABLE EXERCISE AND THE COMMENTS WILL HELP THE STAFF TO IMPROVE THE CODE AND ITS APPLICATIONS.

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Protecting People and the Environment

TRACE Regulatory Applications

Presentation to the ACRS T/H Subcommittee

Mirela Gavrilas
Reactor Systems Applications Branch
Office of Nuclear Regulatory Research

July 7, 2008

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TRACE uses in regulatory activities since the last ACRS review (March 2007)

- ESBWR DCD confirmatory calculations
  - LOCAs
  - *applicability report*
- EPR topical report review
  - LBLOCA methodology RAIs
- Brown's Ferry EPU SER
  - SB and LB LOCAs
- GSI-191
  - scoping analyses

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Plant Decks

Plant	Plant Type	Event	Model Status / Availability Date
Operating Plants			
Monticello	BWR/3	SBLOCA, LBLOCA, SBO	Under development / 2008
Browns Ferry 1, 2, 3	BWR/4	SBLOCA, LBLOCA, SBO	Available
Nine Mile Point 2	BWR/5	SBLOCA, LBLOCA, SBO	Under development / 2008
Point Beach 1, 2	W 2 loop	SBLOCA	Under development / 2008
Prairie Island 1, 2	W 2 loop	SBLOCA, LBLOCA	Work will begin shortly.
HB Robinson	W 3 loop	SBLOCA, LBLOCA, locked rotor	Available
Turkey Point 3, 4	W 3 loop	SBLOCA, LBLOCA	Under development / 2009
North Anna	W 3 loop	Feed and bleed	Under development / 2008
Seabrook 1	W412, 4 loop	SBLOCA, LBLOCA, SGTR	Available
Oconee 1, 2, 3	B&W lowered loop	SBLOCA, LBLOCA	Available
Crystal River 3	B&W lowered loop	SBLOCA, LBLOCA	Under development / 2009
Calvert Cliffs 1, 2	CE 2 loop	SBLOCA, LBLOCA, loss of FW	Available
St. Lucie 1 & 2	CE 2 loop	SBLOCA, LBLOCA	Under development / 2009
Ft. Calhoun	CE 2 loop	SBLOCA, LBLOCA	Unique plant; work will proceed shortly
New Reactors			
ESBWR	BWR	MSLB, BDLB, GDLB, AOL	Available, AOO being developed
EPR	PWR	LBLOCA	Available
AP-1000	PWR	LBLOCA	Available
USAPWR	PWR	SBLOCA, LBLOCA, Transient TBD	Under development / 2009
ABWR	BWR/6	SBLOCA, LBLOCA, Transient TBD	Under development / 2009

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Target Execution Times

Event	One-Dimensional Model TRACE Execution Time[1,2] / Problem Time	Three-Dimensional Vessel TRACE Execution Time[2] / Problem Time
Steady State Initialization	1	0.5 – 3
BWR LBLOCA	1 – 3	1 – 10
BWR SBLOCA	1	1 – 5
PWR LBLOCA	1 – 5	5 – 30
PWR SBLOCA	1	3 – 10

[1] The indicated execution times are goals for the TRACE one-dimensional vessel models.
[2] Typical execution times using an NRC agency PC with a Pentium 4 CPU at 2.80 GHz and 1.0 GB of RAM.

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TRACE – User Office Perspective

ACRS T/H Subcommittee

Ralph R. Landry
Senior Level Advisor, DSRA/NRO
July 7, 2008

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Discussion Points

- Dr. Gavrilas has presented overview of user office applications support
- Two points not discussed are
  - Regulatory result of code errors
  - Specific regulatory issue resolution

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Conclusions

- Code errors can be used in a positive way in the regulatory process
- Code has flexibility to model phenomena such as porous medium flow resistance
- Office of Research has been very responsive to user office needs

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TRACE 5.0 Peer Review

Presentation to the ACRS T/H Subcommittee

William J. Krotiuk
Reactor Systems Analysis Branch
Office of Nuclear Regulatory Research
July 7, 2008

TRACE 5.0 Peer Review

- Tasks
  - Review TRACE code and documentation
  - Produce reports that
    - summarize code strengths and deficiencies and
    - provide recommendations for code changes and improvements
- Priority Objectives
  - Identify major deficiencies that preclude the use of TRACE for confirmatory thermal-hydraulic calculations
  - Identify deficiencies that introduce significant errors in TRACE predictions
  - Provide recommendations for substantive improvements
- Present findings to the ACRS T/H subcommittee

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TRACE 5.0 Peer Review – Panel Members

- International experts with extensive knowledge of thermal-hydraulic code models, methods and applications
  - Dominique Bestion
    Research Director, Commissariat à l'Energie Atomique, CEA-Grenoble
  - Peter Griffith
    Retired Professor of Mechanical Engineering, MIT
  - Marv Thurgood
    CEO/Project Manager, John Marvin, Inc.
  - George Yadigaroglu
    Professor Emeritus of Nuclear Engineering, Swiss Federal Institute of Technology in Zurich (ETHZ)
- Contracts awarded in August, 2007

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TRACE 5.0 Peer Review

- Material Supplied to Peer Reviewers
  - TRACE Documentation
    - Theory Manual
    - Assessment Manual and Appendices
    - User's Guide
      - Volume 1 Input Description
      - Volume 2 Modeling Guidelines
  - TRACE Code
    - TRACE Version 5.0
      - Executable
      - Source
    - Sample problem input and output files

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TRACE 5.0 Peer Review –
General Review Topics

- Capabilities and Limitations
  - Code mission, purpose, objectives, capabilities, limitations and range of applicability
- Numerical Solution Methods
  - Numerical solution scheme
  - Time and space averaging approaches
- Fundamental Equations, Models and Correlations
  - Are original published sources referenced along with supporting data?
  - Is the model or correlation applicable to, and accuracy appropriate for power reactor conditions?
  - Is the model or correlation implementation approach including any modifications sufficiently described?
- General Quality
  - Is the documentation well written, well organized and understandable?

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TRACE 5.0 Peer Review –
Specific Focus Areas

- Detailed review by one or more panel member
  - Conservation Equations Application
  - Thermal-Hydraulic Closure Relations and Physical Models
  - Numerical Solution Schemes
  - Nuclear System Components, Features and Models
    - Pumps, valves, fuel rod models and reactor kinetics
  - Test Assessment Matrix and Results
    - Sufficiency and completeness relative to other T/H codes

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TRACE 5.0 Peer Review -
Specific Focus Area Review Assignments

- Conservation Equations Application
  - M. Thurgood
  - G. Yadigaroglu
- Thermal-Hydraulic Closure Relations and Physical Models
  - D. Bestion
  - G. Yadigaroglu
- Numerical Solution Methods
  - M. Thurgood
- Nuclear System Components, Features and Physical Models
  - P. Griffith
- Test Assessment Matrix and Results
  - D. Bestion
  - P. Griffith

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TRACE 5.0 Peer Review

- Activity Summary
  - Kick-off meeting — Aug. 28-29, 2007
    - Office of Research presentations
    - Discussions/questions by peer reviewers
  - Reviewers draft reports to NRC — Jan., 2008
  - Working meeting — Feb. 27-28, 2008
    - Discuss draft reports and findings with Office of Research staff
  - Reviewers final reports to NRC — May, 2008
  - Presentation to ACRS T/H Subcommittee — July 7, 2008
  - Final Report — August, 2008

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TRACE 5.0 Review Summary

- The following slides developed with the peer reviewers present an overall general summary of their findings.
- Each peer reviewer will elaborate and justify their findings in their individual presentations

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TRACE 5.0 Review Summary

- General Findings:
  - TRACE 5.0 is a good system code with extended capabilities to simulate PWR and BWR LOCAs but is only applicable within the assessment range
    - Conventional PWRs and BWRs*
  - Getting a code as complex as TRACE to provide reasonable answers is an accomplishment

*Note, proprietary evaluation reports which assessed ESBWR modeling and applications were issued in March 2008 and not available in time for the peer review.

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TRACE 5.0 Review Summary

- Specific Findings:
  - Thermal-Hydraulic Closure Relations and Physical Models
    - Improvements needed for some physical models (equations or closure models).
      - Some physical models require further review, analysis and improvement.
    - Include validation matrix for physical models and phenomena.
    - The interface tracking model is innovative and efficient; but user guidance should be provided.

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TRACE 5.0 Review Summary

- Specific Findings:
  - Conservation Equations Application
    - The VVV momentum term is incorrect for side connections, and 3-D vessel flow direction changes.
    - Provide guidance for using the nonconservative form of the momentum equation.
    - Water packing is overly restrictive.

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TRACE 5.0 Review Summary

- Specific Findings:
  - Numerical Solution Methods
    - The SETS* numerical solution is innovative and allows Δts to exceed the material Courant limit.

* Note, the SETS method was previously developed and implemented in TRAC.

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TRACE 5.0 Review Summary

- Specific Findings:
  - Test Assessment Matrix and Results
    - Additional assessments or extensions of assessments are needed to fully address each physical model and all important phenomena.
    - Assessments should be referenced to the SET matrix and PIRT tables.
    - The Assessment Manual should provide information on how well TRACE predicts important licensing limits (e.g. PCT)

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TRACE 5.0 Review Summary

- Specific Findings:
  - Nuclear System Components, Features and Physical Models Documentation
    - A good deal of work is needed to make the Users Manual easy to use
    - The Users Manual should be rewritten to provide recommended modeling and guidelines for system components
    - Better input modeling guidelines with references to assessment modeling are needed
    - Include code uncertainties relative to PWR and BWR transients

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TRACE 5.0 Review Summary

- Recommended Modifications:
  - Items which should be addressed as soon as possible
    - Rewrite the Users Manual
    - Correct VVV momentum term
    - Review indicated closure relations and physical models, and include a validation matrix
    - Continue to expand the code assessments
  - Longer term items
    - Add a liquid droplet field
    - Modify TRACE to solve a conservative form of the momentum equation

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REVIEW OF THE APPLICATION OF THE CONSERVATION EQUATIONS AND OF THE NUMERICAL SOLUTION METHODS USED IN TRACE

Marv Thurgood

General Comments

- I agree with adding a droplet field to the code. The current model is inadequate to address the flow phenomena during stratified/dispersed film/dispersed flow and re-flood. Many of the constitutive models will have to be reworked as will the solution strategy.
- Will the addition of the drop field be adequate or should there be four fields; continuous liquid, continuous gas, dispersed gas and dispersed liquid? There is some indication that the latter is required as an essentially four field model is used by the level tracking model. My recommendation is that we carefully evaluate the complete model needed and set out in that direction to start with, thereby avoiding excessive costs in developing an incomplete model.
- Also, I wonder if some consideration should be given to solving the conservative form of the momentum equations rather than the non-conservative form. It is not always clear that the code will obtain the correct temperature when large pressure gradients exist within the system. It is also not clear if it actually conserves momentum.

General Comments (Cont'd)

- It is stated in the documentation that the code uncertainty for transients in both current and advanced PWR's and BWR's has not been conducted. Is the code usable by NRR with out this?
- I find the documentation generally well written and complete with regard to equations, references and nomenclature.
- There is a description of the code's mission, its purpose, objectives and capabilities. Its range of applicability is also discussed.

General Comments (Cont'd)

- Based on my review of the documentation, I conclude that there is an adequate description of the code limitations.
- The conservation equations are described in complete detail, starting with the time averaged mass, energy and momentum equations and ending with the volume averaged mass, energy and momentum equations which are rearranged for numerical solution.
- References are provided for the origin of the conservation equations used in TRACE along with the numerical methods used to solve the equations. These are adequate to allow a person knowledgeable in numerical modeling to understand the methods being used.

SCOPE OF REVIEW

- I have reviewed the following sections in their entirety:
  - 1: Field Equations
  - 2: Solution Methods
  - 3: Heat Conduction Equations.
  - Appendix A: Quasi steady assumption and averaging operators.
  - Appendix B: Finite Volume Equations
- I have also reviewed the entire section on level tracking, Numerical experiments, the off-take model and Form Loss models. I have also reviewed some of the fluid properties, those of the gas mixture especially.

CONSERVATION OF MOMENTUM

- The V grad V term in the momentum equations is not treated correctly for tees (side connections), connections of 1-D components to the 3-D vessel that face a solid surface, and the bottoms and top nodes of the 3-D vessel which face solid boundaries.
- The velocity in the VgradV term (or area) that is nearest the solid surface should be set to zero for connections that are at 90 degrees to the solid surface. This has been done in a code developers test Version 5.007
- It seems to me that this velocity should be set to Vcosφ for side connections to a 1-D component or for the tee secondary tube.
  - Cos φ is equal to zero when the side connection is perpendicular to the 1-D component wall.
  - It is non-zero when φ is an acute or obtuse angle and removes the same momentum from the side connection that is currently added to the 1-D component receiving the flow from the side connection or when flow is entering the side tube. It is not clear that side connections have been corrected in this way or if this velocity (or area) is set to zero.

CONSERVATION OF MOMENTUM

- The code developers have modified the momentum flux terms in Version 5.07 in response to this review, such that the velocity that is nearest the solid surface is set to zero for connections that are at 90 degrees to the solid surface. Thus the momentum gradient term;

$$V\nabla V = \frac{0.5*(V_{j+1}+V_j)(V_{j+1}-V_j)}{\Delta x_{j+1/2}}$$ becomes;

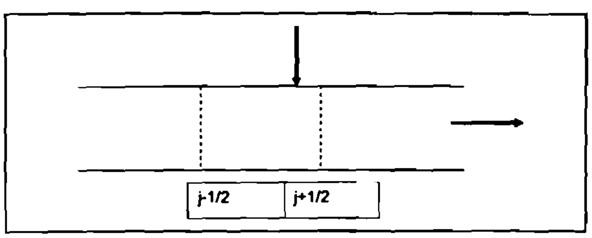
$$V\nabla V = \frac{0.5*(V_j)(-V_j)}{\Delta x_{j+1/2}} \quad \text{if} \quad V_{j+1/2} = 0 \quad \text{and;}$$

$$V\nabla V = \frac{0.5*(V_{j+1})(V_{j+1})}{\Delta x_{j+1/2}} \quad \text{if} \quad V_{j+1/2} = 0$$

This will correctly result in the Bernoulli pressure drop of $1/2\rho V^2$

CONSERVATION OF MOMENTUM

- ASSUME FLOW IN FROM BRANCH, NO FLOW IN MAIN LINE



CONSERVATION OF MOMENTUM

- Momentum in Run of Branch Connection

$$V\nabla V = \frac{0.5*(V_{j+1}+V_j)(V_{j+1}-V_j)}{\Delta x_{j+1/2}} \qquad V\nabla V = \frac{0.5*(V_{j+1})(V_{j+1})}{\Delta x_{j+1/2}}$$

However;

$$A_{j+1}V_{j+1} = A_{j+1/2}V_{j+1/2}$$

$$A_jV_j = A_{j+1/2}V_{j+1/2}$$

$$A_jV_j = A_{j-1/2}V_{j-1/2}$$

$$V\nabla V = \frac{0.5*(\frac{A_{j+1/2}}{A_{j+1}}+\frac{A_{j+1/2}}{A_j})V_{j+1/2}(\frac{A_{j+1/2}}{A_{j+1}}V_{j+1/2}-\frac{A_{j-1/2}}{A_j}V_{j-1/2})}{\Delta x_{j+1/2}}$$

CONSERVATION OF MOMENTUM

$$V\nabla V = \frac{0.5*(\frac{A_{j+1/2}}{A_{j+1}}+\frac{A_{j+1/2}}{A_j})V_{j+1/2}(\frac{A_{j+1/2}}{A_{j+1}}V_{j+1/2}-\frac{A_{j-1/2}}{A_j}V_{j-1/2})}{\Delta x_{j+1/2}}$$

$$A_{j+1} = A_{j+1/2} = A_{j-1/2}$$

$$V_{j-1/2} = 0.0$$

$$V\nabla V = \frac{0.5*(1+1)V_{j+1/2}(1*V_{j+1/2}-1*0)}{\Delta x_{j+1/2}} = \frac{V_{j=1/2}^2}{\Delta x_{j+1/2}}$$

CONSERVATION OF MOMENTUM

- The term may not conserve momentum from one cell to another as the V multiplying the gradient is different from one cell to the next. Does this become a problem when phase change is present or when the flow is oscillating?

WATER PACKING

- Water packing often occurs in several problems.
- Is the level tracking model versatile enough that it can be used in all cases where water packing may occur?
- Is the intent to eventually replace the water packing fix with the level tracking model?

FINDINGS: Level Tracking

The manual states that exaggerated momentum transfer can occur in TRACE 5.0 when a steam/water droplet mixture flows down towards the surface of a liquid pool due to the use of the non-conservative motion equations rather than using the fully conservative momentum equations. It is recommended that the solution, to this problem is to engage the TRACE interface tracking model, when practical.

- The interface tracking model is activated only when the user specifies for it to be used and only when the criteria specified for interface recognition are met.
- How does the user know when he should activate it?
- What are the chances that the user will invalidate the code assessment by specifying this model inappropriately?

Non-Condensable Gases

- Solving n-noncondensable gas equations need not require the addition of any more than one additional equation if the conservation of mass and energy equations are partitioned appropriately.
- The non-condensable gas species specific heats should be temperature dependent. They are currently constant.
- The specific heat of the gas/vapor mixture are calculated incorrectly. The current definition used is based on partial pressure ratio. It should be based on mass fractions:
- The gas mixture properties, viscosity and thermal conductivity, should be based on accepted methods for calculating gas mixtures properties rather than using pressure ratios to define the mixture properties.
- When will the new method for handling the effects of non-condensable gases be available. The current method is wrong and requires that the interface be at the temperature corresponding to the bulk steam partial pressure.

Non-Condensable Gases

- GAS MIXTURE SPECIFIC HEAT
  - Current:

$$c_{pg} = \frac{p_v C_{pv} + p_a C_{pa}}{p}$$

  - Should be:

$$C_{pg} M_{g_{tot}} = C_{pv} M_s + C_{pa} M_a \Rightarrow C_{pg} = \frac{M_s}{M_{g_{tot}}} C_{pv} + \frac{M_a}{M_{g_{tot}}} C_{pa} \Rightarrow \frac{\rho_s}{\rho_{gas}} C_{pv} + \frac{\rho_a}{\rho_{gas}} C_{pa}$$

Non-Condensable Gases

- Transport Properties:
  - Current:

$$\mu_{pg} = \frac{p_v \mu_{pv} + p_a \mu_{pa}}{p}$$

  - Should be:

$$\mu_{mix} = \sum_{i=1}^{n} \frac{x_i \mu_i}{\sum_{j=1}^{n} x_j \Phi_{ij}}$$

SATURATED STEAM INTERNAL ENERGY

- The equation of state for the saturated vapor internal energy is inadequate between the pressures of 1e5 and 2e6 Pascals.
- The derivative of the internal energy with respect to pressure or temperature actually changes sign in this region.
- This is in a pressure range of primary interest for small and large breaks.
- This also results in an error in the superheated vapor internal energy calculation.

Summary of the Review of TRACE V5.0

by D. Bestion

Scope of the review – Method of Work

- Review based on documentation : Theory Manual, Assessment Manual,
- Focussed on field equations (1D, 3D, but not the TEE) and closure models, a few flow process models, and on the assessment.
- The assessment considered SET and IETs devoted to PWR LOCAs.
- For each closure model
  - Importance with regard to safety.
  - Correctness and adequacy of the model with regard to the up to date knowledge
  - Consistency with the intrinsic limitations of the two-fluid model.
  - Degree of empiricism with regard to the physical understanding of the corresponding flow process.
  - Validation of each model in a SET way
  - Adequacy of the section of the Theory Manual
- At last, for each model, recommendations may be given for
  - additional R&D work in view of improving the model,
  - additional validation.
  - improving the documentation

Main Conclusions of the review 1/2

TRACE V5.0 appears to be a good system code with extended capabilities for simulations of LOCAS of PWRs and BWRs.

EQUATIONS & CLOSURE MODELS

- An impressive work done to revisit all closure models, and improve some old correlations
- A coherent set of models.
- Most models seem fully adequate and reflect the present state of the art.
- The degree of empiricism of most models is consistent with the available understanding of flow processes.
  - Mechanistic models were selected or developed when it was possible,
  - some tuning on experimental data was added when necessary
  - pure empirical models were selected when no other approach could do a better job.
- A few models may have an unnecessary degree of sophistication
- A few models may require further analysis and further improvements, such as the DCC, the top-down reflooding, the modelling of noncondensable gases, the stratification criterion, ...
- No big flaw was identified in equations & in closure models which might lead to wrong predictions and to erroneous conclusions on safety issues

NUMERICS:

- The Level Tracking Method of TRACE V5.0 performs remarkably well !

Main Conclusions of the review 2/2

ASSESSMENT:

- SETs and IETs validate many models and covers many physical situations encountered in accidental transients.
- Some validation calculations are not sufficiently analysed
- Additional assessment is still required for a more exhaustive coverage of all models and of all important phenomena encountered in reactor transients.
- No big flaw was revealed by assessment calculations
- Some checks on some models and some additional assessment are necessary to finally demonstrate that there is no flaw.
- The documentation of the physical modelling in the Theory Manual gives not only the selected equations and closure models but also some justification of the choices.
- The documentation of the Validation and Verification in the Assessment Manual presents the general assessment methodology based on PIRT tables and the results of each SET or IET simulation.

Recommendations :

- the analysis of some calculations should be improved
- each assessment work should be related to the PIRT table,
- a cross reference matrix with the models against the SET matrix should be added
- the range of parameters in which each closure law is validated in a separate effect way should be identified.
- some recommendations to users based on assessment work (e.g. recommendations on mesh size and time step) should be added,

High priority recommendations about models (1/2)

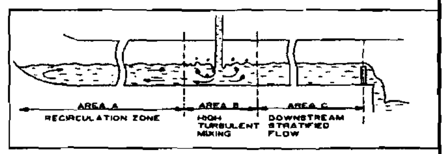
Stratification criterion

$$|V_v - V_l| < U_{crit} = \frac{1}{2}\left[\frac{(\rho_l - \rho_v) g \alpha A}{\rho_v D}\right]^{1/2}$$

- Criterion 1: KH instability
- Criterion 2 : sedimentation of bubbles in turbulent flow
- Criterion 3: based on the Richter flooding limit (UPTF Hot Leg): should be replaced by a user option with free constants (geometry specific)

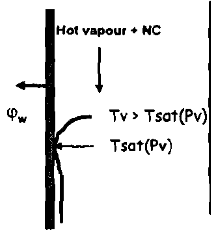
Direct Contact Condensation

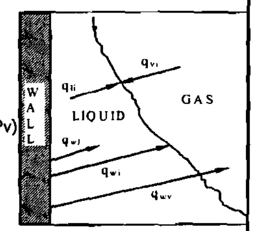
- The enhancement of the condensation due to ECCS jet induced turbulence should be modelled in TRACE and validated in a separate effect way.



Film condensation

- A model for "overheated condensation" should be implemented to allow starting condensation in single phase overheated steam or gas mixture when Tw < Tsat(Pv)
- This should use q_{wi} to create liquid.
- Then when some liquid film exists, condensation can be treated by q_{wl} and q_{li}.



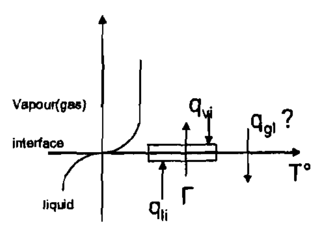


High priority recommendations about models (2/2)

Noncondensable gases

- The q_{gl} term and the multiplication of q_{vi} by Pv/P in presence of NC gases should be reconsidered and possibly limited to some extreme situations (Pv << P & Tv<Tsv or Tv<0°C)

  q_{li} = Ai h_{li} (Tl-Tsv)
  q_{vi} = Ai Pv/P h_{vi} (Tv-Tsv)
  q_{gl} = Ai Pnc/P h_{vi} (Tv-Tl)



ΓHl + q_{li} + q_{vi} = Γ Hv
q_{li} = Ai h_{li} (Tl-Tsv)
q_{vi} = Ai h_{vi} (Tv-Tsv)

Wall heat transfer selection logic

- allow wall to liquid heat exchange for $\alpha \geq 0.9999$
- select the film condensation model with a criterion based on the film thickness rather than on the void fraction (alfa>0.9)

CCFL

- The reason for the misprediction of the Wallis type flooding curve in small diameter pipes should be clarified.
- If necessary the CCFL model implementation in the equations and the solution procedure should be revised.

Medium priority and long term recommendations about models

Medium priority recommendations

Flow regime map
- Add a stratified-mist flow regime (large Dh, HL,CL) for relatively low void fraction

Interfacial friction in pipes
- τ_i in bubbly & cap bubble flow in pipes might be revisited focussing on situations encountered in SG pipes with cooling walls

Momentum equations
- Check the capabilities of the term proportional to gradient of liquid height in horizontal 1D flow in a test case with area change, change of slope.

Core interfacial friction
- Correct the bias on τ_i in the core (α overpredicted) in low P (RBHT) as well as at higher P (THTF). Improve predictions in PWR conditions without degrading in BWR.

Flashing
- add a simple flashing delay modelling in the q_i at least to allow sensitivity tests (+ uncertainty)

Film condensation with NC
- envisage simplifications of the mass diffusion effects on condensation

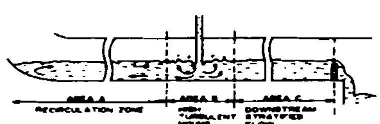
Long term recommendations

Flashing
- implementation of a physically based qi model in flashing conditions would allow a 1D simulation of critical flow without any addition of a chocking model

Momentum equations
- 1D & 3D: implementation of the added mass force and use of a well posed model
- 3D: implementation of a turbulent dispersion force rather than a lift force

High priority recommendations about validation 1/2

Direct Contact Condensation
- ➢ Condensation at ECCS injections should be validated in a Separate effect way in conditions representative of both LBLOCAs and SBLOCAs.
- ➢ Non condensable gas effects in Refill and Reflood should be addressed in assessment calculation



Film boiling in blowdown
- ➢ Investigate possible effects of the mass flux and of subcooling on the overall HT in IAFB
- ➢ Compare range of G & X of THTF tests / reactor LBLOCA. The validation against SET of IAFB modelling should probably be extended to low X & G> 500 kg/m2/s

Downcomer Refill
- ➢ A methodology with respect to the effects of N2 should be defined, and should be used in the validation and recommended for reactor applications.
- ➢ A reference 3D nodalization for the PV should be defined for the core and the downcomer and applied to both the validation calculations and the reactor applications.

High priority recommendations about validation 2/2

Reflood

- A clear policy with respect to the use of non converged 3D nodalization in the PV should be defined. Justify nodalization for Reflood tests, do sensitivity tests to the meshing to evaluate the error due to non convergence.
- The sensitivity to the time step should also be investigated for Reflood calculations in order to give precise recommendations
- Oscillations during Reflood should be addressed in representative conditions by available SETs and IETs. LOFT not representative due to scale distortion, SCTF and CCTF not representative since oscillations were avoided by using LP injection

De-entrainment in upper Plenum and CCFL at UTP during Reflood

- Entrainment de-entrainment in UP and HL and CCFL at the UTP of the Core during reflood should be validated in a separate effect way. SCTF, CCTF not prototypical with respect to this phenomenon. Consider UPTF tests (UPTF-10c,..)

Hot Wall heat transfer in downcomer during Reflood

- Hot Wall heat transfer in downcomer during Reflood should be addressed in representative conditions: Reflooding duration is too short in LOFT tests and the calculated UPTF tests did not address this phenomenon. One may suggest the use of JAERI tests.

CCFL

- The reason of deviation from the prescribed flooding curve? model correction?
- Sensitivity to the meshing & recommendations to users.
- Check in reactor geometry that standard τ_i does not impose a more severe limitation
- Extend validation of CCFL model to SG Inlet Header, inlet of SG tubes.

Medium priority recommendations about validation

Assessment methodology

- **Each assessment report should recall the PIRT table** and the phenomena they are supposed to address in these tables, and conclusions on these phenomena and the related models should be drawn.
- **Cross reference matrix with all closure models & flow process models against tests of the SET** matrix.
- The **range of parameters** in which each closure law is validated in a separate effect way should be identified.
- **Recommendations for input modelling** including nodalization and time step requirements should be given based on each assessment work. Such recommendations should be reflected in the User Manual
- What is presented as "fundamental validation tests" should be either classified in "Validation" or in "Verification" tests according to the standard nomenclature.

+ Some required improvements of assessment work on:

Critical Flow, Stratified flow, 3D power distribution effects in SBLOCAS, Loop Seal clearing, Interfacial friction in core, Interfacial friction in tubes, Convection to liquid, Nucleate boiling, Convection to vapour, Downcomer Refill, Reflood, Film condensation (Appendix 1 ▶)

High priority recommendations about documentation

Field equations

- Separate the derivation of balance equation for the 1D module (and Tee component) and for the 3D PV model

3D Vessel model

- More pedagogy : all simplifications should be listed and justified.
- The scale of space averaging should be clearly specified for each subcomponent of the PV
- The volume averaging of 3D equations should be presented showing how the porosity appears and what simplifying assumptions allow to eliminate it
- Better justify the absence of turbulent diffusion and of dispersion terms and the limits of applicability should be more clearly identified.
- A clear policy with respect to the use of non converged 3D nodalization in the Pressure Vessel should be defined.
  - One possible approach is to define a reference nodalization of the Pressure Vessel and to validate together the physics + numerics + nodalization against scale 1 data.
  - An alternative is to evaluate the non-convergence error from assessment calculations and to use it in uncertainty for reactor applications.

+ *Recommendations on documentation of :* CCFL, Critical flow, Interfacial friction, CHF, Level tracking method, Natural convection heat transfers,...(Appendix 2 ►)

Summary of Conclusions

- ❑ No big flaw found
- ❑ TRACE V5.0 is already a good system code for LOCAS of PWRs and BWRs.
- ❑ A few models require further analysis and improvements
- ❑ Assessment should be extended and improved in some cases

- ❑ Then for long term (additional recommendations not present in the report):
  - Add a 3rd field (droplets)
  - Improve modelling of PV (allowing local mesh refinements)
  - Dynamic modelling of turbulence & Ai

Appendix 1: Medium priority recommendations about validation 2/2

Some required improvements of assessment work

Critical Flow
- Improve the analysis of validation against Marviken tests & Moby Dick tests
- Other geometrical configurations and other upstream flow conditions?

Stratified flow
- TPTF Horizontal flow tests : nature of the flow subcritical or supercritical?
- Add validation of τ_i in geometry and flow conditions encountered in HL or IL of PWRs

3D power distribution effects in SBLOCAS
- 3D power distribution effects under core uncovery situation during SBLOCAs should be validated

Loop Seal clearing
- Loop Seal clearing should be addressed by a SET validation

Interfacial friction in core
- Convergence tests to the noda size : recommendations to users.
- Improve the analysis of validation against THTF Mixture level tests & RBHT transient Uncovery tests

Interfacial friction in tubes
- Extend the validation range of Kataoka-Ishii model.
- The prediction of some α fluctuations in GE Level swell test N°100-3 should be analysed.

Convection to liquid
- Check availability of NESTOR data, (HT coefficient in a real rod bundle)

Nucleate boiling
- SET validation of the ONB & of nucleate boiling model

Convection to vapour
- Further validate wall to vapour Convection model

Downcomer Refill
- Sensitivity tests to DCC in the downcomer and to ΔP from downcomer to the containment for UPTF tests 6 and 7.
- analyse the effect of the τ_i in annular-mist flow regime (high subcooling test UPTF 5)

Reflood
- Additional analysis of SCTF Reflood tests & RBHT

Film condensation
- Assess both the standard (Sklover and Rodivilin) and advanced model for film condensation in presence of NC with UCB-Kuhn tests.
- Extend the assessment against University of Wisconsin condensation tests to other geometrical configurations with downward facing cooling wall.

D. Bestion
July 7th 2008

13/12

Appendix 2: High priority recommendations about documentation

CCFL
- CCFL model implementation in the equations and the solution procedure in the theory manual.

Critical flow
- Choking does not occur when a zero characteristic velocity is reached.
- Make clear that the present flashing model cannot be used for break flow prediction.
- Do not use the terminology "1-D critical flow model" : the TRACE model is 0-D

Interfacial friction
- Clarify if the rod bundle drag models are applicable in the secondary side of SG.

Reflood
- Mention that U tube oscillations validate the capability neither to add any numerical dissipation nor to damp manometer type oscillations (core and downcomer during Reflood)

CHF
- The function of the static quality mentioned in eq. 6-113 should be given.
- Specify how the positive flow is defined in case of vertical, horizontal and inclined flow. (3D?)

Level tracking method
- The level tracking method should be presented in the Theory Manual.

Natural convection heat transfers:
- Reference should be given for the coefficients of the NC correlations.
- Are models applicable to vertical pipes only or also to horizontal and inclined pipes?
- In the 3D module, identification of vertical or horizontal or inclined wall? NC HT takes account of wall orientation?
- A NC correlation established in vertical tubes is used in rod bundles and not for tubes .Why?

D. Bestion
July 7th 2008

14/12

Appendix 1: Medium priority recommendations about validation 2/2

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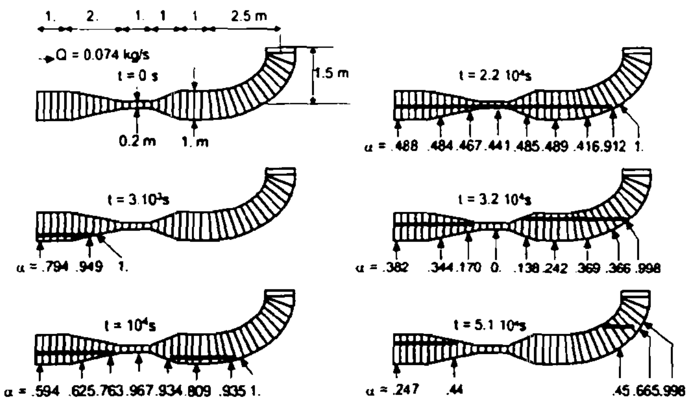
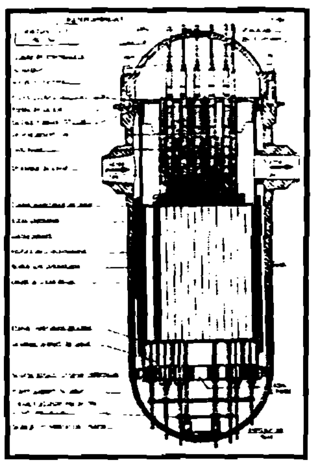
Appendix 3: a benchmark for momentum equations:axial effects of transverse gravity force



D. Bestion, J.C. Micaelli, *A two-fluid stratified model suitable for a pressurized water reactor safety code*, 4th Miami Int. Symp. On Multiphase transport & Particulate Phenomena, dec 1986



Appendix 4: About 3D pressure vessel

$$A \cong \langle A_i \rangle + \delta A$$

$$\langle \chi_i \rho u_j \frac{\partial u_i}{\partial x_j} \rangle_i = \rho \frac{\partial}{\partial x_j}\left[\phi \langle u_i \rangle_i \langle u_j \rangle_i\right] + \rho \frac{\partial}{\partial x_j}\left[\phi \langle \delta u_i \delta u_j \rangle_i\right]$$

Macroscopic convection — Momentum dispersion

$$\langle \chi_i \frac{\partial \rho u_i H}{\partial x_i} \rangle_i = \frac{\partial}{\partial x_i}\left[\phi\rho \langle u_i \rangle_i \langle H \rangle_i\right] + \frac{\partial}{\partial x_i}\left[\phi\rho \langle \delta u_i \delta H \rangle_i\right]$$

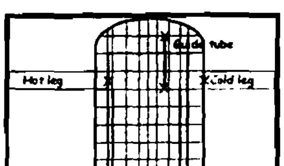
Enthalpy dispersion

In a core during LBLOCA:

Turb. Diffusion < dispersion < interfacial & wall transfers

With coarse meshing:

	impact on PCT
Turb. Diffusion	2K
< dispersion	5K
< numerical diffusion	10K
< numerical error on interfacial & wall transfers	30K
< uncertainty on interfacial & wall transfers models in core	80K
< total uncertainty due to IC, BC, all models	150K



Comments from review of the TRACE manuals, in particular the Physical Models sections

Presentation to the ACRS

G. Yadigaroglu

7 July 2008

1/22

Scope (this Panel member)

Reviewed in detail, in the Theory and Assessment Manuals:

- Completeness and readability of the Theory Manual
- Overall modelling approach
- Particular models:
  - Drag Models
  - Interfacial Heat Transfer Models
  - Wall Heat Transfer Models
- Related, selected Assessment cases
- Also: Appendix A: Quasi-Steady Assumption and Averaging Operators

Some topics covered also by other Panel members are not repeated here

2/22

Warning

- A lot of good work has been accomplished or attempted, or is planned

but

- Comments are made here only for the *negative* findings
- There are no comments for whatever is positive, well done, and fine

3/22

Interactions with the developers

- Excellent information meetings with the developers' team where lots of information was provided and questions answered
- Regret the absence of dialog regarding the interim written comments made: Practically all the replies simply promised action, for the vast majority of the comments, within two years, as "mid-priority issues."
- More interactive dialog would have benefited the process

4/22

State of the code

- We learn by reading the manual that some important changes will be implemented *in the future* to overcome presently detected shortcomings… (some also detected by the Review Panel)
- This is highly recommended

but

- makes the present review tentative…

5/22

Transparency of the top-level modeling approach

- A regrettable lack of the presentation of the top-level "strategic approach" to modeling (in spite of lots of good work for the selection of the best available models and correlations and).
- No *top-level* definition of the flow regimes (although information given in the particular sections)
- Is the top-level selection of flow regimes, phenomena and situations to be simulated, and the corresponding selection of methods and models for these unique and consistent?
- The flow regimes should have been selected in a unique fashion for both hydraulics and heat transfer; this is apparently only partly the case now?
- Recommendation: Present the code logic (flow diagram) used in selecting flow and heat transfer regimes and the corresponding models and correlations, possibly in a clear but detailed graphical form (with references to the corresponding sections of the Manual). Indicate also when these models came from older code versions.

6/22

Level of detail / balance in modeling

- Some (easy to model, but not necessarily very important) items are modeled with (unnecessarily) extreme detail and unwarranted precision, while other, more difficult items are still to be improved
- Some difficulties in converting historically "mixture-based" correlations and models to the two-fluid framework
- Some difficulties with the wall-gas-interface-liquid logic and transfers

7/22

Modeling: "mixing" models and correlations

- Often ad-hoc local adjustments are made to existing models and correlations to adapt them to the needs of the code
- The choice/changes of correlations has often been done in an ad-hoc, piecewise, "local" way (fix an existing problem)
- Very often constants in the models have been modified to better match a *particular* set of relevant data
- The models have often been "mixed" by picking pieces here and there from the work of various authors; consistency is not evident (e.g. Kuhn et al., De Cachard, Chen, ...)

8/22

Transparency of Code Validation

- Lots of new models are proposed: at the end, without extensive developmental validation results one cannot assess their adequacy
- Need a cross-reference table showing how the capability of the code was assessed for each phenomenon considered and where this information can be found
- Provide also information on the "model development tests" (tests at elementary level)

9/22

Readability and contents of Manual

Needed/recommended:

- A Chapter outlining the top-level modeling strategy
- A Chapter defining the flow and heat transfer regimes once and for all (rather than "locally" within the following chapters and several times)
- The historical presentation of model development is interesting and helps understand the choices made, but makes the Manual difficult to read/misleading:
  - e.g., the historical remarks could be printed in smaller font or as footnotes.
  - The reader should be able to find rapidly the model actually implemented
- Numbering of the sections, subsections etc. of the Manual would help the reader and outline the logic of the code
- Create links between a graphical presentation of the flow diagram/logic of the code and the Sections describing its models
- Eliminate repetitions (due to the format and the lack of top-level regime definition)
- Could the user get some help in accessing certain old but important reports (Typically ANL, Ishii and coworkers)?

10/22

Practical limitations of the review work

- It was clearly not possible within the limits of this review to *verify* the adequacy of all the models used and the way they were implemented. For this, one would have, e.g., to consult some of the original publications ... (regarding range of correlations, data used in developing them, etc.)
- Even if more resources were available, it would not have been possible to review the details of the validation work in relation to the adequacy of the models implemented in TRACE. Years of peer validation will be needed for this (CAMP like)

11/22

Particular issues

Most important "problems„ raised here

Numerous additional remarks made in the report

12/22

Extracting Interfacial Shear from Void Fraction Data

$$0 = -<1-\alpha>\frac{dp}{dz} + g\rho_l <1-\alpha> \cos\theta - \frac{P_{wl}\tau_{wl}}{A} + \frac{P_i\tau_i}{A}$$

$$0 = -<\alpha>\frac{dp}{dz} + g\rho_g <\alpha> \cos\theta - \frac{P_{wg}\tau_{wg}}{A} - \frac{P_i\tau_i}{A}$$

multiplying the first equation by $<\alpha>$, the second by $<1-\alpha>$ and subtracting, one obtains

$$\frac{P_i\tau_i}{A} = -g(\rho_l - \rho_g)<1-\alpha><\alpha>\cos\theta + \frac{P_{wl}\tau_{wl}<\alpha>}{A} - \frac{P_{wg}\tau_{wg}<1-\alpha>}{A}$$

The shear terms were neglected. Void fraction from vessel tests was used. What happens in horizontal flows? More validation needed.

13/22

The difference between the true average relative velocity ($<V_g - V_l>$) and the difference of the true phase velocities, $V_r \equiv \overline{V}_g - \overline{V}_l$

- An unnecessarily complicated, approximate treatment in the Manual based on Ishii & Mishima
- Believe that a revisit of the issue from the very fundamental point of view would be worth the effort:
  - Consistency of the physical reality with the assumptions (of the DF model)?
  - How were the relevant data collected?
  - Believe that an exact treatment is possible...

14/22

Interfacial Heat Transfer

- Should recall at the very beginning briefly the logic and the scheme used in the two-fluid framework for heat transfer:

  wall > fluid(s) > interface > fluid

  and make a general statement on how this will be implemented
- Show/identify exceptions to this logic when needed (e.g., IAFB)

15/22

Common film model for annular flow and for condensation

- I believe that a separate treatment is needed for downward condensation in tubes (when the steam could be practically stagnating)
  (This is apparently planned)

16/22

Condensation

- Why is interfacial heat transfer for stratified flow treated separately (and differently?)
- Laminar versus turbulent films treated correctly?
- Justification for "adding" (Eq. (5-49) the laminar and turbulent interfacial htc's
- Why Kuhn vs Bankoff?
- Lots of "mixing" of authors and correlations in the last, retained version of the model...

17/22

Critical Heat Flux

- The Manual mentions some limitations, while the most important one that all CHF correlations are for steady-state situations while CHF occurs during a LOCA under **transient conditions**, is ignored.
- Why CHF is based on a *temperature* difference (and not on a heat flux)?
- **CHF models** seem to be largely untested
- Warning about use of correlations developed for co-current flow in counter-current flow situations

18/22

Post-CHF heat transfer

- The superheating of the vapor is ignored by transferring the heat wall > liquid...
- **Post-CHF Heat Transfer logic:** correct?
- Some apparent inconsistencies: T_{CHF} defined as *the* CHF criterion but later mentioning T_{CHF} and x_{crit} as CHF criteria...

19/22

Rewetting and reflooding

- Belief in the (T_{CHF}, T_{min}) representation ignoring the progression of the QF as *the* controlling mechanism
- Ignoring lots of previous work and starting from scratch with questionable results
- Minimum Film Boiling Temperature: Difference between quench temperature and rewetting (knee) temperature in reflooding "mistreated" (old beliefs remain). Object to the implemented "modification in the vicinity of the quench front to improve ... performance" .

20/22

Modeling of IAFB and DFFB

- New models do not seem to be successful and are largely untested
- Why is all the previous work ignored?
- DFFB, Two-Phase Enhancement of Convective Heat Transfer:
  questionable approach based on turbulence enhancement; it is the presence of the *droplets* that controls the vapor temperature

21/22

Appendix A of Manual

- About the **quasi-steady-state assumption**: Not very useful and relevant – could eliminate

22/22

TRACE Manual Review

Peter Griffith
7 July 2008

Introduction

- Charge to PG
  - Review component models
  - Check constituative models
- CSAU recalled
- Comments on several specific models
- Conclusions

CSAU Recalled

- Most significant uncertainty — Post CHF HTC
- Details of HT package had little effect
- Pump model had very little effect on outcome
- Worst results most often were due to code user errors — the text is very important in preventing this

7 July 2008 P Griffith 3

Calculating PCT

- HSTR -PWR core design
- Nodes .3m on a side
- TRACE for fluid mechanics
- Heat transfer including forced convection, CHF, post CHF
- Radiation - RADENC
- PCT calculated from above

7 July 2008 P Griffith 4

Some problems

- Post CHF from lookup table-fluid properties only-no surface effects
- Concern-radiation might be put in twice-
  - once from lookup table
  - once from RADENC

Instructions should prevent this

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Important editorial changes

- Move out-of-date models to a new list
- Consolidate Component Models and modeling guidelines
- Compare by plotting measured vs calculated PCT
- Rewrite separator text
- Plot variables vs inventory rather than time

7 July 2008 P Griffith 6

Misplaced precision

- BREAK - legislate break flows
- RADENC - Property knowledge doesn't justify this level of detail
- PUMP - Unless pumps run during a LOCA this level of precision isn't justified

7 July 2008 P Griffith 7

Ideal component models

- Schematic showing all flows in and out
- Nodalization diagram
- Identify which constituative relation(s) should be used for which application

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Conclusions

- Rename the subroutines by component
- Get a technical editor for the manual
- Add new component models
  - PWR cores
  - Accumulators
  - Steam generators
  - Separators
  - FW heaters
  - MSRs
  - Flow splitting module

TRACE

TRACE MOMENTUM EQUATION ISSUE

Stephen M. Bajorek, Ph.D.
Senior Technical Advisor for Thermal-Hydraulics
Office of Nuclear Regulatory Research
United States Nuclear Regulatory Commission

TRACE Introduction and Outline

- AT THE DECEMBER 5, 2006 SUBCOMMITTEE MEETING THERE WAS A DISCUSSION ON THE MOMENTUM EQUATION FORMULATION IN TRACE.
- ISSUES AND CONCERNS INCLUDED:
  - WHY IS THE FORMULATION IN TRACE, WITH ITS AVERAGING AND APPROXIMATIONS, AN APPROPRIATE REPRESENTATION OF THE VECTOR NATURE OF THE FLOWS ?
  - DO APPROXIMATIONS INTRODUCE SYSTEMATIC ERRORS ?
  - IS THE FORMULATION ADAPTABLE TO COMPLEX NODES SUCH AS WHEN 1D COMPONENTS ATTACH TO THE VESSEL, TEE-JUNCTIONS, BENDS, LOWER PLENUM, etc. ?

TRACE Introduction and Outline

[illegible]

- ITEMS TO BE COVERED:
  - DEVELOPMENT OF A SET OF TEST PROBLEMS [illegible]
    [illegible]

TRACE Test Problems

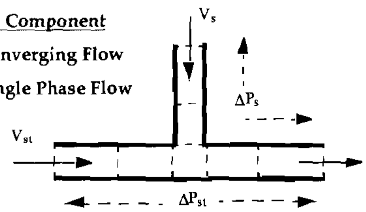
[illegible]

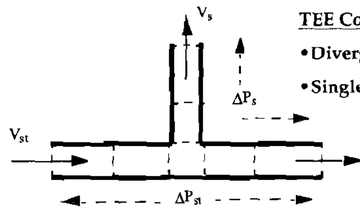
- EACH PROBLEM INVOLVED A SIMPLE GEOMETRY [illegible] AN INDEPENDENT ANALYTICAL SOLUTION FOR [illegible] DROP WAS AVAILABLE.
- EXAMPLES OF TEST PROBLEMS INCLUDE:

[illegible]

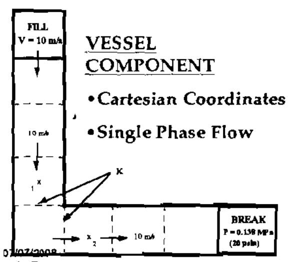
TRACE Problem Areas

TEE Component
- Converging Flow
- Single Phase Flow

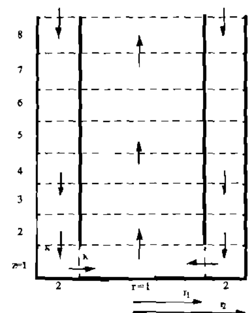




TEE Component
- Diverging Flow
- Single Phase Flow



VESSEL COMPONENT
- Cartesian Coordinates
- Single Phase Flow



VESSEL COMPONENT
- Radial Coordinates
- Single Phase Flow

07/07/2008
smb4@nrc.gov

5

TRACE Plant Model Steady-State

Upstream Location	Downstream Location	Comment	TRACE (psi)	Expected (FSAR) (psi)	SMB Memory (psi)
11	12	Hot Leg ΔP	1.8	1.3	
12	13	SG Inlet Elbow ΔP	10.6		
13	14	SG Tube ΔP	32.3		
14	15	SG Outlet Nozzle ΔP	3.2		
12	15	SG ΔP	46.1	40.1	45
15	1	Loop Seal DP	2.3	3.3	
1	2	Pump ΔP Rise			
2	3	Cold Leg ΔP	2.7	3.4	
3	5	Vessel Inlet Nozzle ΔP	16.4		3
5	6	Downcomer ΔP	4.9		1
6	7	Lower Plenum ΔP	21.4		2
7	8	LCP ΔP	1.4		2
8	9	Core ΔP	29.6		30
9	10	UCP ΔP	2.3		2
10	11	Upper Plenum ΔP	2.4		2
3	11	Vessel ΔP	83.9	47.6	

TRACE Version 5.0 steady-state predictions showed excessive pressure drop at several locations.

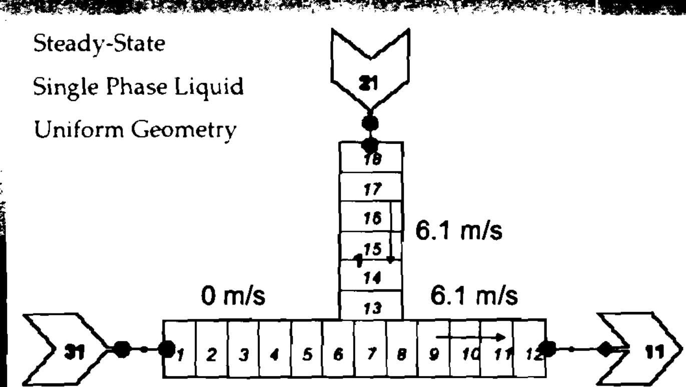
90° bend

180° bend

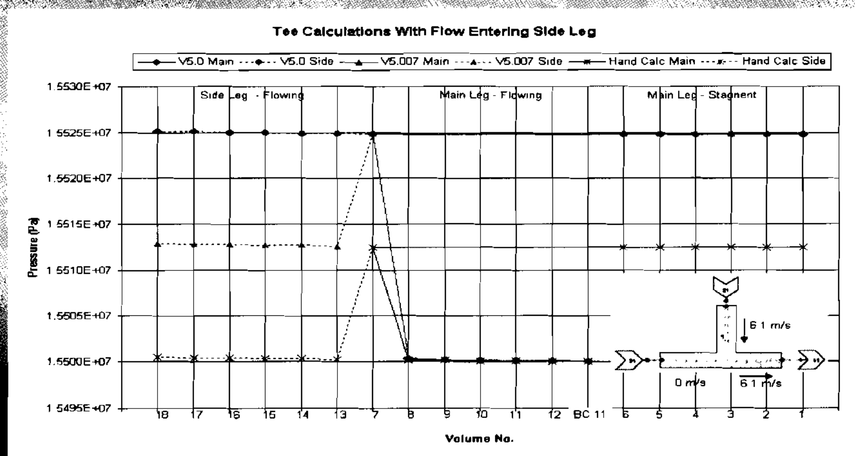
TRACE Formulation Errors

- Side Connections in TEE Components
- Locations where flow encountered a solid surface
  - ✓ Top and Bottom Nodes of the 3D VESSEL

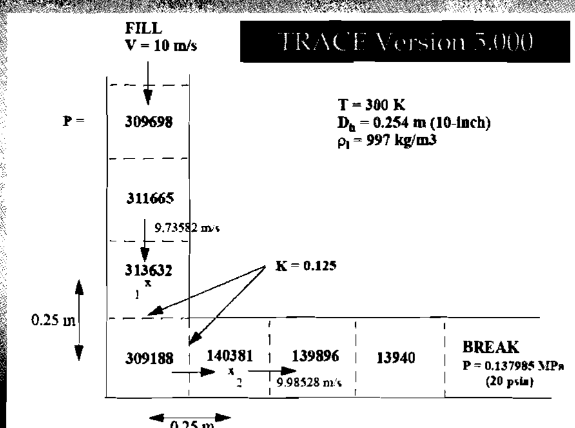
TRACE Flow Split Test Problems



TRACE Flow Split Test Problems



TRACE VESSEL Cartesian Elbow

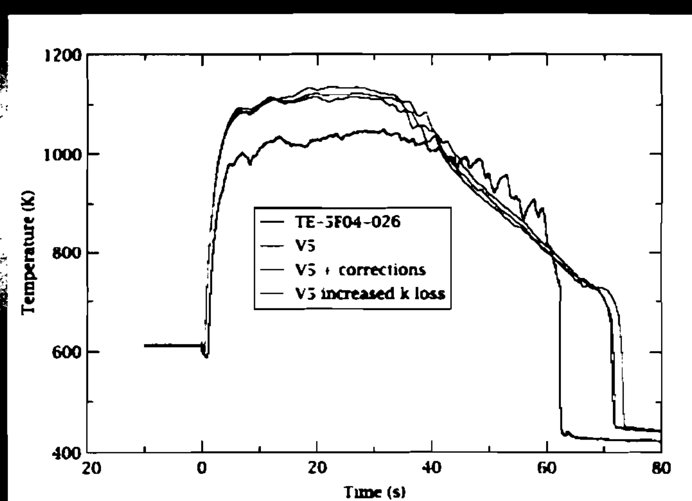


- TRACE Version 5.000
  ΔP = 25.1 psi
- Hand Calculation
  ΔP = 1.6 psi
- TRACE Version 5.007
  ΔP = 3.5 psi

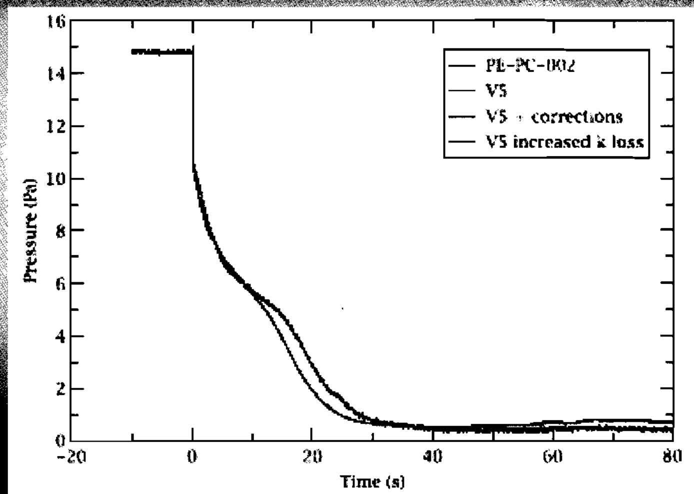
TRACE — Effect of Errors on Calculations with TRACE

- LOFT L2-5 WAS A LARGE BREAK LOCA INTEG[illegible] SIMULATING A COLD LEG DEG WITH EARLY PU[illegible]
- [illegible]

TRACE — LOFT L2-5 SENSITIVITY



TRACE LOFT L2-5 SENSITIVITY



TRACE Conclusions from LOFT Sensitivity Study

- THE LOFT L2-5 SENSITIVITY STUDY SUGGESTS THAT THE EFFECT OF THESE MOMENTUM EQUATION DEFICIENCIES ON A LBLOCA TRANSIENT ARE SMALL.
- THE UNCERTAINTY ASSOCIATED WITH THE MOMENTUM EQUATION DEFICIENCIES APPEARS SMALL COMPARED TO UNCERTAINTIES RELATED TO CLOSURE MODELS.
- A SMALL EFFECT IS EXPECTED IN PWR & BWR LOCA CALCULATIONS, BUT THIS SHOULD BE VERIFIED BY ADDITIONAL CALCULATIONS ONCE THE CODE REVISIONS ARE COMPLETE.

TRACE Additional Actions Taken

- CONSERVATION EQUATIONS TO PROVIDE A MO
DESCRIPTION OF THE MOMENTUM EQUATION

TRACE Summary & Conclusions

- REVISIONS TO TRACE HAVE BEEN PROPOSED AND ARE
BEING TESTED. IF REVISIONS PROVE TO BE UNACC
TRANSITION TO A FULLY CONSERVATIVE FORM OF