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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS (ACRS)

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SUBCOMMITTEE ON THERMAL-HYDRAULIC PHENOMENA

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TUESDAY,

DECEMBER 5, 2006

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

MEMBERS PRESENT:

SANJOY BANNERJEE Chairman
SAID ABDEL-KHALIK ACRS Member
THOMAS KRESS ACRS Member
JOHN D. SIEBER ACRS Member
GRAHAM B. WALLIS ACRS Member

ACRS STAFF PRESENT:

RALPH CARUSO

ALSO PRESENT:

STEPHEN BAJORK

BUTCH BURTON

JOSEPH KELLY

JOHN MAHAFFY

MARINO di MARZO

CHARLES MURRAY

C-O-N-T-E-X-T

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

8:31 a.m.

CHAIRMAN BANNERJEE: The meeting will now come to order. This is a meeting of the Advisory Committee on Reactor Safeguards, Subcommittee on Thermal Hydraulic Phenomena. I am Sanjoy Banerjee, Chairman of the Subcommittee. The subcommittee members in attendance are Tom Kress, Graham Wallis, Michael Corradini who seems to be absent and Jack Sieber. Said Abdel-Khalik is participating via video conference for this meeting.

The purpose of this meeting is to review the continuing development of the TRACE thermal hydraulic computer code. The subcommittee will hear presentations by and hold discussions with representatives of the NRC staff, the contractors, regarding these matters.

The subcommittee will gather information, analyze relevant issues and facts and formulate proposed positions and actions as appropriate for deliberation by the full committee. Ralph Caruso is the designated federal official for this meeting.

The rules for participation in today's meeting have been announced as part of the notice of this meeting previously published in the Federal

Register on November 15th, 2006. A transcript of the meeting is being kept and will be made available as stated in the Federal Register notice. It is requested that speakers first identify themselves and speak with sufficient clarity and volume so that they can be readily heard.

We have no received any requests from members of the public to make oral statements or written comments. So I think with that, we can now proceed with the meeting and I'll call upon Mr. Bajorek of the NRC staff to begin.

DR. BAJOREK: Good morning, Dr. Bannerjee. I'm going to start off in a minute. My name is Steve Bajorek from the Office of Research, but in order to give just a brief introduction, Butch Burton, our Branch Chief, is going to speak for just a few moments.

MR. BURTON: Thanks, Steve. As Steve mentioned, my name is Butch Burton. I serve as the Chief of the Code Development Branch in the Office of Research. Some of you may remember me from awhile back from my license renewal work. Others I haven't had the opportunity to meet but I hope to meet you shortly.

I wanted to talk briefly about meeting

objectives and some introductory remarks. What we hope to accomplish today is to bring the subcommittee up to speed on some of the recent activities that the staff's been engaged in with regard to the development of the TRACE code and we'll also touch briefly on the graphical user interface work that we've done with our SNAP code.

Steve Bajorek and Joe Kelly will provide the lion's share of the information in those areas. Joe will also be talking about some of the developmental work we've done with regard to some of the constitutive models. Throughout the presentation, one of the things that we'd like to get from you all as subcommittee members is to identify topics for future meetings. We recognize that this is going to really provide sort of an overview and status of our work, but as we go through, it would be very helpful to us if you would identify areas of particular interest or concern that you'd like to have a more fuller discussion on in the future.

Also, as you're aware, it's been almost a year now that you received the latest of several issues and concerns that were submitted to you all anonymously. We responded to you all last March that we would be addressing those and this is really our first opportunity to do that. And in order to do that, we've asked Professor Mahaffy from Penn State University to come and join us and he'll be giving a presentation responding to some of those issues and concerns. Also as we were preparing for this briefing, we were informed that there were some issues with regard to the momentum equations and their development and application. So Dr. Wallace, we hope to address some of those issues for you today also. And again, Professor Mahaffy will be discussing those.

Finally, we'll also be providing a discussion to you with regard to some of the scaling distortion issues that have been of issue and concern recently. A little bit of history; as many of you know, TRACE development really began in the mid to late `90s. The driver for that was at the time we had several thermal hydraulic codes, TRAC-P, TRAC-B, RELAP-5, all of which were being developed, assessed, maintained, in parallel as well as a neutronics code and in the mid-`90s there was a decision that for efficiency sake, we would try to consolidate those. The work has progressed more slowly than we would have liked and there have been a number of reasons for that, some of which is that some of these legacy codes had challenges that we had not anticipated and that we had to deal with.

We have a relatively limited staff to address some of the developmental issues. In fact, in terms of the actual staff, lead staff on this, we really have five people. In addition to Steve Bajorek, who provides overall technical direction, we have Joe Kelly, who is basically the lead in model development; Joe Stoddermeyer, who is involved with deck development and numerics; Chris Murray who is involved with configuration control and has developed a number of tools to help with efficiencies, and Chester Gingrich, who is our lead with the graphical user interface work.

In addition to those leads, we had a very limited support staff of junior engineers who have really been coming on very nicely and very recently, and when I say recently, within the last couple of months, we've brought on new staff, primarily folks out of school but some with some experience and we're hoping to develop them over the next couple of years to join our staff which will allow for much more in-house work, much more of the work to be done in-house and with the associated reduction in resource commitments.

MEMBER WALLIS: Butch, can I ask you

something now?

MR. BURTON: Certainly.

MEMBER WALLIS: We recommended in our research report that TRACE becomes the tool for the agency. We recommend TRACE should actually become the mature code used by the agency all over the place and we wanted to see it mature and you say it's going to be universal documentation in 2007, but what was sent to us to review seemed to be a hodgepodge of all kinds of stuff. What I want to review is a draft final document, not a hodgepodge of stuff which I have to figure out - not even dated. I don't even know whether some of the documents are old or new or what they are. That's not very helpful to us.

MR. BURTON: We understand, and let me respond to that. We are -- right now we are working very hard to try to get an executable version of the code to NRR by the end of this year, basically by the end of this month. And in doing so, and in working with some of the other problem areas, the documentation has fallen behind but we have made a commitment to try to get some of the supporting documentation, including the theory manual, the assessment report, things like that, we expect to have that issued by March of 2007.

But you are correct, what you initially received was not the most organized and comprehensive version of the documentation. But as you'll see during our presentation here, that is going to be our -- has been and will continue to be a priority for us and we hope to have all of that together by the end of March. And in terms of future topics, again, as you listen to the presentation, if that is an issue that you would like for us to get into, you know, in further detail, at that point, we would be more than happy to do that.

MEMBER WALLIS: Well, when you say "issue", is it going to be issued in final form and how can you have -- I mean, you're going to have a peer review and an ACRS review. Is this going to be issued for public comment or something or when is it going to be final?

MR. BURTON: Okay, well, let me go back to some of the initial issues that you had brought up in your review of the research program earlier. We recognize and we agree that the documentation is absolutely essential even to support the peer review and that's why you see in the one bullet about the peer review being conducted and it's going to be done in later 2007 and early 2008.

MEMBER WALLIS: But suppose the peers come back and say there's a problem with it? You can't issue a code for use and then have the peers tell you that parts of it are no good.

MR. BURTON: Okay. One of the things that we'll be discussing today is some of the assessment results and one of the things -- we recognize that in terms of the chronology of how these things are unfolding, it may not be the way we would like, but the NRR staff, they have been looking for the TRACE code to support some of their reviews for the advanced -- the certification reviews, specifically for ESBWR. So we have really tried to prioritize our work in order to support them in what they need. So what we plan to do is to provide to them an executable version of the code with the user guide. And the user guide actually is in pretty good shape at this point. As we get the documentation together, we are going to provide them with the support that they need in -- you know to answer questions, things like that as we get the documentation together. I don't want to steal Steve's thunder but we do have plans to provide that support. Later on, and you're right, you know, as they perform the peer review, if there are areas that need to be addressed, we will certainly address them but what we plan to provide to the NRR staff to support their review is a code that we have been able to go through. We know where the strengths are with the code and we also know where the problem areas are with the code. And in our discussions, in our consultations with NRR, as long as they can understand where it works and where there's still areas for improvement, they can deal with that. That's sort of the understanding that we've reached with them. But we know that we are playing catch up with the documentation. That's why we really tried to schedule the complete the schedule the completion of the documentation along with the peer review, provide the funding and the resources we need to support that over the next year or so and again, we think that all of that will come together appropriately a little bit later on.

But some of the issues and concerns that you're raising are valid and in the presentation we will try to address those.

MEMBER SIEBER: How valid is the information that you folks provided us on this tape or this CD. This has a user's manual, has a theory manual, has a source code and a bunch of other documents. Can I rely on this or should I believe that I've wasted some of my time reading it or how should I interpret this?

MR. BURTON: Boy, you all are really ahead of here, but Steve, did you want to speak to that?

DR. BAJOREK: I think the best way to describe the documentation right now is a preliminary draft that we're working on so that we can begin this peer review in the first quarter or thereabouts in 2007. The documents that you have now is the best document that we have available right now to represent the theory manual, the state of the assessment, the user guide and some of the other issues that we hope to deal with today, which are related but not directly -- do not direct impact the --

MEMBER WALLIS: Well, can I ask you about that? I turned to theory manual and what I got was something from LANL dated July 2000, which said nothing about the integration of RELAP into TRAC. It was all about TRAC. It said nothing about TRACE whatsoever and it said it didn't have the capability to model BWRs. And it's nothing like the final version. Why did we get something that's six years old?

DR. BAJOREK: When we first set up this meeting, our objective and goal was to have that theory manual in draft from some time in November. As Butch mentioned, in order to get the code in a releasable form by the end of December, we did run into some problems over the last quarter of this year. Most of the staff has been working to resolve that problem in order to meet NRR's objective and goal to be able to assess the ESBWR.

CHAIRMAN BANNERJEE: What were these problems?

DR. BAJOREK: As I'm going to describe, what we are attempting to do is to run some 500 different assessment cases all with a frozen code. As we go through this rather large assessment matrix, we'll invariably find some cases which fail, some in which we aren't satisfied with the results and that causes us to stop, go back, change the code and then redo the assessments. We thought we had it. We thought we had it pegged back in August or September. There were a couple of cases notably I think it was UPTF, for ECC Bypass that was suddenly giving us some very peculiar results. I think that Joe Kelly is going to talk a bit about the wall and the frictional drag models today and he noted that there were some problems as they were being applied in the down comer. We've been making those changes. We think at this point, we're very close. We think that now we're going to be able to get all of the assessment in the state that it is producing reasonable results and that all of these cases run through. We feel that our primary goal is to make sure that when we present the code to NRR, it's reasonably robust, that all of those highly ranked processes, especially for ESBWR are well-represented in the code and are giving us reasonably good assessment. But that has held us up.

I think that in the overall approach that many people take to code the code development, documentation slips behind because you're always waiting to get that final model fixed. Until you are satisfied with that, in some cases, there's the feeling that it's not worth writing up some of the models because you may well have to change those.

CHAIRMAN BANNERJEE: Do you document quite well inside the code itself?

DR. BAJOREK: I would say that right now, most of that documentation is within the code itself and in the developer's draft sets of notes. Our goal between now and February, March, April when we hope to have that complete, is to get all of that updated into a completely new theory manual.

Now, the 2000 education that was put out be NANL, some of those sections will still apply. The two sections in which we are making major revisions, the section that discusses the closure models will essentially be entirely rewritten. At least 75 percent of those have been changed and we'll talk about some of the reasoning behind that as we go on with the presentations today.

There have been some additions in some of the sections that talk about the code feature. The section on the field equations has been revised substantially. But at this point, even though we have made those revisions, they weren't in the shape that we could release to this committee in time for this meeting.

MEMBER WALLIS: It seems to me you need a peer review now because the way it's presented is really to the insider, who knows the history of these codes and what's going and you really need to present it in a way that the outside world can understand.

DR. BAJOREK: We hope to have it --

MEMBER WALLIS: But some expert who is not in the field but knows a lot about fluid mechanics and computation, looking at this thing, doesn't decide it's some strange animal but actually says that it's a useful thing.

DR. BAJOREK: We agree. we see this as the document that's going to begin the peer review. We expect to give this to a peer review committee and we expect them to find some things that they don't like, some problems and deficiencies that eventually need to be corrected. Our view of the code development process is that you need to start somewhere. We need to issue the code. We need to identify what we think are the problem areas, begin this process of the external and the peer review, and then continually improve the code over the future to address some of those things that both we, you and the peer review committee see as being the most vital things that we need to get correct.

CHAIRMAN BANNERJEE: You know, in the old days when all these codes were being developed, the peer review group which was then the Advance Code Review Group, went hand in hand with development. I don't recall how useful they were but for some of the constituted equations of closure relationships I think they were very useful. Why was that process stopped?

DR. BAJOREK: I guess I really can't answer that one. since I've been with the NRC, that part of the process has not been there.

CHAIRMAN BANNERJEE: But it seems at least that you carried a group of pretty eminent fluid dynamitists and computational people along with you and depending on how much time they put in, you've got valuable feedback or maybe less valuable feedback because it was done on a continuous basis. So you were never sort of introverted; whereas here there's always a possibility that you become introverted, you pick your own favorite correlations or whatever. I'm not saying that's what you do, but without actually subjecting it to some external sort of criticism. What Graham is saying is the peer review should be, I think, I don't want to paraphrase it, a part of the process and not just something done right at the end.

MEMBER WALLIS: I'm also thinking it's too bad that George Batchler isn't around any more but you've got to be able to respond to the sort of criticism that he would give you even though it may not be appropriate. It may be that he's being too persnickety and academic and all that kind of stuff but you still have to respond to the sort of thing that a guy like that would have said about some aspects of this. And there are still people like him around. I think you ought to invite some of them to comment.

CHAIRMAN BANNERJEE: Well, in particular, his counterpart you might think of as Andy Akrovous. And Andy is now retired, living happily now in Stanford, left City College. He works at more than normal --

MEMBER WALLIS: So he knows that the West Coast is more congenial than the East.

CHAIRMAN BANNERJEE: Right, but nonetheless, it's people like that you want involved who are not part of the sort of mafia here, you know, who only talk to each other and write their little equations or large equations or whatever, follow their own numerical methods and hope for the best.

DR. BAJOREK: All I can say is, your point is well-taken. We believe that with the issue of the code and this documentation, we can start to integrate that process into our code development. We'd be interested in your suggestions on people who could do this task.

We've talked about some names among ourselves. We don't want to mention them here in this meeting just to avoid any contracting difficulties.

CHAIRMAN BANNERJEE: One of the things with the previous peer review group that you had was that they were not so-called industry insiders. You know, they were not Los Alamos people or Idaho people, or people who have been in this and doing the thing. They were people like Garrett Burkhoff or Peter Lacks, you know, Abe Duckler or like that and I think you should strive for that same level of people rather than somebody who's written a quote in Idaho and make conjoin. You know, keep those -- really make it a peer review, I mean, an external review.

DR. BAJOREK: Okay, we will certainly attempt to try to accommodate that. As far as the individual makeup, we'll have to work on that. I would have to think that when it comes to getting people who are working on these codes, we may have a question or a problem with independence in that a lot of the people who are in the business of this large scale LOCA code development have either some association with the NRC or some of the people that we regulate and we have to make sure that we keep those two groups apart in this case.

MEMBER WALLIS: Let me ask you, who are the users? You talked about NRR. This code is going to be available outside? I mean, it's going to be available to anybody who wants them for free?

DR. BAJOREK: It will be made publicly available, yes.

MEMBER WALLIS: So it might be used in a

course in a university? And you don't want to students to say, "How can you do that", when they see certain parts of this document. It's got to be something that is robust when it gets into those sorts of hands.

MR. BURTON: Yeah, let me speak to a couple of issues that you've raised. First of all, you are absolutely right, the peer review of the code has to be better integrated right from the beginning. A big part of this -- and to speak more broadly to some of the things that you're bringing up is that the code has to be credible with the users. And right now, for many folks RELAP-5 is the code of choice. And just internally amongst the staff, we've discussed how are we going to market the code and kind of overcome those what we call barriers to entry.

And a big part of that is the credibility of the code. It's got to be able to do what RELAP-5 does at least as well, if not better and more user friendly, a number of different issues, but the credibility issue is key to integrating this into both the staff as well as people beyond the NRC.

And one of the -- we do have user groups. We have the -- if you're familiar with the CAMP Program, the Code Application Management Program, which involves primarily, actually RELAP-5 users all over the world. And we meet twice a year and what we found at the last meeting which was in November, is that they are starting to exercise the TRACE codes. they're doing a lot of comparisons between RELAP-5 and TRACE and in many instances, they're finding favorable results, not in all, but that's also one of the ways that we begin to get the code into user's hands and to begin to get feedback and areas for improvement, identified bugs to be fixed, things like that. But, yeah, our intention over the next year or two is to really market the code, but in order to be able to market it credibly, we do have to have that documentation in place and we do need to have the peer review. And we do recognize that one of the weak areas is it would have been much better to integrate that peer review right from the beginning and have it all along. We weren't able to do it for this version for a number of reasons, but as we begin to develop subsequent versions, Version 6, 7 and 8, we want to try and integrate the peer review right from the beginning. And the issue is identifying those people that are truly independent and have the technical expertise that can really add value to that.

DR. BAJOREK: I want to make sure we do

keep the code in context. Our primary user for TRACE and really any of the tools that we develop over here is NRR and other groups within the agency. Their mission is to be able to use this code to do audit calculations of the Westinghouse, the AREVAs, the GE's their applicants in order to look at new and advanced plants and changes to those conventional plants.

So our marching orders are generally to try to make sure that we can be -- we can supply the tools that they need in order to do their regulatory functions. It is not our goal to produce the best, state of the art LOCA tool so that it can be used at universities or by the international groups. We'd like to do that. In fact, we do have this code in use at a few universities and we'd like to encourage that, that actually --

CHAIRMAN BANNERJEE: What's the difference

DR. BAJOREK: The difference is that the applicant's code and their analysis is the analysis of record. We like to try to maintain rigor, that is about consistent with what the applicants are providing but we do not -- we don't feel that we are required to lead the state of the art. We want to make sure that our models capture the important phenomena and can handle the features which are necessary for some of the plants, you know, some of the new things which are in the advanced plants. This helps us ask the right questions to the applicants. It helps us understand whether their codes, which represent the analysis of record, truly capture the phenomena. So there is a -- kind of a gray area in how far we can actually advance this code to be the state of the art compared to where the industry and our applicants, applicants to the staff go.

MEMBER WALLIS: I think you need a careful introduction to this code, and the code is going to be tuned with these 500 assessments and that's going to be proof of the pudding that with its defects that it still works for your needs, but it may well not work for oil wells or biochemical reactors or a whole lot of other things for which people would like to use a code like this because it's been very much focused your applications.

DR. BAJOREK: Well, these codes, I think you can say that about TRACE, RELAP, the various flavors of TRAC. They're semi-empirical in nature. No matter how detailed you want to make your numerics, your nodalization, various features, you ultimately have to go back to the closure relations for heat transfer, wall friction, interfacial friction. Those are based on mainly subscaled experiments and because of that there are going to be uncertainties associated with your analysis.

MR. CARUSO: Steve?

DR. BAJOREK: Yes.

MR. CARUSO: One area that -- I don't know if you're thinking about this but as we go to more risk informed regulation, the risk analyses are founded upon analyses, reactor analyses about how the machine actually behaves, not how risky it is, but how the water flows and how the heat gets transferred. And is it your intent that this code will be used to support probabilistic risk assessments? Because if that's the case, then it has to be much more realistic maybe than you're describing.

DR. BAJOREK: It certainly has to be realistic. And we're working in that direction. I think it's mainly to the degree at which we really consider this to be the state of the art that we have to be careful, that's all.

CHAIRMAN BANNERJEE: What do you mean by, you said that two or three times that it won't be necessarily state of the art.

MR. CARUSO: What won't it do? And will people who are trying to remodel the behavior of the plant know when they go places they're not supposed to go with this code?

DR. BAJOREK: Yes, we're going to try to be very explicit in what the code can be used for and what it can't be used for.

MR. CARUSO: I mean, if you hand it over to a PRA person who doesn't understand even what heat is just about, they'll just run the computer code and they'll believe the numbers the way they believe the numbers that come out of their PRA calculations. So if the code is telling them something which is physically impossible because it's been driven beyond its capabilities, what are they going to do with this?

DR. BAJOREK: Hopefully, they aren't using it in that type of a manner. We think there is some maturity that needs to be there on the part of the user in order to apply a code like this. So hopefully they aren't using it as a black box.

MR. CARUSO: The history of the other code shows that they are misapplied.

DR. BAJOREK: Hopefully, yes.

CHAIRMAN BANNERJEE: Well, I suppose some of this will become clear as we go on, right, what are the limitations and where it can be applied, where it can't be applied. Though I must say that it's hard to read about the code when the last document we can reference is in the year 2000.

DR. BAJOREK: We agree, we agree.

CHAIRMAN BANNERJEE: So I don't know how much feedback you really want from us if we don't have an up to date document to review. I think you will get much better feedback once we have your draft manual or whatever, manual. Anyway, why don't we --

MEMBER WALLIS: Well, I would try writing the --

MEMBER SIEBER: Well, let me ask just a couple more questions. If some vendor were out there writing this code and wanting to submit it to the NRC for approval as a code of record, you would require of that vendor more than you're requiring of yourself; is that correct? For example, I understand you don't plan to do V and V.

DR. BAJOREK: I'm sorry, I couldn't hear. We have a plan to do V and V?

MEMBER SIEBER: Yeah, do you?

DR. BAJOREK: Yes, we do, yes. There's a rigorous procedure that's used to put models into the code to check those out and then these some 500 cases then represent, you know, a test then of the code and the --

MEMBER SIEBER: And so you'll be able to determine from that work what the range of applicability of the code is. Will you be able to meet most of the other requirements that you would place on others to qualify this as a code of record?

DR. BAJOREK: Yes, we think so. I think the big difference is if a -- let's say an applicant were to take TRACE and want to use this for their safety analysis, they would have to submit this to NRR and it would have to go through the rigorous type of code review that has been done for the other evaluation models. Now, at this point, we hope to get a lot of that same type of questions, raising of the issues, from the peer group but TRACE will have not gone through that formal application process to designate it as an evaluation model that an applicant would typically do.

MEMBER SIEBER: But it will be documented that you did the work, right?

DR. BAJOREK: Yes.

MEMBER SIEBER: Okay, I think that's important because if you intend to put this code out as an available public document and people begin to use it, you end up with a problem. I think if something goes wrong whatever application that somebody might make of that, you have an obligation to do a rigorous job of authenticating your code.

MR. CARUSO: I have seen licensees take NRC codes, misapply them and then file Part 21 reports against the NRC for the code because they claim that the code was deficient because it didn't work in the application they wanted, although the NRC that developed it said it wasn't intended for that use.

MEMBER SIEBER: Well, I guess my --

MEMBER ABEL-KHALIK: Can I ask a question? Excuse me. I'd like to go back to the issue of being state of the art. Let's say you're going to be 100 percent successful in your plans and the question then is, if I were to compare the capabilities of your completed code against the capabilities of the codes that we had when you started in the mid-`90s or late `90s as you state, are there any advancements beyond the capabilities of the collection of codes that were available at that time?

DR. BAJOREK: There are in terms of the model corrections that we've made to TRACE and some of the newer refinements we've put in there to model the advanced codes, I mean, excuse me, the advanced plants. I believe about a year ago, we had a presentation to describe the modeling for condensation processes. These were some models that were not in the code previously and give us the ability to model things like the PCC heat exchangers in plants like the ESBWR and some of the drywell processes that go on the ESBWR scenarios.

However, you should keep in mind that the code consolidation process that began in the late `90s was to consolidate the features of the TRAC-B, the TRAC-P, the RELAP and the RAMONA. It was not intended to improve upon the constituent models. The idea was to consolidate those features, put them into a new code, originally called TRAC-M, that gravitated or evolved into TRACE, and once we have everything into that code, now being TRACE, that is when we would start to improve those constituent models based on deficiencies and problems we see in the code and things that we've learned from tests that we've done at Oregon State, Penn State in the RBHT program, UCLA in the sub-cooled boiling.

We think we have a fairly good ides on what those models and problem areas are in the code. That's going to be the next step. But I think I would not characterize the main intent of the consolidation process to improve those individual models. That's coming up.

CHAIRMAN BANNERJEE: Thank you.

DR. BAJOREK: Which did you want to --

MR. BURTON: I think we're well into it now.

DR. BAJOREK: Okay. Okay, let me just sort of to finish off maybe things we haven't touched on. One of the things that we do want to hear from the committee are what are those topic areas that you feel are most important within development of some of the codes like this. You've made it very clear, you don't have the documentation, but if you did have that in front of you right now all together, the assessment manual itself is over 1600 pages, the theory manual probably not quite that size. There's --

MEMBER WALLIS: I don't really need 900 pages. If I don't like the first 20, then I begin to say what am I doing with this document, you know.

CHAIRMAN BANNERJEE: To give you an idea for CATHARE, I can get a pretty good idea of what's in the code, out to solve it and almost everything useful in about 20 pages and they write peer reviewed papers so they have to have that discipline.

MEMBER WALLIS: Yeah I was going to suggest that someone try to write that sort of short document which explains what the code is and what it does and what it sets out to do.

DR. BAJOREK: Well, Ralph, I think I've sent e-mails to Ralph that I think what might serve this committee best I the long run is really a series of technical papers, those articles six to 15 pages that either go into some of the details or describe a particular phenomena and how it's modeled, how it's assessed. You know, I agree with that. I think that's the most efficient use of your time.

MEMBER WALLIS: Yeah, but if you described what does TRACE do, how do you -- it's designed to analyze reactor transients, amongst other things. Okay. So what's a reactor look like? It looks like this and we're going to divide it up into various control volumes which we call nodes and they look like these various shapes, and we're going to write some equations which approximate what happens in those control volumes.

But then if you start writing down a whole lot of differential equations with grad divs and scrolls and things in them, that's nothing to do with how you model the control volume. So I immediately have a -- it's a disconnect between what I think you're applying it to and what you're writing down as a fundamental theory. Now, that's a very fundamental thing and I'm sort of surprised it's still there like that, that someone hasn't clarified what's going on.

CHAIRMAN BANNERJEE: Well, the message anyway is that things which are not incredibly wrong, I mean, 900 pages is hard to digest but I think you can put it together in much shorter documents which we can read, or any reviewer can read. So you could see the sort of thing that CATHARE has done. They've published in decent journals, I wouldn't say they're outstanding. I would prefer to see you publish in a little bit higher quality journals like Journal of Fluid Mechanics or Physics of Fluids, but if worse comes to worse, Nuclear Engineering and Design will do. It must be properly peer reviewed and not by your buddies and not just by the editor, you know. It should go out and be peer reviewed and then that would be fine, if it gets through that at least. That's the first minimal set of requirements.

DR. BAJOREK: Okay, two other things that we want to take care of in this meeting. This committee has received several anonymous letter raising some concerns in TRACE. John Mahaffy is here today to talk with you about some of those and hopefully resolve the issue.

Another issue that we've --

MEMBER WALLIS: How much attention do you give to these anonymous letters? It seems to me there's a great deal of attention given to anonymous letters, perhaps more so than to comments from this committee.

DR. BAJOREK: No, I don't think that's necessarily true. We take the committee's comments very seriously and we try to address those as best we can. The anonymous letter I think originally when we put this together it came across as like an allegation. So I think from the beginning, there's been a treatment of this that has been somewhat special.

MEMBER WALLIS: Maybe if I want to effect you guys, I should go away and write an anonymous letter, which I've never done in my life and never intend to do.

CHAIRMAN BANNERJEE: Maybe it is your anonymous letter that they located.

MEMBER KRESS: You could change your name to anonymous and they'd think you were great there.

CHAIRMAN BANNERJEE: Okay, carry on.

DR. BAJOREK: Okay, and finally, what we would like to talk about is the issue of, I think it's been termed pi group ranging. This was an issue, a concern that was raised originally back in the time of AP1000 review. There are a number of different scaling evaluations that were proposed in almost across the board. Everyone said, well, if pi group is between .5 and 2, it was acceptable outside, it was unacceptable. There really wasn't a basis for that. We want to talk about that issue, propose a methodology that helps to define, you know, what is that acceptability for --

MEMBER KRESS: You're going to convert those numbers into some sort of level of uncertainty associated with the thing, is that the approach?

DR. BAJOREK: More like a level of impact. In one way what happens when you look at the importance and the range, acceptable range of the pi group, you start really getting into CSAU. You want to know whether -- if something is totally deficient, you know, a scaling group maybe it's three or something that you would have said, but if you put it in your model and if you put it in your code, or if you look at it in an experiment, did it really have an impact. What Marino di Marzo is going to describe this afternoon is a way of developing a relatively simple model for the process, doing a ranging of those important scaling parameters determine whether the things which are within --

MEMBER KRESS: So a sensitivity analysis on the pi groups.

DR. BAJOREK: I think that's probably the best way of describing it.

MEMBER KRESS: That might work but you'd have to do that for each case almost.

DR. BAJOREK: But that's why I mean why you're starting to get closer to CSAU, okay, but we'll move into that.

MEMBER WALLIS: You've got to be careful about taking these pi groups too seriously. I mean, if you have say a pipe connected to a vessel, a dimensionalist group is the length of the pipe compared with the damage of the vessel, right?

Okay, now suppose that I have a pipeline running across Canada transferring oil from Alaska to Winnipeg or something, it doesn't make much sense to take that length and compare it with the size of the vessel that's going into and say I've got a pi group but you can. It doesn't make sense to try to duplicate that in an experiment, that sort of ratio and yet it is a pi group. So you've got to be careful that you've picked the right sorts of parameters to measure the scaling to make some sense.

CHAIRMAN BANNERJEE: Well, I guess when the idea of pi groups was developed, it was to see the effect of these non-dimensional groups on some specific parameters such as core inventory of coolant or something like that which had the most impact. So what I hope will be addressed in your meeting will be how these facilities -- because they always relate it to facilities, and the use of facilities to provide a data base to validate and allow for scale-up using quotes and we've always had a great deal of concern with these facilities which are not full height, in particular and have severe distortions and the reason for that has been different phenomena are important in different transients, different parts of transients. So we didn't see easily how some sort of one-quarter or one-third height facility could meet the requirements for all phases of the accident. Could be one and pi groups sort of showed us that this didn't happen as well if you did it right. So I'd like to see the arguments because all the NRC facilities, as I can see, are quite distorted.

MEMBER KRESS: The other thing is with sensitivity analysis, one quite often sees each say pi group, if you're doing those, done one at a time. I'm pretty sure that the sensitivity of one pi group will depend on the value of another pi group. And you have to figure out some way to deal with them as a group. We'll wait to see what Professor di Marco tells us.

DR. BAJOREK: Okay.

MEMBER ABEL-KHALIK: I'd like to just backtrack for a moment, if I may and so that I can understand the process by which this development is working. The people who are developing the models, are they the same people who are doing the coding?

DR. BAJOREK: In some cases they are. In general, however, the people who are developing the models are giving that suggested coding over to a person who is a little bit better at the software and they put those models into the TRACE code.

MEMBER ABEL-KHALIK: Now, is this transfer process from a model developer to a coder, documented?

DR. BAJOREK: Joe or Chris, I guess I'd have to ask you guys as to how the documentation is at that particular step.

DR. KELLY: Joe Kelly from Research and the first thing I would say is that -- well, when I give my presentation on the constitutive models, I can talk about that a little bit more, but we didn't really -- we don't have a lot of model development work going on. It's been more model remediation.

Remember, the focus of this was code consolidation but what -- and I am primarily the model developer. Most of the models that have been changed have been done by me.

In some cases, I have gone ahead and done the coding myself and then handed it off to someone else to do the testing. In other cases, I've written a document explaining what I thought the model should be and why I thought it should be that way and then handed that off to a junior co-developer for implementation and that was just a matter of being more effectively using my time, and then the other staff member would do the implementation and the testing.

So was it documented at that time, yes. Has that document necessarily been kept in an archive, no. What would then be archived would be what the person that did the implementation. He would prepare a software design and implementation document. And that would capture some of what I gave him but then also the programming details and the testing that he did when he put the model into the code.

MEMBER ABEL-KHALIK: But isn't there a standard QA process for large code development that requires sort of a paper trail from -- you know, even though you call it minor tweaking in the models, it is still a modification of the model to the point where somebody writes lines of code and making sure that t his process is done as the model developer wants it to be done. And it just seems kind of too ad hoc to me, the process by which this code is being developed.

DR. BAJOREK: As we go along, the notes are retained. We keep these on a website that --

MEMBER WALLIS: I support what SAID is saying. I have some experience with the TRAC code. The TRAC code evolved and then trying to figure out what it was and why things were in it the way they were was almost impossible because there wasn't this paper trail.

MR. MURRAY: This is Chris Murray from the NRC Research and I'm the code maintainer, so I have a close hand with the QA process. If you want to see our QA process, it is documented. There is an LO number we can, you know, send you to see what that is. There are actually two code QA documents that sort of govern the development of the TRACE code. The first is a general software quality assurance for thermal hydraulics codes that was written by I think Frank Odar back in 1999 or 2000.

One of the aspects of that document

dictates that for each code we develop a code specific QA plan and that document exists. It was written by Vince Musoe and Simon Smith back at the time this process started. And that sort of outlines how we go through a development process from the developer writing a software requirements document to then -- in a test plan, to then writing a software design and implementation document and submitting all those to NRC for review at each step in the process.

And once the coding has been developed, that comes into the code custodian, that's me. I review to make sure all the necessary pieces are there. Part of that they write a sort of HTML web page that describes the model or the change that was made. If it's a new model, then there's some description usually and in some cases, you know, when Joe mentioned about writing these model descriptions that's part of the package and usually just a link to the pdf or something like that. But there's a summary of th update, what changes were actually made in each sub-routine with a description of what changes were made, whether any model or rather documentation changes that needed -- had needed to be made and then describe what the verification testing was and what the test results were for those -- for that particular update.

And that all goes onto a TRACE development website. It's an internal sort of website that only our contractors and NRC staff have access to and you can see the whole history from Day 1 Version 3 of TRAC-M through Version 4.1 or 2.9 something for TRACE at this point. You know, some thousand some updates or whatever. And so there is a clear history and all of those updates do go into a version control system so you can go back and see exactly what changed from step to step. And we've presented on that before. some of the new members may not be aware of that process but that -- and the validation side of VNV comes in from that assessments.

MEMBER WALLIS: I wasn't trying to say that you were doing a bad job but I was saying how important this job was and it had to be done right.

MR. MURRAY: I was trying to address the other gentleman's comments.

MEMBER WALLIS: All right.

DR. BAJOREK: I'm going to go through -- we've actually done a pretty good job of racing ahead for some of the -- some of the slides I have here but let me just hit on a couple of these briefly. Just in terms of the code development process, up until about

2003/2004 most of the work had been going into consolidating the code. At that time it was called TRAC-M, modernizing, going from older versions of FORTRAN to FORTRAN-90, making it -- setting the code up so it would be easier to change and maintain in the future.

MEMBER WALLIS: How -- what does consolidation mean? If you're consolidating RELAP with TRAC, is it that they have exactly the same basis in which case there's not much consolidation required or if they have something fundamentally different about how they're based, I'm not sure how you consolidate it.

DR. BAJOREK: Well, the first --

MEMBER WALLIS: I don't understand what consolidating two codes means and just what kinds of things do you have to do to consolidate?

DR. BAJOREK: Well, there's two things that need to go on. First you need to consolidate all of the features and things that one code can do into a common platform, TRACE, okay.

MEMBER WALLIS: But they have the same base but then some can do some things and some can do others. You can just sort of incorporate all of the things they can do into this new code, by adding them in presumably, but if they have something which is fundamentally different about how they solve things or how they formulate things, I don't know how you consolidate that. Is there no problem of that type? Is it just adding in another correlation or something?

DR. BAJOREK: No there was a selection process they had to go through in order to come up with models for TRACE. In most cases, those models did come from the TRAC series of codes. However, they did have to be implemented in a way that they could do the --

MEMBER WALLIS: Can you give an example of how TRACE is better than TRAC, because of something that came from RELAP?

MR. MURRAY: TRACE species tracking, TRACE species tracking was one aspect, I think that TRAC didn't have that RELAP had on multiple non-condensible gases, modeling non-condensible gases.

MEMBER WALLIS: Add-on things.

MR. MURRAY: There is add-on. You know, TRAC only had the concept of a T component and RELAP had sort of a branch with multiple side junctions. And that was something --

MEMBER WALLIS: TRAC has multiple side junctions?

MR. MURRAY: Now, a single cell can have more than one side junction. That's something that RELAP had that TRAC didn't have.

MEMBER WALLIS: Maybe that should be said up front in the documentation. So TRAC had the ability to handle multiple junctions that RELAP did not have?

MR. MURRAY: No, RELAP did but TRAC did not.

MEMBER WALLIS: Okay. And what do they do with weird shaped junctions? Was one better than the other?

MR. MURRAY: I think we'll get into that later.

MEMBER WALLIS: Later on.

CHAIRMAN BANNERJEE: Let's avoid it right now.

MEMBER WALLIS: Okay, I'm just trying -- I'm still not quite sure. It looks as if it was very easy. You simply said something that RELAP could do, multiple gases or something which stick it into TRAC and everything is easy.

CHAIRMAN BANNERJEE: A lot of this -- correct me if I'm wrong, Steve, was associated with the fact that you wanted the final product to be able to run on decks and so on that had been generated say for RELAP-5 in the past, so you --

MEMBER WALLIS: So it's input.

DR. BAJOREK: There's a lot of work had gone on to developing those facility models and plant models with RELAP. We needed to be able to preserve that. So TRACE had to be able to largely take those legacy decks as we call them, read those in and still be able to do the simulations, incorporate all of the features.

CHAIRMAN BANNERJEE: Are these mainly legacy decks with RELAP-5 or were there some with TRAC as well?

DR. BAJOREK: Some with TRAC as well. Some of the TRAC-B decks had a different format than what TRAC-M or TRACE now uses. So there was a conversion process that had to be done for some of those TRAC-B decks. That, however, was less onerous and difficult than porting over the RELAP decks.

Now most of the work for doing that conversion of the decks is done with the SNAP input processor or the SNAP graphical user interface, as I'll talk about later. That's actually proven to be fairly difficult to get everything to translate over from RELAP over to TRAC -- or to TRACE, excuse me.

We have a project ongoing right now, where we are taking the plant decks that NRR is most interested in and we are running that through SNAP, then running TRACE calculations in order to make it more of a turnkey operation, so that the users, the analysts in other places of the agencies don't have to become experts in SNAP or TRACE in order to do their jobs.

CHAIRMAN BANNERJEE: Can you give us a rough idea of what fraction -- you were showing this program, what fraction of this went into say developing something that allowed you to -- things like SNAP and what fraction went into actually doing the closure relationships or making sure they were compatible, bringing them together?

What amount of effort went into the numerical part, so let's say the physics, the numerics and the pre and post processing. So if you took those broad categories, how would you have split this and some of it, I imagine, into validation and assessment as well. Make that five.

DR. BAJOREK: In terms of just the overall weighting and resources, I think if you go back to pre -2000 --

CHAIRMAN BANNERJEE: Put the slide back up because your slide vanished.

MALE PARTICIPANT: That's all right, he'll get it. They're all together.

DR. BAJOREK: Thank you. I think if you go back to the 1998 to 2003, it was virtually all divided between software development for TRACE and the rest of it probably in the development of the SNAP processing. It's basically a software project.

CHAIRMAN BANNERJEE: And what do you mean by software development for TRACE?

DR. BAJOREK: Modifying the code, doing that conversion to FORTRAN-90, updating the structure, really putting the code together. Okay, assessment and applications at that time were -- there were some going on but that effort was fairly small, so --

CHAIRMAN BANNERJEE: You started with TRAC, right?

DR. BAJOREK: Right.

CHAIRMAN BANNERJEE: In 1990 there was a FORTRAN-90 version of TRAC. Okay, so why did you have to do so much work with FORTRAN 90 at this point? TRAC already existed, right?

DR. KELLY: TRAC existed but at that point it would have been in FORTRAN-77 and also would have had a lot of stuff in it to make it work on a CRAY computer, which by the time we started this program really didn't exist any more. The consolidation, it's kind of foggy here but there's really two things. It depends on which code you're talking about.

TRAC-P and TRAC-B shared the same base code. Now, to give boiling water reactor capabilities to TRACE what we would do is actually import a model from TRAC-B, something like for example, the jet pump, and basically, just bring the coding over and then, of course everything did have to go into FORTRAN-90 because at that point everything was in 77 or even earlier, but for RELAP-5 and ROMONA, that was something different. I'll talk about ROMONA first. That was a thermal hydraulics code and an neutronics code all built together. Instead what we did was incorporate the capability. We took a modern reactor kinetics code, that's the PARCS code, and built an interface between TRACE and PARCS so that we could run the two concurrently.

That allowed us to recover the capability of doing three dimensional reactor kinetics simultaneously with the thermal hydraulics. For RELAP the idea was not to bring RELAP models and stick them inside of TRACE, but to make the TRACE code have the same calculational capabilities as the RELAP-5 code.

Up until the time of this consolidation, TRAC-P was the agency's large break LOCA analysis code. RELAP-5 was the agency's small break LOCA analysis code. And that was pretty much the division between the two.

Well, you know, some of the models are different but a lot of it is, you know, very similar. And so the idea is, if the TRAC models are good, they should be able to do a small break LOCA just as well RELAP-5. It turns out some of the models weren't so good and you'll see some of that when you see my presentation.

And so as we went through the assessment process, we've tried to identify which models needed to be replaced. In some cases, we may take a model direct -- you know, constitutive model directly from RELAP or another code, like for example, CATHAR, and bring that into the TRACE code. And so I'll talk a little bit more about that when I get to my presentation.

CHAIRMAN BANNERJEE: CATHAR made a significant effort to make the equations hyperbolic which you don't seem to have done. Why did you make that decision?

DR. MAHAFFY: Can I make a comment on that?

If you look at CATHAR and how they made it hyperbolic, there's not a strong physical basis to begin with, all right. There's some mathematical tricks that mathematically say it's hyperbolic. If you go back to the old paper by Bruck Stewart and Bur Windruff I think was involved in it at one point, what they showed was for the size of meshing that you use typically in reactor safety and well below that, it's not a practical issue. So that rather than put some non-physical terms in there to please some mathematicians, we just left it as is.

We had not seen throughout the history of this code and its predecessors, problems with the non-hyperbolicity simply because of the numerical implementation.

CHAIRMAN BANNERJEE: Yeah, CATHAR, of course, in some areas is physical.

DR. MAHAFFY: Yes, but --

CHAIRMAN BANNERJEE: Did you incorporate all the physical ones?

DR. MAHAFFY: The one zone where it's definitely physical is if you're looking at what happens in horizontally stratified flow, right, and we have that. That's there.

MEMBER WALLIS: You have no mass term for instance?

DR. MAHAFFY: We do not use that at mass term. We've addressed that here before and I'll talk about it later on.

CHAIRMAN BANNERJEE: Is this something which -- let me -- there is no not invented factor showed, right?

DR. MAHAFFY: This -- we have tried to be inclusive of, you know, everything that is appropriate balancing it with time considerations and level of effort for this.

DR. MAHAFFY: So if you release a bubble in in an invicit (phonetic) fluid, you'd get an infinite acceleration in your code?

MEMBER WALLIS: If the bubble has no mass.

DR. MAHAFFY: Well, a bubble does have mass but --

MEMBER WALLIS: No, it doesn't necessarily. It may have a very, very, very, very, very, very low mass.

DR. MAHAFFY: It's very small. No, you're right, you'll get the wrong accelerations. No, I'll concede that. You still have value terms that interplay.

MEMBER WALLIS: Well, I want to go back to my question about consolidating. When I read this TRAC, I find in the numerical methods there's a mysterious W which is a weighting factor for some sort of averaging which is presumably twiddled. Then there's a mysterious beta which is some sort of a weighting factor to make things more or less implicit in handling velocity. There's a SETS method, which is somewhat difficult to follow. There are various other things like conserving convected momentum and stuff. Is that all comparable with RELAP or did RELAP do it some other way and why did you choose this way rather than the RELAP way?

DR. MAHAFFY: The SETS method is -- exists in both codes. In RELAP they call it nearly implicit. In TRAC, I'm the guy that invented that circa 1978, so I --

MEMBER WALLIS: So the betas and the W's are also in RELAP, are they?

DR. MAHAFFY: I can't attest to that. I'd have to go back and look at their documentation to see how much of that --

MEMBER WALLIS: But there's nothing in TRAC that tells you why it's done this way. They simply say this is it. They don't have a sort of a paper trail that says the beta was put in and this is some sort of a pedigree for using this beta approach which is verified in some way.

DR. MAHAFFY: If you read deeply enough into the TRAC documentation, if you go back to the Journal of Computational Physics article on SETS what you will find is that beta is -- you can regard it as semi-empirical. It is there simply because it makes the behavior of the code more robust in certain situations when boiling is present.

MEMBER WALLIS: There are questions asked and if I read the document, it says beta is there, okay, when the nodes are all in a straight pipe and all the same size, maybe you can handle it with a certain W and beta and so on, but if the nodes have different sizes, there's no explanation about how you then start weighting your averaging. So there's a whole lot of questions I have about this stuff and we can't spend all the time. But did this come from RELAP or did it come from TRAC or is it --

DR. MAHAFFY: Most of that stuff is from the TRAC side of the --

MEMBER WALLIS: No insight that you got from how RELAP does this stuff?

DR. MAHAFFY: Oh, no, I understand what

RELAP does. RELAP's near implicit method is based -- and this is something I tried out circa 1978, RELAP's flavor of this methodology is driven by the fact that you have the virtual mass terms in your momentum equation and that forces you to order your equations in a certain way if you're going to solve them smoothly. Once you remove the virtual mass terms or neglect to ever put them in, then the particular formulism that you have in sets is from a computational standpoint more efficient. That's what drives that. If you would like to pass on your specific questions after reading through that --

MEMBER WALLIS: No, I'm just trying to figure out how you took these two codes and consolidated them and preserved the best features of both and I got the impression that you simply went with TRAC.

CHAIRMAN BANNERJEE: That probably is the truth. Your developer was from TRAC and therefore, you went with TRAC.

MEMBER WALLIS: That's the answer. That's the answer. I don't need any more. I just need to know that, that's all.

DR. KELLY: Okay, at the time -- this is Joe Kelly again. At the time that we started this program, I was the team leader, so I can address how we made those decisions. We had three options available to us. One was starting with a brand new code, writing it from scratch and then trying to bring in the best of all of the extent codes.

Option number two was start with RELAP-5 and then build into RELAP-5 everything we needed to do to consolidation. And number three would have been starting with TRAC-P. My favorite was starting with a new code because I didn't want to live with the problems that --

MEMBER WALLIS: So you started with TRAC. It's all based on TRAC.

CHAIRMAN BANNERJEE: You wanted to start with --

DR. KELLY: Right, right, and for good reasons that path wasn't followed. There was very high risk and a potential for a much higher cost. So that brings us to using either RELAP-5 or TRAC as a base. And at that time -- well, there is three reasons why we selected TRAC over RELAP-5 at least to the best of my memory, okay, because this was a few years ago now.

The first is that TRAC had a working three-dimensional capability; whereas in RELAP 5 at that time, the three-dimensional capability was very much experimental and completely untested. Whereas in TRAC it had existed from the beginning of the TRAC code and had been well-assessed during the time of the CSAU program.

The second one was that the numerics in TRAC were much more robust than in RELAP-5 as far as -- especially with the SETS method when you want to run with time steps that are beyond the KARANT (phonetic) limit which you really need to do for small break LOCA or for these passive plants. So those were the two reasons, and the third had to do with leveraging funding, if you will because the Department of Energy and Naval Reactors had already committed to do the modernization of the one-dimensional components in the TRAC code. So we could build upon their effort and only have to fund the modernization of the three-dimensional.

So as best I can recall, those were the three primary reasons that we selected TRAC instead of RELAP-5 as a starting point.

CHAIRMAN BANNERJEE: Let me ask you, Joe, RELAP was more sort of used for small break LOCA's, right? Yet, you say that the TRAC methodology numerically was more robust for the small break LOCAS.

Is today RELAP-5 used in industry still more for the small break LOCAS than TRAC?

DR. KELLY: Well, you said "used in industry", so I can address that from one particular standpoint because I worked for Siemens for awhile before it became Framatome and then AREVA. They have their own version of RELAP-5 called S-RELAP-5 and it's descended from RELAP-5 Mod 2.5 whereas the current NRC code is 3.3, I believe. They've had that code in-house and done their own development work over a number -- a fairly large number of years. And they're responsible for doing the assessment and making sure that the code works. Then, as you know, they did a best estimate large break submittal and did the -- you know, the end certainty methodology, et cetera, and presented it here. Likewise, they have an evaluation model approach for small break LOCA.

Now, I assume at some point that will become a best estimate but an evaluation model is both the code and how the code is applied. And then how the code is applied is a lot of different assumption about what occurs in the transient and what the initial states of the plant are and that was approved by NRR as well.

So it's different. You know, the RELAP 5s are different.

CHAIRMAN BANNERJEE: You mean the solution methodology used in S-RELAP is different from the RELAP 5 or --

DR. KELLY: To the -- I don't think it has the nearly implicit scheme. They have -- they've modified -- they've had the code in-house for 15 years or something. So over that period of time, it has grown up in their own environment, and you know, the development paths have been different since that point in time.

CHAIRMAN BANNERJEE: What sort of scheme do they use now numerically?

DR. KELLY: I know there's --

CHAIRMAN BANNERJEE: I should know, but I've forgotten, it was so long ago they came out.

DR. KELLY: I know they still have the semi-implicit. I don't remember if there's any kind of partially implicit scheme at all. I simply don't remember. It's -- you know, when I was there, I worked on the assessment of the physical models for large break LOCA and I don't remember the numerics, I'm sorry.

CHAIRMAN BANNERJEE: Anyway, so historically, whether for better or worse, you picked

TRAC and that's what was done. And therefore, the basic structure of the code and numerical schemes are to a large extent what was in TRAC-P at that time.

DR. KELLY: Yes.

DR. MAHAFFY: Can I interject something here? The key word that I just heard was structure and that's not the case. Understand that we have been using an evolutionary programing approach. TRAC was a convenient place to start but we were operating under three guidelines from on high. The code had to be easy to read, easy to maintain and easy to extend. Neither RELAP5 nor TRAC were particularly easy to read, okay. You're nodding your head, you've been there and tried to do that. Have we succeeded completely, no, but it's a lot easier. I can give examples of graduate students mucking around in these various codes and I can tell you TRACE is a lot easier for a novice graduate student to work with. But, you know, this code has evolved. As you've noted, I'm an old TRAC developer. This is not TRAC any more. It's something else. Inside the data structure is totally different. The computational flow is different. You will recognize bits and pieces of code if you really knew it. They're still there but it's part of what Joe talked about. You know, do you write a new code from scratch or do you evolve a new code? And we went with the evolution. I think in terms of verification capabilities, it's proven to be a pretty good course.

DR. BAJOREK: Okay, I'm going to try to move ahead quickly here to get us back on schedule. Since about 2003, to answer your original question, it has moved more towards assessment in terms of where most of our resources that fixing those models which you're finding deficient. In the future, we thing more of that work is going to be on the model development and assessment end of things, kind of getting back to where we really want to be in making the code better.

What it's applicable to --

CHAIRMAN BANNERJEE: Do you want to go back to that previous slide?

DR. BAJOREK: Do I want to?

CHAIRMAN BANNERJEE: Let's get the answer to the question in my mind at least. In the early days, you spent a lot of time making a sort of software so that different decks could run on the same code, RELAP could be used on TRAC, I mean, TRACE and so on. And that sort of finished, when about 2000?

DR. BAJOREK: About 2003.

CHAIRMAN BANNERJEE: Oh, 2003.

DR. BAJOREK: 2003.

CHAIRMAN BANNERJEE: Now, you've got a code which has sort of got a --

DR. BAJOREK: Now, we essentially -- at that time we had what I like to think of as basically a new code and then we need to assess that to see how well it works. Notice one thing that is --

CHAIRMAN BANNERJEE: It's not frozen though.

DR. BAJOREK: Not necessarily frozen because we were continually finding things that needed to be fixed. One of the things that you'll note that was absent really from the CSAU work that was done in 1989, 1990 up through 2003. There was virtually no assessment done on TRAC which was the base for this -- for much of what we were working on.

So when we started the initial assessment, 2002, right about the time the consolidation was completed, we found many models that needed to be fixed in order to make the code run with the assessment matrix, which now has grown to where it needs do to TRAC-P, TRAC-B, RELAP all of those cases simultaneously.

At that time is when we found that, hey, many of the models, many of the closure models, that had been put into TRAC-M and TRACE needed to be revised and that's kept Joe Kelly very busy here over the last two, three, four years. I think that --

CHAIRMAN BANNERJEE: That's post-2003.

DR. BAJOREK: Post-2003. Today with revision of many of those models, completion of much of the assessment and keep in mind there was a lot of deck conversion that had to be done for the assessment cases as well, we're now at the point where we can do the assessment, look at the models, go back and see whether they are good enough in order to move forward. So I would say that at this point, most of our resources are being spent on assessment, maybe 60, 70 percent, the remainder on SNAP development, refinement, and fixing the models in TRACE as we find those to be problematic.

CHAIRMAN BANNERJEE: When is the code going to be frozen in some version or has it already been frozen now?

DR. BAJOREK: We have frozen it a couple of times over the last couple of months. By the end of this week, actually I think the team, we're going to meet and hopefully agree upon what we call the next release candidate and send it through this batch of 500 assessments. When we did that last month, we found a couple of problems that needed to be fixed. We are hoping that with this next round of revisions, the code can be rigidly frozen. That is what will become TRACE 5.0 and then we can completely move on to finishing the documentation.

CHAIRMAN BANNERJEE: Now, will you have gone through the verification procedure that the equations in the code are actually the equations that are supposed to be there, independent verification and this sort of stuff? This was an issue raised in one of the anonymous letters.

DR. BAJOREK: The line by line review type of procedure?

CHAIRMAN BANNERJEE: Yes.

DR. BAJOREK: Yes, we've done much of that. I can't say we've --

CHAIRMAN BANNERJEE: Is there a sort of QA document, a procedures document that you followed? What sort of controls do you have for people to say, yes, this has been done other than your word for it?

DR. BAJOREK: Oh, as far as a written guide, I --

CHAIRMAN BANNERJEE: Or just a log or whatever. I mean, do you have some record that this verification procedure has occurred?

DR. BAJOREK: I don't believe there is any.

MR. MURRAY: I mean, a lot of that was captured on that website that I was mentioning earlier. You know, step by step, as we've gone through and involved this code from the TRAC-P days, every time that we've, you know, a programming structure, a data structure change is made to underline numerics, that's gone through -- John has gone through a rigorous verification process that's documented through either the HT summary pages that are in our website or you know, some of the documentation that we supplied that is on that CD that was mentioned a little earlier, there's an SQA documentation directory that contains, you know, as much of that documentation as exists.

CHAIRMAN BANNERJEE: Perhaps one thing that would be helpful would be for somebody to put this all together in some form that it's in one place so anybody coming in can look at it and see that all the verification exercises or whatever you've done is there and that it's something which stands up to scrutiny.

DR. BAJOREK: Okay.

MEMBER WALLIS: The user will get the

source code so the user can look at the details and will the user be clear when he looks at a piece of the code which is supposed to do something about what's going on in that part of the code?

DR. BAJOREK: I think in the past we have released --

MEMBER WALLIS: A source code --

DR. BAJOREK: It's not a blanket release.

MR. MURRAY: Repeat the question once more, I'm sorry?

DR. BAJOREK: Will the user get the source code?

MR. MURRAY: The users generally get the source code when we release the code to --

MEMBER WALLIS: The user wants to know how you handle some particularly difficult aspect of the numerics or something, then this user can to go part of the source code and look at it and figure out that what you're doing there is --

MR. MURRAY: That is true.

MEMBER WALLIS: And there is some description in the theory manual which goes along with it so he can check that one is consistent with the other?

MR. MURRAY: I mean, that will be the

intent.

MEMBER WALLIS: In the --

CHAIRMAN BANNERJEE: That is the intent.

MEMBER WALLIS: -- because it didn't run when solving certain problems, people would insert things to make it work without explaining what they were doing and then it became very difficult to figure out.

MR. MURRAY: That's always a challenge, I think, to always get people to document why something was done. It's very easy and historically, people have added comments that say what they've done. It's saying why they've done something and what the implications that are always a challenge in any software development and certainly we strive to always capture that -- answer those whys, either in the theory manual itself, the user manual, the user documentation, the code itself or the code software, you know, quality assurance documentation.

DR. BAJOREK: That's one of the issues that we have taken very seriously though, is where do we go from here with respect to the documentation in our assessment. We've taken a lot of pains over the last couple of years to really automate the entire process. We want to get out of this mode of making changes to the code, assessment them some day and then documenting them some day further downstream, where by that time the code developers may have come up with some better ideas. We think we've arrived at a procedure now where the theory manual, our draft versions, the assessment report, the input decks are all put on to, I guess what I would describe as a data base. The user could check out the input decks, make his changes, check those back in. When he does all of this and if he runs a new code, there are automatic -- the software will automatically change figures in the report. The theory manual is there so that as we make changes, we can very quickly go to that theory manual now and update it with those models as they change.

Our goal is that once we get past TRACE 5.0, and this initial glut of information, as we make changes to TRACE and it evolves into 6.0, 7.0, the time frame for turning around the theory manual, the assessment and everything that's associated becomes very short. So you know, if a new user picks up a TRACE Version 6.0, in a year or two, he's going to know very quickly what were those changes in the theory manual, what does 6.0 do that was different than 5.0 because he's going to have that assessment readily available to him. But at this point, it's --

you know, we have the process in place and it's a matter of freezing the code, which we hope to do this month and then completing the documentation which our target is the -- is early part of 2007.

MEMBER WALLIS: I know it's assessed with what's been done in the past but when you're evaluating something like ESBWR, we have -- just thinking out loud, there's a chimney in there and there may be sort of bubbles of a size that's never really been assessed before in the chimney. Are you going to just run TRACE as a black box or are you going to modify parts of it to handle a new geometry like that or something or what? What are you going to do?

DR. BAJOREK: No, it's not going to be run as a black box. I was looking ahead here in terms of, because I do have --

MEMBER WALLIS: What sort of thing do you do when you get a new thing like that which is different from a previous design?

DR. BAJOREK: One of the later presentations are going to outline that in a little bit better form. We're going to come out with this assessment report which for better purposes, I would refer to as a generic assessment report manual. That would include virtually all of the processes that people have indicated are highly ranked in the various parts for large break and small break. Okay, that's where you'll find assessments for reflood, heat transfer, mixture level swell and the likes. We will also be producing documents for ESBWR, EPR and any of the other advanced reactors that come along which we refer to as applicability reports. In those documents, we would document the things like for ESBWR as an example, things like Puma, Panda, Giraffe.

MEMBER WALLIS: Maybe -- are you going to say to use this code for the ESBWR you have to change certain lines in the code?

DR. BAJOREK: No.

MEMBER WALLIS: No?

DR. BAJOREK: No.

MEMBER WALLIS: No? Well, how did it take -- maybe TRACE doesn't handle the bubble size that we expect in ESBWR. How do you do it then? It must change something if it can't handle it.

DR. BAJOREK: We have done that assessment using two different types of tests to look at the bubble size and behavior in large diameter pipes.

MEMBER WALLIS: You feel it's already in

trace.

DR. BAJOREK: Yes.

MEMBER WALLIS: Well, there must be some questions which come up which are not, perhaps, already answered in Trace. Then you're going to have to produce modifications to the code or is that never going to happen?

DR. BAJOREK: If we --

MEMBER WALLIS: Suppose they change the design of the EBSWR, suppose the core catcher has certain features in it that they want to model with TRACE. You're going to have to put in some new lines of code, presumably.

DR. BAJOREK: Well, the core catcher would be dealing with a severe accident and I believe that's all external to the code.

MEMBER WALLIS: Well, I mean, I'm just trying to think, there must be some situations which you haven't assessed which come up with new designs.

DR. BAJOREK: If they change the design and it arises in processes or capabilities that the code doesn't have, we would fix the code and we revise it.

MEMBER WALLIS: You would revise the code.

DR. BAJOREK: We would revise the code.

MEMBER WALLIS: You hand out to the user something to -- a new code or you hand them some bulletin which says change certain lines using --

DR. BAJOREK: We would -- let's say they change something in ESBWR that added a new process and we had to change the code substantially, we would come out with a TRACE Version let's say 5.1 that had that new model in there. We would repeat certainly all of the assessment that goes along with ES --

MEMBER WALLIS: It wouldn't be like upgrades. I mean, you can upgrade Microsoft stuff. They just send you stuff all the time and say it's going to improve your code. You have no idea if it will or if it won't, but they do it all the time. You're not going to do that sort of thing.

DR. BAJOREK: No, we would be rerunning the assessments, starting with the ESBWR series of tests that we've put together. We would run all of the -- we'd try to run all of the generic assessments, okay.

CHAIRMAN BANNERJEE: That's a big job.

DR. BAJOREK: It is a big job. One thing to keep in mind is because of the automation and new techniques available, we've been able to speed that up. Turning around some of these integral tests, in the past, may have taken you months to do. We can do that in a couple of weeks now. We can turn around this 500 set assessment in under a month. You know, there's always going to be a couple of cases that you have to scratch your head and work at a little bit, but --

MEMBER WALLIS: And the figure of merit is also automated in some way?

DR. BAJOREK: Not all of them. We're working on that but in many cases the things like the scatter plots, the things that we do to judge the merit of the code are also automated so that we get most of that on the rerun of those simulations. There's still some work that we'd like to do to try fully automate that, but that's going to be work for the future. But that is one, I think, very important feature about what we have been doing over the last couple of years is that we aren't thinking that 5.0 is going to be frozen in time for the next 10 or 15 years. We expect new plants to come into the agency. We expect people to uprate and modify their conventional plants that maybe we need to look at new range of conditions. And we want to be able to address that very quickly.

So if things come out that require a 5.1, we're going to be very -- we're going to be very able to change the code and the documentation so that a user knows what its applicability are and how good the code works with those changes. When we come out with the --

MEMBER WALLIS: You have five people doing all this?

DR. BAJOREK: No, it goes beyond five. We have a number of people with contractors to help us with the assessments at this case -- at this point, excuse me. We're bringing in a number of new people in the agency.

MEMBER WALLIS: I'm thinking about Fluent. (phonetic) Fluent is a few miles from my house and they have hundreds of people.

CHAIRMAN BANNERJEE: They only have 20 people doing the development.

MEMBER WALLIS: During the real development, okay.

CHAIRMAN BANNERJEE: The rest do sales and marketing.

MEMBER WALLIS: And customer relations and stuff.

MR. CARUSO: Do the transients that you're listing here include stability transients for ESBWR?

DR. BAJOREK: At this point, no, but we are working on that. We have a project going on where we're looking at the Peachbottom Turbine trip, Ringhals turbine trip, we're currently assessing a series of integral tests that have been done using the Puma facility to investigate stability. So TRACE 5.0 I would not say is ready for doing BWR stability but we would complete that assessment in one of the subsequent versions.

MEMBER WALLIS: It's not ready for that yet?

MEMBER KRESS: Is NRR evaluating -- NRR is reviewing ESBWR stability as we speak, correct?

DR. BAJOREK: I believe that's correct.

MEMBER KRESS: Are they using Romona for that?

DR. BAJOREK: I think they are using the LAPUR code.

CHAIRMAN BANNERJEE: That's just a linearized --

DR. BAJOREK: At this point --

CHAIRMAN BANNERJEE: Why don't you do fluorogeam. I mean, Graham's point was that ESBWR may have fluorogeam related instability because of the large size of the chimneys. Now, in principle, we know that -- I know this from oil/gas pipelines, that if you have a fine enough nodalization, you can actually see slug formation and capture severe slugging without doing anything else. What it means is, if you've got the right equations and the type of fluorogeam transition, is captured within those equations, then simply by going to fine enough nodalization you should be able to resolve some of these, not all of them, but certainly slug flows you can.

Now, with BWR chimneys, whether you do get fluorogeam oscillations because the experiments show if you look at the Ontario Hydro Experiments, correlated void fraction oscillations which go over long lengths of pipe. And whether this is actually going to be seen in the ESBWR we don't know, but we'd like to be able to use something like TRACE with a very find nodalization if you see it or not.

DR. BAJOREK: Right now, well, right now with TRACE, we would -- I think we would venture cautiously that TRACE PARCS coupled should be able to do stability. However, because we haven't completed our assessment in that work, we would say that you could use it but you would have to use it with a lot of caution. We're focusing our work --

CHAIRMAN BANNERJEE: Without the added mass, I don't know if would give the right thing.

MEMBER WALLIS: Well, we're reduced to believing or not GE's version of it?

MEMBER KRESS: And Dr. Marsh Leuba.

CHAIRMAN BANNERJEE: But he does linearized analysis.

MEMBER KRESS: Well, that's the independent chair.

DR. BAJOREK: Our development is often at the direction or needs defined by our stakeholders, NRR in this case. They've made it very clear they want TRACE to be able to do large and small break analysis for ESBWR. They have not made stability a priority and because of the amount of work that we have to do just to do that large and small break LOCAs for conventional plants, we have not made stability a priority at this point.

MR. CARUSO: What about ATWS? What about ATWS for ESBWR? Does it do ATWS for ESBWR?

DR. BAJOREK: ATWS.

MR. CARUSO: ATWS, Anticipated Transient without Scrap.

DR. BAJOREK: We've actually used an

earlier version for TRACE to do some investigation in that area. It actually worked fairly well in comparison to codes like MAP that we're trying to do similar predictions. So, yes, we could use this code for ATWS.

CHAIRMAN BANNERJEE: Then stability is probably important. Stability probably that's in there, right?

MEMBER WALLIS: Also one stakeholder is the public and it would really help to reassure the public if you had a code which you could run independently to check something which is handed to you by General Electric.

DR. BAJOREK: Uh-huh. Okay, but with respect to applicability for 5.0, we feel it's going to be adequate for conventional PWRs, BWRs, ESBWR, large and small break LOCA.

MEMBER WALLIS: It's our major jobs in the future of this committee, the ACRS is going to be assessing new designs. And it would be very helpful if you could actually run this code when questions come up about the performance of these new designs.

DR. BAJOREK: Okay.

CHAIRMAN BANNERJEE: Now, you have EPR there which is -- we don't know exactly what it will be but for sure it will look quite different in terms of its reliance on reflux condensation, on control cool-down of the secondary site, the lap of accumulators, at least the versions I've had a quick look at.

DR. BAJOREK: Yeah.

CHAIRMAN BANNERJEE: This is quite a different plant. We'll have four trains of emergency cooling which is something else, but how will you be able to handle some of these new phenomena which you don't see in the conventional PWRs?

DR. BAJOREK: Jumping ahead a little bit, there are going to be two other documents that are going to be produced in our directorate that would accompany the generic assessment manual, and ESBWR applicability document that would look in some of its unique features in its assessment. We're also planning one for EPR. Because of those features that you just mentioned, we are doing some added assessment to look at steam generator performance in reflux condensation. Now, at this point, we don't think there is a reason to believe that TRACE is not applicable to EPR. Now the range of conditions over which we want to apply the code may be somewhat different than conventional plants but because we have not started the design certification, we don't have all the information available to us to really define that range but we've started that assessment looking at some very elementary scaling considerations in EPR to try to characterize where we think they're going to be with reflux condensation and flow patterns and the hot --

CHAIRMAN BANNERJEE: You get flooding, right, roughly and hold up in these tubes if you're flooding velocity is exceeded at the inlet?

DR. BAJOREK: Yes, we should. We do a good a pretty good job on the situations where we do see flooding, in some of out other assessments, in some of the other small break assessments.

CHAIRMAN BANNERJEE: So it's in the steam generator tubes.

DR. BAJOREK: Yes. Yeah, if we look at tests like ROSA, semi-scale where there was reflux condensation, it doesn't look too bad. Now, of course, it's difficult to characterize some of those because it does come from integral tests.

CHAIRMAN BANNERJEE: Yeah, there are some separate effect tests.

DR. BAJOREK: Which is why our newer assessment is trying to use those separate effects tests to try to really focus in on some of those models, so you rule out the compensating error question that you can have when you're looking at only at --

MEMBER WALLIS: About performance, if you have a falling film in a pipe and you -- then you get flooding. Then there's a jump in behavior completely. I mean, when the pressure drop may increase by orders of magnitude.

CHAIRMAN BANNERJEE: And that's the issue that --

MEMBER WALLIS: Can you get a sudden -- I don't know if TRACE can handle these sudden changes in behavior like that.

DR. BAJOREK: That's why we're doing that assessment.

MEMBER WALLIS: I'm just looking at the time. Are you going to be here till lunchtime?

DR. BAJOREK: I certainly hope not but I'll speed this up no matter how long it takes.

(Laughter)

We want to make sure that users are aware of what it's applicable to and where you do need to be careful. Okay. Westinghouse 2-Loop plants, BNW, AP-1000, we recommend additional assessment before we would apply the code.

MR. CARUSO: There are rumors that AP-1000s that are going to be ordered are not going to look quite like the ones that were certified. Geometry is going to change. So what is the staff planning to do to provide what what --

CHAIRMAN BANNERJEE: What's going to be certified?

MR. CARUSO: What are the TRACE people doing to insure that the staff that's going to evaluate those changes is prepared to do that?

DR. BAJOREK: We will communicate with our colleagues in NRR and NRRO and when those changes come, we'll make any modifications necessary but until they come in, I guess we can't try to anticipate things that haven't happened yet.

CHAIRMAN BANNERJEE: I guess what Ralph is saying is that even small changes because these flows are so dependent on gravity, can have very significant effects on cooling. So would TRACE be able to handle and assess these, because even small piping changes can lead to a big change.

DR. BAJOREK: Sure. You're dealing with gravity heads and very small resistences.

MR. CARUSO: Are you also going to provide the ability in TRACE to model head loss through sump screens in the AP-1000 when that gets reviewed?

DR. BAJOREK: That's merging two different meetings together. That's -- sump screens, I think are clearly unfair at this point. But no, we are --

MEMBER WALLIS: It doesn't apply to sump screens.

DR. BAJOREK: It does not apply to sump screens.

MEMBER WALLIS: Okay.

DR. BAJOREK: Unless someone develops a correlation and gives us an adequate head loss through those sump screens. Good. We do have a plant model for AP-1000 and we have been talking with NRR about getting that plant model and the additional assessment prepared here over the next year so if and when those changes do come to AP-1000 in another year or two, we have it done beforehand and it doesn't become some type of a fire drill. Yes, we're looking ahead on that.

MEMBER SIEBER: I they change portions of the deign that are important, doesn't that decertify the --

MR. CARUSO: They have to go through rulemaking.

MEMBER SIEBER: Pardon?

MR. CARUSO: They have to go through rulemaking, but I've heard that some things that were thought to fit in the building will not fit in the building, so they have to choose between, you know, rulemaking and the alternative which is unpleasant.

CHAIRMAN BANNERJEE: Make the building bigger.

MR. CARUSO: That's non-trivial, that's rulemaking as well. It won't fit.

CHAIRMAN BANNERJEE: How did it fit before in the designs?

MR. CARUSO: You know, on paper it fits, but it --

DR. BAJOREK: We've talked about our paper here quite at length. The message I just want to leave you with on this is that we're taking this very serious. We're going to freeze the code. Documentation is becoming our priority. We expect to have --

MEMBER WALLIS: I don't understand this. I would think you'd have to develop your theory before you did any code writing at all. And you shouldn't have difficulty figuring out what the theory is.

DR. BAJOREK: The theory is developed.

It's there in sets of notes, internal documentation. It's just not in the format that we could hand it to a peer review group or even to this committee in a format that you'd be satisfied with. We think we're still a few months away from that.

CHAIRMAN BANNERJEE: Are we almost done?

DR. BAJOREK: I think we are. We're getting fairly close.

MEMBER WALLIS: We went through this so quickly here. What are we doing?

DR. BAJOREK: Peer review.

CHAIRMAN BANNERJEE: Maybe it's worth spending a little time on --

DR. BAJOREK: We are going to start the peer review in 2007. We're going to ask this group, which is yet to be defined, give us a critical review of the models, comment on the assessment, the matrix, what they see there. Comment on the documentation, its clarity, thoroughness, ease of use.

CHAIRMAN BANNERJEE: Can we make -- at least can I make a suggestion, that trying to put this together that we have some interactions in ACRS and whoever is doing this, so that you get some suggestions as well from us.

DR. BAJOREK: Yes.

MEMBER WALLIS: That's much better than you coming up with a final version and us writing a long critique of it, much better if there's more interaction along the way.

DR. BAJOREK: We would like that --

CHAIRMAN BANNERJEE: We've always tried in our peer review groups at least in thermal hydraulics, not to have too many National Lab and internal people. Even if you look back at the CSAU group, you know, when it was put together, Graham and I were on it and Neil Todreas and people, and we lived through that, so there is no problem. So there's a -- it shouldn't give the appearance of being too inbred.

MEMBER WALLIS: It's not just that. It's that you're often more -- it's more effective to have someone from outside because they help you to avoid mistakes which you sort of develop blinkers about. So you want to invite people who may appear to be critical but actually are really being very helpful.

DR. BAJOREK: Well, it helps. It gets a fresh look on the situation because we start to focus on some things. They may have some fresh ideas, so that would be good. So we are --

CHAIRMAN BANNERJEE: One of the big ideas around right now which seems to be getting some traction is this idea of plug and play. And the chemical industry has been developing this called a keep open (phonetic) framework, where you can plug various components in and the backbone is ASPEN or HISYS, which in this case could be your TRACE code. But ultimately, where you need some 3-D stuff they put in whatever it the qualified 3-D code or a stress analysis code. This is something that in the forward thinking, you might want to think about.

MEMBER WALLIS: So we don't get a break until 11:15, is that --

CHAIRMAN BANNERJEE: 11:15, you need now?

MEMBER WALLIS: So half time.

CHAIRMAN BANNERJEE: All right, let's take a break for 10 minutes, then we'll start you, Joe?

DR. KELLY: Sounds good.

CHAIRMAN BANNERJEE: All right, so let's see. We'll reconvene at 25 to 11:00.

(A brief recess was taken.)

CHAIRMAN BANNERJEE: Okay, we're back in session.

DR. KELLY: Okay, as you know by now, I'm Joe Kelly. And I'm going to be talking about the upgrades that we've made to the constitutive models in

TRACE. And this is just -- I'll start with a very brief introduction, telling you just a little bit of an overview of what we have changed. Then we'll talk about two models in particular, two-phase wall drug and interfacial drag. On two-phase wall drag it's broken really into three parts; annular flow which is the reason we did this to begin with. Then bubbly slug, some corrections for the effects of long nucleation, interfacial drag models, we changed them from vertical pipes for rod bundles and then for horizontal stratified flow and I had the foresight to realize I wasn't going to be able to talk about all of that in two hours, so for this one I was only going to present results only. Hopefully, I'll get somewhere close to that before we have to stop, and then the future plans.

What I'm going to tell you on this slide is two things; what we intended to do and what we did and they are somewhat different. As we were winding down the consolidation program, it became obvious that the ESBWR was going to be submitted. And so we had to take a look at that and decide what in TRACE would most likely need to be changed in order to be able to have a credible calculation of the ESBWR, and it was condensation with non-condensible gases, both for the

PCCS tubes and for the containment walls.

So we put a new model into the code for that and I've presented that here on a couple of occasions in the past. But other than that, the plan was to retain the legacy models from the TRAC-PF1 code that we inherited. Then, go through a very comprehensive PERT based assessment process for all the applications of interest and all the highly ranked phenomena, make one complete pass through that assessment. Then review the results of the assessment, identify the models where you think you had problems and prioritize those for the model improvement needs.

Then as you go down your priority list, either develop or select a literature (phonetic) model to make that needed improvement. Then repeat the assessment and cycle back through this until you've managed to meet at least all of the high priority phenomenon. Well, that's what we planned to do. It didn't work out that way. And the reason is that very early in the code assessment process, either the code would not able to complete the transient, it would simply roll over and die or the accuracy would be so poor there was no point in even continuing. And a lot of those -- well, sometimes it's the input model, about 50 percent of the time and sometimes it was because the physical model was just so poor.

MEMBER WALLIS: This code has been used for decades, hasn't it?

DR. KELLY: Not so much Mod 2. Mod 1 was and there's some history there. Mod 1 is the code that was used for the CSAU and as part of the CSAU it was reviewed basically by this committee and there were a lot of, you know, recommendations on models that needed to be improved and that's what was done for Mod 2. Almost all of the physical models got changed between Mod 1 and Mod 2.

MEMBER WALLIS: That made everything worse.

DR. KELLY: Well, this code was never used very much and certainly was not assessed very much. It was kind of put on a shelf until we dusted it off for the AP-600 and then for the code consolidation program.

So the result of this was that the model remediation -- I don't want to really call it development because in a lot of cases it wasn't. It was just fixing things that were broken, but had to be done in parallel with the code assessment process. So you're always, you know, chasing your tail.

You know, by the time I got one model fixed, I had a list of two or three others that, you know, there was always a continuous backlog. And in the end, we've changed about 75 percent of the models in the code. If you had asked me when we began what we would have changed, I might have gone with 20 percent. There is no way I would have guessed we would end up here.

CHAIRMAN BANNERJEE: Seventy-five percent is all models or you're just talking about some specific types of --

DR. KELLY: Seventy-five percent of the constitutive models, like the heat transfer correlation, a wall drag model, that kind of thing, not the numerics. I'm just dealing with the physical models.

CHAIRMAN BANNERJEE: No, no, I realize that but that's a lot.

DR. KELLY: It's incredible. And as a result of that, and this is -- you know, comes down to me being a bottleneck, I haven't gotten the documentation finished. I have started it, but again, every time I finish one model and I start to work on the documentation, something else breaks and I have to go off on it. But we're very near the end of that process now. You know, there's very few little things that we have to fix, do the assessment one more time and then I'm 100 percent on documentation.

MEMBER WALLIS: But TRAC's been approved before these original models were replaced for various accident analyses? Have we been relying on a rather weak read for a long time here?

DR. KELLY: Well, you actually mean the TRAC P-F1 card?

MEMBER WALLIS: TRAC is TRAC, really, it seems to me TRAC is TRAC and you can't just have different mods which are so different that you have to replace 75 percent. This is telling me something about the state of the art, isn't it, if you have to change 75 percent of the models in a code?

DR. KELLY: Well, what happened, remember Mod 1 is the code that was used for CSAU. That's the code that existed at the time of all of the large experimental programs.

MEMBER WALLIS: Well, weren't its physical models essentially the same as Mod 2?

DR. KELLY: No, that was -- Mod 2 did -- it made some improvements to the numerics, like the set scheme, but the other thing, Mod 2 replaced almost 100 percent of the physical models from Mod 1.

MEMBER WALLIS: They must have put in worse ones if you then had to --

DR. KELLY: In some cases, yes.

MEMBER WALLIS: Okay.

DR. KELLY: Now, I've discussed the reflood model here before and what I said was it was well-intentioned and that was the case. They tried to build a lot of, you know, physical insight into it but they didn't realize that it really wouldn't work in a computational framework because of some of the things that it was based on.

So this is how we got to where we are today. This is just a very quick list of some of the main models that have been changed in the code. The first that we had to work on was what's called the interim reflood model and it reason it's called interim is that we never intended to do this. We intended to develop a new reflood model based upon the experimental results from the RBHT program. but at the time that we started doing the assessment and the existing model was so poor that we couldn't complete the assessments, we had to come up with a fix and that became what's known as the interim reflood model?

MEMBER WALLIS: What's the problem with these models that they were tuned to different systems, than the systems to which you wished to apply them, that maybe they were based on some simpler system like straight pipes in university labs instead of real reactors or something?

DR. KELLY: In this -- well, in this case the fundamental flaw with this model, the flow regimes in it were based on distance down stream of a quench front and all of those distances were computed based upon a criteria for a capillary jet breakup which is actually the wrong phenomenon. This is the idea of, you know, how do you get from inverted annular to disbursed flow and when you have breakup and that's not really the governing phenomena in what we have because we have a situation where the vapor generation rate is increasing expedentially as the liquid saturates in that core. But -- so they used this capillary breakup model and it depended upon the velocity of the jet of course, to give you all these links. Well, if any of you have looked at a calculation and looked at the core inlet velocity in a gravity reflex situation, it oscillates like crazy both in reality and even more so in the code.

Well, that velocity is now what's giving you these links, so all these links are doing all these crazy things and it's just, you know, like I said it was well-intentioned. We were trying to build a more fundamental level of physics in the code but it was a fundamentally bad idea.

And when I say reflood model, it's not one model. There's actually about 20 different constitutive models that have to work together, because you have interfacial drag, interfacial heat transfer, wall heat transfer and all the different regimes.

So the next one, and this one we did plan to do once the ESBWR became evident and that was a model for a condensation with non-condensible gas. The first thing I did was check the existing model against some of the Berkeley PCCS experiments. It was very poor so we developed a new one.

Interfacial heat transfer, this is primarily direct contact condensation. I mean, there are other models in it, but the ones that gave us trouble were primarily those. And those were either replaced or in some cases the implementation of it was fixed. And the reason we had to do it was excessive condensation in both the co-legs of LOFT and CCTF, for example, during the accumulator injector period in LOFT we had odd pressures and the co-leg that was sub-atmospheric and that doesn't happen. We had flow coming in the break and you know that's not right.

Two-phase wall drag and interfacial drag, those are the ones I'm going to talk about today and I'll show you the motivation for those.

MEMBER WALLIS: Earlier on we talked about flooding and if you've done an experiment on flooding, you can go from a situation of smooth falling film with essentially no interfacial drag and then it becomes disrupted and it bounces all over the place and you're interfacial drag goes up by orders of magnitude. I'm not sure how you would predict something like that.

CHAIRMAN BANNERJEE: They just back it out from the flooding correlation.

MEMBER WALLIS: But then --

DR. KELLY: In TRACE we don't try to do that from any kind of fundamental. We build in CCFL correlations. You specify this --

MEMBER WALLIS: So you assume it's already started then, it's already happening. You don't have the smooth falling film in there at all because CCFL assumes it's already flooded.

DR. KELLY: Right.

MEMBER WALLIS: Whereas it may, before it floods actually be in regime where there's very little interfacial drag.

DR. KELLY: Uh-huh. And we do that. I will show the results of one. It's actually for the UPTF hot reg.

CHAIRMAN BANNERJEE: The -- don't they just back out the interfacial drag? Well, they do it even for just it reflects more if they back it out.

MEMBER WALLIS: Yeah, that's right.

DR. KELLY: Yeah, I think that's right for the --

MEMBER WALLIS: At least one vendor does that, too. They fudge the interfacial drag to fit flooding.

DR. KELLY: Well, that's what I do for the hot lag, but for most case -- most -- and steam generators tubes are different but for most of the places where we worry about CCFL is like in a tie plate and so that's very geometry specific.

MEMBER WALLIS: It's a local phenomenon, it's not --

DR. KELLY: Right. Okay, now I'm going to start the details of the presentation, talking about two-face all drag. And the first thing you have to ask yourself is after all these years, why on earth would I be talking about two-face wall drag? And the reason was that when I went into put in a PCS, PCCS condensation model, I needed at least a pretty good prediction of the film fitness because that's the primary characteristic dimension, at least if you're not Nogadessylis (phonetic).

And this is the result using --

MEMBER WALLIS: I think that's unbelievable.

DR. KELLY: This result there?

MEMBER WALLIS: That's unbelievable. I mean, NURSOL (phonetic) is so basic.

DR. KELLY: I'll show you why on the next slide.

MEMBER WALLIS: It's so wrong.

CHAIRMAN BANNERJEE: Blue is the NURSOl solution.

MEMBER WALLIS: No, NURSOL is the red one.

DR. KELLY: This is a fine film. This is a pure steam condensation face.

MEMBER WALLIS: He's reduced the viscosity of an order of magnitude.

CHAIRMAN BANNERJEE: So why should the TRACE be different from NURSOL at least for laminar flow?

DR. KELLY: Oh, the equations -- I hope you can see the equations better than I can. Here's why. Remember, we don't -- this is not a CFD code. We don't resolve laminar and turbulent viscose shear stresses. Instead we use constitutive models, in this case wall drag.

MEMBER WALLIS: And you use some kind of a mixture average to something or other, too?

DR. KELLY: Here it is. If you figure out what the frictional pressure gradient is in TRACE, using the old model before I changed it, okay, it has two components, one to the vapor, one to the --

MEMBER WALLIS: It has wall drag due to the vapor?

DR. KELLY: Yes, always. The best thing you can say about this model is it's correct at the limits, alpha equals zero and alpha equals one and that's about it but this is what was in fact --

MEMBER WALLIS: It would depend on which fluid is on the wall and things like that.

DR. KELLY: That's reality. This is what the fluid was.

MEMBER WALLIS: Okay, okay.

DR. KELLY: Okay. This is also what REROC (phonetic) was, it's what COBRA TRAC was, et cetera, et cetera. You have to look --

MEMBER WALLIS: You mentioned reality. I have to go back to something I picked up in your user's guide. It says the purpose was to develop solutions to real and hypothetical transient scenarios. Does that mean that these hypothetical scenarios are all unreal? I didn't understand that statement.

MR. MURRAY: Well maybe you should -- maybe you should say known in an hypothetical.

MEMBER WALLIS: That's different from real and hypothetical, yeah. Anyway that's --

DR. KELLY: Yeah, the --

MEMBER WALLIS: I'm astonished, you see. This is the problem with TRAC from the beginning. People putting something just out of the air. This sort of a correlation here makes no sense. It has no basis in reality whatsoever.

DR. KELLY: And when we did COBRA TF, the very first versions, we borrowed this straight from TRAC.

MEMBER WALLIS: But who would invent something like that? I don't think it's ever been used anywhere except in TRAC.

DR. KELLY: Well, it's also in RELAP. I can't tell you about CATHARE because I don't remember. So this is actually one of the reasons I looked at it in TRAC because they had to fix it in RELAP. So anyway what supposedly makes this two-phase is using a friction factor based upon a mixture of viscosity. And you know, this just isn't very good. For annular flow, this term, even though it exists, is really negligible and you see this term and it comes to an effective liquid two-phase multiplier of just one minus alpha to the minus one power. And as you know, this should be square down here.

Now, when your void fractions are 95 --

MEMBER WALLIS: If mu "G" is zero, that gives you an infinite thing on the right-hand side, and you take it to the minus one, you get zero. So if mu "G" is zero, the mixture of viscosity is zero even if the bubbles are in the goopiest liquid imaginable. It makes absolutely no sense.

DR. KELLY: Couldn't agree with you more, which is why it's gone, it's history.

MEMBER WALLIS: Yeah, but you see, that's a problem I have is that, is this the way all the codes were before you came along?

DR. KELLY: Pieces of some codes. So this is what we have now and you know, I've wrote it in terms of a liquid two-page multiplier here, just for recognition and here's the --

MEMBER WALLIS: Well, the problem is those guys never had peer review. Must be something like that. How could something like that be used?

DR. KELLY: Well, you know wall drag wasn't very -- if you did a PER (phonetic) and looked at highly ranked phenomena, wall drag really wouldn't show up. You'd typically have form analysis and things like grid spacers, area changes, but about the only place wall drag is significant, really is in the steam generator tubes. But for most, you know, TRAC, at the time of the CSAU, its application was large break LOCA. It didn't really matter.

MEMBER WALLIS: Because there were no long straight pipes in reactors.

DR. KELLY: Right, the pipes tended to be like --

MEMBER WALLIS: Except in steam generators.

DR. KELLY: Right.

MEMBER WALLIS: Okay.

DR. KELLY: Especially, long small diameter straight pipes.

MEMBER WALLIS: So you could be way off and it didn't matter.

DR. KELLY: Right. It was unimportant. That's not the case if you're trying to predict the film on the inside of a condenser tube. So for the two-phase multiplier, one minus alpha to the minus two, and you do the math and you know, it's a very simple formula.

MEMBER WALLIS: You're going back to Martinelli.

DR. KELLY: Right.

CHAIRMAN BANNERJEE: So you're just going back to Martinelli.

DR. KELLY: Yes. Then for the friction factor, this is friction factor versus Reynolds number. Hey, if it's a good idea, you might as well recycle it, you know. Something that's simple and gives a good answer is a lot better than something complex that's shaky. So this is friction factor versus Reynolds number. What is normally used for pipes in TRACE is the Churchill correlation and there you see the laminar and turbulent behavior.

MEMBER WALLIS: Well, what's your definition of a Reynolds number?

DR. KELLY: The Reynolds number here for

the annular flow regime --

MEMBER WALLIS: Is base on the liquid viscosity.

DR. KELLY: -- is, yeah, the liquid viscosity and the liquid mass flows and you can rewrite that as, you know, four times the flow rate per unit surface area.

MEMBER WALLIS: So if there's no liquid there at all, there's no friction.

DR. KELLY: Right. Well, there's ramp from you know, annular flow to single phase vapor. You have to try to cover all those possibilities. So once the --

CHAIRMAN BANNERJEE: This assumes the wall is wet.

DR. KELLY: Yes, I'm talking about annular flow at the moment.

CHAIRMAN BANNERJEE: Yeah, post-CHF it will go to go to gas.

DR. KELLY: That's actually one of -- it didn't used to, it does now. Now, it looks to see where quench fronts are. Between the quench fronts where the wall is dry it puts all of the drag onto the gas phase. Above and below it puts it on the liquid phase. So, for --

CHAIRMAN BANNERJEE: Is this two Churchill or which Churchill is this? I've never --

DR. KELLY: I don't remember --

MEMBER WALLIS: Winston.

DR. KELLY: -- but when you get the documentation I can guarantee the reference will be in there. But, you know, it's a approximate -- it has like three different things that go together and it gives you the shape through the transition region.

CHAIRMAN BANNERJEE: By why pick this? I mean, there are 900 correlations. What did this do for you that was --

DR. KELLY: I didn't pick Churchill. That's the one that was in TRAC and what it does do, though, is it covers laminator transition and turbulent.

MEMBER WALLIS: This was also in RELAP you said?

DR. KELLY: I don't know about the Churchill correlation.

MEMBER WALLIS: But the previous slide you said that alpha, that was in RELAP, too.

CHAIRMAN BANNERJEE: It's only to give you one correlation through the laminar and turbulent regions.

DR. KELLY: Right, but for annular flow, there's no evidence that you have this. If you look at, you know the book by Huling, Hall, Taylor (phonetic), Duckworth's calculations, et cetera, you get this nice smooth shape and so the smooth shape is what I implemented in TRACE and I did it by using a power wall weighting or just a laminar and turbulent. So this black line is what we actually use for the annular flow regime.

And so this new TRACE model, if you do it as a two-phase multiplier, and this really is phi with alpha square, so it's phi sub L but not with the square, prodded against liquid fraction, and I have some upflow data and downflow data and then what the model would be and so it, you know, obviously, I took it from this kind of thing, so it looks pretty good.

But it also gives an excellent comparison against falling film fitness data. This blue line -- well, this is non-dimensional film thickness against the film Reynolds number.

MEMBER WALLIS: This is for a non-disturbed film, it's not post-CCFL.

DR. KELLY: Right, this is a simple falling film on a wall. But the blue line was calculated by TRACE. There are about 500 data points there from, you know, Shannon Abel (phonetic). There's some --

MEMBER WALLIS: And if the old TRACE had done it, it would have been off by a huge amount.

DR. KELLY: Yes.

CHAIRMAN BANNERJEE: Well, the low Reynolds numbers you have the nozzle solution, right?

DR. KELLY: Exactly. And this is what I showed before. It's a pure steam condensation case. Obviously, you're starting the tube here, coming down this way. That's the old solution. This is what falling film would give you and this is what we get in TRACE and this is what you expect because there is some interfacial shear and --

MEMBER WALLIS: I wonder what TRAC-G has done to something like this. Has TRAC-G got the same kind of glitches in it that the told TRAC had?

DR. KELLY: I know they put in a model for condensation and non-condensible gases but I can't speak to whether --

MEMBER WALLIS: Well, they've got a model in the ESBWR.

DR. KELLY: But I haven't reviewed it, so I can't say. I really don't know.

MEMBER KRESS: As best I recall, it had

the old model that he showed first.

MEMBER WALLIS: The old model?

MEMBER KRESS: Yes.

MEMBER WALLIS: Like that, so the --

MEMBER KRESS: With the void fraction in it and the gas flow and liquid flow because we discussed that and had a large objection to it one time.

MEMBER WALLIS: This is why you guys have to have your own code.

DR. KELLY: Well, we now met what I wanted to do was get it to work for annular flow, so I could do the --

MEMBER WALLIS: That was a simple problem, though.

DR. KELLY: Well, you can't do that -- but now we come to the bubbly slug flow regime and the first thing we could do is just keep the model that's there because remember, we're trying not to change physical models unless we have to. And I had to change the one for annular flow. Well, looking at the legacy model, just looking at the formulation, I know that it gives drag to both phases when it shouldn't. And I can look at a two-phase multiplier and know that it's going to under-predict the wall drag. I can do that just by looking at it.

So I know it's wrong and about where you would have to fair this into the annular flow, you could be off by a factor of five or 10 on the wall drag. So I didn't -- I knew it was going to be inconsistent and I didn't want to have to build in a ramp between the two when I knew this wasn't that good. So I thought, okay, let's replace it.

Plan B, you can get a two-phase multiplier from the literature and the one that most people recommend these days is by Friedel. One problem with this, or one serious problem, it's based on flow quality. That's great for a steady state, you know, co-current up-flow test. For transient situations, it's meaningless for counter-current flow and for a case closer to the pool boiling. It's also a very complicated function of mass flux pressure and diameter and I'd still end up with something that was inconsistent with the annular flow model and have to ramp it in somehow or other.

So what I decided to do was seek a two-phase multiplier if it's a function of void fraction.

CHAIRMAN BANNERJEE: These are all sort of steady state things in the literature, right?

DR. KELLY: Uh-huh.

CHAIRMAN BANNERJEE: But what happens if you have a rapid change in flow like in a large break LOCA?

DR. KELLY: Well for wall drag I'd have to say fortunately, it's not important.

CHAIRMAN BANNERJEE: For what?

DR. KELLY: For wall drag, I'd have to say fortunately it's not important.

CHAIRMAN BANNERJEE: It's not important.

DR. KELLY: But for other models, that's a concern. You know, you tend to use fully developed steady state data to do an a model and is that applicable in a rapid transient and --

CHAIRMAN BANNERJEE: Would be like people added mass terms, right?

DR. KELLY: Well, but I'm talking about if you're using say DDIS (phonetic) bolt for heat transfer, you know, does that apply if you don't have a transient term on it? It's not perfect but it's probably not bad, but the key to that is you do the assessments for that particular application and you have to demonstrate that you cover the full range, you know, as best you can.

So anyway an example of a wall drag model that fits that is Lockhart-Martinelli.

MEMBER WALLIS: I don't quite understand that. If you're trying to model, say a falling film, it doesn't know what the gas is doing, it doesn't know what the void fraction is. It just only knows what its thickness is.

DR. KELLY: Uh-huh.

MEMBER WALLIS: So why should void fraction have anything to do with a falling film?

DR. KELLY: Well --

MEMBER WALLIS: And the pipe could be infinitely wide and it's still a falling film on the wall. The void fraction is one.

DR. KELLY: Uh-huh.

MEMBER WALLIS: So I don't see why you need a void fraction to model a falling film. Maybe in this case bubbly slug but if you look at some simple thing like the previous slide, it doesn't make sense.

DR. KELLY: Well, except that void fraction is what the code solves for. That's one of the primary independent variables in the code. And so you have to then convert that void fraction you know, just through geometry, into a film thickness, but you also have to allow for the possibility of interfacial friction.

For example, in the PCCS condensers you have co-current downflow. And so then you're solving the two-fluid momentum equations along with mass and energy. And it's the -- it's gravity, interfacial friction and wall drag that combine to give you the film thickness.

You know, I could just write what the film thickness is for Nusselt. Now in some codes you'll see Nusselt use for a condensation model. But then you have to review all the assumptions that went in to generating Nusselt, like no interfacial shield.

MEMBER WALLIS: What I just said is untrue in this business about the film on the wall because of the way file square is defined, it's based on the pipe being full and if the pipe is humongous then you have sort of infinity over infinity. By the time you've done it, you get back to a falling film if you do it right.

DR. KELLY: Right, exactly. So for bubbly slug I wanted to see, you know, if I could come up with a two-phase multiplier that was a function of void fraction. So step one, go get some data.

MEMBER WALLIS: Looks like a Martinelli plot.

DR. KELLY: It is in effect. It's the

two-phase multiplier versus the liquid fraction. This is some 80 vatic steam water data from Furrell and McGee. The dotted blue line is Lockhart-Martinelli. And the black line is the one that goes through the data. And the exponent here is minus 1.72. So it's very close to minus 2.

That was Step One was going and getting some data and looking at it. I looked at more data than this but this was the one that had the most points.

MEMBER WALLIS: This is a log plot.

DR. KELLY: It's log on the vertical axis one and 10/20, and it's linear on the liquid fraction.

CHAIRMAN BANNERJEE: It's got quite a deviation at the --

DR. KELLY: At those points, yeah.

CHAIRMAN BANNERJEE: Yeah.

DR. KELLY: But, you know, that's -- you know, any experiment --

CHAIRMAN BANNERJEE: It's bubbly flow, right/

DR. KELLY: Pardon me?

CHAIRMAN BANNERJEE: That's bubbly flow.

DR. KELLY: No, this is -- yeah, you're right. That's bubbly flow there. And I had some other data that, you know, in other places fitted better or worse. But at any rate, this looks pretty good. And one of the things down here is quite often the pressure drop is not very large the frictional pressure drop.

CHAIRMAN BANNERJEE: It's hydrostatic.

DR. KELLY: Right, so the error becomes very large.

CHAIRMAN BANNERJEE: These are for vertical tubes?

DR. KELLY: Yes.

MEMBER WALLIS: Well, in vertical slug flow the wall friction could be negative.

CHAIRMAN BANNERJEE: Yeah.

MEMBER WALLIS: Because it's holding up the film around the bubbles.

DR. KELLY: Yeah.

MEMBER WALLIS: And you can't plot negative on this plot, so it's not there. You can't plot it on a log scale.

DR. KELLY: Well, the one thing I didn't -- in the -- when I showed the pressure gradient, I went ahead and squared the velocity. In the code, it's the absolute value of the velocity times the velocity. So you get the direction in it. But, of course, we would only get negative wall drag if the liquid was falling down.

MEMBER WALLIS: Well, it is around the bubbles.

DR. KELLY: Right, but we don't really go to that scale. And slug flow is something you don't really see in reactors either with the exception of steam generator tubes.

CHAIRMAN BANNERJEE: Well, I think you will see them in the ESBWR because their not quite slug.

DR. KELLY: Caps.

CHAIRMAN BANNERJEE: They're annular.

MEMBER WALLIS: Right, yeah.

DR. KELLY: You have big vapor structures maybe 60 millimeters in diameter, that kind of thing but you won't have slugs that are a meter.

MEMBER WALLIS: They're unstable, those slugs are unstable.

CHAIRMAN BANNERJEE: Well, at that size, you won't get slugs.

DR. KELLY: Right.

CHAIRMAN BANNERJEE: If you would argue you can't get slugs in large pipes.

MEMBER WALLIS: You can if you make it --

the first one is a slug but then after that --

DR. KELLY: That's true.

MEMBER WALLIS: You can make one, make it in static liquid, but once the liquid gets disturbed, you won't get any more.

DR. KELLY: So Step one was get some data and see if it made any sense for you to continue. Step two was going to the literature and seeing if you could find some models, and I found two; one for up-flow and one for down-flow. For up-flow --

MEMBER WALLIS: It's like going the Bible, you just go to the right chapter and verse and you can find a correlation that you want.

DR. KELLY: Yeah. But it was surprising in the realm of two-phase multipliers, almost all of them are correlated versus flow quality and they get to be very, very complicated. You don't see any of them that look like this and the reason is --

MEMBER WALLIS: You don't see any that look like that?

DR. KELLY: Not as a function of quality, none that are this simple.

MEMBER WALLIS: You've never read my book?

DR. KELLY: Well, okay, but even then it's more details than this.

MEMBER WALLIS: I've got a whole pile where the experiment from one to two, I think, depending on your model.

DR. KELLY: Yeah, but the point is, if it's void traveling it can be very simple. If it's quality, it can, and that's because the fundamental dependence is really on the void fraction. And if you're correlating against quality, what you first really have to do is translate the quality to void fraction and then correlate it.

MEMBER WALLIS: So you had to go to Japan to find the correlations that you wanted?

DR. KELLY: Well, those were the two I found in the literature.

MEMBER WALLIS: But there are hundreds of literatures, so why did you pick those two?

DR. KELLY: Well, these are the two I found that --

MEMBER WALLIS: It was the closest to what you wanted, right?

DR. KELLY: Because they were correlated in terms of void fraction instead of quality and that's what I was looking for.

CHAIRMAN BANNERJEE: I want to ask you a

little bit about this approach, you know, sort of this is a `50s way of doing things and the `50s way basically says that you have flow through a pipe with a friction factor without one phase or the other. And then you take sort of a ratio and then you get that. Now, that was done because you knew nothing about the true liquid velocities and things like this. Now, your code is producing a liquid velocity for you, right and a --

DR. KELLY: A cross-sectional averaged.

CHAIRMAN BANNERJEE: Yeah, cross-sectional average liquid velocity, a cross-sectional average gas velocity.

DR. KELLY: Right.

CHAIRMAN BANNERJEE: And a void fraction as well. Now, is there nothing that is a little bit more mechanistic that correlates with those liquid velocities than this?

DR. KELLY: Here it is.

CHAIRMAN BANNERJEE: Right.

DR. KELLY: That's all it is. All I was doing --

CHAIRMAN BANNERJEE: But now how is the friction factor --

MEMBER WALLIS: Because VL is related to

JL buy one minus alpha.

DR. KELLY: This is your normal single-phase liquid friction factor.

CHAIRMAN BANNERJEE: Yeah, but shouldn't that be --

DR. KELLY: That's all it is.

CHAIRMAN BANNERJEE: -- a range .005 or something?

DR. KELLY: Yeah, that's all it is and this is what we use.

MEMBER WALLIS: Well, if you use that, you get two instead of 1.75 if you assume the same friction factor for the two cases.

DR. KELLY: Except --

MEMBER WALLIS: If you assume a RLYS (phonetic) number dependence, then you can get a different --

DR. KELLY: Yeah, exactly.

MEMBER WALLIS: So it's gone back to the `50s as my colleague says. This is the most elementary thing you've been teaching to students for a long time, but it's very, very, very simplistic.

DR. KELLY: It's also --

CHAIRMAN BANNERJEE: All right, so that's effectively what you're doing and it works.

DR. KELLY: Yeah, and it works and it's a lot more correct than what was there before.

CHAIRMAN BANNERJEE: Sure. Depending on which phase is wetting the wall. You have to know that.

DR. KELLY: Right, that switch is there, too.

CHAIRMAN BANNERJEE: Yeah.

DR. KELLY: Now, I thought this looked pretty good. Any time I can take a model in the code and simply it and get a better answer, I'm all for that. Well, I made the mistake of looking at more data.

MEMBER WALLIS: That's right, well, you should always do that.

DR. KELLY: In particular, I looked at data where the wall -- with wall nucleation.

CHAIRMAN BANNERJEE: That would, of course, be quite different, yeah.

DR. KELLY: And that's what this is. So again, the two-phase multiplier versus the liquid fraction. This covers actually three different pressure levels.

MEMBER WALLIS: Now, are they actually measuring void fraction in this test or are they doing it for energy balance?

DR. KELLY: Yes, no, they measure the void fraction with a gamma densitometer. And actually it was very hard to find pressure drop tests where they measured the void fraction.

CHAIRMAN BANNERJEE: Who did this work?

DR. KELLY: This was be Ferrel and Byland. It was -- and the other one was Ferrill and McGee. I think it was North Carolina State. I think that's right but this was in the `60s.

MEMBER WALLIS: All the good work has been done in the `60s. We know that.

DR. KELLY: We can't afford to buy a gamma densitometer these days.

CHAIRMAN BANNERJEE: I know how to make them very cheap.

DR. KELLY: Okay.

CHAIRMAN BANNERJEE: I'll do it for you.

DR. KELLY: So it had three different pressure levels between four and 17 bar.

MEMBER WALLIS: So now your x-axis is liquid fraction, it's not void fraction. It's the other way around.

DR. KELLY: Right, and that's what was on the previous one.

MEMBER WALLIS: When you think you've got all liquid, you've got a little bit of bubbles on the wall, that's when you get this big error on the right-hand side there.

DR. KELLY: Right, this is about a factor of four.

MEMBER WALLIS: Well, it's a bigger -- when we get to the end there, it's -- yeah, I guess it's a log scale, it's always a factor of four.

DR. KELLY: Yeah, right. And you see there's a little bit of a mass flux effect. This is like at 500 and this is about 1700. And you --

MEMBER WALLIS: So it's down with max flux?

DR. KELLY: Pardon me?

MEMBER WALLIS: The correction goes down with max flux, increase in max flux?

DR. KELLY: Uh-huh.

MEMBER WALLIS: It's just it's hard to read it. Okay.

DR. KELLY: Yeah, I'm sorry about that. They looked great on the computer screen.

MEMBER WALLIS: That's all right, I can see it now.

DR. KELLY: But so what I did was

introduce a correction factor, one plus the correction factor for nuclear boiling and this is where we get off on a little bit shakier ground but if you want to try to match that data, you have to do something and I did compare this to Friedel and the previous model is more accurate than Fridel.

MEMBER WALLIS: Now, is this because they have an error in the acceleration pressure draw? To get this friction thing, you have to take away gravity and acceleration, don't you?

DR. KELLY: Yeah, I actually reduced this data and I --

MEMBER WALLIS: Putting in the void fraction?

DR. KELLY: Well, no they reported the void fractions from a gamma densitometer. Okay.

CHAIRMAN BANNERJEE: But not the wall void fraction, just the overall, right?

DR. KELLY: Just the overall, the cross-section average.

MEMBER WALLIS: So then you took way the acceleration pressure drop. Was that a big effect?

DR. KELLY: In this case it was -- it wasn't a factor of four. It was more 10, 20 percent kind of number, okay. I don't know the physical phenomena that causes this.

CHAIRMAN BANNERJEE: Probably wall roughness, right?

DR. KELLY: Well, maybe.

MEMBER WALLIS: It also stirs things up and increases it.

DR. KELLY: That's what I believe. You know, if you go and like read Collier's textbook, you talk about sub-cool boiling and wall roughness. Well, maybe but -- I don't have the plot to show you but one of the plots I made when I was looking at this was I colored the points as to whether they were sub-cooled or saturated. Sub-cooled points laid right on top of the saturated.

MEMBER WALLIS: But to makes ships slide through the water better, they put bubbles through the hull and it decreases the friction. When you put bubbles in your boiling tube, it increases the friction.

DR. KELLY: Confined versus -- internal versus external.

MEMBER WALLIS: Well, I don't know.

DR. KELLY: But I think this is what you were talking about. If you had nuclear boiling going on, you have bubbles that are, you know, if you will, in the boundary layer, moving out into the center of the pipe.

MEMBER WALLIS: Stirring things up.

DR. KELLY: Stirring things up in a high velocity liquid.

MEMBER WALLIS: Stirring momentum transversely.

DR. KELLY: Exactly. That's what I think. I --

MEMBER WALLIS: Does this have a turbulent flow?

DR. KELLY: Yeah, because the mass flux is there.

MEMBER WALLIS: If it was laminar, it's not going to be laminar for long with all those bubbles.

DR. KELLY: Right, but now, these are pretty mass flux.

MEMBER WALLIS: Now, is this sub-cooled boiling at the end there or is that -- sub-cooled sort of shakes things around without actually --

DR. KELLY: In this region, up to about here, some of these points are sub-cooled, some of them are saturated.

MEMBER WALLIS: They appear and go appear and go, so they really stir things up presumably.

DR. KELLY: But you know, just like when you're trying to explain why heat transfer is so good in nuclear boiling, the bubble departs and the liquid rushes in behind it, well, that would effect all drag as well. That's what I think is going on here but this wasn't a fundamental, you know, investigation into how to model wall drag. It was just trying to quickly get over a problem that TRACE had. So what I did was develop an empirical model using the data from this one source because that was the only source I had.

You ask if this was laminar. The mass fluxes are relatively high. The lowest is about 500 kilograms unit square per second going up to close 2,000.

MEMBER WALLIS: Everything you do, of course, is in SI units?

DR. KELLY: Yes.

MEMBER WALLIS: That's true, that's a true statement?

DR. KELLY: Yes.

MEMBER WALLIS: So when you --

DR. KELLY: Well, bar, I should put MPA, but you know, I use those two. Occasionally, I'll give the English units in quotes but --

MEMBER WALLIS: What is G then? G is --

DR. KELLY: Mass flux.

MEMBER WALLIS: Milligrams per square meter per second?

DR. KELLY: Yes. So these are pretty high. And these tubes were like -- they were like three different diameter tubes. They were around a half inch to three-quarter inch. So this is highly turbulent flow. One of the reasons for that is you have to get it to these kind of mass fluxes or wall drag is not large enough to measure.

CHAIRMAN BANNERJEE: So when you had no boiling, everything worked and Lockhart-Martinelli worked well --

DR. KELLY: Yeah, slightly different than Lockhart and Martinelli.

CHAIRMAN BANNERJEE: Yeah, whatever, close to it, and now, you've got boiling, so you would think you'd use something like Martinelli-Nelson, right, because have of that quality. And the line there is what, Lockhart-Martinelli still or --

DR. KELLY: Yes.

CHAIRMAN BANNERJEE: But that shouldn't apply to a boiling system exactly because there is additional, as you say, turbulence generated by boiling. But there is Martinelli-Nelson, right, where he has this series of curves for different --

DR. KELLY: Yeah, again, but that's going back to correlating in terms of mass flux and then you end up with either -- you know, tables or very complicated models. Now, I compared this simple, you know, Lockhart-Martinelli type model.

CHAIRMAN BANNERJEE: Diabetic, I mean --

DR. KELLY: Adiabatic flow. I compared that against Friedel for both the adiabatic and the diabatic case. This simple model did a better job than the two-phase four multiplier that everyone is recommending using. Now, I --

MEMBER WALLIS: But if you talk to Tom Handratti (phonetic), he doesn't use anything except his models, so you'd better use his, now see what happens there.

DR. KELLY: Well, I used one of his models in the code and in one case I tried using one and it didn't work. But so I developed a relatively simple empirical model for that correction factor and I used the bubble departure diameter that came from Levy, but you'll notice a lot of it is the function of the void fraction. It has a maximum of about 50 percent void and goes to zero --

MEMBER WALLIS: This is surface tension, Saul Levy has surface tension and bubble up the wall.

DR. KELLY: Yeah, there's a surface tension and a wall shear stress. Okay. Yeah.

MEMBER WALLIS: Okay, I remember that. It goes back a long way, late `50s or something.

DR. KELLY: Yeah, so this gave me -- this resolved the mass flux effect.

MEMBER WALLIS: What have people been doing since the `50s? They've been just screwing everything up?

DR. KELLY: Well, I was at a presentation, I don't remember what the conference was but Professor Hewitt was talking and he was talking about pressure dropping pipelines. And he started out and he showed the HEM model and he showed the comparative data and you know, it looks reasonable. It has the right trends.

And then he said, then along the way, we thought about this, whatever this phenomena was, and you know, it kept getting bigger. And as it went through time, it got bigger and bigger and bigger, until the correlation covered, you know, several slides. And then he showed performance and accuracy versus time. Accuracy versus time went down as the models got more and more complicated.

And I don't remember exactly what his quip was, but it was basically like the more and more we learn, the less we knew.

MEMBER WALLIS: It's like students in universities, they can do -- they can do less math when they graduate than when they came in.

DR. KELLY: I can't do math at all any more. So at any rate, when you put this in and you compare it against the data. Now, again, this is the data that it was developed from, it's very accurate. It had nearly a zero average error and an RMS error --

MEMBER WALLIS: That's what bothers me a bit about just using this Ferrill volume, I mean, there's a lot more data out there.

DR. KELLY: You'd be surprised --

MEMBER WALLIS: It's a lot of work to collect it and test it all, validate against it.

CHAIRMAN BANNERJEE: Is this in that tube, this data?

DR. KELLY: Yes.

MEMBER WALLIS: Straight tube?

DR. KELLY: And it's surprising how little pressure drop data there is where the void fraction is measured.

CHAIRMAN BANNERJEE: I thought there was some --

DR. KELLY: There's a whole lot of pressure drop data --

MEMBER WALLIS: Well, it doesn't matter because you're going to calculate the void fraction in TRACE and then you're going to feed it back into it.

DR. KELLY: Right.

CHAIRMAN BANNERJEE: Isn't there some Canadian data that -- I know they used neutron densitometers. I don't know if --

DR. KELLY: Maybe but I didn't find it when I looked.

MEMBER WALLIS: Yeah, but you'd have to have measured the void fraction in order to test the model because now you've got the model, you can predict the void fraction and complete the loop and just test -- compare friction pressure draw.

DR. KELLY: And when we have an application where wall drag becomes a high priority phenomenon, we'll do exactly that. But now, remember, I got it to work for films in condenser tubes and that's what I wanted to do. And then along the way, I just went ahead and improved the model from what was there before because I considered it a glaring deficiency. I did not do this work because wall drag and bubbly slow was giving us any trouble.

But you mentioned that we could use the void fraction and compare the data. Well, you can only do that if you interfacial drag models are any good. And so now we're making a change. We're going to talk about interfacial drag and we had basically two problems with the legacy models that we inherited, accuracy and unphysical oscillations. And I'm going to show you examples of both of those.

I know these are hard to read up here. This is calculated versus measured void fraction and this is some steam water pool data of Berringer and you'll notice one point --

MEMBER WALLIS: What do you mean by pool data now?

DR. KELLY: It's steam bubbling up through a pool. The liquid isn't flowing.

CHAIRMAN BANNERJEE: It is a level swell.

DR. KELLY: In effect, yes.

MEMBER WALLIS: The bubbles are usually bigger than you think.

CHAIRMAN BANNERJEE: But drift flux does a pretty --

MEMBER WALLIS: And they follow each other. They get caught in each other's wakes and things.

DR. KELLY: That's where we're headed.

MEMBER WALLIS: But you know, boiling data usually gives you a higher velocity of bubbles than if you just take something out of an air/water data. That's what I think you're showing here, isn't it? The void fraction is less.

DR. KELLY: Well, let me answer that as we go through this.

CHAIRMAN BANNERJEE: Yeah, but this was using the interfacial drag models in --

DR. KELLY: From the old code, the models we inherited. And this is steam water pool. You see it consistently over-predicts. And then there's this very funny bump in here once you go above about 10 percent in the data and so --

CHAIRMAN BANNERJEE: It's transition, I guess.

DR. KELLY: Exactly.

MEMBER WALLIS: Those are bigger bubbles, bigger bubbles.

DR. KELLY: Well, remember, this is the code model not the data. This is calculated versus measured. So we should be lying right along this line.

MEMBER WALLIS: So what's the basis of the calculation?

CHAIRMAN BANNERJEE: It's an interfacial drag correlation.

DR. KELLY: The one that was existing in the code and I'm going to show you a part of it but I'm not going to go through the details because I don't want to spend all the presentation time --

CHAIRMAN BANNERJEE: Then you eventually get to drift flux base.

DR. KELLY: Right, because I'm going to throw this model away. This is just showing you why I'm doing the work.

MEMBER WALLIS: Well, the real question is, what's the bubble rise velocity?

DR. KELLY: We're getting there. This one is void fraction versus elevation. This is for a rod bundle. You know the other was for what a six-centimeter pipe and this is for a rod bundle. It's basically just a -- it's like a low pressure boil-off type condition. And two things to notice in this, one is a large over-prediction in this bubbly, slug whatever kind of area and in this part you'll notice the void fraction level is out at 75 percent. The wasn't calculated with TRACE. It was done on a spreadsheet but I put the TRACE --

MEMBER WALLIS: Usually that results because of a C1 or something that once you get a high void fraction it's all dominated by that distribution coefficient that Novack has that --

DR. KELLY: Yeah, we'll get to that.

MEMBER WALLIS: But we're there already, though.

DR. KELLY: Yeah, the reason this is flat here is because it comes from the spreadsheet solution. That actual models in TRACE oscillated so badly that I didn't know what value to put here. If you take that same calculation and I show you one of those elevations versus time, it looks like that, and yeah, there are some oscillations in the data but they're not this big. So I had two problems. One was accuracy and the other was unphysical oscillations.

This shows how you get at least the oscillation problem. I took those models --

MEMBER WALLIS: One way to get oscillation is just to have a flow regime map where the computer can't make up its mind and it goes into annular and it says annular is unstable, you'd better go back to slug and it says slug is unstable, you'd better go to annual, and so it just hops around between the two forever.

DR. KELLY: Uh-huh, and in effect, what was done -- this in interfacial friction coefficient versus void fraction and it's a log linear scale.

MEMBER WALLIS: Three orders of magnitude?

DR. KELLY: Yes.

MEMBER WALLIS: This is like a materials plot.

MEMBER KRESS: Or a PRA.

CHAIRMAN BANNERJEE: PRA.

MEMBER SIEBER: It's not that bad.

DR. KELLY: And you know, it comes up and hits the peak about right. There's this funny, you know, transition here. But what's really the problem is this --

MEMBER WALLIS: This is predicted, this curve here?

DR. KELLY: Yeah, that's what you get using the old TRAC-PF-1 models and they use a linear interpolation of a drag coefficient between a void fractions of 50 and 75 percent. So this is a linear interpolation from whatever they would calculate here to here, to an annular floor model. I won't mention what annular floor model.

MEMBER WALLIS: What's going on here? You said interfacial friction coefficient of 10^4.

DR. KELLY: Yeah.

MEMBER WALLIS: It doesn't make sense to me.

DR. KELLY: Well, it's not the point that the --

MEMBER WALLIS: CF is 10^4 .

DR. KELLY: Well, in other slide I'll show you what the interfacial drag coefficient is. This is not the .005 number. Okay, it has the interfacial area for unit volume built into it.

CHAIRMAN BANNERJEE: Plus you have a form drag here.

MEMBER WALLIS: Even so.

DR. KELLY: But at any rate, the problem here is there's three orders of magnitude and they're doing a linear interpolation which means the minute you move onto that ramp, this changes by orders of magnitude. So it's exactly what Professor Wallis said, the code would say, okay, I'm a void fraction of 75 percent, it has to be annular flow. The annular flow drag is so low it couldn't support the liquid.

The liquid falls down and goes oh, I'm in bubbly slug, you hit it with this huge one. You throw the liquid up and keep going back and forth.

MEMBER WALLIS: Which is actually what happens --

CHAIRMAN BANNERJEE: Which is actually --

MEMBER WALLIS: Actually what happens in the pipe.

DR. KELLY: Yeah, but this is for different reasons, okay.

MEMBER WALLIS: It happens in the pipe.

CHAIRMAN BANNERJEE: It has turbulence.

MEMBER WALLIS: It slides down the wall until it's unhappy and then it gets all mixed up and it goes up and then it comes back down again and --

DR. KELLY: But we're not doing slug tracking, we're doing, you know, volume and time average equations.

MEMBER WALLIS: If you had this fine nodalization my colleague talked about, you'd probably capture all that. You've got all these transients that look like slug flow bubbles.

CHAIRMAN BANNERJEE: That's what happens in GE chimneys. They're right in this regime, 60 to 75.

DR. KELLY: Yeah, I have some data that I'll show later in the presentation and then we can talk a little bit --

CHAIRMAN BANNERJEE: It would be nice to do a chimney calculation with TRACE with fine nodalization.

MEMBER WALLIS: Slug annular transition is very interesting because, in fact, you've got two regimes in the pipe at the same time. And trying to model it with one is fraught with some difficulty.

DR. KELLY: And what's been done in the past in all of these codes is you say I think I know this one, I think I know this one. Let's do some kind of interpolation between them. And I'm not going to much of --

MEMBER WALLIS: Part of it's been the pressure to deliver something. You have to do something so you assume you interpolation because you have to get on with the problem, deliver something to the NRC.

DR. KELLY: And in this case, the linear interpolation causes a problem.

MEMBER WALLIS: Are you going to interpret it in your paper or --

CHAIRMAN BANNERJEE: He's going to change the model.

MEMBER WALLIS: He's going to change it anyway. He's going to change it anyway.

CHAIRMAN BANNERJEE: He's going to use the slides.

MEMBER WALLIS: But you seem to be changing so many things here.

DR. KELLY: Yeah, and I never intended to, but you know, if you look at void traction in a rod bundle, at a low pressure boil-off condition, this is what you see in AP-1000 during the passive cooling phase. If you look at this, how large this error is then --

MEMBER WALLIS: Well, this is why we do large-scale experiments in order to get our feet on the ground properly.

DR. KELLY: This is a huge error in inventory and in a small break LOCA, whether it's you know, a passive plant that's depressurized or not, inventory is that name of the game.

MEMBER WALLIS: Where does all the water go when you get that difference?

CHAIRMAN BANNERJEE: Out the break.

MEMBER WALLIS: Always out the break?

DR. MAHAFFY: Where else? There's nowhere to go.

DR. KELLY: So --

MEMBER WALLIS: Why didn't you take pictures instead of this sort of --

DR. KELLY: I should have but these are my cartoons and they're not very good but they give you an idea. These are the four flow regimes that we basically consider. I'm going to take about these three because these are the three I changed.

CHAIRMAN BANNERJEE: Where is churn annular which is a huge flow regime?

DR. KELLY: Yeah, that's kind of between these and that's -- I'll talk about that transition as well but that's where we don't know much.

CHAIRMAN BANNERJEE: That's what will happen in the chimneys.

DR. KELLY: And I'll show you how well we do or don't in just a few slides. I showed you the basic two problems with the legacy models, accuracy and oscillations. So to improve the accuracy, I'm going to implement a drift flux base interfacial drag model. So for dispersed bubble regime a simple turbulent model, for the slug or Taylor cap bubble regime, the Kataoga-Ishii and for rod bundles, a mono Bestion, this was actually an early Catar model that was published in the open literature so I could use it.

And then for my transition, rather than do linear weighting, I'm going to use a power wall weighting scheme and I'll show you that in a minute. Step one is you have to take a drift flux model and turn it into an interfacial drag coefficient. So this gives you an idea of what the interfacial drag coefficient is. Bring this relative velocity squared over here and you get the force per unit volume, the interfacial drag force per unit volume. So it's just IC times V relative square. In other words, it has the density and it has the interfacial area inside it.

So the first thing I did, and I'm not the only one that's done this, but I basically copied some other work. I equate it to the buoyancy force and that gives me the interfacial drag coefficient as a function of void function and relative velocity. Well, that doesn't help me because I don't know the relative velocity but I can get the relative velocity.

MEMBER WALLIS: Drag force is what, per unit volume of the stuff or per unit volume of the pipe or what?

DR. KELLY: Per unit volume of the -- yeah, the flow area.

MEMBER WALLIS: The pipe.

DR. KELLY: The pipe, right.

MEMBER WALLIS: Okay, so if I'm thinking about one bubble, I have a lot of difficulty figuring that out because I have to have one minus alpha or something in there to get the force on the one bubble.

DR. KELLY: Right.

MEMBER WALLIS: Okay, that's why it looks a little funny here.

DR. KELLY: Right. So yeah, this is a buoyancy force in a pipe. Well, what I want to do now is express that relative velocity in terms of the drift flux model. Well, the first thing you have to realize is that in a code like TRACE, the velocities that we talk about which we normally just say are velocities, are actually void weighted velocities. They're actually void and density weighted because alpha times rho times V equals the mass flux and it's really alpha rho V you know, at cross-sectioned averaged. So the little vertical lines means these are void weighted. That's what you get form a TRACE calculation if you look at the velocities.

CHAIRMAN BANNERJEE: You can take the densities to be uniform across the pipe.

DR. KELLY: Right.

MEMBER WALLIS: Why does density come into it at all?

CHAIRMAN BANNERJEE: It's the way the equations are set up. They separate the -- what happens is you should have products of averages equal to -- averages of products. It comes to that. This is fabric averaging basically.

DR. KELLY: Right.

MEMBER WALLIS: But that means that when you have a VG with a bar it's not the average velocity of the gas.

DR. KELLY: Right.

MEMBER WALLIS: This is getting very confusing.

CHAIRMAN BANNERJEE: It's void.

MEMBER WALLIS: Yeah, but it gets something which has sort of strange poles when you go to extremes and things that don't make sense, right?

CHAIRMAN BANNERJEE: I haven't looked at that.

DR. KELLY: So then what you do is you go to -- you can show from a drift flux model --

MEMBER WALLIS: This -- now, I understand what you mean by real and hypothetical scenarios.

DR. KELLY: Well, this is the area

averaged velocities that people talk about in the drift flux model.

MEMBER WALLIS: You know, there are some bogus theories about this now based on false averages that you've got to be careful about.

DR. KELLY: I'm trying.

MEMBER WALLIS: And I think you're doing a good job but you've got to be careful about these different sorts of averages and things.

CHAIRMAN BANNERJEE: This ground has been covered by a number of people including the KATAR people.

DR. KELLY: Right.

MEMBER WALLIS: You're relearning it all, though.

CHAIRMAN BANNERJEE: It's not relearning but it's --

DR. KELLY: Fortunately, I spent two years working there and I learned a few things while I was there. So when you plug this in, you get the interfacial drag coefficient as a function of void fractions and the drift flux velocity and then the second term over here, which is the ratio of the cross-sectional average velocity to the relative velocity in the code. That second term in TRACE we define it as a profile slip factor so going here --

MEMBER WALLIS: What on earth does that mean?

DR. KELLY: It means, if you ignore this, you see we have something that's a function of a drift flux velocity. The second term which we've -- you know, it's TRACese, if you will, is going to bring in the distribution coefficient or distribution parameter, a C sub zero and that's how this is done.

This comes out of a paper by Ishii and Mushima that relates the two and you do the algebra, this is what it ends up being as a function of c sub zero alpha and the individual phase velocity as the TRACE computes.

CHAIRMAN BANNERJEE: Let me ask you a question, here. This approach is quite similar to what is done in other codes. How does your final product that comes out of this compare with say KATAR and things like that?

DR. KELLY: It should be very -- well, this should --

CHAIRMAN BANNERJEE: Is it identical?

DR. KELLY: It's not identical to KATAR because they don't do the C sub-zero thing exactly. They correlate -- they don't correlate C sub-zero.

They have a different distribution. They have their own distribution parameter that they use. In earlier versions of KATAR, they did it exactly this way.

CHAIRMAN BANNERJEE: Why do they use a different distribution coefficient?

DR. KELLY: I don't know. It's not one --

CHAIRMAN BANNERJEE: Do they have a different data base or what?

DR. KELLY: Well, they --they don't use a distribution coefficient in the Zuber-Findley definition, okay. They use a -- let me see, let me go to --

CHAIRMAN BANNERJEE: They use it --

DR. KELLY: In this relative velocity that you multiply this coefficient by, there's like a c sub-one multiplying the liquid velocity, it's something like that. They correlate that C sub-one. So it's not direct -- you can't take the KATAR correlation, the most recent one, and turn it, and go back and do a hand calculation like you can with the drift flux philosophy.

CHAIRMAN BANNERJEE: Let me ask a more general question. KATAR has been through this exercise in enormous detail and with all their verification, validation stuff like that. And they have a list of closure relationships and so on. How -- you're coming up with a set with an independent assessment. How does your set and theirs compare at the end of the day? And if there are differences, why are there differences, different data sets?

DR. KELLY: And --

CHAIRMAN BANNERJEE: They're smart guys and they've been looking at it a long time.

DR. KELLY: Yeah, I considered using the drift flux correlation from KATAR in this but there were some questions as to whether we could or not, you know, whether it was proprietary information because, remember KATAR is co-funded.

CHAIRMAN BANNERJEE: It's all published.

DR. KELLY: Not the details. One of the models that I am going to use is the Bestion model for rod bundles and that was from an earlier version of KATAR that was published in Nuclear Engineering and Design. But the models in the more recent KATAR version are not published in the open literature or they may give you part of the model. You know, like they would say -- they might show you the drift flux and they's show the C sub-one in it, but then not give you the correlation for C sub-one.

CHAIRMAN BANNERJEE: So for example, have you seen the lecture that Bestion gave at the NRC here?

DR. KELLY: No, did not.

CHAIRMAN BANNERJEE: I have the text if you want and all the tables and stuff. It seemed that he had every correlation --

DR. KELLY: He might, I don't know.

CHAIRMAN BANNERJEE: -- that was used in KATAR there and all the methodology.

DR. KELLY: But at the time that I was doing this, I had to go into the KATAR code to pull out the details of the model.

MEMBER WALLIS: There's someone you should have on your peer review.

DR. KELLY: That would be a very good idea, if Dominique is available.

MEMBER WALLIS: And how about this idea which we've floated several times including in our research report that we need more international cooperation on these things, both in the code development and in the test facilities because they're expensive. Is that something which we can make happen or are you going to be working sort of independently the way you appear to be doing now?

DR. KELLY: Well, in test facilities, we

certainly are trying to do it. And for example, we participate in the ROSA program and that's a very important one because they do things like the cooling -- the cool-down transients in the steam generators. So that would be very important when we start working on the --

MEMBER WALLIS: Well, I get the impression that you plus Bestion might be a very good combination rather than both of you working independently

CHAIRMAN BANNERJEE: Team, yeah.

DR. KELLY: All right, I have worked with Dominique before and I really did enjoy it.

CHAIRMAN BANNERJEE: Especially on the constitutive equation side and then on the numerical side they've got some really top people as well. I mean, they wrote it fully implicit to start with back in `79 or whenever they started this thing.

DR. KELLY: Yeah, and which worked well for the one decomponents but not for 3-D. Then they actually implemented sets for the 3-D vessel.

CHAIRMAN BANNERJEE: The 3-d, you know, frankly, I think the 3-D stuff or any 3-D stuff is going into extremely detailed questioning.

DR. KELLY: Multi-region.

CHAIRMAN BANNERJEE: But some of this stuff is okay, I think.

DR. KELLY: Yeah, I would welcome --

MEMBER ABEL-KHALIK: Can I ask a related question to the two questions that were just asked? You have said earlier that you have changed roughly 75 percent of the constitutive relations in the code. And have you just simply purged the code, purged these correlations out of the code?

DR. KELLY: You mean, as opposed to leaving them in with a switch?

MEMBER ABEL-KHALIK: No, as opposed to giving the user options to use different constitive relations. And the reason I'm asking is presumably some of the constitive relations that you have discarded or sort of judged to be inadequate may still be available in the vendor codes of record. Is that correct?

DR. KELLY: It's correct that some of those models may still be in a vendor code but --

MEMBER ABEL-KHALIK: Okay, so --

DR. KELLY: -- now, what I did -- what I did, depended upon the model. If, for example, the way the wall drag was done was so wrong that we took it out but we took it out in stages. We left it in for awhile with a switch, so we only applied the new wall drag model if it was flagged as a PCCS-2 and then gradually as we went through the assessment, we expanded that.

One of the reasons that we don't do what you're suggesting or what I think you're suggesting is something called the user effect. And if you go to the international standard problems, one of the things they did were blind calculations. So they would run a test, lock up the data, give the initial and boundary conditions to all the participants and have them run the various different codes. And what you found by looking at the code results was often that the differences between calculations with the same code were much larger than differences between the codes and the reason was attributed to something called the user effect. And sometimes that's from geometry modeling. You know, if some people just make a mistake or they choose to do something differently, but it's also sometimes from selecting different models and if you give the users a lot of flexibility, they'll use it.

MEMBER ABEL-KHALIK: But my question originated from a different perspective. Presumably you're developing code so that the NRC can have an independent capability of evaluating calculations that are done by the vendors. And let's say at the end of the day, you have a large difference between what the NRC codes predict and what the vendor predicts. Now, how do you go about the process of finding out who's right and who's wrong or where the differences come from?

DR. KELLY: Okay, you know, that's the job of NRR but we would assist them in. But I can tell you what I would do. And if you find a difference, then you -- you know for a particular transient, and you look at where that difference occurs and when it occurs, you know, is it a certain component at a certain time caused by a certain phenomena that's not being modeled well in one of the codes or the other.

Then you go to the assessment basis for both codes and you try to find the assessment that was done for that phenomena in that range of conditions and then it's that which proves which of the two models was superior or not, which one was right and which one you shouldn't believe.

Now, I can't speak to that because I don't work at NRR and I don't know that they've had to do that yet or if they've made a decision based on that.

MEMBER WALLIS: I'm not sure they know

how to do it because you had difficulty yourself doing it, figuring out why the codes were giving different results. And they'd have to have your kind of expertise and I don't think they have it. So it's going to be a mystery to them why the two codes give different answers.

DR. KELLY: Well, of course, yeah, but of course, we are available any time they come to us and ask. I mean, that's our job is to support NRR. And if, you know, they were to come and say, "We ran, you know, Code X on this new plant and we got this answer, yours is completely different, which should we believe", then we would spend some time with them to straighten it out.

MEMBER WALLIS: Well, then there's the specter of running TRACE on the old plants and finding out that something that was approved before now is in question.

DR. KELLY: Yeah, well, you know, for most of what was approved before was done very conservatively. So as our models get more accurate and their comparisons get more realistic, what you should discover is that there historically was a very large margin. That margin is steadily being eroded by power upgrades and you know, things being taken off line so they can be repaired. And that's one of the reasons that the codes need to have a much higher accuracy than they used to have.

MEMBER WALLIS: Let me go back to an earlier question; does RELAP have all these things that you've corrected here which were like the old TRACE before it was corrected?

DR. KELLY: It has some and that's why I knew where to look in TRACE.

MEMBER WALLIS: But should we assume that if RELAP ran these things you've indicated here, it would give the same kind of inaccurate answers.

DR. KELLY: Well, for example, for the film condensation, it had exactly the same. I say had, and at the time of the SBWR submittal, we were going to be using RELAP-5. And my job at that time was to look at PCCS condensation with RELAP-5 and sure enough, the same thing, the liquid films, you know, fall several meters a second which is completely unphysical. And it was because of alpha -- you know the one minus alpha rho V squared and the pressure drop. That one minus alpha when that alpha goes to one, doesn't work.

MEMBER WALLIS: You're getting me concerned here, because I've been on this committee longer than I thought I would be, and at times along the way we have approved vendor codes, which when you look at the -- if you could get ahold of it, the documentation, that is about as shaky as the stuff you presented here, and yet they presented most of the comparisons and stuff and they made a case for themselves and eventually we got persuaded but it may be that if we had the sort of knowledge you have now, we'd have been much more critical.

DR. KELLY: You know, for me, you have to look at the intended application and what's important for that application and that's why you do the PIRT based assessment and you have to do a good job of that.

MEMBER WALLIS: Well, there has to be someone with the sort of knowledge you have looking at the problem. You can't just have ACRS looking at what a vendor gives us and we can't just have NRR saying well, it looks okay to me compared with a few experiments.

CHAIRMAN BANNERJEE: But the vendor also gives it to NRR who presumably asks you for some discussion of detailed points.

DR. KELLY: Well, now a days, NRR actually runs the vendor codes and compares them sometimes to experiments that are not part of the vendor's assessment base as well as doing a cross-comparison to either RELAP-5 or TRACE.

MEMBER WALLIS: When they do a cross-comparison with RELAP-5, it's still got these old glitches in it. What's the value of this comparison if it has these errors of an order of magnitude that you showed with this falling film?

DR. KELLY: Well, but I corrected --

CHAIRMAN BANNERJEE: But that's very specific.

MEMBER WALLIS: It's a very specific thing, right.

DR. KELLY: Yeah, and that specific one I've corrected in RELAP-5.

MEMBER WALLIS: But you've corrected 75 percent, of a large, very large number of model.

DR. KELLY: Right, but there was a difference between TRAC-PF-1, Mod 2 and RELAP-5 and the difference was TRAC-PF-1 Mod 1 was modified extensively as a result of reviewer comments.

MEMBER WALLIS: It was tuned.

DR. KELLY: No, it was -- all the models were completely changed basically. And then the code was pretty much put on a shelf because large break

LOCA was not an -- you know, a focus at the time.

MR. CARUSO: It was not reassessed.

DR. KELLY: Right, exactly. And wasn't, you know, used by the international community very much either.

MEMBER WALLIS: So what screwed it all up was the reviewer comments?

DR. KELLY: Well, that's what the code developers would say but I don't believe that. What the problem for me is, is that the level of effort was not continuous. You know, it doesn't have to be a huge level of effort but it has to be continuous. You have to build a talented team, keep it together.

MEMBER WALLIS: But you have some people in management who believe that it's all over, that these codes were mature, nothing has really happened since the `70s or something. We don't need to do any more work on them.

MR. CARUSO: Closure.

MEMBER WALLIS: How do you change that?

DR. KELLY: Show them the ACRS comments from 1993 when you guys reviewed applying RELAP-5 to AP-600 for the first time.

MR. CARUSO: Closure is the word. That's what they like.

DR. KELLY: We're not dealing, very obviously, in terms of errors in the third significant digit.

CHAIRMAN BANNERJEE: Joe, we are going to need to finish in about 15 minutes. You've got --

DR. KELLY: I can actually --

CHAIRMAN BANNERJEE: -- a whole bunch of slides on interfacial drag, I see.

DR. KELLY: Right.

CHAIRMAN BANNERJEE: Why don't you pick out the ones that you think are the most important.

DR. KELLY: Right, I can do that. So, I won't go through the details of these models but they're pretty simple. Instead I'll show you some of the results. There is one kind of major point in here that threw me for awhile. If you're going to use a dispersed bubble flow regime and then transition to either a slug or a seal cap bubble, depending upon the tube diameter, basically, you have to have some transition criteria, and this plot shows void traction versus a non-dimensional gas superficial velocity and that is for air/water pool data. And you'll notice down here, this is very well fit by a churn turbulent model. This is pretty well fit by a cap bubble model but you have to get from here to there and back again.

And what I did was a very simple linear interpolation of a drift flux velocity between 20 and 30 percent void fraction.

MEMBER WALLIS: What's the X axis here?

DR. KELLY: The X axis is a non-dimensional gas velocity.

MEMBER WALLIS: How is it made non-dimensional, with surface tension?

DR. KELLY: Basically by a bubble rise velocity.

MEMBER WALLIS: It's a surface tension thing.

DR. KELLY: Yeah, it's this.

MEMBER WALLIS: Yeah, it's that.

DR. KELLY: The sigma G delta rho of a rho squared of a quarter power.

CHAIRMAN BANNERJEE: Single bubble rise velocity.

DR. KELLY: Right. It works great for this. I checked it on some other air/water data. It worked great, but again, I looked at more data and it doesn't work so great. If you look at steam/water date, now, this is some the old Wilson bubble rise data. Void fraction again, versus non-dimensional gas velocity. The black diamonds are the data. You'll notice it's very well fit by the cap bubble flow regime. It's not fit at all by the dispersed bubble regime and if you follow the red line, that's the transition that I proposed based on air/water data.

CHAIRMAN BANNERJEE: That's quite high pressure data, isn't it?

DR. KELLY: Yes, this is four megapascals and it's a 10 centimeter pipe. Okay, so this is fit by cap bubble even down at void fractions, you know, below 20 percent. When you think --

CHAIRMAN BANNERJEE: How much was the pressure in the previous data that you showed?

DR. KELLY: This is atmospheric.

CHAIRMAN BANNERJEE: The ratio is quite different.

DR. KELLY: Yeah, but I looked at other air/water data and the 20 to 30 percent void kind of fit, because, you know, once you get up above 20, 25 the bubble densities are so high, the bubbles coalesce and you go to cap bubbles. For steam, these are adiabatic tests but it's still -- you seem to be getting things that are cap bubbles at void fractions where you don't expect it.

And this is steam water pool data over a range from atmospheric up to, I think that's four megapascals and you see the same kind of thing. I showed this -- well, I would have showed this earlier. Now we fit in the cap bubble regime very well using the Kataoka-Ishmii model. This portibation comes about when we're transitioning to the dispersed bubble which you think you ought to do at wall void fractions.

MEMBER WALLIS: Is this porated water?

DR. KELLY: In these cases, probably not.

MEMBER WALLIS: Well, I'm just saying if there's some sort of conglomeration of these bubbles and it depends on how they're made and what their original size is. It may depend on the water chemistry as well as the other things.

DR. KELLY: Right, well, I came up with two explanations for why they might work like cap bubbles. The first came out of the paper of Zuber and Findley, because they presented the distribution parameter and said it works great for all of this data but these other three tests it doesn't work so well.

MEMBER WALLIS: If you put a few drops of dishwashing detergent in there, you'll get a --

DR. KELLY: Yeah, but I think it's this because if you go into the lab and set up a boiling experiment, and I remember very clearly being in

Professor Dhir's lab looking at this sub-cool boiling experiment, and you just look at -- you know, you have water flowing up, your nucleation starting and you're in sub-cool boiling regime. You see these little bubbles. They're about .2 millimeters in diameter. They're almost perfectly spherical. There can be a lot of them. The void fraction can be 20, 30 percent okay, but there are these beautiful little bubbles. Then you get to the point, the elevation where you've reached Tsat in the liquid and it's like an explosion occurs.

MEMBER WALLIS: At low pressure, yeah.

DR. KELLY: Yeah, but all of a sudden the bubble radius goes from these .2 up to about four millimeters, just bang, over a very short axial distance.

MEMBER WALLIS: Also they get caught in each other's wakes which helps to increase their speed.

DR. KELLY: But, you know, all of that happens in air/water data. What I think is happening here is flashing. The bubbles are growing because they're basically at that point in a super-heated liquid environment. And you have that interfacial area, that's where the vapor generation is, is at the bubble surface. And the bubbles rapidly get large and act more like cap bubbles rather than spherical bubbles.

Well, at any rate, I had these, you know this model worked great for air/water data. Didn't work for steam/water data. So what I did for sub-cool two-phase flow --

MEMBER WALLIS: You haven't got any Russian stuff in here? There's all kinds of Russian -- Ludwin Soff (phonetic) and all sorts of people did work on this kind of stuff. I remember Criara (phonetic), we had to go to them to get correlations that worked for some of these the flashing bubbles. I don't remember all the names but there was --

DR. KELLY: Some of the void fraction data that I have in pools, I think is Russian, but this is Wilson, which was Allis-Chalmers, I think. I mean, we're going back aways and I'm not sure where this was from, but by using different transitions, I got much better --

MEMBER WALLIS: So what parts of the reactor circuit does this apply to?

DR. KELLY: Well, this is for pipes, not for rod bundles.

MEMBER WALLIS: And what parts of the

reactor circuit does it apply to?

DR. KELLY: Anything vertical.

MEMBER WALLIS: Are there many vertical pi-pipes in reactors that are of any length?

CHAIRMAN BANNERJEE: SBWR.

DR. KELLY: Not too many but we actually use this for horizontal as well.

MEMBER WALLIS: See, that's another concern with all of these codes, is you can perfect these correlations for a well-defined situation but then in real geometry of a real reactor system, there's all kinds of stuff and the shapes are not straight vertical pipes.

DR. KELLY: That's true.

MEMBER WALLIS: So it's again a great leap to take these correlations and methods and apply them to any part of reactor circuit.

DR. KELLY: Well, you asked about the SBWR chimney --

MEMBER WALLIS: Yeah, but that's because we have a concern about that, yes, but you see that's another question I have about all these codes is yes, you can twiddle them as much as you like but then you're going to apply them to everything.

CHAIRMAN BANNERJEE: But those are also in very large pipes and --

DR. KELLY: That's the point here. See, this is a half meter diameter pipe and it's hard to find the --

CHAIRMAN BANNERJEE: Is that the Ontario hydro data?

DR. KELLY: Not this, this is Wilson bubble rise. It's steam water pool, so the water is not moving here. You're bubbling --

CHAIRMAN BANNERJEE: Now that you've developed this correlation, it would be a good cold test. You've got the Ontario hydro data now.

DR. KELLY: We've done that. You're talking about the one case where they reduced the inventory in the loop and keep pumping the flow around and have void fraction as function of time.

CHAIRMAN BANNERJEE: Well, they have pressure drop void fraction and for different pressures. I'd have to look at the report again but it's quite a lot of data.

DR. KELLY: That may be different than what we have. What we have as part of the TRAC-B assessment, they presented an assessment against some Ontario hydro data. It was a void fraction in large diameter pipe, but it was presented as void fraction versus time and there was really one part and there's about five different plateaus on it where they operated at five different inventory levels. And we do have that as part of our assessment.

CHAIRMAN BANNERJEE: How did that agree with the --

DR. KELLY: It agreed --

CHAIRMAN BANNERJEE: Was it a large pipe?

DR. KELLY: Yeah, it agrees -- oh, darn it.

CHAIRMAN BANNERJEE: Because that's what GE is using for the -- for the --

DR. KELLY: I pressed wrong. It agreed very well in here. Okay, up to void fractions on the order of 60 some percent.

MEMBER WALLIS: Now, this is your new version you're showing us here.

DR. KELLY: Right.

CHAIRMAN BANNERJEE: And then up to the 70, 75?

DR. KELLY: Up in here rounded 80 percent, the data would have been 70 and we predict 80. And that doesn't sound that bad but --

MEMBER WALLIS: Well, doesn't C_o say you can't get above a fraction with one over C_o or something, you can't get above a void fraction with a C_o the way you have it in there. You can't get above a void fraction of one over C_o, I think. So if C_o is 1.2, there's no way you're going to predict above .85.

DR. KELLY: Well, let's see --

CHAIRMAN BANNERJEE: But C_o is of course, not going to be 1.2 in churn annular.

MEMBER WALLIS: Well, I don't know what he's got for -- I don't know what he's got for --

DR. KELLY: Yeah, this is how we transition in. The bubble slug model is going to go to zero pretty rapidly in here. And if you remember that profile slip factor, we don't let it get lower than .05. That's the reason for this inflection. For these conditions, that meets that annular flow model and --

MEMBER WALLIS: That annular flow doesn't have any C_o in it, does it?

DR. KELLY: Right, no.

MEMBER WALLIS: It's a different thing all together, different basis all together.

DR. KELLY: Right. They cross over at a void fraction of about 9 percent for those conditions and what we do is a power-law weighting of the two and we get this curve in between. So we'll be doing this and transitioning to annular in here.

MEMBER WALLIS: Now, what you're doing here is very interesting. It reminds me of a large number of Ph.D theses that were done at one time comparing different models and things like that and somehow or other, yours is going to be better than all these other ones.

DR. KELLY: Well, in this case, I'm using literature based models. I'm not developing a new model. I'm just putting literature models into the code.

MEMBER WALLIS: A few selected ones which the most sort of robust or something.

DR. KELLY: Right, right, I try a lot of them. You know, I built this into a spreadsheet where I could quickly put a model in and try it out against a lot of data and also try out different transition schemes and that's what I came up with for rod bundles.

CHAIRMAN BANNERJEE: This is why I was saying, it would be worth doing a comparison with another comprehensive set if you could access it and see what the differences are, if any.

DR. KELLY: Yeah.

CHAIRMAN BANNERJEE: I mean, maybe in

effect you are the same.

DR. KELLY: Yeah, well, this worried me when I did this comparison because there was a consistent trend.

MEMBER WALLIS: You know, for 40 years I've seen things like this and you find that you get some Russian paper and he's got a wonderful correlation of everything and everything works fine. And then you see someone else's paper and it seems to look with the same data and everything else and it correlates on some other scheme all together. This has been going on forever. And so I'm just wondering how is it that yours is going to somehow be so robust that it isn't going to be subject to the same problem, that when you apply it to something else, it doesn't work and all that stuff.

CHAIRMAN BANNERJEE: With some new data or some different situation, clearly it may or may not work.

MEMBER WALLIS: So the stuff you want to apply it to is what's most appropriate to the application you have in mind.

DR. KELLY: Yeah. Now, what I want to say is about this section because this bothered me. And when we ran the Ontario hydro test, the one test that we got the data from GE from, we compared very well except for one data point which was up in this region and okay, 70 percent versus 80 percent doesn't sound like that big an error.

MEMBER WALLIS: The bubbles are going slower than you predict, is that right?

DR. KELLY: Let's see, we're predicting too much water, so we're saying the water is going too slow, so the liquid fraction has to be too high. And what that --

MEMBER WALLIS: The void fraction is too high so the bubbles are hanging around too long and they move too slowly.

DR. KELLY: Well, our void fraction -- this is the calculated. The calculated is under-predicting.

MEMBER WALLIS: The real one is higher.

DR. KELLY: Right, so what that means is that --

MEMBER WALLIS: The bubbles are going slower then?

DR. KELLY: In reality. We have too much slip, if you will. The reason this is of concern to me you know, 70 to 80 percent for a lot of things, that's close enough but what we're really talking about is 20 versus 30 percent.

MEMBER WALLIS: In terms of liquid fraction, that's huge.

DR. KELLY: It's a 50 percent error. And if this is what -- and the void fraction versus elevation is pretty much constant in the chimney.

MEMBER WALLIS: But here the error is -- there's more than a factor of --

DR. KELLY: You know, axially.

MEMBER WALLIS: -- two in liquid fractions.

DR. KELLY: Oh, great.

MEMBER WALLIS: Are you silenced now?

DR. KELLY: I dropped the battery out. That's what you get when you're up here, but so what I did you know, we have the Thermal-Hydraulic Institute. Actually, we're kind of between contracts now. We're finishing off a contract. So there is -- I have a task order in place at Purdue University to first go through the data, you know, try to establish a data base of large pipe diameter data that we can compare the interfacial drag models to. And it turns out they had just built a facility, I don't remember what it was for, but someone else paid for it and we ended up with very large diameter pipe, close to a foot in diameter and a very large pump and they can get their air supply from the wind tunnel. So we can go up into this regime in a pretty large pipe.

CHAIRMAN BANNERJEE: How long is the pipe?

DR. KELLY: I think it's about 30 feet long. It's however long the airport building out at Purdue University is.

MEMBER WALLIS: Well, oil wells have a -- and gas wells have big pipes that are very long, all kinds of data there, all kinds of void fractions. It's not so easy to get hold of though is the problem.

DR. KELLY: Yeah, so, you know, their first task, which they're working on now, is to try to find whatever data they can in large diameter pipes and the reason is, to look at these conditions for the --

MEMBER WALLIS: The other thing to do is to run a full scale test and take the answers you get.

CHAIRMAN BANNERJEE: There's not going to be steam water high pressure.

DR. KELLY: No.

CHAIRMAN BANNERJEE: That was the beauty of the Ontario hydro data and you should -- my suggestion would be if those data are good, because they had beautiful gamma densitometers, and they had two gamma densitometers, I think, and it was very well instrumented.

DR. KELLY: Well, I'll have to see -- you know, all I have are the literature papers and the very short section from the TRAC-G assessment.

CHAIRMAN BANNERJEE: Right, and they had also the fluctuations in the void and the --

DR. KELLY: Well, remember they're running this through a pump. They're running a two-phase mixture through a pump.

CHAIRMAN BANNERJEE: Right, right.

DR. KELLY: And they have to infer the inlet conditions to that pipe by assuming HEM flow coming out the pump.

CHAIRMAN BANNERJEE: Well, whatever it is they have the data, yeah.

MEMBER WALLIS: But it develops in a short distance they claim.

DR. KELLY: Uh-huh, so to go ahead and finish this up, I talked about oscillations earlier, with the power wall weighting. This is the answer.

MEMBER WALLIS: Is that the right answer?

DR. KELLY: Yeah, you'll see in just a second. What I did for rod bundles and this is where I took the Bestion model, notice how very simple it is. It looks like a slug flow model with the exception that it has the gas density in the denominator, not the liquid density.

MEMBER WALLIS: Whoa, wait a minute. What is this?

DR. KELLY: This is the model that Dominique Bestion developed --

MEMBER WALLIS: Describing a bubble.

DR. KELLY: -- for --

MEMBER WALLIS: That's got no density at all.

DR. KELLY: For rod bundles, it works.

MEMBER WALLIS: That's baloney, baloney. I mean, if rho G goes to zero, this goes to infinity. It makes absolutely no sense at all.

DR. KELLY: Well, but rho G doesn't go to zero, it goes to --

MEMBER WALLIS: The bubble, the density of the bubble doesn't matter, you can fill out the rest of the sentence.

DR. KELLY: No, I mean, I know what you're saying is that it should be --

MEMBER WALLIS: There's a big different.

DR. KELLY: You think it should be a liquid density.

MEMBER WALLIS: Unless you've got a density ratio of 1,000 or something, it doesn't matter whether it's a million. Density cannot effect bubbly flow at all.

DR. KELLY: I don't know why this works. There are questions --

MEMBER WALLIS: Probably because the whole model is wrong and you should be using rho G v G squared and using some other kind of a model based on the gas drag on something. It's the only way this could make sense if you're holding up droplets with a big gas or something.

DR. KELLY: Yeah, that's what it --

MEMBER WALLIS: Are you sure this isn't a typo? This isn't a typo somewhere?

DR. KELLY: Yeah.

CHAIRMAN BANNERJEE: Show us what happens.

DR. KELLY: Okay. The other -- this is the drift philosophy. The other part is the distribution parameter.

MEMBER WALLIS: This must be a steam water at high pressure.

DR. KELLY: Most of the data was at low pressure but I'm going to show you both.

MEMBER WALLIS: If I go to something like a distillation plant from --

CHAIRMAN BANNERJEE: Wait, wait, let him show the data.

MEMBER WALLIS: -- with very low gas density, I'm going to get an enormous velocity.

DR. KELLY: This is on the order of meters a second.

MEMBER WALLIS: Yeah, but if I make rho G very small.

DR. KELLY: I'm talking about one atmospheric --

MEMBER WALLIS: Like we'll do fractions of an atmosphere because of distillation of the sea wall or desalination or something, you're going to get --

DR. KELLY: Well, this is an empirical model, so this is not -- this is empirical, not you know --

MEMBER WALLIS: But no one is going to believe it.

CHAIRMAN BANNERJEE: Well, let's see the data.

DR. KELLY: Okay, for the distribution parameter, it was initially developed with a constant factor of 1.2. I first put it into TRACE using the one suggested by Ishii which goes from 1.2 to one as you go towards the critical point. But then as we were doing data comparisons, there was a recommendation by Coddington and Masian it's a pretty recent paper, to use C sub zero equal one for rod bundles. When you do that, this is what you get.

This is at basically BWR conditions. Five megapascals, mass fluxes from 700 to 1500, heat fluxes typical of a boiling water reactor. This is calculated void fraction versus measured. This was TRACE calculations using C sub zero from the issue model and this is using C sub zero set to one.

MEMBER WALLIS: That's using his bogus velocity?

CHAIRMAN BANNERJEE: What about low pressure?

DR. KELLY: Yeah. Getting there. And you can see that the average error and RMS error are greatly improved by going to C sub zero equals one. That looks pretty good.

MEMBER WALLIS: This goes for void fraction of zero, the gas density matters are the void fraction of zero when there's no gas there at all? That makes absolutely no sense.

DR. KELLY: I don't know why that works

but it works fairly well.

CHAIRMAN BANNERJEE: Next slide.

DR. KELLY: This is at low pressure boil-off conditions. This is two different flex C set cases. One is at 20 psi, the other is at 40 psi and you have void fraction versus elevation. Obviously, this is where boiling starts. These are decay heat levels, yo know, like five percent kind of power so there's really no sub-cool boiling. And so the data taken with DP cells are the red diamonds and when you see this kind of behavior, this is where a grid spacer is. That's why these two values, for example, on the data are low relative to the other values of the data.

The dash blue line is the Bestion model with you know, C sub zero basically equal to 1.2 and the black line is when C sub zero equals to one, and it fits the data quite well.

MEMBER WALLIS: Did Bestion apply to anything else?

DR. KELLY: It's only applied to rod bundles. We put in a -- the old TRAC model used the same correlations for tubes and rod bundles. We knew rod bundles behaved differently so we put in, you know, this model for rod bundles. And then this is high pressure boil-off at THTF, four and a half megapascals but you know, the lift was almost stagnant. And again, the red diamonds are the data and they well-fit with C sub zero equaled to one.

CHAIRMAN BANNERJEE: Was this model published in his Nuclear and Design Paper?

DR. KELLY: Yes.

CHAIRMAN BANNERJEE: Strange it go through. I wonder who reviewed it.

DR. KELLY: And that takes us to horizontal stratified flow and here I'm going to show results only. What we did --

MEMBER WALLIS: If you put a bubble in an air water tank it goes with a velocity of five meters a second as I calculate from this?

CHAIRMAN BANNERJEE: It comes under the square root.

MEMBER WALLIS: There's no way. I mean, it goes -- I took the square root of a thousand and I got 30. Five meters a second, I mean, we know that it goes to the velocity, which is you know, there's all kinds of plots of bubble velocities.

DR. KELLY: Yeah, around 20 centimeters.

MEMBER WALLIS: Twenty centimeters a second. So there's something really -- very peculiar going on here. It is a good note to quit so we can get over this.

CHAIRMAN BANNERJEE: No, no, I think we should finish this.

DR. KELLY: I'm actually almost finished.

MEMBER WALLIS: Okay.

DR. KELLY: In TRACE --

MEMBER WALLIS: Are you going to tell us anything more fantastic?

CHAIRMAN BANNERJEE: It's not his fault.

DR. KELLY: And you know, when you look at horizontal flow, these are basically the type flow regimes people talk about. Again, I'm idealizing them with cartoons. In TRACE we really only model stratified smooth and then what I call a normal flow regime. And then similar to KATAR or RELAP 5, we interpolate between the normal regimes and a fully stratified and that interpolation factor becomes if you will, the interfacial drag for the regimes in between. The reason I looked at this was poor accuracy for high pressure conditions, large oscillations and a loft in CCTF in the lags and problems with ESBWR horizontal vent clearing.

Looking at co-current stratified flow, the TPTF facility, this is well, JAEA and Japan used to be JARED (phonetic). It's an eight-inch diameter pipe and what you have is calculated versus measured void fraction. This is at four different pressure levels from three megapascals to 8.6 and this is TRACE before I changed the model. You'll notice it consistently over-predicts.

After we changed the model, it does a pretty good job. For counter-current flow in a PWR hot leg, we looked at the looked at the UPTF data. This is a full scale facility, so this hot leg is full scale, 36 inches or something in diameter. Here's a simulator for the steam generator. You have the reactor vessel over here, providing a steam source. You establish a steam flow. Then you turn on the spray to model a fall-back from the steam generator. So you have liquid flowing counter-current.

There's one thing that's not prototypical for USPWR and that's the hutze, which is the hot -- they have hot leg ECCS in Germany and this is where this --

MEMBER WALLIS: The hutze, I thought it was a huftze with an F.

DR. KELLY: It's H.

MEMBER WALLIS: It's an H? I thought an F instead of a T. I never -- maybe I got it wrong. It's hutze.

DR. KELLY: So this does effect the CCFL, if you will, that occurs here but that was the best data the we had to look at.

MEMBER WALLIS: So you're saying that the counter-flow flooding is a natural consequence of the drag model, it's not something which is an instability?

DR. KELLY: Well, what I'm doing here is the transition which is basically a Titel (phonetic) Ductworth type criteria which does have to do with an instability but I also built in a CCFL model into the transition.

MEMBER WALLIS: Oh, so you also built it in.

CHAIRMAN BANNERJEE: You don't put it through Tao I (phonetic) and VGJ or anything like that.

DR. KELLY: Not directly. It's the -- you've determined a fraction of a stratification factor if you will and you use that factor to do actually a log ramp between interfacial drag for stratified and non-stratified flow. This, again, was just an improvement to the model that was already there.

CHAIRMAN BANNERJEE: Yeah, I should tell

you something which has been recently found. It's just been published in the International Journal of Multi-Phase Flow for Stratified Flows. Rad Isha at Imperial, I think wrote a paper where they took just the usual stratified flow equations, yours must be some version of it, with whatever is normal interfacial drag. And they did a very find nodalization so they were looking for oil/gas pipelines. And they were able to develop slug flow naturally.

DR. KELLY: By having instabilities propagated.

CHAIRMAN BANNERJEE: Automatically and the slugs died automatically. So if they got it to fine enough nodalization, they modeled a 30-kilometer pipeline and they had many, many measurement locations and Jeff Shubert (phonetic) is doing it right now. They were able to predict the slug -- the severe slug distribution, everything just from the equations. They didn't have to put in any flow regime transitions, nothing. Sort of interesting.

MEMBER WALLIS: Well, it gave the non-uniform void fraction in the pipe then.

CHAIRMAN BANNERJEE: Yeah. Well, when there's slug, here's slug.

MEMBER WALLIS: But these guys tried to predict a void fraction which is the same all along the pipe going to -- these codes.

CHAIRMAN BANNERJEE: They just gave that inlet conditions.

DR. KELLY: Yeah, and let it develop as a function of time and space. Yeah, of course, we're using nodes that are on the orders of meters long.

CHAIRMAN BANNERJEE: Well, this is one of the things that Jeff was telling me. He said the current state of the art is that they can use for a 30 kilometer pipeline nodes which are like this. How is it that they're able to do it and we can't do it?

DR. KELLY: They're modeling one pipe and not a reactor system.

CHAIRMAN BANNERJEE: A pipe, I mean, it doesn't seem that different. They have terrain changes. They have all sorts of things coming in and out and --

MEMBER WALLIS: But the fine nodalization is somewhat different isn't it? Still, the length of the node to its diameter is reasonable. It's not a tiny little slice.

CHAIRMAN BANNERJEE: It's of the order of the pipe diameter.

MEMBER WALLIS: Yeah, but it's not a slice, yeah.

DR. KELLY: Yeah, you know, I'm not saying we shouldn't look at that for the next generation. But remember what I'm trying to do here is fix something that's broken as quickly as possible so the assessment can continue.

MEMBER WALLIS: So your next slide shows that works, right?

DR. KELLY: Yeah.

MEMBER WALLIS: You were able to make it work.

DR. KELLY: Yeah, so this is a flooding plot, J sub G non-dimensional square root, J sub L. This is actually the Richter model is the black line, which is the sum of those is equal to 0.7 if I remember right. The red --

MEMBER WALLIS: It's empirical, right.

DR. KELLY: Yes, the red squares and triangles are the data. You'll notice they follow this line quite well until this point and this is the no flooding point where all the liquid just comes down from the steam generator into the vessel. This is the results in blue of TRACE with the previous models. At 15 megapascals, at least there's some flooding, okay, but at the three megapascals almost all the points allow no water coming back into the vessel at all. It was pretty bad. When I changed the model, I then get a pretty good comparison, not perfect but certainly much improved.

MEMBER WALLIS: Well, it's about as perfect as you can get.

DR. KELLY: Well, if you -- you know, each one of -- like this point corresponds to this point in the data because the way the tests are run are by establishing a steam flow, then turning the water on and seeing how much water collects in the vessel.

MEMBER WALLIS: This is from a smooth interfacial drag model? No.

DR. KELLY: You use a smooth interfacial drag but what controls it is the interpolation to the normal bubbly type flow regimes.

MEMBER WALLIS: Oh, that's what controls it.

DR. KELLY: Right.

MEMBER WALLIS: Well, that's not what happens in reality.

DR. KELLY: No, but that's what I had to work with.

MEMBER WALLIS: But you can make your

predict, but I think your physical model doesn't describe why it floods in reality.

DR. KELLY: Yeah, I built the CCFM model into the transition criteria.

MEMBER WALLIS: I mean, I can make an annular model predict bubbly flow by fiddling the coefficients. It's not necessarily a very good model that way.

DR. KELLY: I tried to look for wavy stratified models and in particular I used one from Hanratty and when I put that in, I couldn't get the counter-current flow at all. You know, it just always held the water up. So that's what we did, at least what I can show you today in the time I had in order to do TRACE 5.0. But looking beyond 5.0, we know where some of the models aren't terribly good and where we should spend some time and in some cases we already had done experimental programs and we just haven't had time to generate a model or select a model using that data to go into the next code version.

So one thing that we've already started doing is putting a component model in TRACE for the ESBWR suppression pool to try to account at least in the first order for the thermal mixing -- for the thermal stratification and mixing in the suppression pool. So it will have a bubble plume model and a turbulent buoyant plume model in it.

MEMBER WALLIS: That's a tough one actually.

DR. KELLY: That's a very tough one, but anything is better than complete mixing. We did have an experiment as part of the Thermal Hydraulic Institute with one-tenth sector model and an extensive thermal couple cage in it. So we have some data to use on that.

MEMBER WALLIS: Don't they say they get 100 percent condensation or something in this?

DR. KELLY: A hundred percent condensation, yes, but that --

MEMBER WALLIS: Is that realistic?

DR. KELLY: Well, 100 percent up to whatever the partial pressure is above the interface. That's -- I think that's realistic. The question though is, what is the temperature of the water at the interface. And that has to do with how much of the tank participates in the mixing process.

And we ran two sets of experiments at Purdue. One we good the PUMA facility and put a thermal couple cage into their suppression pool, and just quickly ran some tests in that. And so if we ran those tests with pure steam coming in, then it would depend upon whichever vent is cleared, but say we're just talking about the top vent, basically all of the water above the top vent mixed. It was pretty much complete mixing. Now, we're not the same lateral scale as the ESBWR but that's what we saw.

If you added just half a percent, you know, mass fraction of air to that, what you get is a bubble plume because the bubbles don't -- I mean, the air doesn't condense. The bubble plume is very effective at mixing it. With that very small air fraction, it mixed the entire suppression pool in the PUMA facility. When we did the 1/10th sector, we saw exactly the same thing. And so which in retrospect makes sense because you use bubble plumes to break up thermal stratification in reservoirs.

So there will be a very simple model, it will be better than complete mixing. The next one is interfacial drag in rod bundles. We have two new data sources. One is the RBHT. We ran a series of interfacial drag tests in that, just, you know, basically boiling water and measuring the drag, the void traction, actually measuring delta Ps and inferring void fraction from that.

But over and above that, there's a new

data source that was made available to us as part of an international standard problem and it's boiling water reactor -- actual real boiling water reactor bundles.

MEMBER WALLIS: This RBHT is the Penn State?

DR. KELLY: Yes.

MEMBER WALLIS: That's all low pressure?

DR. KELLY: Yes.

MEMBER WALLIS: And they get their -- they assume it's all a constant uniform pressure or -- that's --

DR. KELLY: I don't know. I haven't looked at how they -- when I do it, I'll start with the raw data and make sure, you know --

MEMBER WALLIS: Do you know whether they measured the pressure of they system?

DR. KELLY: Yes.

MEMBER WALLIS: Things like that, which we raised when we read their report? Okay.

DR. KELLY: You know, and I will check my answers to their answers as to when I infer what the void fraction is. But this is actual BWR conditions. Heat fluxes, pressures and mass fluxes, it's the best data set we've ever had.

CHAIRMAN BANNERJEE: Where was that taken?

DR. KELLY: It was taken in Japan, I believe it's funded by NUPEK (phonetic). It's proprietary data, so we can't release it but we can use it.

CHAIRMAN BANNERJEE: It's in reactor data?

DR. KELLY: Actually no. They took a rod bundle and they put it inside a CAT scan machine, so it's an electrically heated rod bundle, full eight by eight and they put it inside a CAT scan machine to get sub-channel distributions, which we're not worried about, but they also had three other gamma densitometers so you have area average values for the void fraction at four different axial elevations. It's the best, most productive data we've ever had, so we want to use it. The new re-flood model, again, using the RBHT data and the post-CHF data from UCLA, the --

CHAIRMAN BANNERJEE: What is the post-CHF? We haven't heard about that.

DR. KELLY: It's the first program was just on a flat plate, but we have a little three by three rod bundle with a refrigerant and what we're going to try to do, I'm not sure we're going to be successful, but what I want to do is not do reflood test but do steady state, low quality film boiling test and if you remember back 20 some years ago, Renaveld and others developed a hot patch technique. Well, you can do that on a tube, but as we've looked at in one of the previous presentations, film boiling in a rod bundle in a tube is very different. And so what we want to do is in effect, put a little hot patch at the beginning of these rods and that's one of the reasons for using a refrigerant, by having a very short high heat flux region, at the beginning of a rod, you can trip it into post-CHF and stay at film boiling conditions down stream of that even though you're at low powers.

CHAIRMAN BANNERJEE: But with the refrigerant, is this V.J. Dhir's program?

DR. KELLY: Yes, it's one we're funding through the Thermal Hydraulic Institute. Now, it hasn't been funded for awhile, so it just started back up and I don't have results yet. I don't know if it's going to work but I wanted to try it because that would give a steady state film boiling data in a rod bundle and that greatly enhances your ability to generate a constitutive model from it.

The next one is looking at grid spacer effects, both how it effects convective heat transfer and what it does to drop diameters model like what's in COBRA TF but using our RVHT reflood and drop injection test data. Sub-cool boiling, we ran an experiment at low pressure at UCLA and we haven't had a chance to go back and use that data yet and finally critical flow in an orifice and --

CHAIRMAN BANNERJEE: Going back to the sub-cool boiling, we had a presentation a few years back from UCLA and at that point we raised the question as to how this very detailed modeling and experiments was going to find its way into TRACE. And you guys, I seem to remember, said that you were looking into this and that was two or three years ago. So where are we on this now, same point?

DR. KELLY: Now, we're in almost the same point and that's because I have been doing this other stuff. I haven't had a chance to get through that.

CHAIRMAN BANNERJEE: Is this just a lack of manpower or is it --

DR. KELLY: It has --

CHAIRMAN BANNERJEE: What's the real problem here?

DR. KELLY: We've had two problems. One is lack of co-developers if you will, people that know how to program, what two-phase, what two-fluid models are and what, you know, something about two-phase flow. So we've had a lack of people to do code development. We've also, and this is just as serious a problem. We've had a lack of analysts and those are both capabilities that we're trying to develop.

CHAIRMAN BANNERJEE: What's needed here is to take the data and the models and translate it into a form which properly can go into your quote "structure" if I remembered that.

DR. KELLY: Right, uh-huh.

CHAIRMAN BANNERJEE: That's true of a lot of these things, with these experiments that are being done. You can analyze these experiments and then you have to cast it in a way that it will go into the code, into the structure of the code through your constitutive equations or whatever.

DR. KELLY: Right, exactly.

CHAIRMAN BANNERJEE: And that step, I thought somebody was going to take. If you can't take it here, that should be part of the task of the researchers because I see that they're doing beautiful work on single bubbles or 10 bubbles or having lovely correlations which are detailed based on these. But eventually it has to get into the code.

DR. KELLY: Right.

CHAIRMAN BANNERJEE: And maybe their job should be to get it into that form.

DR. KELLY: The problem has been that most of the people that do good experiments and have good knowledge about two-phase fundamentals, don't understand the codes, and so if you just task them to do a model, more often than not, that model won't work in a two-fluid framework which means someone that does understand it, has to work with them very closely. They don't have to necessarily do it, but they have to supervise it and we haven't been able to do that.

CHAIRMAN BANNERJEE: That's part of your contract to these people is they should involve some people who know fluid monitor. It's not rocket science, I mean, ultimately, they can figure it out.

DR. KELLY: Yeah, what we're now -- well, once we get passed this 5.0 release, and I get the documentation done, I have two to three new staff that are going to be working with me and that's how they're going to learn. We're going to work our way through this. And things that we don't have time to do, we're going to do just what you said except someone like me will like be on the student's thesis committee to make sure --

MEMBER WALLIS: What good does it do to

have someone develop a model which is not compatible with the framework of TRACE --

DR. KELLY: It doesn't.

MEMBER WALLIS: -- in analyzing that data?

CHAIRMAN BANNERJEE: You write a paper in Journal of Heat Transfer and graduate.

MEMBER WALLIS: That's right, that's what they do.

DR. KELLY: Which is why we're not really going to be doing very much --

MEMBER WALLIS: And just doing experiments doesn't really help unless they focus on the parameters which are most important for TRACE in some way.

DR. KELLY: Well, and that's what --

MEMBER WALLIS: There has to be that connection.

DR. KELLY: The experiments that we started, were because of a need that arose when we did RELAP 5 for AP600 and has such terrible results on void fraction in the core, et cetera. And the intent was always for basically me or someone like me you know, to work with the experiments and get the data into the code in a timely fashion. We never imagined we'd have as much problem with extent models in TRACE as we did.

MEMBER WALLIS: If you look at the RBHT, they didn't measure interfacial drag at all. All they did was measure pressure drop.

DR. KELLY: Right.

CHAIRMAN BANNERJEE: Could they measure void fraction?

MEMBER WALLIS: No, they didn't measure void fraction. They inferred from the pressure drop some assumptions about how all these different components work together and that was a long way from getting a good measure.

CHAIRMAN BANNERJEE: Did they have some device, I remember way back, some sort of shadow graph device where they put --

DR. KELLY: That's for droplets.

CHAIRMAN BANNERJEE: Droplets, the dispersed flow regime, right?

DR. KELLY: Yeah, and that works pretty well to get droplet diameters.

CHAIRMAN BANNERJEE: Have you got that data as well?

DR. KELLY: For droplet diameters in a reflood.

CHAIRMAN BANNERJEE: Yeah, have we been able to use that data?

DR. KELLY: Only in assessment. We haven't used it in model development yet and that's because we've been trapped in this cycle of model remediation, which we're finally about to, you know, finish and do the documentation and then this is where we're going to be working next with the exception -- you'll notice the others down here at the bottom, we know there are going to be applications like EPR coming up. There will be phenomenas such as reflux condensation that may be very important for that reactor that we haven't had to worry about yet. So we've generated some, you know, systems data on facilities like OSU and we're going to see how TRACE does on it.

CHAIRMAN BANNERJEE: But there is historical data in this area.

DR. KELLY: Right, and we'll look at that, too. So reflux condensation now goes up onto the assessment matrix and we have to assess the code against it and see if it works. If it's a significant deficiency, then we look at the data that we have and see if we can develop a model from it.

CHAIRMAN BANNERJEE: One of the things

that, Joe, is going to be not just the single or few tube behavior because probably there is data on that, but it's going to be the distribution in these plant and what goes in there. It's going to be a complicated 3-D problem to assess. I don't know how you're going to do that.

DR. KELLY: Yeah, I know. Some tubes have a forward flow, some backwards.

CHAIRMAN BANNERJEE: I asked this question at the tripartite meeting and -- of the French and Michael Reacreaux came and said, "You know the answer to this of course, which is that we can't do it". You know, that's the reality that you have to do it.

MEMBER WALLIS: You've got critical flow in here. Is critical flow in good shape?

DR. KELLY: The TRACE critical flow model works pretty well, but what it doesn't --

MEMBER WALLIS: I mean, there was this strange Mahaffy paper saying that it depended on downstream conditions and things. That's all been fixed, has it?

MR. MURRAY: That was a bug that was fixed back in 2002.

MEMBER WALLIS: But there's no data on the paper. I mean, it has been fixed?

DR. KELLY: Yeah.

MEMBER WALLIS: But TRAC for all that time predicted critical flows depended on downstream pressure, up until 2002?

DR. MAHAFFY: Well, the simple answer is yes. It was a question of how you -- well, careful. The biggest issue was actually the downstream nodalization of your break component. There was a slight dependence on pressure. And it became more important in various configurations, but we've wiped that out.

MEMBER WALLIS: You wiped it out, okay.

DR. KELLY: The reason this is on here, the model works pretty well, but it doesn't work great for very thin orifice plates because there's a thermal non-equilibrium thing or lack of, you know, going to equilibrium. And the become very important in experimental facilities. Like if you're trying to model ROSA and do one of their small break experiments, they're inventory driven, so if you don't get the break flow right, you can't look at all the other phenomena that you wanted to look at. And so what the Japanese have done is put in a special critical flow model for their thin orifice places that they use in ROSA and that's the first one I'll look at is the possibility of just plugging in so that when we use -- you know, when we try to do their integral effects test, we can use a model that gets us a better answer for the break flow.

CHAIRMAN BANNERJEE: Okay, thanks, Joe. We're going to --

(Applause)

CHAIRMAN BANNERJEE: Anyway, we are running well behind schedule as usual. So let's take a break for what, an hour, and come back at quarter to 2:00. And Steve, you and John Mahaffy and DiMarzo need to figure out how you can finish in some reasonable time.

DR. BAJOREK: I think we've jumped ahead on a few of these issues and talked about them. What I'm going to do is go through some of the assessment very quickly, try to get through this with hopefully a minimum of questions, you know, so we can just summarize some of the main points.

Joe has gone into some of our planned development. I think I can cut back on that. We do want to get to John Mahaffy's presentation talking about some of the issues on the momentum equation.

CHAIRMAN BANNERJEE: Right, so we'll try to move quickly to that. I'm sure that Graham will be very anxious to --

MEMBER WALLIS: I didn't say anything about momentum. I said, how do you go from differential equations to control volumes? That's my first one.

CHAIRMAN BANNERJEE: Now we're going to adjourn this, all right.

(Whereupon, at 12:46 p.m. a luncheon recess was taken.)

CHAIRMAN BANNERJEE: The meeting will now come to order. We are on Item #3.

Stephen, you are on, but you are not going to do assessment results now?

DR. BAJOREK: What I'm going to do to try to get us back onto schedule is to talk briefly on the assessment, and then jump to some of the use of TRACE, or some of the work that we're doing to make sure that this is the tool that the agency can use in the future.

There are two presentations that have gone around. You have all of the overheads and the slides for this.

What I have done in the PowerPoint is to merge the two to hit only what I think are the more important features.

MEMBER WALLIS: You have consolidated the two?

DR. BAJOREK: Yes.

CHAIRMAN BANNERJEE: It hasn't taken three years, right?

DR. BAJOREK: Yes, it was consolidated.

One of the things I wanted to try -

MEMBER WALLIS: Let me turn out these lights.

ASSESSMENT RESULTS

DR. BAJOREK: What I wanted to do in the presentation on assessments is talk about our assessment matrix, how we've changed it, expanded it over the last couple of years.

The two points that I want to make sure do come across is that by and large we are following the CSAU process. We have taken a look at the processes that have been published for large break and small break PIRTs, and we've arranged our assessment matrix to hit all of those processes which are highly ranked.

We have selected tests that cover both a broad range of phenomena and a broad range of scale.

Earlier today Joe talked about some of the developmental assessment, where we are looking at a lot of single tubes type of data.

Where we think that the real acid test of the code then comes when we start to apply it toward full height bundles, full height test faciities, and a range of integral test facilities -

MEMBER KRESS: Bethsy is is one to 100? That seems enormous.

DR. BAJOREK: Bethsy is - the information I had listed is one to 100. I thought it was closer to one over 48.

CHAIRMAN BANNERJEE: No, that's ROSA.

DR. KELLY: ROSA would be one over 48, and I think one over 100 is about right for Bethsy. Bethsy is full height, but that's volume.

MEMBER WALLIS: Full height? Okay. Because it's huge otherwise.

DR. KELLY: Right, it's full height, full pressure.

MEMBER WALLIS: Okay, that's what it says.

DR. BAJOREK: These are volume scales here.

CHAIRMAN BANNERJEE: The yellow means of course they are not full height, right?

DR. BAJOREK: No, these are going from small volume scale to larger. The couple of facilities that were noted here in the lighter color are from our own test programs.

MEMBER WALLIS: What's this LWR test facility you have over here?

DR. BAJOREK: No, this is just to represent the full-scale light water reactor. It's not a test facility until you realize that UPTF is essentially a full scale vessel.

But one of the things that we have done over the last couple of years is, we've tried to learn some of the lessons of not assessing a code like TRAC PF1-MOD2. And what we have done is, we've looked at the code. When we have the models in there that we think are correct, we've gone through the assessment matrix.

As Joe Kelly pointed out, when we went through that assessment matrix the first time we found numerous deficiencies. These had to go back into the model and development stage. In some cases we had to go back to the test programs that we've been running at the Thermal Hydraulics Institute; develop improved models; put them back in TRACE, and go back through the assessment again.

In the process of automating things, we have gone through this loop actually numerous times, and it's only now that we feel we have enough fidelity in the models, enough accuracy in the models, and replaced the ones that are truly bad, so that we can start to release this code to real applications.

We think in the long run, since this is intended to be a best estimate realistic code, we are going to develop uncertainty methods, implement those in the TRACE as well, so that we can run numerous applications for a full scale application in order to examine the model and sensitivities, so it isn't just one calculation that is done, but we see some of the impact of being off in some of those thermal hydraulic models.

MEMBER SIEBER: Has any reviewer assessment activity sent you back to the PIRT itself?

DR. BAJOREK: Not yet, no.

MEMBER SIEBER: That's a possibility, though, correct?

DR. BAJOREK: It's a possibility. I'm trying to think in case of a new reactor, say EPR, where reflux condensation, which is one of those things which are important in the PIRT, but now it becomes emphasized in there.

So in each one of these new applications there is a PIRT process that is followed to try to identify which models are going to be more important, accentuated in that application, and then we start to target the assessment, and we are going to make sure that the code -

MEMBER WALLIS: Well, you have all these models that you fix up for things like interfacial drag. It could be ways to fix up the momentum bounds for Ts. That would be something that's in the code itself. Is that maybe the sort of thing you are thinking about? Because all of those things have an influence on the answer.

DR. BAJOREK: That's true.

Show a little bit of a difference on what has been done over the last couple of years as opposed to how some of the assessment had been done in the CSAU and throughout the `90s. Typically if we use ECC bypass as an example, most previous assessment when they would choose the code would focus on one of the Test 6 simulations. UPTF looking at ECC bypass, this was uniform injection in each of the three intact loops that flow with bypass go out the broken loop.

We've expanded that to take a look at all of the test cases in the Test 6 series, because some plants do wind up with asymmetric injection because of their safety systems. We've expanded that to take a look at a series of tests, Test 7 in UPTF, in which there was asymmetry in the injection.

Test 5 is one which had very, very high subcooling. You wouldn't really expect that in somie of the plant calculations. However, we found this one to be very useful because it really puts a premium on getting your condensation models correct in the up or downcomer. So we focused on this, and added in Test 21, which is a direct vessel injection test.

One reason I wanted to bring this one up, and there is another one in the package looking at a reflood test -

MEMBER WALLIS: Excuse me, but when you bottle this downcomer, it's a three-dimensional model, is it?

DR. BAJOREK: Yes.

MEMBER WALLIS: With radial nodes?

DR. BAJOREK: In the downcomer itself you would not have radial nodes. You would have radial nodalization going across the vessel -

MEMBER WALLIS: It's just circumferential and axial.

DR. BAJOREK: Circumferential and axial.

CHAIRMAN BANNERJEE: But it's not really a three-dimensional model. It's something close.

DR. BAJOREK: Really, the way the downcomer is modeled, it's two-dimensional.

MEMBER WALLIS: It's a nodalization of the downcomer, but it's not really 3-D.

DR. BAJOREK: Three theta, whereas you take advantage of other aspects of a 2-D type nodalization within the -

MEMBER WALLIS: It doesn't model, for instance, the ECCS fluid say impinging on the inner wall and running down or anything like that?

DR. BAJOREK: No, volume average.

Now partly because of those types of concerns, the reason I wanted to bring this overhead in here is we do want to try to rule out the user effect inasmuch as possible.

So we've taken a look at our conventional models for PWRs and BWRs, and we've applied that nodalization to the test facilities as much as we possibly could.

So we look at the lower plenum in a plant model, it would look very much like the lower plenum in downcomer in a UPTF.

Also in the package, if we take a look at - I'm going to jump ahead, okay - the core nodalization which is shown here, using RBHT as an example, the axial nodalization, the way we would model the core in a plant is very much like we would apply that model in all of the test simulations that we've performed.

And that would be our recommendation for anyone setting up a model. That way we're at least staying very close to the assessment base.

What we've done for each of these simulations, we would go, in the case of UPTF, we would define figures of merit. In this case we're looking at the penetration time. We are looking at the filling rate, in this case of the lower plenum. And we'd also be looking at the condensation efficiency, what we predict and what we measure.

This is an example of one of the UPTF cases.

MEMBER WALLIS: Is this is the sort of switching where the staff to start with it's all held up, and then it comes down as fast as it goes in?

DR. BAJOREK: Yes. Well, not exactly.

MEMBER WALLIS: More or less?

DR. BAJOREK: Not exactly. Some of it is still being swept out --

MEMBER WALLIS: That's why it's slightly curved.

DR. BAJOREK: - during the penetration period.

But rather than focusing solely on a single test, we've run a relatively large number of these in order to characterize how well the code is doing.

Now you can see in this case we've predicted that lower plenum fill rate generally within plus or minus 20 percent. In comparison to the work that had been done for CSAU, the penetration time, that ratio of predicted to measured was 2.76, meaning it was predicted relatively late. We think we are getting that about correct in TRACE now.

The filling rate TRAC-PF1-MOD2 was about a point five in the CSAU study. Here we think we're getting things about right, without a significant bias as a group. There are problems. We see in some cases if we have injection done asymmetrically, the code does tend to want to sweep, flow in the opposite side of the downcomer out, excessively, when compared to the data.

Now we've looked at these various simulations, and for the most part the code is doing the right thing in that it tends to bypass that loop close to the break, while those loops far away from the break on the other side of the downcomer tend to do the penetration.

CHAIRMAN BANNERJEE: -- most sensitive to which correlation? There must be 100 of them being exercised, but which one actually gives you the most effect on the results?

DR. BAJOREK: I would have to venture that it would be the drag on the droplets that are entrained within the downcomer.

CHAIRMAN BANNERJEE: What about the correlation that says how many droplets had changed? There must be some correlation like that, right?

DR. BAJOREK: There is a correlation in there that accounts for that. As to which one is having the largest effect, I really don't know.

CHAIRMAN BANNERJEE: So let's say how the droplets enter in. Where does that correlation come from? Is it from something like this? Or something completely different? Like is it from annular flow correlation for entrainment?

Maybe Joe can speak to it.

DR. BAJOREK: That's been a recent topic.

MEMBER WALLIS: it's not clear to me that bypass is a result of droplets being entrained anyway. It's more a macroscopic slushing out of stuff.

DR. KELLY: What I saw when I looked at these tests the last time was that the elevation where the cold legs and hot legs were, the void traction was fairly low, on the order of 40 percent.

And it was below that that it was in the annular flow regime. But it was, as Steve said, it's primarily the interfacial drag model that holds the water up, because the velocities are on the order of a couple of hundred meters per second, because it's a pretty low pressure steam being blasted through the downcomer to go into the cold leg.

MEMBER WALLIS: Don't you get a lot of oscillations in this downcomer?

DR. BAJOREK: Some tests, yes.

MEMBER WALLIS: Because I mean they already did tests on downcomer, fifth scale or something. I looked at those; I was there. And we had all sorts of thermocouples and things. And there was a tremendous amount of unsteady flow going on in there.

DR. BAJOREK: That's true. And that is especially true if the water coming in is subcooled. Some of these tests were nearly saturated.

MEMBER WALLIS: It would collapse and all kinds of stuff.

CHAIRMAN BANNERJEE: In comparing these, it is interesting to see how much makes it down, how much goes out, but do you have more detailed data, like how much is - what the void fraction distribution looks like?

I don't remember UPTF now.

DR. BAJOREK: Well, there is information on the global mass distribution, how much is in the lower plenum, how much is in the downcomer, how much remained in the cold legs, and where that penetration occurred.

By looking at thermo-couples which were embedded in the core barrel and the vessel wall.

They really inferred where the penetration was occurring from the thermo-couple information as opposed to a direct measure.

MEMBER WALLIS: Did you compare with those CREARI tests?

DR. BAJOREK: No, we haven't done that. That is part of our looking at the assessment damage, in that when we have a limited amount of resources to spend on certain problems we have tried to look at that full scale first and then expand to lower subscale tests.

CREARI I think was one-fifth and one-fifteenth.

MEMBER WALLIS: But there is a lot of data on the variation of temperatures and void fractions and things at say different places over time, which you could also compare with your predictions. A lot of detail as well as the macroscopic penetration rate, if you want to do that.

Of course you may just want to get the answer you want, and if the details don't match we don't care.

DR. BAJOREK: Well, we want to make sure that the code is not scale dependent, and in the long range it's likely we will look at some of those tests.

But in order to get the code out and finished, if we had to make a choice between something at part scale versus full scale for UPTF -

MEMBER WALLIS: Not to mention the phenomena of changing much, though, between pretty big and really big. There's not much that can change.

ADMINISTRATIVE JUDGE PEACOCK:

CHAIRMAN BANNERJEE: I guess my question would be, are you getting the right answers for the right reasons in terms of the mechanisms? So I don't know what you have to do to do your own internal due diligence about this.

Because I would say that if you were getting this type of result because the phenomena your code was predicting was similar to what you saw in the experiment, that's really wonderful.

But if you were getting it due to say some mechanism which wasn't really what was then the experiments, but you got the right results anyway, that would be more of a concern.

So I would ask you to look at everything from that point of view.

DR. BAJOREK: What we try to do is not focus on a single figure of merit.

CHAIRMAN BANNERJEE: Right.

DR. BAJOREK: I mean you can't just look at the penetration time. You've got a CCFL breakdown. As I mentioned, we tried to look at three things: when you start to get the penetration; how quickly you fill the lower - what's the penetration rate; and what's the condensation efficiency.

Now in earlier versions of the code -

CHAIRMAN BANNERJEE: And why are you getting the bypass.

DR. BAJOREK: And why you are getting - in earlier versions you may get one of two fo those parameters correct, and the other one would be way off. Like I remember one of the earlier versions, the bypass time and the filling rate weren't all that bad, but the condensation efficiency was 1.0, and we knew that wasn't correct, compared to what the experimental data was.

When we fixed that, well, we were off on the other two. It was really the refinement of several models before you simultaneously get all of your figures of merit correct. And we are sensitive to that, and that's why we try to look at several things within not only the context of what was measured in there, but in taking a look at things like the interfacial drag, where the bypass occurs, things that the code is predicting that you could make some engineering judgment on, but you may not necessarily have a -

CHAIRMAN BANNERJEE: For example if in this case most of the bypass was occurring due to entrainment of CROPS, that's what the code was predicting, and that's what experiments somehow were doing, that's fine.

But say most of the bypass was predicted by the code as drops. In experiments it was some periodic slushing phenomenon that was getting it out. Then you've got the right result for the wrong reasons.

And I think that there is no way we can ever go into every experiment and look at it in the same detail as you can obviously. So every time you show us something like this, we'd like to be reassured that you've got that answer because you've got the right mechanism. It's didn't come due to some other mechanism.

DR. BAJOREK: We agree.

MEMBER WALLIS: Because for example you can fudge the condensation efficiency with a correlation and make the points come closer to a line. It doesn't mean to say you've really fixed the right mechanism.

DR. BAJOREK: And what we've found is that when we have made adjustments in those models affecting one, when we do make that mistake, and correct one model and not think about something else, it pops up in some of these other figures of merit.

So we are looking at all of those simultaneously, and we're not just looking at the global results only. We are looking at the details.

MEMBER WALLIS: What you are plotting here is a penetration rate which is the slope of the line that you showed on the previous graph?

DR. BAJOREK: Yes.

MEMBER WALLIS: Which is not a straight line, though, so what is the - is it the average penetration rate or something?

It's a curve; it's not a straight line. So what is the penetration rate there? Which one - the slope doubles when it goes up from the bottom to the top.

DR. BAJOREK: We've relied on some evaluations that had been done during the 2-D/3-D program.

MEMBER WALLIS: What is the penetration rate here?

CHAIRMAN BANNERJEE: Is it the average of this? Or what are you taking?

DR. BAJOREK: We look at the time after penetration occurs until the time just before the lower plenum is filled. So it's the average filling rate during oh approximately 55 and 75 seconds here.

MEMBER WALLIS: Because the TRACE prediction is actually about double the rate at the end as at the beginning.

DR. BAJOREK: This is - this is some nodalization question here.

MEMBER WALLIS: No, no, just the slope of that curve, the rising curve. The slope is a lot bigger at the top than the bottom.

DR. BAJOREK: Yes, but we are looking at that average over this evaluation period.

CHAIRMAN BANNERJEE: Now, for each of these, do you write up a little report just to describe what it was you found, from the viewpoint of the phenomenon, and then more detailed results? How do you document these?

DR. BAJOREK: Each one of the assessments we've tried to standardize this format. We describe the facility itself, what those experiments were. So the reader can be informed about what was going on.

We describe the TRACE model then, and hopefully from this we can see how it changes from one facility to the next, or how it's standardized when you have similar phenomena.

We have the assessments shown, then compare the code predicted results to the measure. And then there is a section that performs basically an evaluation of that comparison, describes what went right, what went wrong, tries to identify if there is a sensitivity in the code, let's say to subcooling, and comments on those simulations.

So that somebody who is interested in ECC bypass can go through there and make a conscious decision as to whether the code is doing a reasonable job for the right reasons, or whether I have a model that if I have very high subcooling, there is something I want to be -

MEMBER WALLIS: Look at this again here, your trace curve is continuous. The data says that stuff goes into the low plenum and comes out again, it sloshes in and out in a very dramatic way. I mean there are times when it is actually coming out as fast it went in; the down part of those spikes.

So there is something going on here about it going in and out of the lower plenum which isn't being modelled at all by TRACE.

DR. BAJOREK: Well, these tests were measured with I think roughly four DP cells at the bottom of the lower plenum. There was a lot of motion, a lot of sloshing going on.

I think what we were picking up in the DP cells, there is a lot of that sloshing motion.

MEMBER WALLIS: Picking up some momentum rather than some gravity; is that what you mean?

DR. BAJOREK: Yes. In fact, I think you see it down here. As soon as you turn on the test, you do start to see an offset -

MEMBER WALLIS: No, but I'm talking about the big oscilations.

DR. BAJOREK: You are talking about these?

MEMBER WALLIS: Right.

DR. BAJOREK: Yes.

CHAIRMAN BANNERJEE: Would it could be partly gravity, partly momentum.

MEMBER WALLIS: Right.

CHAIRMAN BANNERJEE: I guess the broader issue is, is there discussion of the qualitative phenomena seen in the experiments, and whether there is similar qualitative phenomena in your calculations.

There is qualitative and quantitative in this case, and the quantitative aspect of it, you know, sometimes you can get a few things right, but because you can adjust things, or even if you are not adjusting things it could be just some completely wrong reason.

Jeff Hewitt has a famous talk about this. Have you ever heard this? It is how you can change the models over incredibly wide range of results, and if you know the answers, you can always get the right answers. This is a talk he gives.

So taking that into account, and some of them are due to completely the wrong mechanism, you know. So the issue is really, are you getting that result due to the right mechanism or not?

MEMBER WALLIS: And do you care?

CHAIRMAN BANNERJEE: We care.

DR. BAJOREK: We think we are now getting the right results for about the right reasons. We try to write that up in the assessments. Are we successful in doing that for every single simulation? Probably not. But we try to do as thorough a job as we can in describing how well the code is actually performing for that particular task.

MEMBER WALLIS: Another obvious question is, what's the rate at which you are adding the ECC? Is that TRACE slow at the top there putting it all in? In which case, comparison with the code doesn't really tell you very much. It's simply saying it all gets down. It's not telling you something percentwise. It's just saying it all gets down.

DR. BAJOREK: That's why for this particular - these particular tests, our interest is really on this part of -

MEMBER WALLIS: It's not all getting down.

DR. BAJOREK: - down there. Once you are out here, and the steam flow is gradually decreasing in these tests, and you've got CCFL penetration, and you've filled the lower plenum, we really don't care. We could have cut this off at about 80 seconds and not really have lost much information in terms of how well the code is behaving.

We talked about this. We try to go through, and this is an example of some of the reflood test results, we've expanded the test matrix. Instead of just taking a look at one or two tests, we now look at a very broad range of conditions that look at pressures -

MEMBER WALLIS: D we have those?

DR. BAJOREK: Yes, you do. Page nine. It's in there. I am skipping through this.

MEMBER WALLIS: Slide 18. Yours is 10. Your numbers aren't the same?

DR. BAJOREK: No, not on this one, no.

But comparing the things that were measured in the test, in this case the code is in red.

MEMBER WALLIS: This is remarkably good for a very messy phenomena.

DR. BAJOREK: Thank you.

CHAIRMAN BANNERJEE: Is this pre-test or post-test?

DR. BAJOREK: No, this is all post test. In fact SEASET was run in the last `70s. Most of these tests have been in a way almost a standard type fo problem for reflood behavior.

MEMBER WALLIS: But Joe Kelly has massaged the code a lot to make it fit these tests, hasn't he?

I mean he had a lot of concern about the model and he kept adjusting it.

DR. BAJOREK: Well, let's jump one more. This is kind of what we started with.

MEMBER WALLIS: This is after Joe fixed it?

DR. BAJOREK: This is before.

MEMBER WALLIS: This is before, but what we see on this -

DR. BAJOREK: This was a 2001 report that was done basically with the reflood model in TRAC-PF1-MOD2. It was renamed TRAC-M at about that time. We really hadn't changed any of the physical models.

MR. CARUSO: It's conservative.

DR. BAJOREK: It's conservative. It's not realistic. Would you say that's conservative? At the upper elevation?

MEMBER WALLIS: Which one are they going to show us?

DR. BAJOREK: Well, if you were a vendor, you'd want to see this one, and you probably wouldn't get to look at this one.

The point is, this is really the state of the code reflood before the interim reflood model. Okay?

MEMBER WALLIS: 1600 degrees K is pretty

bad, isn't it?

DR. BAJOREK: It's very bad. It's very bad. But the point I want to make is, that's what we started with a couple or three years ago.

There have been numerous changes to the reflood model. We are still looking at that.

CHAIRMAN BANNERJEE: Now you have been over this, but can you in a nutshell tell us again what is the major change that you have to make?

Is there one controlling phenomenon that was really wrong?

DR. BAJOREK: Oh, I don't know if there was one that was very wrong. The model was very, very sensitive to that oscillatory flow rate.

I think a large part was the inverted annular behavior near the clench front.

CHAIRMAN BANNERJEE: You mean the precooling due to it?

DR. BAJOREK: How that inverted annular column broke up, and what was the key transfer that occurred, and how it affected the downstream development at that point.

MEMBER WALLIS: Is it really inverted annual, or is that just what people think it is?

DR. BAJOREK: I think that's what the old model -

CHAIRMAN BANNERJEE: Well, I guess in gravity reflood, whatever, you were getting these oscillations. So this tongue was being made and broken, made and broken, all the time, right?

DR. BAJOREK: Yes, so I think when you were getting that downward flow on the gravity reflood, the code may have been thinking it had been in an invert -- breaking that up and giving us different dynamics.

CHAIRMAN BANNERJEE: Now how did you fix it? You still have oscillations.

DR. BAJOREK: We still have oscilations. We still have a lot of work to do in the reflood model.

In your handout which we won't be able to get to in the interests of time, we were looking at some of the CCTF simulations. And we think we have identified why those aren't in agreement, between the code and the data. That's grounds for future work.

But one of the reasons we're satisfied with TRACE 5.0, and feel it can be publicly released at this time is because we think it does a reasonable job for most of these assessment, and those places where it does have some problems and deficiencies, we can identify them.

CHAIRMAN BANNERJEE: Where are the problems and deficiencies?

DR. BAJOREK: I'll get to that in just a -

CHAIRMAN BANNERJEE: Who didn't get a chance to tell us what was the new deficiency in the TRAC-M, though?

DR. KELLY: Well, actually Steve did a pretty good job of that.

CHAIRMAN BANNERJEE: It was oscillation?

DR. KELLY: Because they were using an inward velocity to tell them the lengths of the inverted annular. So just like you said it would go away, which meant everything would be treated as droplets, swept out of the bundle, and the bundle would sit there and fill back up and throw all the water away.

MEMBER WALLIS: When you model these tests you have to model the rest of the system, not just the bundle?

DR. BAJOREK: Not for this particular test. This is a relatively simple separate effects test, so it's just the bundle.

The point I do want to leave you with on this -

MEMBER WALLIS: Don't the oscillations

depend on the rest of the system as well?

DR. BAJOREK: Yes.

MEMBER WALLIS: How do model that?

DR. BAJOREK: In CCTF and STCF, we modeled the entire system for that test.

MEMBER WALLIS: But for select CSET you don't have to do that?

DR. BAJOREK: No, this was a forced flooding test.

MEMBER WALLIS: It's forced, okay.

DR. KELLY: But even with that the Banerjee M model had these oscillations that threw the water away.

DR. BAJOREK: Yep. As we go through and do the simulations, we don't just look at the cladding temperature. This was typical of what some assessments had done in the past.

We are looking very closely at the heat transfer coefficients that are predicted by the code. We have been able to either get those from the experimental data, or we have been doing some work to actually develop those from our data for the newer tests.

But we are also taking a look simultaneously at things like the bundle mass, its distribution, and the overall clench profiles.

That was TRAC-M. Problems, improvements: We aren't going to claim at this point that TRACE 5.0 is perfect. We think there are a number of things that we would like to fix in the near future.

Joe did a nice summary earlier on where we think some of the model improvements are going to come from.

If we take a look at all of these tests in mass, where we have - some of our biggest concerns are with the core reflood model, and its lack of space or grid models.

If we look at the details fo some of those experiments - we don't have time to look at those - we don't capture the local behavior that occurs downstream of the grids.

This is due to the convective enhancement, the droplet breakup, and in some cases, early rewed of the grid spacer itself.

So when we look at some of the CCTF and SETF experiments, we expect with TRACE right now to overpredict some of those temperatures at the higher elevations. And we think that when we get in the spacer grid models, and are able to model the phenomena that they would impact, we would be able to improve those considerably.

We want to put in the spacer grid models in TRACE 6.0. We already have the capability of adding in a third field, which will give us the capability of modeling not only the gas field, the continuous liquid field, but having a separate droplet field.

That is going to give us a distinctive advantage then at being able to model droplet breakup higher into the core, and droplet entrainment, the entrainment both in the core and in the upper plenum for those tests like UPTS 10 and 10-B where you need to know what that distribution is.

If we took at all of the tests gravity and forced reflood, we can probably say at this point our carry over, how much of that liquid -

MEMBER WALLIS: Is overpredicting it?

DR. BAJOREK: It's overpredicting it.

MEMBER WALLIS: But I would think that the drop breakup mechanism would break them up smaller and make them carry over even more?

DR. BAJOREK: It would break them up and evaporate them.

MEMBER WALLIS: Well, they'd either evaporate or carry over. But they certainly would be carried up as steam or water.

DR. BAJOREK: Well, we're hoping they'd be carried up, break up the droplet, increase the interfacial area, evaporate and drop the steam temperatures, which are too high right now.

When you look at some of the tests like SCTF, there is too much water in the upper plenum and being generated in the steam generators.

MEMBER WALLIS: Once you've dropped the steam temperature, though, the steam is simply carrying up whatever didn't evaporate.

In that case smaller drops would carry move to the upper plenum. Yes.

DR. BAJOREK: Yes.

Downcomer, interfacial and wall models, I left that on there. This has been one of those last things that we've had to pin down. We've seen some problems there, and we are going to look at those in more detail.

And curve robustness. We are somewhat plagued with transients going out, going in very small time steps. We know we've got to fix that if we're going to be able to use this code to do the 59, 94, or 124 simulations which are typical of the best estimates statistical approach.

CHAIRMAN BANNERJEE: Come again with robustness? I just didn't quite get what the problem is.

DR. BAJOREK: If we run 500 simulations, we will find that there are going to be a handful that will stop. A lot of time the analysts can get around that by taking some creative liberties with the time step, making it small before they are getting it through.

We want to be able to run all of these cases, all of the plant calculations, without having to nurse it through.

MEMBER WALLIS: It doesn't reverse the time when it stops, doesn't actually go backwards, it just stops.

DR. BAJOREK: It just stops, yes.

MEMBER WALLIS: What is that due to, in general?

DR. BAJOREK: It can be several different things.

MEMBER WALLIS: Something blows up? Some matrix which is singular or something, something odd happens, and it just can't go any further?

DR. KELLY: A good example is if for some reason in say a downcomer where you would have a level, the condensation rate gets very high for awhile, and a solid wall of liquid hits another boundary, and then you get a pressure spike, and that's called water packing and so on.

MEMBER WALLIS: I'm just curious about that. In the preamble to this TRAC thing that I tried to read, it said they could model water hammer. Is that true? I really doubt that

DR. MAHAFFY: This is John Mahaffy. There is historical precedent for that. There were some calculations done by what was then called CAMA in the Netherlands using TRAC to look at water hammer phenomena in various odds and ends trains of piping and reactor systems, where you'd have a flow, you'd slam a valve closed and look at it.

They actually surprised me. They did better than I expected.

The reason really is, when you think about the phenomena with the shock waves and the rarefactions, you'd normally think you really want a good set of fully conservative mass energy momentum equations.

But that kind of thinking comes from kind of ideal gas situations in shock tubes. When you are dealing with shock waves going through water where the density doesn't change, you can get away with a lot less and still get good answers.

MEMBER WALLIS: You need a pretty small time step?

DR. BAJOREK: Oh, certainly, you have to. You are following the shock wave. You got to resolve it.

Most of the assessment work that we've been looking at has been generic in its applications. It covered the highly ranked phenomena and the PIRT.

As I mentioned, we do have test series to take a look at those tests which are most important to ESPWR, to focus in on phenomena that we know is important in that plant, and what we expect is going to be important in a plant like EPR.

So we are trying to look ahead.

CHAIRMAN BANNERJEE: Well, ESPWR, there is no - GIRAFFE is full height or not?

DR. BAJOREK: GIRAFFE is pretty much full height, yes.

CHAIRMAN BANNERJEE: And is now actively being used?

DR. BAJOREK: I don't believe so, no.

MEMBER WALLIS: it's a giraffe.

CHAIRMAN BANNERJEE: So all these - the

rest of the stuff - you are not involved with PONDA anymore, right?

DR. BAJOREK: Not presently, no.

CHAIRMAN BANNERJEE: There is a new set of requirements being planned for PONDA?

DR. BAJOREK: There is a new set of experiments. I believe the NRC has decided not to participate in that.

CHAIRMAN BANNERJEE: Right. So all you have are these PUMA experiments which are reduced height, right?

DR. BAJOREK: Reduced height. We have several different types of tests in PUMA, and GIRAFFE we use proprietary.

CHAIRMAN BANNERJEE: You know one of our major concerns is this chimney. How are you going to address that?

DR. BAJOREK: Well, in terms of the assessment.

CHAIRMAN BANNERJEE: Well, to be sure that your code works.

DR. BAJOREK: We can shoiw you the assessment against the Ontario Hydro. There may be some additional Ontario Hydro tests that we ought to be looking at.

CHAIRMAN BANNERJEE: Well, they may be the right diameter, but they are not -

DR. BAJOREK: Not necessarily the full height.

CHAIRMAN BANNERJEE: The full height, and then they are not part of the system. And the sort of thing that was worrying us was, you get fluourogime type oscillations which actually cover the whole chimney.

DR. BAJOREK: Well, one of the things that I would suggest is that as we get closer to audit calculations of ESPWR, we come in and we talk to you about the assessment and phenomena in ESPWR, and how TRACE performs against those.

And I don't think we're prepared right today to really look at the results and comparisons for Ontario Hydro or for the interval tests.

MEMBER WALLIS: When we look to GE's work on stability they had a Courant number which was not right for the chimney. And this really spread out the void fraction variations as you remember. There's artificial diffusion numerically. Does TRACE have the same problem, or does the SEASETS method prevent this diffusion.

CHAIRMAN BANNERJEE: The differencing would give us the diffusion.

MEMBER WALLIS: It was very noticeable in the GE results, that Courant number was way off, and htye came in with a certain perturbation, and it just attenuated itself as it went up the pipe with no mechanism to create it except the numerical diffusion.

DR. BAJOREK: I haven't seen those results.

MEMBER WALLIS: You haven't seen those?

CHAIRMAN BANNERJEE: Well, part of it was they very crudely normalized the chimney.

But even if the code was capable of having these fluorogime-related oscillations in void fraction, it couldn't sustain them because it was so crudely -

MEMBER WALLIS: But even without that they couldn't sustain the perturbation in void fraction.

DR. BAJOREK: One of the things that the ACRS -

CHAIRMAN BANNERJEE: It could go back to EPR? So ROSA of course is full height, full power, everything is right.

DR. BAJOREK: Well, CSET was full height. APEX is quarter height. But we're using this basically as a separate effects facility where we can have some more detailed measurements as the steam enters the uphill part of the steam generator.

MEMBER WALLIS: This is being done now, you said?

DR. BAJOREK: Some of those tests have been done, and we still have - we have some plans for doing some additional tests in 2007, yes.

CHAIRMAN BANNERJEE: In the plenum of the steam generator, you mean the inlet plenum or what?

DR. BAJOREK: What we've done is we've taken the flow from - it boils off the intact side, steam generator; blocked off that part; brought that steam into the hot leg, and let it go into the faulted side steam generator, the one that we are doing the measurements, with DP cells in both the uphill and the downhill side, where we can have collection tanks to see what is the split of condensation on the uphill or the downhill side.

We could range it then over a variation of Reynolds numbers, inlet Reynolds numbers, which are somewhat consistent with EPR, where we expect it to be.

We'd also like to be able to put in some noncondensables, because what is kind of interesting - oh, actually one of our Bethysi tests -

MEMBER WALLIS: Is it normally done so it will accumulate in the steam generator?

DR. BAJOREK: Yes. We see that in one of the Bethysi tests, and also one of the ROSA tests. I don't have it listed up here at this point, but when they had this rapid depressurization of the secondary side of the steam generator, they started to collect noncondensibles, which I think came out of solution into the steam generator tubes, that actually reduce some of the condensation.

MEMBER WALLIS: These are hydrogen? What are the noncondensibles?

DR. BAJOREK: In our test?

MEMBER WALLIS: In a real reactor. I guess in the real reactor.

DR. BAJOREK: Real reactor it'd be hydrogen.

MEMBER WALLIS: What are they in the APEX, noncondensibles? What are they?

DR. BAJOREK: Nitrogen air.

MEMBER WALLIS: Nitrogen? Is this air in the water?

DR. BAJOREK: In the tests that we're running we're putting it in.

MEMBER WALLIS: You're putting it in?

DR. BAJOREK: We would put that in.

They suspect that in the ROSA and the Bethysi tests where they saw those phenomena it was nitrogen coming out of solution.

CHAIRMAN BANNERJEE: We have no full height facilities of our own, non-potentially our own.

DR. BAJOREK: Potentially yes. One of the things that we want to try to do is to perform some of the assessment with TRACE as it is right now. We want to see how well the models behave.

If the models are poor, they're getting the wrong answers for the wrong reasons, or even the right answers for the wrong reasons, then we have good justification to propose to our management to go to the Thermal Hydraulic Institute and set up separate effects test in order to focus in on the phenomena.

But we're finding that if we were to just simply say, we need resources to run some tests, because we think a model is bad, we aren't going to be very successful.

CHAIRMAN BANNERJEE: It's good to have ROSA data anyway, right?

DR. BAJOREK: Yes, we have the ROSA data. We have already obtained this - well, we have the reports for FLECHT-SEASET trying to get that electronically.

We have this data in house.

CHAIRMAN BANNERJEE: Wait and see.

DR. BAJOREK: Yes.

One of the issues that was raised in the ACRS letter was that we needed to do things to try to make TRACE an integral part of the agency tool box.

In looking at this over the last year, one of the things - a couple of things we realized is that when the users in NRR or any of our other offices try to use TRACE, they are kind of in a time bind. They need to have an answer in several weeks, and they don't have that opportunity to convert decks, set up plant models, in some cases learn code. So what we've attempted - what we are trying to do now is to make this much easier for new users.

We have started training workshops. We had one last year, it was either April or May. We plan to continue those at one to two per year. I think last year we had something like 30 or 40 new users; have a hands-on workshop to use TRACE and SNAP, and step through how you would set up, use and -

MEMBER WALLIS: Now, I've already gotten an NRR, and I want to use TRACE to model ESPWO. What I would want from you would be a TRACE code which already has all the input stuff, and all the special stuff I need for ESPWO. So there's an absolute minimum of stuff I need to put in myself. I wouldn't have to put in the geometry of ESPWO, or something like that. I want it to be there already.

I can start asking questions about, what happens if this pressure is higher than that, or this temperature - play with it. But I don't want to have a lot of time putting in information.

DR. BAJOREK: That's what we're trying to make easier. NRR, I took the responsibility of setting up the ESPWO model. We also, we've taken that and been using it.

But what we want to try to be able to do, whether it's ESPWR or any other -

MEMBER WALLIS: Well, have you figured out how to nodalize and all that stuff, and I won't have to fiddle with that?

DR. BAJOREK: We would do that. We would do the nodalization. We would have the plant decks, NRR, or any other stakeholders then are free to go do some of those sensitivities, or explore some of those questions - what if I have a different type fo noding here? What if I change my initial suppression pool temperature, or what if I have a higher resistance in this line than I may have anticipated?

MEMBER WALLIS: They can go to some line in the code and put in a different coefficient of heat transfer, can they do that?

DR. BAJOREK: I hope not.

MEMBER WALLIS: You hope not?

DR. BAJOREK: We want to try - they can put in loss coefficients and things like that.

MEMBER WALLIS: Well, I've got this one minus alpha for the 1.75 or 1.8 or 1.92. I want to change that and see how sensitive it is to that. I can't go into the code and do that?

DR. BAJOREK: No, you can't.

CHAIRMAN BANNERJEE: That is a Joe Kelly thing. It's a Joe Kelly decree.

DR. BAJOREK: You'll have Joe mad at your too.

DR. KELLY: But what we are going to do, and Steve alluded to this earlier, when we start doing the uncertainty methodology, we will provide as input multipliers on a number of physical models.

But then a user can set those multipliers to do exactly those kind of sensitivity studies you are talking about now.

Would they be on every physical model? No. But they will be on the ones that were judged of high importance in the PIRT for the various transients.

DR. BAJOREK: Right. You'd look at the CSAU. You may want to arrange heat transfer coefficient in the core. You really don't want to go and arrange heat transfer coefficient everywhere within the system.

So what we are - our plans are, when we merge this with uncertainty methodologies, is to set it up so the user can go and put multipliers on various models and various correlations.

But no, he will not have the flexibility of pulling out one correlation, dropping in another.

CHAIRMAN BANNERJEE: Yes, I guess what you are trying to avoid is what commercial CFD codes do, where they have user defined subroutines. So if I wanted to go in and change a correlation in fluid, I can do it in 10 minutes or 10 hours.

DR. BAJOREK: It was in the assessments that we saw. You get one right or wrong based on what's in the code. You didn't get that assessment correct, because instead of using the recommended model for interfacial drag, I used my own.

That's somewhat deceiving. What we are trying to do, by freezing the code and limiting user access, is to make it consistent with the overall assessment, so you see how that model package is doing not only in the particular assessment, but in any of the other types of assessments in which it may have played a role.

CHAIRMAN BANNERJEE: I think you have a completely different objective from a commercial CFD code.

DR. BAJOREK: Right.

CHAIRMAN BANNERJEE: Where you actually have to qualify it and assess, so that's fine.

One of the things I wanted to ask you, though, is a lot of these designs, even if they are certified, like say AP 1000 or whatever, even small changes in these designs can make a big change in the answer, because they are gravity driven.

So are you going to give NRR - I guess this is like Graham's question - the AP 1000 thing all properly nodalized and stuff like that, and then they can play with it to see what these changes which a vendor might make will do to it?

DR. BAJOREK: We are working on that now. In fact we've been talking with the NRO office about AP 1000 in anticipation of potential changes.

What we are going to do is resurrect our

AP 1000 model, make it consistent with the other assessments on how we nodalize things, or how we think it should be nodalized to AP 1000; make that deck consistent with the final set of information that Westinghouse gave us and the design certification; do additional assessment for core makeup tank, so we know how exactly we should be modeling those with TRACE 5.0, and then give that model to the users if they want to apply it either for audit calculations or additional -

MEMBER WALLIS: Now is one of the users someone like the Union of Concerned Scientists, where they can get TRACE and they can ask their questions with TRACE? Someone who might want to check out how robust it really is, in order to get some confidence in it. Someone who perhaps is more critical than you guys might be.

No, I don't think that's possible.

DR. BAJOREK: Maybe more critical than you guys might be.

MEMBER WALLIS: More self critical. I mean you are self critical; you do a good job at that.

You get some public credibility if you give something to someone who is dubious. I mean he could say, gee whiz, everything is fine. That's wonderful.

MEMBER SIEBER: Or they could get in there and do some mischief too.

CHAIRMAN BANNERJEE: They get object code, right?

MEMBER SIEBER: Right. Licensees are more likely to go to their reactor vendor. That's what they're doing now, and that's how they audit their safety analysis.

DR. BAJOREK: Well, if the Union of Concerned Scientists would like to run the code, we have some assessments -

MEMBER WALLIS: Well, it's the National Academy of Science, maybe they could get a subgroup, and say, run this code and see if you believe it.

CHAIRMAN BANNERJEE: We suddenly get 30 reactors, the concerns, the National Academy might -

MEMBER WALLIS: Might want to do it.

CHAIRMAN BANNERJEE: - want to do it.

DR. BAJOREK: To make it easy for them to use, we are continuing the develop the SNAP tool. One of the things I like to do is ask some of the people who are newer why they call these things card images to the input data. Some people don't know.

A more efficient for newer people to use the code and learn the input is by a menu-driven structure that is built into SNAP. So as people are setting up a model they put the input into the volume, the length and arrays, and things like that, and SNAP automatically puts this into the right format, allows them to do the nodalization.

It gets around a lot of the confusion and headache that was typical of setting up code models in the past. So along with the plant model they'll have a SNAP mask so that when they start to look at the plant model, they are able to make changes in efficient fashion, and not have to stumble on inputs that change in one part of the input structure, and you forgot about something else elsewhere. It's to make life easier for them.

MEMBER SIEBER: SNAP is a front end process. Do you have an equivalent back end process?

DR. BAJOREK: SNAP actually does both the front end and the back end, so there is a way of visualizing the results.

We have initiated a project. We've talked with NRR. They've indicated which plants they are most interesting in seeing -

MEMBER WALLIS: Well, let me ask you about input decks. Suppose I want to model the primary circuit of a PWR. I have the big pipes coming out of pumps and going into cold legs and stuff.

When you develop an input deck, do you say this is a pipe of a certain length? Do you put in the bends? Do you put in the details of the geometry, or just say it's a pipe with a certain volume and a certain length?

I'm missing some of the physics of how you do that.

DR. BAJOREK: For example if it's a hot leg, you have to define how long it is, how many cells you need to have it -

MEMBER WALLIS: But suppose you say it has a bend in it. Does the TRAC know that a bend, which is a bit steeper than another bend, makes some difference?

DR. BAJOREK: Yes.

MEMBER WALLIS: It does?

DR. BAJOREK: There is input there to tell where the cell changes elevation, and what should be the orientation.

MEMBER WALLIS: When it changes direction, does that appear in there?

DR. BAJOREK: I'm sorry?

MEMBER WALLIS: When the cell changes

direction in going around a bend, does that appear in TRAC?

DR. BAJOREK: Yes.

MEMBER WALLIS: It does?

DR. BAJOREK: Yes.

MR. CARUSO: If I recall, AP 1000 has these unique sort of curved -

DR. BAJOREK: The corkscrew pressurizer?

MR. CARUSO: No, not the pressurizer, the cold legs go back into the vessel work curved; they weren't straight.

DR. BAJOREK: Actually a conventional plant has an elbow there too. They are curved in AP 1000, but there are other conventional plants where there is actually a 40 or 50 degree elbow going into the vessel.

DR. KELLY: But we don't try to solve three-dimensional momentum equations in the pipes. Those are 1-D momentum equations, and elbow comes in pretty much in two ways, the one Steve alluded to, and it changes your elevation change per unit length run of the pipe. But the other is, you input a loss coefficient for the elbow.

MEMBER WALLIS: There is nothing about the flow regime, and we know that when stuff goes around a corner it centrifuges the liquid.

DR. BAJOREK: Not yet.

MEMBER WALLIS: None of that is in there at all?

DR. BAJOREK: Not yet, no. There is nothing like that.

But we are working with NRR -

MEMBER WALLIS: What I miss in this whole literature when you present these codes is a discussion of the kind of nodes you have, and what things happen in them which you are not modeling.

CHAIRMAN BANNERJEE: It's coming, right?

MEMBER WALLIS: Is that going to be in your documentation eventually?

CHAIRMAN BANNERJEE: Isn't John going to talk about that?

MEMBER WALLIS: Is it going to be in the documentation? It's going to be a fair presentation, saying these are the kinds of things we're modeling; these are the aspects of them which we are modeling; and these are the things which we are not modeling, but which really happen.

Are you going to present things like that in your documentation? Because I didn't see any of it now.

CHAIRMAN BANNERJEE: Honesty is great. You can get away with everything if you just say what you can't do.

MEMBER WALLIS: That's right, and then people will make it more credible. Otherwise people will say, well, how about this? And how about that? And why didn't they do that?

CHAIRMAN BANNERJEE: The problems you often have, at least you'd have with me or with equivalent people, is if you claim too much.

You know we know what these codes can do more or less. They can't be everything. So that's fine. I think we should clearly say what it can do and what it can't do, and what it does badly, and where model dimension effects are important.

It doesn't mean that the code is not useful, but you may as well acknowledge it up front.

DR. BAJOREK: Okay.

MEMBER WALLIS: That's the whole problem. You say here are some differential equations. You write them down as if they were Navier Stokes equation, everyone believes them. And it gives the impression that this is some sort of an exact representation of the physics, whereas we know that there are all sorts of things that are not represented.

Just make a clean breast of it at the beginning, and it would help.

CHAIRMAN BANNERJEE: Are you almost done, Steve?

DR. BAJOREK: I think so.

One of the things in comparison to the past, we think we have a very large exhaustive assessment matrix. Even though code is not going to be perfect, we think we have enough information in our assessment matrix to help us identify where there are deficiencies, where there are problem areas you should be aware of.

We want to use that then as a means of prioritizing model changes to the code; where we need to make improvements.

We think that we have automated the process. We think that in the future we are not going to get into this situation of having the documentation lag by months or years. We are going to be able to turn this around.

MEMBER WALLIS: What makes you think that the future will be any different from the past?

MR. MURRAY: There have been a lot of - in terms of maintaining the documents, there have been a lot of I'd call more mechanical or technical glitches with the documents and how they are stored electronically; a lot of equations coming over from Macintosh to PC have gotten jumbled; and have made it difficult for us to have a developer update the document at the same time.

And once we have a framework for the documentation that we feel comfortable in, I mean as a code caretaker, I view it as may job to ensure that when a code update comes in, I better see some concurrent documentation that comes in at the same time that really updates that.

And I have been - because of some of the other technical glitches, we have to sometimes make a tradeoff on progress versus completeness. And that's not going to be a problem in the future that I see.

DR. BAJOREK: Then finally, with respect to bringing this code into agency use, we are trying to go that last step, making sure there are plant input decks available, there is tools like SNAP available, so that a new user can get the training, and he has the things available in order to process his input and output in an effective manner.

MEMBER WALLIS: Do you have experience with training people?

DR. BAJOREK: With training people?

MEMBER WALLIS: You say you are going to have training workshops.

DR. BAJOREK: We have been conducting re-up training for several years.

MEMBER WALLIS: But that's the code you are throwing away. So how about TRACE workshops?

DR. BAJOREK: We had the training workshop back in April.

MEMBER WALLIS: And there is evidence that people learn quickly and effectively?

DR. BAJOREK: What would you say would be suitable evidence?

MEMBER WALLIS: That they can use it after so many days or hours or weeks or years and get the same answers that Joe Kelly gets.

DR. BAJOREK: Hopefully they leave the workshop with a - being able to use it.

MEMBER WALLIS: But do they? Is there ever an instance when they can't?

MR. CARUSO: Success would be if they came to us for a power upgrade with a TRACE calculation.

MEMBER WALLIS: Yes. Success would be if they used it and they came to us with the results.

MR. CARUSO: There's power upgrades coming up, Susquehanna and Hope Creek this summer. If they came to the committee with a TRACE calculation, that would be success.

CHAIRMAN BANNERJEE: That might be - if not that, something like that would be a good objective.

MEMBER WALLIS: But the real thing would be, if we asked them in the morning and they came in the afternoon.

CHAIRMAN BANNERJEE: That sounds a little ambitious.

DR. BAJOREK: That may be, but we think that within about the first quarter of this year we are going to have that capability - not turning it around in the very morning. But there will be no excuse not to use TRACE to look at those types of audit calculations.

MEMBER WALLIS: Now the brief experience that fluent has is that you have to have a lot of customer service; you have to hold the hands of your customers, who are always having trouble using the code.

And unless you have that, you can't be in business.

CHAIRMAN BANNERJEE: That's of course Fluent's business.

MEMBER WALLIS: Not just saying that we have Fluent. Without the customer service, it wouldn't work.

CHAIRMAN BANNERJEE: And charging them at least $250 an hour, at least $500.

MEMBER WALLIS: But you are going to have the customer service NRR, presumably. And you have to probably dedicate some people to doing that. I don't think you have the people.

CHAIRMAN BANNERJEE: These guys are actually trying to make a code which works. Fluent's objective is a little different. It is how much money.

DR. BAJOREK: I don't want to go there.

MEMBER WALLIS: You are being much too cynical.

DR. BAJOREK: We try to work closely with NRR. We don't just throw this code over the wall and let them suffer the consequences.

MEMBER WALLIS: Well, I wouldn't underestimate the amount of interaction you are going to have to have with them as they use it.

CHAIRMAN BANNERJEE: I have a slightly different question. You guys have been supporting quite a bit of research, and at least eventually that research needs to make its way into TRACE or something equivalent.

One of these is the interfacial area. So what we haven't heard right now from you is, and maybe we can hear later, we don't have time today, what is your five-year plan for this code if you like? When are we going to see all this work with interfacial area, work that VJ -- and company are doing on boiling and --

DR. BAJOREK: We put together -

MEMBER WALLIS: -- all that sort of stuff.

DR. BAJOREK: We put together a roughly a five year plan about a year ago. First goal is to get 5.0 out. Beyond that versions which we might refer to as 6.0 or 7.0, to turn on the droplet field; make improvements to the other constituent models; make use of the data from UCLA for subpool boiling; RBHT for reflood; a number of those models that Joe Kelly talked about this morning.

Beyond that, a version like 8.0, more four to five years out, is when we would take advantage of that information from Purdue to put in the interfacial area track.

So we have thought about that, but at this point we are trying to go full speed ahead to 5.0 on this.

CHAIRMAN BANNERJEE: I think what you are trying to do is fine. But we have never even heard from some of these programs, like Ishii has been happily working away on interfacial area for the last 10 years I think and never appeared in front of ACRS to tell us what he's been doing. And you guys have been funding him, presumably because of TRACE.

So I think even if you don't appear with a version of TRACE with this stuff in it, we'd like to know where you are.

DR. BAJOREK: And that's something that we can rectify, and present the status of the work at a future ACRS meeting. But at the moment we don't have a contract for the Thermal Hydraulic Institute. We are issuing a new RFP pretty soon, and we'll see how the contractor is.

MEMBER WALLIS: But we have already -

DR. BAJOREK: Generated data for certain - well, that contract is ending now.

MEMBER WALLIS: Can we at least get the status of that?

DR. KELLY: Yes, and as Steve said, that's probably going to make its way into TRACE 8.0, and that's when having an elbow in a pipe will make a difference, because there will be a model for what happens to the interfacial area as a two-face flow goes through the elbow. And then downstream of the elbow, it has to recover.

It turns out it recovers fairly quickly, at least from the data we've taken so far. But yes, that's showing you the progress in the program is certainly something we could plan for a future meeting. We'd be happy to.

CHAIRMAN BANNERJEE: Right, it doesn't have to be that you have new work done. Where does it stand right now?

DR. KELLY: Okay.

DR. BAJOREK: That is something we'd like to do, and I think several of us are actually anxious to start looking at those programs, and looking at other model improvements once we get the 5.0 workout.

MEMBER WALLIS: Have you got it documented, as you are closing out the old contract, have you got all this stuff well documented?

DR. KELLY: Yes, we get annual reports from the Institute as well as individual data reports from each test series, and those are archived.

MEMBER WALLIS: Okay. So it won't be lost?

DR. KELLY: No.

MEMBER WALLIS: Maybe we should make this one of the programs that the ACRS reviews. We review three or four as you know, the history of the ones we've looked at every year. Instead of looking at the ones you tell us to look at, maybe we should look at this one because we're curious about it.

DR. BAJOREK: I keep suggesting - well, Bill Shack isn't here today - I wanted to suggest the chemical effects program as one to look at.

MEMBER SIEBER: He'll be here tomorrow.

DR. BAJOREK: Okay.

CHAIRMAN BANNERJEE: We've already looked at it some in the past. But you know we've looked at UCLA work. We've worked at Penn State work. And we've been generally quite happy with what we've seen in the past.

It's this effort which has been going on for a long time, and which we are quite interested in, because we'd like to see the progress, and we haven't seen it at all. And potentially it has a high impact.

DR. BAJOREK: Okay. So with that I'd like to turn it over then to -

MEMBER WALLIS: So you say you are going to schedule a presentation on Ishii's work?

DR. BAJOREK: If you would like that, we'll try to work that in.

MEMBER WALLIS: Yes, that would be good.

DR. BAJOREK: Because Joe mentioned that is going to have to wait until we do get the contract.

CHAIRMAN BANNERJEE: But even if you don't have the contract, I mean whatever has been done up to now you can talk about that.

DR. BAJOREK: Okay. Even if you don't have the contract, I mean whatever has been done up to now. You can talk about that. I mean even if Ishii doesn't come up, because it doesn't cover his travel, you guys can tell us.

MEMBER WALLIS: Now are you held up by this continuing resolution business in awarding these contracts? Is that part of the fall out of Congress taking forever to get through its budgeting process?

DR. BAJOREK: Yes, and no. I don't think that that will hurt us on 5.0, but when it comes to making progress with some of the future enhancements that we want to make, the SNAP, the Thermal Hydraulic Institute, dealing with advanced reactors, a lot of that assessment, any model development that might be associated with that, that could get impacted by the continuing resolution.

CHAIRMAN BANNERJEE: I should turn the

chairmanship over to you .

TRACE MOMENTUM EQUATION

CHAIRMAN BANNERJEE: John, are you going to now delve into the momentum equation?

MEMBER WALLIS: Is someone going to tell us how you go from differential equations to finite control volume?

DR. MAHAFFY: That is the purpose of this talk, the momentum equation was singled out because it's more complicated.

MEMBER WALLIS: It's more difficult.

MEMBER WALLIS: But even with something that is a scalar like energy, if you have funny shaped nodes of different sizes and shapes, then there is a question about how you do upwind differencing and so on, and how you do some of this averaging with the betas and the Ws and -

DR. MAHAFFY: The betas and the Ws are, that's really not finite volume. That has to do with your time leveling.

MEMBER WALLIS: Well, ridiculous averages - if you have a small node and a big node, to take some average which is independent of the ratio of the size of these nodes doesn't seem to be right

DR. MAHAFFY: I'll talk about that a little bit as we go through what I'm about to show you.

MEMBER WALLIS: Maybe I should just be quiet and listen to what you have to say

DR. MAHAFFY: Well, no, don't be quiet. Because what you have to say is very important.

Part of your problem with the documentation is that I haven't really shifted on full time to rewrite the documentation on the fuel equations. So that anything you want to contribute in terms of structure or content to the fuel equations section of the documentation you are certainly welcome.

Just as an aside, while I'm up here, and waiting, in terms of Ishii's work, as we speak I have a graduate student sweating, putting in some of Ishii's results in a version of TRACE. It's something not funded by the NRC. This is internal money from my laboratory. Just for fun to see what happens.

So that information will be available to the NRC as they move forward and try to make additional plans. It's just a massive thesis; it is nowhere near a complete piece of work.

CHAIRMAN BANNERJEE: Enlighten me a little bit about this, John, because in most fuel codes, where they use some sort of Euler or LaGrangian approach, the equation for the problem is a density function for the drops or whatever.

It's effectively an interfacial area equation. Because this usually is written in a way, let's say F is the probability density function between a certain velocity and a certain spatial location. So you write the usual type of equation, with a sink and a source function for coalescence break up, collisions, et cetera.

And this has been used in -- Williams work was done in `59. So what is so new about this? This is used in coats like Fluent or god forbid StarCD and everywhere else --

DR. MAHAFFY: There is nothing particularly new. Joe Kelly can talk to you about the things they did in COBRA in the late 1970s with droplet fields. All of this is well established. All we are doing is trying to take the results of experiments that Ishii did to come up with appropriate source and sink terms in your area transport equations.

CHAIRMAN BANNERJEE: That's all it is

DR. MAHAFFY: That's the bottom line. There is no whiz bank technology in terms of the fuel equations. In fact, what we're doing, the droplet field is implemented right now in a yet-to-be version of TRACE. The updates are still in the holding bin. That is not implemented as a single droplet field.

You can put - if you want to spend enough hours waiting for your computer to come back, you can put in 20 or 100-droplet fields. And what we are doing with the -

CHAIRMAN BANNERJEE: But that's in an Eulerian context?

DR. MAHAFFY: That's in a Eulerian context. We're not doing anything - we are not trying to superimpose LaGrangian calculations here. These are Eulerian interfacial area and associated mass equations with your whatever field.

But with minor modifications, that now can be changed so that those fields are not droplets but they are bubbles. And with a little bit of extra work there you can have some bubbles, you can have some droplets, and track as many of these as you want.

CHAIRMAN BANNERJEE: The problem with that, it's a little digression, is that the areas are vectors in a sense. So if the area is normal to the flow, or parallel to the flow, they have a different effect in terms fo something like drag.

So this is why I wanted this whole subject aired, before we go too far with this

DR. MAHAFFY: That is another level of resolution. I don't see us getting there in a reasonable way. It's just some kind of an average effect.

CHAIRMAN BANNERJEE: Right. With drops and bubbles I can understand how you can do this. But with a generalized area, whether it's parallel to the flow, or normal to the flow, you are going to have a slightly different - so this is why it's better to talk to us early than late

DR. MAHAFFY: This is why I tell my students right now that two-faced flow is a great field to get into, because I can guarantee there is still a lifetime left of research left to do.

MEMBER WALLIS: Right

DR. MAHAFFY: All right. First, getting somebody to pay for it.

MEMBER WALLIS: It will come back. All this nano bio.

DR. MAHAFFY: Yes.

Let me draw you a volume, and we are going to do a momentum equation here.

One thing to notice -

MEMBER WALLIS: So this is all one dimensional? It's all going in one direction

DR. MAHAFFY: We are going to do it in a generic way so that you can interpret it as one dimensional or three dimensional, and we are going to have to talk around the issues of what happens if your pipe is bad.

MEMBER WALLIS: What about going into a lower plume, say, where there is a huge change in geometry?

DR. MAHAFFY: Yes, I've got a little bitty abrupt area change in this. I'm pointing at the wrong thing.

MEMBER WALLIS: Also I have a problem when you say, the center of the volume. I don't quite know where to draw A1 and A2

DR. MAHAFFY: A1 and A2, these are my flow boundaries.

MEMBER WALLIS: They go through the center of gravity or the center of volume of the volume, and they are perpindicular to the flow, so you know which way the flow is going and all that

DR. MAHAFFY: We can talk around the issue of perpindicular. In this case I've drawn them as perpindicular, and we will work in that sense.

If you get into the case, in TRACE there are two modes for sort of non-straight one-dimensional flow. The traditional TRAC approach is that let us say at area two, when you tack on the next volume, there is a bend there, and it moves into another volume, and you've got to think about what that then means.

If it were RELAP-5 - or excuse me, RELAP-5 bends here at the corners. In TRACE it actually bends here at the center plane.

MEMBER WALLIS: Okay, in this pair you've drawn this little tapered thing

DR. MAHAFFY: I was just trying to draw some generic -

MEMBER WALLIS: So that taper, we continue that taper back. Is A1, where is A1? How do I know where A1 is in that cone that's coming in

DR. MAHAFFY: It's where it's drawn.

MEMBER WALLIS: How do I know where to put it in evaluating my - it's somewhere? It's just somewhere?

DR. MAHAFFY: What's happened - good question - what I was trying to say before we wandered off on this train is that in TRACE you have to think about two different classes of volumes, because we have a staggered mesh.

There is a set of volumes I haven't drawn, and -

MEMBER WALLIS: I think you should draw the whole volume.

DR. MAHAFFY: There is another - what you do is, you follow this taper on out here -

MEMBER WALLIS: That's the J volume, that thing.

DR. MAHAFFY: And that's my J volume, and that is a mass volume.

A1 is running through the geometric center, in terms of distance, not in terms fo volume weighting.

MEMBER WALLIS: Just in terms fo distance

DR. MAHAFFY: That's the way we think of it.

MEMBER WALLIS: But it makes a difference

DR. MAHAFFY: Only when you get to the level fo CFD.

MEMBER WALLIS: What happens if A1 cuts A3? I can ask an infinite number of questions

DR. MAHAFFY: That's right, and we can get into infinite amounts -

MEMBER WALLIS: Well, you see, that's the problem I have, I see these very simplistic definitions, and I can think of about 10 questions right away about how do you do it.

CHAIRMAN BANNERJEE: I guess what would be useful John is to - unfortunately we don't have a board here - maybe you could just sketch - because it's a staggered mesh, what would be notionally a mesh where you show the staggered mesh as well

DR. MAHAFFY: You want me to draw something up here?

CHAIRMAN BANNERJEE: Yes, if you just draw this.

MEMBER WALLIS: I understand that for a straight pipe. I understand the Js and the J plus ones.

CHAIRMAN BANNERJEE: Since it's a staggered mesh, just to show the other mesh

DR. MAHAFFY: We can do that. It's not directly relevant.

One thing I want to address, Graham, is that as you see how the equations develop, there is not anything in there that talks about exactly where A3 is. If A3 overlaps -

CHAIRMAN BANNERJEE: You can do it without A3 now.

DR. MAHAFFY: We could do it without A3.

MEMBER WALLIS: But if I have ECC injection

I want to know -

DR. MAHAFFY: Okay. So let's work in -

CHAIRMAN BANNERJEE: Forget the A3 for the moment.

DR. MAHAFFY: I'm going to draw you a similar picture. These are two of my mass-energy values. And I'm going to write some center point here. I'm going to be vague about that. We tend to think about it geometrically -

MEMBER WALLIS: Yes, but the user needs to know where to put it.

DR. MAHAFFY: If you look at what's done in TRACE, what it says is that this point is halfway along whatever this axis is.

MEMBER WALLIS: So it's a curved pipe, it follows what? It follows the metal

DR. MAHAFFY: If it's a curved pipe, we will talk about this briefly, you could think of me going into some very odd new coordinate system. It was a curved coordinate system.

MEMBER WALLIS: Well, suppose it comes in in the X direction and goes out in the Y direction. Where is the middle?

DR. MAHAFFY: Where is the middle?

MEMBER WALLIS: When I go into the lower

plenum, I come down the downcomer, and then I go out horizontally. How do I draw the middle of something which has a kink like that?

DR. MAHAFFY: If you're in the vessel, that's different, because you really do have a three dimensional cell structure, and the cells have a -

MEMBER WALLIS: In the lower plenum

DR. MAHAFFY: Yes. I mean if you look at the vessel in TRACE, you have a very clearly defined geometry of your cells. There is no ambiguity there.

MEMBER WALLIS: I'd need to see the cells that you have in the lower plenum to understand that.

DR. MAHAFFY: What happens - let's finish this one.

CHAIRMAN BANNERJEE: Yeah, why don't we go one thing at a time.

DR. MAHAFFY: Let's do one thing at a time.

So here's what I've done for a momentum cell.

MEMBER WALLIS: So it's all one dimensional. You know where to draw the perpendiculars, right

DR. MAHAFFY: Yes. I'm working in one dimension here. What I'm going to do to finesse this is say, okay, you believe there is a bend in there, I'm going to construct some kind of distorted -

CHAIRMAN BANNERJEE: But let's go forward with this.

DR. MAHAFFY: Okay. So this is where we are -

MEMBER WALLIS: And there is no W power when you do the averaging?

DR. MAHAFFY: If you are interested in Ws, that's another lecture, and that has to do with your choice of time differencing, time leveling.

MEMBER WALLIS: But as I read the instructions, it says that there is a Y J plus a half, which isn't just the average; it's a W J plus a half of Y J.

DR. MAHAFFY: That's a particular -

MEMBER WALLIS: It doesn't appear in here

DR. MAHAFFY: No, that's the SETS method.

MEMBER WALLIS: That's something else altogether?

DR. MAHAFFY: That has to do with your choice of time leveling in the SETS method, all right? That has nothing to do with your breaking up of space for finite volumes.

MEMBER WALLIS: I have a problem here, because this Y J plus a half, which is defined in the

SETS method is different from the J plus a half value you are going to use in this balance that you are going to talk about?

CHAIRMAN BANNERJEE: Let him finish this. Because what you are going to do is convect, let's say a property like density or enthalpy is put at the center.

DR. MAHAFFY: It's at the center here.

CHAIRMAN BANNERJEE: All pressure

DR. MAHAFFY: All thermodynamic variables are at the centers fo what I'm calling my mass and energy variables.

CHAIRMAN BANNERJEE: So H, P and Rho are there?

DR. MAHAFFY: Yes.

CHAIRMAN BANNERJEE: And the flow rate

DR. MAHAFFY: Flow rates are here. The proper term is velocities.

CHAIRMAN BANNERJEE: The velocities.

CHAIRMAN BANNERJEE: So what is Row in this J plus a half finding?

DR. MAHAFFY: Well, that's the interesting question. You have to define - that comes to the heart of your definition of your differencing scheme, and how you are deciding your order of accuracy and your differencing scheme.

CHAIRMAN BANNERJEE: So now, that's not at the center of mass of this, it's at the center of the G coordinate?

DR. MAHAFFY: Yes, it's at the center of the G coordinate.

CHAIRMAN BANNERJEE: So if you think about that upstream node as a mixing volume, the density or enthalpy or pressure, is it supposed to represent whatever is for that full volume

DR. MAHAFFY: I'll address this here as we move through the approximation, but we are, within TRACE, and this gets to your worry about publishing in journals, we do not publish anything we do in TRACE in any respectable fluid dynamics journal, because it is first order accurate space.

CHAIRMAN BANNERJEE: Tell us what you do then

DR. MAHAFFY: First order accuracy basically says that, yes, in effect what I'm assuming is -

MEMBER WALLIS: Engineering journal? I don't understand why it's not publishable in an engineering journal.

DR. MAHAFFY: Oh, you go to any fluids

journal, and they talk about their requirements for publication of both numerical methods developed and computations using a numerical in that, that they do not want you to use first order methods.

There is an exception to that.

MEMBER WALLIS: You can publish it in other journals which are engineering based and utility based.

DR. MAHAFFY: As I said, SETS was published in 1982 in the Journal of Computational Physics, and that was before people got off of first order methods, and into higher order methods, and so it was perfectly acceptable then.

But -

CHAIRMAN BANNERJEE: Let's carry on from this point.

DR. MAHAFFY: Yeah, this point, think of it as a geometric center. There is a density here, and at first order one way of interpreting what the first order method of approximation is, I assign a value here, and I'm going to treat it as if it's constant over this entire volume.

And I assign some values here, and I'm going to treat them as being constant over this entire volume. That is, in terms fo kind of modern numerical methodology, I'm choosing a basis function here.

MEMBER WALLIS: But then when you have the other volume, which is the composite of the two, it's got two pieces of volume, it's different

DR. MAHAFFY: It's got two pieces, and you will see me do that in some of the integrations I go through.

I've got a density that is constant over --

MEMBER WALLIS: But how do you work out a d rho dV or something if it's not constant throughout the volume?

DR. MAHAFFY: I'm doing an integration.

CHAIRMAN BANNERJEE: More problematic is with the pressure, right, when you want to drive the flow across the boundary.

DR. MAHAFFY: Yes.

CHAIRMAN BANNERJEE: I may accept the density and the enthalpy being uniform, but what word would the pressure -

DR. MAHAFFY: What I'm going to do, and you will see me do it, is that I am going to sit down and I am going to look at an integral over this volume of a void fraction times the gradient of a pressure, DV.

MEMBER WALLIS: But I know that if I didn't have the void fraction there, this would be the same as the integral PDS. But if it was averaged with an alpha in there, I'm not quite sure what it is

DR. MAHAFFY: Well, and you fit the one point of what we do. We take this alpha and we pull it out, with some mean sense -

MEMBER WALLIS: How do you pull it out

DR. MAHAFFY: It's one of these things, it's this business that everybody does, and you will see me do it here, at some point in any of these methods you talk in a low voice very quickly so people don't hear you -

MEMBER WALLIS: It's unacceptable

DR. MAHAFFY: - and you say, the average of the sum is equal to the sum of the averages, or the average of the product is equal to the sum of the -

MEMBER WALLIS: But we know that's not true

DR. MAHAFFY: We know that's not true.

MEMBER WALLIS: So the question is, how good is it?

DR. MAHAFFY: The question is, how good is it.

CHAIRMAN BANNERJEE: Okay, let's see what you do. You pulled out the alpha

DR. MAHAFFY: Yeah, we pulled this out.

I'm going to show you this integral in a minute. But what happens if, if you look at this -

MEMBER WALLIS: Which volume are you going to integrate it over now?

DR. MAHAFFY: I'm going to integrate it over this volume?

MEMBER WALLIS: So what are you going to do about that slice of wall there, where it says perpindicular up there?

DR. MAHAFFY: Oh, that is just part of the integration.

MEMBER WALLIS: But you don't know what the pressure is up there?

DR. MAHAFFY: That's the key.

MEMBER WALLIS: That's the key

DR. MAHAFFY: The fundamental assumption, and I'll talk to this later or we can talk to it now, I told you what my assumption is. I told you that I've assumed that the pressure is constant over this entire volume.

My first order of approximation basis function tells me that over this entire volume, I've got a constant pressure.

MEMBER WALLIS: That is very untrue though. You have a pressure recovery

DR. MAHAFFY: That's right. That's right, and we'll talk about what it means.

CHAIRMAN BANNERJEE: Anyway, you assume that pressure is acting on those walls.

MEMBER WALLIS: I thought that wall goes with the downstream volume and not the upstream volume?

DR. MAHAFFY: This wall is going with this volume, right?

MEMBER WALLIS: But we know that when a jet comes out of a hole, the pressure near the jet is the pressure upstream pressure, not the downstream

DR. MAHAFFY: Absolutely true. But you know it's more complicated than that. And I'm running out of space to go up, but if you think about it, I've got a jet coming out of here, and it follows some trajectory, and there's a reattachment point if you will, right?

And the hole -

MEMBER WALLIS: There's a vortex up in the corner or something.

DR. MAHAFFY: I've got some recirculation pattern here in the corner. And if I knew this profile, and I made some assumptions about what my density was doing, I could make a pretty decent estimate of what my pressures were all the way along this line, just playing some games with Bernoulli's equation.

But I don't know what that reattachment point is. This is a problem that we're not going to resolve at the level that we do finite volume work in trays.

MEMBER WALLIS: Now you've got two phases, too. So the liquid is on the wall, the gas isn't. But somehow the gas knows what the pressure is on the wall? I mean there are all sorts of questions like that.

DR. MAHAFFY: You can ask the questions, and what I'm telling you is, if you want to resolve the questions in terms of what's going on in the pressure profile along this wall, you've got to be doing a full up -

MEMBER WALLIS: You see, it would really help me in your preamble to this whole code if you'd draw things like this and explain what's going on so we could see what kind of assumptions are being made. Then we'd know what's going on.

When it's presented as sort of differential equations, and it's obviously true, that really doesn't help me very much

DR. MAHAFFY: Well, we admit that the differential equations are only true to a point too.

MEMBER WALLIS: But the differential equation doesn't apply to a control volume. So it's a huge bridge there that's missing

DR. MAHAFFY: No, and I'll try to bridge it.

MEMBER WALLIS: All right, and I better be quiet and see what you do.

DR. MAHAFFY: No, keep talking. But my problem is, I'm not taking notes fast enough.

MEMBER WALLIS: There will be a transcript

DR. MAHAFFY: There'll be a transcript, and we'll work from that.

But what I'm trying to tell you, if I could do CFD, okay, I'd be okay. And this people talked earlier about state of the art, and does the NRC try to do the state of the art.

If you want to know what the state of the art is right now, it's the Neptune project, where people are trying to set up to do full up two phase CFD for an entire reactor and a reactor transient.

And they are honest enough to tell you that it's going to be 20 years in the future before they can do one of these things, both in terms of the development of the physical numerical model set that goes into the software, and in terms of the computer hardware that will let you solve all that mess. It's a long horizon item.

We're here wanting to finish this story. And if you'd like, it's probably a good idea in the manual, if we are going to start drawing these pictures, what we can do is talk about some approximations here.

If you think about it, my first order assumption, where I've got this constant pressure over this entire volume, that is going to induce the lease losses.

MEMBER WALLIS: Then there is nothing to change the momentum of the fluid going through there, too. It's okay.

CHAIRMAN BANNERJEE: The put a loss factor on it.

DR. MAHAFFY: That's exactly what we do. Because we have no physically based model to tell you what this pressure profile is here, as we go through this detached zone of the flow -

MEMBER WALLIS: You put in a loss factor

DR. MAHAFFY: - we put in a loss coefficient.

MEMBER WALLIS: But the loss coefficient has nothing to do with these differential equations. It's a macroscopic -

DR. MAHAFFY: It's a macroscopic engineering correction.

CHAIRMAN BANNERJEE: You know, to move it forward, I think what we will do today is, we will go forward and try to understand what you do, without necessarily saying - we are not going to comment too much.

I understand exactly what you are doing, for better or worse.

MEMBER WALLIS: You understand? I don't understand. This loss factor, if I look at the differential equation, it has an FI and a FW in it. Once you've integrated a differential equation - presumably the loss factor is some kind of an integral of FW in there. Otherwise it's not in the equation at all.

DR. MAHAFFY: If you want to be physical about it.

MEMBER WALLIS: Oh, I want to be physical about it. So this FW or something, that's where the loss factor would come from, as a pseudo integral FW?

DR. MAHAFFY: This is something -

CHAIRMAN BANNERJEE: For a single phase case it would just be FW.

DR. MAHAFFY: If you want to really talk about this in one more level of detail, we could have started - I'm one step beyond Navier-Stokes equations here. And what I've trying to capture is the idea that - you see I don't have my stress tensor in here at all any more.

What I am telling you is that in TRACE and in all of its cousins in reactor safety, all the way on through KATAR, you do not worry about certain aspects -

MEMBER WALLIS: Well, does the loss factor come from the delta P, the grad P, or from the FW, or some combination of those

DR. MAHAFFY: The loss factor -

CHAIRMAN BANNERJEE: This is an integral of an average solved equation. So the gradients are gone. FI and FW are the interfacial drags -

MEMBER WALLIS: Gradients haven't gone because they are multiplied by an alpha. So your loss factor is some kind of - it's a combination of the grad P term and the VFW term

DR. MAHAFFY: The less factor, really, the best way to look at it is physically is that I simply have not gotten the pressures at the wall correctly.

MEMBER WALLIS: Okay, so the grad P, that term is not properly -

DR. MAHAFFY: That's the best way to think about it, because if - it's influenced by other things that are happening with the detached flow and whatnot.

AS you are probably well aware, there is a classic way to derive the loss coefficient for an abrupt expansion. It's a simple integration momentum equation, like I've done here, but what you do is, you assume that along this wall the pressure is equal to the pressure here, in the narrow part of my flow channel.

And if you assume any kind of flow like this, that's a decent assumption. And if this thing is perpindicular to my centerline, it's a really good assumption.

CHAIRMAN BANNERJEE: You use the mechanical energy and the momentum equation.

MEMBER WALLIS: And the distribution of alpha has nothing to do with it

DR. MAHAFFY: No, alpha is -

MEMBER WALLIS: Alpha is inside the integral here. So that has nothing to do with it

DR. MAHAFFY: It's going to have an effect, too.

MEMBER WALLIS: So that's also captured in the loss coefficient somehow

DR. MAHAFFY: And you think of that as, it's probably your two phase multiplier on your loss coefficient, all right?

CHAIRMAN BANNERJEE: You had a question, now if you introduce the A3 there, what happens, in the previous slide?

MEMBER WALLIS: I'm not sure I want to have anything to do with that. I just have problems with the integral you have written down there, how that turns into momentum balance for a control volume. Is that actually going to appear somewhere eventually?

DR. MAHAFFY: Yes.

MEMBER WALLIS: Because you have got all these integral signs in all these slides, but you don't have anything that tell me how to evaluate J1, J2 and all that stuff.

DR. MAHAFFY: Well, that comes down towards the end.

MEMBER WALLIS: Still all differential equations. And that really isn't what you're analyzing. You are analyzing control volumes

DR. MAHAFFY: Yeah, but look what happens.

As I told you, the purpose of this exercise is to deal with the volume integration, the finite volume methodology. I am not trying to deal with your choice of time level.

MEMBER WALLIS: Why don't you just start with the volume? Why do you start with differential equations? Start with the volume balance of some sort.

DR. MAHAFFY: The differential equation is basically here.

MEMBER WALLIS: I know. Then you integrate it to get a volume.

DR. MAHAFFY: I integrate it over my volume.

MEMBER WALLIS: And you've got something which you could have got most of the terms by writing them down.

DR. MAHAFFY: That's right.

MEMBER WALLIS: And then you've got some other terms.

DR. MAHAFFY: Which terms do you want to talk about.

MEMBER WALLIS: Well, I don't know, because I don't see the results yet.

DR. MAHAFFY: Okay, all right.

So we start - all I've basically done here, I've basically done a volume integral over an differential equation -

MEMBER WALLIS: You've taken an initial equation -

DR. MAHAFFY: This was your divergence term, and I've turned it into a surface integral. This is your integration, your pressure. This is momentum transfer due to the phase change. This is your basically your sheer stress at the interface. This is your washer stress contribution.

And down here we've got gravity.

MEMBER WALLIS: So the A3 would appear in that second interval?

DR. MAHAFFY: The A3 is right here, and you can see down here what I do is, now say, okay, this is a -

MEMBER WALLIS: And this is a vector equation?

DR. MAHAFFY: This is a vector equation as written now.

MEMBER WALLIS: And you are going to turn it into a scalar equation of some sort

DR. MAHAFFY: We don't get there with this derivation, but if you think in terms of the alignment of the primary axis, yes.

But here is the term that we sum over three different areas. There could be 10 different areas here that are leaking, we'll run them in.

Okay, now what I do, it's a classic maneuver, I'm trying to convert this to a form that we normally use in TRACE, and part of that is processing the mass equation. And this is -

MEMBER WALLIS: And if I have this hufta thing, or the hutze or whatever it is, momentum comes in at an angle and then it's diverted along the pipe. This is an external force that changes the momentum. If I just drew your box here I wouldn't have that.

The fact that I have a hutze dynamics is different, isn't it?

DR. MAHAFFY: You've lost me on that one, but go ahead.

MEMBER WALLIS: Well, the direction of the momentum that affects the flow depends on whether there is a hutze in there or not. And if you just draw a control volume it doesn't tell you whether there's a hutze.

DR. MAHAFFY: What I haven't done, really -

CHAIRMAN BANNERJEE: Next picture, you can have another one.

DR. MAHAFFY: I have some entry here, but it may be because the pipe is coming in like that.

MEMBER WALLIS: There may be a hutze in there or something that determines it, and that's an integral of a pressure on a surface of some sort

DR. MAHAFFY: Yes.

CHAIRMAN BANNERJEE: The problem is, you are trying to take very 3-dimensional things and trying to make them one dimensional.

MEMBER WALLIS: That's one of the problems

DR. MAHAFFY: Anyway, we are going to process in some mass equations here to convert from a fully conservative form of the momentum equation to a nonconservative form, all right?

That's what I'm doing here, okay.

MEMBER WALLIS: Make the substitution

DR. MAHAFFY: These are the mass equations, on two different volumes that are adjacent. It's hard to read the font, but it's integrating over the right side and the left side. That's what we go in here -

MEMBER WALLIS: Then you combine these two in some way?

DR. MAHAFFY: And we are going to do that. I'll just change some notation. This is a repeat of what I wrote before, but instead of showing some integrals, I've started putting bars over things.

I'm going to rearrange the mass equations, and I'm going to solve for my mass fluxes at the right face and the left face of my momentum cell; mathematical games, that's all we're doing here.

I'm going to look at this business of averages, and this is where I say that, okay, we are going to make some assumptions about the average of a product being the product of averages, and I note that this doesn't always work.

If you are thinking single phase flow, and you look at a momentum flux in an integral sense with a standard fully developed turbulent profile, what will happen is that this assumption will give you about a two percent error roughly.

MEMBER WALLIS: So you are going to take the average of grad P in there? These are all average things now, are they

DR. MAHAFFY: Yeah, the average bar - I've just dropped the bars, and in dropping it, in places, we are basically saying average of product -

MEMBER WALLIS: I don't think under average grad P, though. You are going to turn it into a surface interval.

DR. MAHAFFY: You are going to do that.

MEMBER WALLIS: But you can't average it first. You've got to take the interval, grad P -

DR. MAHAFFY: We'll go down and do that in a minute.

MEMBER WALLIS: But you've already averaged it here. It looks to me.

DR. MAHAFFY: I'm going to back off that, all right. I'm going to take this product and re-express -

MEMBER WALLIS: Go back to the interval and re-express that.

DR. MAHAFFY: We'll do that in a minute.

MEMBER WALLIS: Maybe you should leave it as interval PDV.

DR. MAHAFFY: We can do that.

So -

MEMBER WALLIS: On the pressure, are both phases the same?

DR. MAHAFFY: The pressure on the left side of the momentum volume is different than the pressure on the right?

MEMBER WALLIS: On both phases, the liquid and the gas. Are the liquid and gas at the same pressure?

DR. MAHAFFY: Yes. Two phases -

MEMBER WALLIS: Which they don't have in the bubble.

DR. MAHAFFY: Yes.

Okay, so this basically, we're changing the notation. You got to be honest about this, even if you are in single phase flow. I've made a mistake here with this product business if I'm in laminar flow.

You can do the derivation and you will find that this guy is getting a near result from a momentum transfer that's about 25 percent low for any momentum flux turn.

CHAIRMAN BANNERJEE: Most of this is what Bird, Stewart and Lightfoot were, you take the profile, and you can get injectors and all sorts of things.

MEMBER WALLIS: That's just for single phase. And that's for straight pipes

DR. MAHAFFY: When we're doing these kinds of averages, again, these are the mass equations here that I'm going to be doing -

MEMBER WALLIS: Do you have some simple examples showing that this works? I remember George Batchelor was very critical of this stuff. And he had some examples, an exact solution to a single bubble, showing that this gave all the wrong answers.

Have you faced that sort of criticism and got answers to it? Because I think you have answers to it.

DR. MAHAFFY: We've got answers to it in that it all boils down for a bubble to how you choose this little fellow up here, okay.

If you choose that right, you will get the right bubble rise in a vertical standpoint of liquid.

MEMBER WALLIS: But I mean there is the classic thing, if you take a bubble in a pipe and you simply hit it with a hammer, the bubble moves three times the distance of the pipe or something. You would never predict that from here.

CHAIRMAN BANNERJEE: He doesn't even have -

(Simultaneous voices)

MEMBER WALLIS: So a lot of classic problems, which the hydrodynamicists like.

CHAIRMAN BANNERJEE: You oscillate this -

MEMBER WALLIS: That doesn't work

DR. MAHAFFY: No, if I've got to oscillate a bubble, if I've got to rapidly accelerate a bubble, it's not there.

We talked about this last time -

MEMBER WALLIS: There should be a discussion about the fact that it doesn't matter, because it's only when you have acceleration bigger than a certain order of magnitude that this term is bigger than the drag force, or matters at all

DR. MAHAFFY: That's right.

MEMBER WALLIS: And even in a flashing flow through a nozzle, the drag force usually tends to dominate the added mass term.

CHAIRMAN BANNERJEE: It doesn't in near critical flow.

MEMBER WALLIS: Well, that's what I'm talking about, too. Unless you have an orifice or something, very rapid.

DR. MAHAFFY: There are very limited exceptions. This is something, NRC is very PIRT based. If at some point virtual mass becomes a critical phenomena, in some kind of analysis, then we go back and we install it in this case, and we use it.

MEMBER WALLIS: So what you are doing is, you are combining these in a way that you can actually combine the differential equations to use a continuity equation with an overall momentum equation to get an equation of motion. Is that it

DR. MAHAFFY: That's what we're doing. We are headed for an equation of motion.

MEMBER WALLIS: You can do that at the differential level without integrating anything

DR. MAHAFFY: Yes. And what happens is, when I combine everything in here with my mass equation into my momentum flux terms, I get a revised summation over my three momentum flux terms here, and you will see that it simplifies into a rather curious form. It appears in trays. There is in effect your on axis V delta V term, and here is the contribution from the momentum being injected from your sidelight here, your area three phase.

MEMBER WALLIS: So you are playing with the momentum flux?

DR. MAHAFFY: Yes.

MEMBER WALLIS: And you are assuming that the vaporization all occurs at the velocity of each phase. Therefore, this is as irreversible as possible essentially?

DR. MAHAFFY: Yes, this comes in just from the way I've substituted -

MEMBER WALLIS: Because in reality the phases have the same velocity at the interface, where the evaporation occurs. There is no reversibility due to evaporation in reality

DR. MAHAFFY: The reality here, there is a difference between reality and what really happens.

MEMBER WALLIS: Say that again

DR. MAHAFFY: Yeah.

MEMBER WALLIS: Say that again.

CHAIRMAN BANNERJEE: For the public record

DR. MAHAFFY: It's on the record already, folks. There is a difference between reality and what actually happens in TRACE, RELAP or any of a number of codes, and it's right here.

I tried to capture what you just said in this term. We have got a velocity at the interface that's being transported across.

MEMBER WALLIS: That's the velocity at the interface, there?

DR. MAHAFFY: Yeah.

MEMBER WALLIS: Oh.

DR. MAHAFFY: This is not a velocity of a gas. It's not a velocity of a liquid as written.

MEMBER WALLIS: But we need to know what it is.

DR. MAHAFFY: But to be fair, if you look at any of these codes, what they will end up doing is, for that velocity at the interface, if it is boiling, that will be set to the liquid velocity. It will be set to the vapor velocity.

And if it's condensing - I'm doing this backwards - yeah, in this case on the vapor side. If it's condensing it's going to be set to the vapor velocity.

MEMBER WALLIS: I think some codes, actually I've seen them, have one where it's the average velocity of the phases at the interface

DR. MAHAFFY: You can do that.

MEMBER WALLIS: I can do it, but I mean I don't know whether it's good or not, but some codes do it.

DR. MAHAFFY: What it amounts to, if you think about it, if I choose the extreme as depending on the direction of my phase change, either going with the bulk liquid or the bulk gas philosophy, what I am doing is I'm basically saying there is a portion of this integrated pure stress at the interface that I am not going to account for. I'm subsuming it into this change in usage of the velocity.

Now people don't do that correctly, I'll admit that. But the basic idea is that if you look at what's going on here, there is enough uncertainty in that that whatever happens here is small potatoes.

MEMBER WALLIS: Condensation on the drops in the flow pulls the boundary there in, retards separation and reduces the drag. And that is not necessarily in your code.

Okay, so this looks like a Bird, Stewart and Lightfoot type of thing.

DR. MAHAFFY: What we've done is, we've come up with - this is a set of surface integrals now combined with the results of volume integrals that tell me what these surface momentum fluxes transfer to.

And when I take - if I take what I've gotten here, okay, and I start combining these with some similar terms, okay, this guy was already present in my original equation, and now I'm taking these two that have appeared because I've folded in my mass equation.

And I look at it. And yes you can argue about details, or the means of this or that, but basically what I've ended up with is an expression of what -

MEMBER WALLIS: That's what Bird, Stewart and Lightfoot do at the differential equation.

DR. MAHAFFY: You do it at the differential equations. I'm just doing it rigorously within the context of the finite volume formalism that we've ended up with.

And a similar thing will happen now as I deal with the momentum transfer due to phase change. All these terms come together into a single term that looks like this.

So we've got a rated phase change times the difference between the interface and the bulk velocity of the gas.

MEMBER WALLIS: Which is all charged to the vapor in evaporation and charged to the liquid in condensation.

DR. MAHAFFY: Right, one of the reasons you go with these extreme interpretation, where keep on condensing, if I am taking vapor away, and I set this to the gas velocity, what that is doing for me is that if I botch my interfacial drag, at least by the act of removing vapor, I'm really not affecting the mean velocity of this mass. And that's really a decent physical assumption in terms of what's going on there.

So I combine all of these things, and I end up with a set of equations that look like this. I've got my time derivative here.

MEMBER WALLIS: And the gas and the liquid all have to be going in the same direction. And it's still one dimensional is it

DR. MAHAFFY: Well, there is nothing that said the gas and the liquid are moving in the same direction, no. But what I'm telling you what my momentum transfer terms look like. This is my main long access transfer term. This is what is coming in from the side.

Here is the action of my pressure, and I haven't faced up to that yet, and the rest of my assorted terms.

CHAIRMAN BANNERJEE: These are still all vectors, right?

DR. MAHAFFY: These are still vectors. They'll be vectors all the way to the end here.

Now, here's where - I've got the words here, we talked about this earlier, the fundamental constant pressure over a volume assumption in our first order approximation.

One other thing we do do -

MEMBER WALLIS: Wait a minute, the second term there, the VG2 minus VG1 is simply the flow rate through the thing times its change in velocity

DR. MAHAFFY: Yep.

MEMBER WALLIS: Doesn't it allow it to have a different flow rate out than it has in? Because really the momentum out is affected by a change in flow rate out from in. Here you've simply got the flow rate times a change in velocity

DR. MAHAFFY: That's right.

MEMBER WALLIS: - there's a change in flow rate.

DR. MAHAFFY: Think about what's happened. Think in terms of the differential equations -

MEMBER WALLIS: Just think of this A3. This A3 is giving in some momentum

DR. MAHAFFY: Yes.

MEMBER WALLIS: And that means - and then some mass, so there is more mass going out than comes in. I would think that has to appear in that second term somehow; there is more mass coming out than going in.

DR. MAHAFFY: It's done here.

You know you need to go through it, and that's one of the reasons I went through this step by step -

MEMBER WALLIS: It doesn't seem right

DR. MAHAFFY: It may not seem right, but that's why we went through all these steps.

MEMBER WALLIS: It still doesn't look right

DR. MAHAFFY: That part of it is. There are other bits and things where you may want to -

MEMBER WALLIS: AC is what

DR. MAHAFFY: AC is the area at the center of my momentum cell, at the J plus one half position.

MEMBER WALLIS: So you are really saying that the flow rates in and out are characterized by the flow rate in the middle.

DR. MAHAFFY: Yep. Think about what happens when you go in the differential form from a fully conserved momentum, to a motion equation. And you go to this V del V formulation. This looks an awful lot, if you think about it, like our old friend V del V.

MEMBER WALLIS: And it's assuming that things aren't changing too rapidly. I mean if stuff comes in as steam and goes out as liquid, you are in all kinds of trouble.

DR. MAHAFFY: In principal it's taken care of down here.

MEMBER WALLIS: Okay.

DR. MAHAFFY: The face change terms were in the derivation, and they all ended up down in this term.

Okay, so we've talked about constant assumptions. One thing we haven't talked about, it's kind of a basis function, is because we can have discontinuous changes in area -

MEMBER WALLIS: So this is delta AC. AC is a vector?

DR. MAHAFFY: Yep. Area.

MEMBER WALLIS: So I need to know where to draw it.

DR. MAHAFFY: In principal, if I've constructed my coordinate system right, this dot product is really just the product of the magnitudes of these two vectors.

MEMBER WALLIS: Where is AC in your figure on the left here? Is it the area of the inside? It's the outside?

DR. MAHAFFY: It's the area right here.

MEMBER WALLIS: Why is not the big one, going to the outside? What isn't it the big one at that boundary, the one that goes -

DR. MAHAFFY: Oh, from here to here?

MEMBER WALLIS: Yes.

DR. MAHAFFY: Because there is no flow here or here.

MEMBER WALLIS: It's just simply the area of the middle that you are talking about

DR. MAHAFFY: It's the area available to flow.

MEMBER WALLIS: Is that what it has to be

DR. MAHAFFY: Yes.

MEMBER WALLIS: I thought it was simply the area of the middle of the volume

DR. MAHAFFY: This area across -

MEMBER WALLIS: Suppose I had a slightly tapered ball, then I'd draw it half way up that, wouldn't I?

DR. MAHAFFY: Yes, you would. But I've got a discontinuity.

MEMBER WALLIS: Suppose I didn't have a discontinuity; I just had a taper. Then it would be the actual physical area, wouldn't it

DR. MAHAFFY: Yep. This is where my momentum -

CHAIRMAN BANNERJEE: - assumption.

MEMBER WALLIS: I think there is some assumption there.

CHAIRMAN BANNERJEE: I don't know if -

MEMBER WALLIS: I'm not sure we're making much progress.

CHAIRMAN BANNERJEE: I think probably what is worth doing is writing down all the assumptions, and reach the point that you reach, and -

MEMBER WALLIS: Have it peer reviewed.

CHAIRMAN BANNERJEE: - and have it peer reviewed.

MEMBER WALLIS: Because it's got to be robust. And it may well be that it is.

CHAIRMAN BANNERJEE: And it's okay if it's first order -- because you are making certain approximations, it seems to me that it has some effect on accuracy, but if it's adequate, who is going to complain

DR. MAHAFFY: This is how we got there.

CHAIRMAN BANNERJEE: But I think it needs to be written out in a way which can be looked at by somebody or some group of people and they can say, okay, given these assumptions, given the objective of reaching this accuracy level, everything is okay.

And I think that is all that needs to be done here. And the next step that you haven't taken yet is to actually take it from a vector form, that one can follow what you are doing I think, with the velocity becoming a scalar form, where despite the fact the velocity is changing direction - I guess that is something which you want to discuss

DR. MAHAFFY: What you end up doing to get to the scalar form is that you assume that on the average this vector is aligned with the center line of your glove. And then you go from something that, by the time you get to the bottom line, each of these vectors, this vector ir orthogonal to this vector - or not orthogonal, it's parallel -

CHAIRMAN BANNERJEE: Let's say the center line if it was curving now, let's say, then the velocity vector is continuously changing, so you've got something like a centrifugal force obviously

DR. MAHAFFY: But think about the centrifugal force; that's a good point.

CHAIRMAN BANNERJEE: I mean it would act on the walls of the pipe.

DR. MAHAFFY: It's acting on the walls, and where it ends up - we were actually talking to somebody who has done two-phase flow simulations in helical heat exchangers, and it's an interesting problem.

What happens is that you are going to end up - this is all along the primary direction, so your centripetal force terms don't feed into this momentum equation per se, except that what happens is they impact this little character here, and they also cause you to introduce terms that look like terms in horizontally stratified -

MEMBER WALLIS: What is the primary

direction in something like a Y junction?

DR. MAHAFFY: You have to pick one.

MEMBER WALLIS: That's the whole problem we had with Retrend. Retrend tried to do it, and they got into all sorts of difficulties. And they actually had examples which claim they gave the wrong answer. It's very simple examples, like just flow around a bend. They had a bend which behaved like a pump. Which didn't really make any sense. That's what they predicted.

CHAIRMAN BANNERJEE: Up to this point, everything you've done, within the limits of the assumptions you've made - averaging and first order everything - in some way is completely okay.

Now you have to move down to this point, and I think that's where I get lost. Up to the other point I'm with you.

DR. MAHAFFY: You get lost beyond this equation?

CHAIRMAN BANNERJEE: I can write the equation, but I don't know I can tell those vector equations into scalars. I can take a dot product itself and write a kinetic energy equation. That actually makes sense.

MEMBER WALLIS: So you are with this

business of the flow at the middle times the velocity difference?

CHAIRMAN BANNERJEE: Well, it's not with it, I simply think that it's a set of approximations you've made by averaging all of those volumes. They lead to -

MEMBER WALLIS: The problem is - I've learned this - that when you average two different things, you are making two different assumptions in those equations.

When you combine them to get something new, the thing you get sometimes has a completely spurious thing, which is simply a result of value average.

CHAIRMAN BANNERJEE: Yes, distribution coefficients are being ignored.

MEMBER WALLIS: Right. Then you start treating that as if it's real.

CHAIRMAN BANNERJEE: But if you write down the assumptions, within those assumptions, you have to be consistent. So you've come up with something. You've made an assumption that your distribution coefficients are like turbulent flow flat essentially, I think, assumed some things regarding the distribution of density, pressure, ends up being those control volumes.

And you have come to a conclusion based on those assumptions. You can write that down probably four or five of them.

But now you are going from a vector equation to a scalar equation, the only way I know how to do it is to take a dot product itself. There is no other way to do it that I know of.

So if you do that, what happens? You get a mechanical energy equation, correct

DR. MAHAFFY: Right.

CHAIRMAN BANNERJEE: Where would you go from there?

DR. MAHAFFY: Well, it's not fully developed. I didn't finish it before this meeting, and I wanted to go through the momentum side anyway.

The file that I gave to Ralph on the end of this, in fact, we can scroll down and see, I've actually gone through the same derivation based on the kinetic energy arguments, and you end up at roughly the same point.

CHAIRMAN BANNERJEE: But you have got an equation which we are agreeing to, let's say. Now you can take your dot product or whatever of that

DR. MAHAFFY: We can do the dot product of that on the center line.

One thing - let me back off and address this question of a Y more seriously.

If you wanted to do a Y, and I'll be blunt, TRACE cannot do it right now, because it only admits one side junction with an angle other than 90 degrees. This is just a limitation in the code that we've never had high enough priority to fix.

If I had to do a Y junction, here's what would happen. This character here would be zero. As would be my A1, and I would have two flavors of side junction coming in with these terms that would be vector quantities, and I would treat the angles with respect to my primary direction for my V2, my V center, in an appropriate way.

MEMBER WALLIS: I really think that term is wrong.

CHAIRMAN BANNERJEE: You have to check it

DR. MAHAFFY: You can check the math. I believe it does drop out correctly. I've been through this derivation from a number of different angles.

MEMBER WALLIS: I think it has to do with the way you're averaging. And then you hit something spurious about it, because you are letting the velocity in the middle be something which then isn't really there. And then it's typical, assuming that the velocity in the middle is typical of the velocity going in and coming out in that second term, and it isn't.

CHAIRMAN BANNERJEE: And it could be more the one on the left hand side.

DR. MAHAFFY: Take a look at this, and I'd appreciate any comments you want to send to me. Obviously you want to think about it in a little more depth.

MEMBER WALLIS: This is actually the basis of TRACE?

DR. MAHAFFY: This is -

CHAIRMAN BANNERJEE: The vector equations seems right.

MEMBER WALLIS: No, but this equation with the VGC and the VG2 minus VG1 -

DR. MAHAFFY: Yep, that is the basis.

CHAIRMAN BANNERJEE: And you've just done this now into a scalar equation. That's all

DR. MAHAFFY: And we turn this into a scalar equation. Think of it as dotting everything into a unit vector along the center line.

CHAIRMAN BANNERJEE: Dot Z

DR. MAHAFFY: Yep. That's what happens.

CHAIRMAN BANNERJEE: As long as it's a straight pipe. All right. And as long as that A3, I don't know what you do with that. But leaving that term out -

DR. MAHAFFY: Well, again, if it's not a straight pipe, I redefine my coordinate system.

CHAIRMAN BANNERJEE: That's where you start to lose me.

MEMBER WALLIS: What if it's a T

DR. MAHAFFY: If it is a T, then there is a combination of two straight pipes, and this particular thing I'll do in the primary side of my T, and this term right here takes care of the contributions from the T side junctions to the flow in the primary side of the T.

MEMBER WALLIS: Why does it only go to the VT1 in there?

DR. MAHAFFY: It has -

MEMBER WALLIS: Suppose all the gas comes in the side, then it has no effect? Well, we could go on like this for far too long.

CHAIRMAN BANNERJEE: Yes. I mean what is the concrete action coming out of this? I think one action is that we are very interested. It's a look at your handout. Sends you comments. And we do that at least up to the point of this one last shocker, because we haven't really had the time or perhaps the energy to go through the rest.

And Mary, you haven't even developed the scalarization of this vector equation

DR. MAHAFFY: No. Everything you see here has been pulled together fresh since somebody sent me an email, what was it, last Wednesday.

MEMBER WALLIS: So I need to see this really in the form of a draft final document rather than a set of slides, because I think there is more to it that you just presented here.

And I'm very concerned about this, because here we are, this is the foundation of your whole code, and we are still arguing about whether it's valid or not.

This should have been resolved years ago.

DR. MAHAFFY: It was.

MEMBER WALLIS: If it had been resolved, perhaps, and we're revisiting it, then we don't know how it was resolved. So maybe you should document how it was resolved.

DR. MAHAFFY: It goes back to the peer review process. Everything in TRACE, as you say, went through the code review group, and was peer reviewed at the time.

Did they miss something? Perhaps. This is my attempt at cross-checking it from a completely different direction and getting the same answer.

MEMBER WALLIS: I assume I'm a fairly typical representative, a fairly knowledgeable mechanics person with some experience. And I assume that somebody else with my similar experience would look at this in the same way. It has to be credible for those kinds of people.

CHAIRMAN BANNERJEE: From a macroscopic controlled volume point of view, most people would appeal to, say, the macroscopic balances and something like Bird, Stewart and Lightfoot, where you, let's say have a bend. You have it coming in; you have it going out. So then you dot your velocity with your area, which is a bend on each side, and you get a force on the bend.

MEMBER WALLIS: It doesn't look like this, though.

CHAIRMAN BANNERJEE: I don't know if it's been done for a two-phase flow, but for a single phase flow -

MEMBER WALLIS: But then you've got the force on the bend. There is no force on the bend in this equation.

CHAIRMAN BANNERJEE: You'd get the force on the bend. But that's all right. You may say, I don't care about the force on the bend

DR. MAHAFFY: Well, I do care about it, but that goes back to something I said up front, and that is, that we have varied all of the effects of the force of the bend inside these engineering loss coefficients that you can think of as being hidden off down here in our F sub W.

MEMBER WALLIS: If you look at Bird, Stewart and Lightfoot, when they analyze a monometer, where the momentum on one side is the opposite direction from the other, and there is a bend, they are very careful not to use any momentum equation; they use an energy equation, because they don't know how to do it with the momentum equation

DR. MAHAFFY: I understand that.

CHAIRMAN BANNERJEE: They use a mechanical energy.

MEMBER WALLIS: Yes, they don't know how to do it with momentum.

DR. MAHAFFY: You probably don't remember, but the last time that I was in front of this subcommittee, what I said was that when you are in one-dimensional modeling of fluid flow within a piping system, the correct approach is to talk in terms of the kinetic energy equation rather than in terms of the momentum equation because your momentum really is not a meaningful quality until you nodalize that thing up to a full three dimensions with a CFD solvent.

And that's why I went on beyond this with a kinetic energy derivation which is not really finished.

CHAIRMAN BANNERJEE: But I think we have seen various points we could have tried this kinetic energy derivations, and with certain assumptions about distribution coefficients, that they are flat, they will begin to start looking like momentum equations -

DR. MAHAFFY: That's right.

MEMBER WALLIS: - with loss factors and with force terms. These force terms sort of get stuck into loss factors. And but it will be good if it was systematized, put down so that we understand what assumptions go in. Maybe they have to be flat distribution coefficients. So then you get rid of these VQ terms of V squared and stuff, okay. Whatever it is. But let's get these assumptions straight. Get all the equations straight. So we at least know under what circumstances the equations work.

There could be circumstances they don't work, in which case we know at least what their limitations are.

So if there are very bad distribution coefficients, they are probably wrong. It doesn't matter. We assume they're flat. And we move from there.

But it would be good if we got it all down. I think this is a good start. You've started to move in this direction. And we just need to finish the job.

And we need to probably ourselves look it over, give you some feedback. But it needs to get finished.

DR. MAHAFFY: Well, like I said, January is my month to write all this up in the documentation of the field equations.

CHAIRMAN BANNERJEE: Once we've got this done, and at least our committee has taken a look at it, you've got our feedback, and then I think the next step will be when we get the whole document down it will be peer reviewed.

MEMBER WALLIS: This VG2 minus VG1, is that part essential to TRACE?

DR. MAHAFFY: This?

MEMBER WALLIS: That term, I was having trouble with, is that essential to TRACE

DR. MAHAFFY: Yes, that is your V del V term in your motion equation. If you look at that, this is difference -

MEMBER WALLIS: It's a V del V, but when I integrate it, I have to get the velocity in - I have a different mass flow going in than coming out. When I actually do the control volume balance, I don't get a VGC. I have VG going in squared if you like, and a VG going out squared. I have a different formulation.

CHAIRMAN BANNERJEE: You're looking at it from the physics point of view.

DR. MAHAFFY: Again, go back to the Bird, Stewart and Lightfoot, wherever you want to go, or do it by hand yourself, purely, go to single phase flow to do a simple problem -

MEMBER WALLIS: Well, VGC is what, the average of VG2 and VG1?

DR. MAHAFFY: No.

MEMBER WALLIS: What is VGC

DR. MAHAFFY: That is the velocity -

MEMBER WALLIS: But my computation scheme only calculates it at certain points

DR. MAHAFFY: It calculates it here, here,

here -

MEMBER WALLIS: So VG2 and VG1 are averages

DR. MAHAFFY: VG2 and VG1 are averages, because we have yet to determine -

MEMBER WALLIS: So they really aren't defined except in terms of the VGCs on the other side of them.

DR. MAHAFFY: That's right.

MEMBER WALLIS: So that term doesn't mean anything to me yet until I see it related to what you actually use.

DR. MAHAFFY: Not until I specify an averaging technique. But within the context of my derivation, it still stands as a meaningful number added at the edge of a volume.

CHAIRMAN BANNERJEE: VG1, let's say, is notionally an average between VGC and whatever is upstream of one, and VG2 is notionally an average of between VGC and something which is -

DR. MAHAFFY: Let me be very explicit about what it is. It's in some of the words -

MEMBER WALLIS: This V del V thing is only true if you don't add mass along the way

DR. MAHAFFY: Yes, and what I'm trying to tell you, based on the derivation, this is the adjustment to it if you add mass along the way. Okay?

You've got to go through the derivation. You've got to do the derivation.

MEMBER WALLIS: We are spending much too much time on this.

CHAIRMAN BANNERJEE: Can we then -

MEMBER WALLIS: I'm very nervous that what you are going to get as a critique is something that you don't like.

CHAIRMAN BANNERJEE: That's okay.

MEMBER WALLIS: No, it's not okay. Because I want to sign off that this is a good code, and it's based on something that -

CHAIRMAN BANNERJEE: We'd all like to sign off that this is a good code. But do you want to finish up, John before moving on to the anonymous - do you have anything more to say on this one

DR. MAHAFFY: Well, we wandered around this quite a bit. I've got some specific comments. But they really aren't - they are details you can look at and talk about what's going on.

Because if you look, there is a step between where my derivation ended and what actually happens in TRACE. TRACE says, G, I've got an alpha rho G here, an alpha rho G here, and they cancel out, because TRACE originally came from a finite difference, rather than a finite volume derivation for these terms.

You will see something similar in the gravitational force terms.

So there are some things there that we need to talk about.

The other thing you need to know, it's in words in this, if you go back and look at it, I didn't talk to you about the basis functions for the velocities, and there rather than assuming velocity is constant over the volume, because we are admitting the possibility of discontinuous changes in area, what we take as the product of area and velocity, the volumetric flow, is constant over some volume stretch.

We used that - if you want to get back - we haven't even gone into the whole issue of upwinding and whatnot. But when I actually need to get a velocity here, I'm consistent with upwind differencing. So what I'm going to do is, I'm going to use the velocity upwind that's actually calculated to generate the velocity at this position. And I'm going to use my constant volumetric flow rate to go from a velocity at this position to a velocity there. That's my interpolator.

A similar thing going from the velocity here, which is calculated, to this edge of my momentum velocity thing.

CHAIRMAN BANNERJEE: Provided there is no inflow in between, right?

DR. MAHAFFY: Yep.

CHAIRMAN BANNERJEE: All right, John, I think we need to either take a short break, or how long do you have?

MEMBER WALLIS: Let me ask you about this anonymous thing.

ANONYMOUS LETTER

CHAIRMAN BANNERJEE: The anonymous thing. We need to do that. Do you want to do that?

MEMBER WALLIS: We need to do that. That's something you need to resolve. But I don't have an interest in whatsoever.

DR. MAHAFFY: I have been told by the NRC that this was an item that was directed from higher levels.

MEMBER WALLIS: But do we need to worry about it on this committee? I mean you guys are resolving it somehow.

MR. CARUSO: In a letter we used to transmit this to them, we asked them to tell us how we were to resolve it.

MEMBER WALLIS: I see.

MR. CARUSO: So we have to -- we asked for it, so --

CHAIRMAN BANNERJEE: Why don't we go forward until 4:30, see if we can resolve this by then, then we'll take a break.

MEMBER WALLIS: Well, we have an hour and a half scheduled for anonymous letter. I don't think it's worth anything like that.

CHAIRMAN BANNERJEE: No, let's do it for 20 minutes.

DR. MAHAFFY: I've got eight slides here, all right?

MEMBER WALLIS: Good

DR. MAHAFFY: Okay, in the anonymous letter, there are some fairly direct statements. One says, the approach in the code does not in any way represent a solution of any kind for the EOS. The approach is not mathematically correct. The linearization of the EOS, and does not represent any correct or reasonable engineering approximation.

MEMBER WALLIS: What is EOS?

DR. MAHAFFY: Equation mistake. And he breaks this up into three different categories.

MEMBER WALLIS: I think they are talking about how you represent the thermodynamic tables

DR. MAHAFFY: No, what he is talking about, let me be very specific about what happens. Here is what being contested. At the very end of a time step, after the stable - and this is only true for SETS. Remember the TRACE - well, it actually has three methods, but you only see two of them right now. There is the SETS method, and the semi-implicit method, in terms of your time leveling, that we haven't dealt with.

This is not relevant for semi-implicit, number one. If you choose to run the code in a semi-implicit mode, what's being discussed here doesn't happen. It's only in SETS. After you have completed the stabilizer, mass and energy equations, what happens is that the natural solution of those equations is to give you values for the products down here that I'm outlining, alpha rho G, et cetera. That's what naturally comes out of the conservation equations in SETS form.

We would like to retrieve from that, from the standpoint of really flow regime stability, a void fraction. As a sidelight, we could get pressures and temperatures, but we choose not to. They would only be used as initial gases in the next time step.

What we use as initial gases instead are the new time pressures and temperatures we get from the semi-implicit.

So we are going through here. We are trying to get ourselves a void fraction, and yes at the same time, we're getting estimates for changes in temperature and pressure.

And here is what's happening. We set up an equation set here, so I've got four functions of four unknowns. And as you know, the method of choice for this, since these are nonlinear expressions, is to go to a generalized Newton solution procedure.

So that's what happens. I go in here, and I generate a Jacobian matrix, based on my partial derivatives of each of these functions that I've written now.

MEMBER WALLIS: Excuse me, what does the EOS have to do with this?

DR. MAHAFFY: Well, this is an EOS. It then - this is the expression - I've chosen my independent variables -

MEMBER WALLIS: That's where the EOS comes in.

DR. MAHAFFY: Yeah. This is the equation

that stayed here.

MEMBER WALLIS: Was that the beta form?

CHAIRMAN BANNERJEE: No, it's how he solves it.

MEMBER WALLIS: It's how you solve it, okay

DR. MAHAFFY: And all I've done is, I've gone in here, and I've set up, this is a class Newton information. I've defined my function. So I'm saying, this as-of-yet to be determined product is equal to this number, right.

This is some expression I'm getting through changing pressures and temperatures and void fractions.

CHAIRMAN BANNERJEE: But all you are trying to find there is the void fraction, right

DR. MAHAFFY: All I really care about is the void fraction. I will get changes in pressure and temperature that I can use, too.

CHAIRMAN BANNERJEE: so go ahead

DR. MAHAFFY: So this is it. This is a Jacobian for Newton iteration. I think you should recognize that. And these will be my changes over the iteration. Here's my right-hand side.

These are my residuals.

CHAIRMAN BANNERJEE: And you try to drive it to zero? You iterate to zero

DR. MAHAFFY: We don't.

CHAIRMAN BANNERJEE: You just do one step

DR. MAHAFFY: All we do is a one shot. What we've done is, first of all, let me talk about the verification process for this particular operation.

CHAIRMAN BANNERJEE: Why is he saying what you are doing is wrong, though? Even if you just take one step.

DR. MAHAFFY: I'll try to get to that. But I think most people will agree that a Newton iteration is a well established way to solve nonlinear equations.

When this was originally implemented in TRACE, this was done as a full up iteration. And part of the verification process, we did really two things, and this is fairly standard within the way I operate.

Number one, these Jacobian elements were generated in two different ways. They are obtained think of it as analytically.

CHAIRMAN BANNERJEE: All by perturbation

DR. MAHAFFY: Okay, but every time I do a Jacobian anywhere, I always do a second backup with perturbation, and check to make sure that the two answers match. That's been done.

The other thing that was done was that during Newton iterations, the procedure through the iteration was observed in a number of representative cases, and you could see the classic quadratic convergence of the Newton iteration going on.

So we had confirmation that the Jacobean was okay, and that the Newton iteration was behaving the way it should.

As we worked with this over a period of months, what became obvious was there was really in this case no advantage to going more than one iteration. That first change in void fraction was all you really needed to get things to a reasonable approximation and quit.

So that's why right now there is just the single shot linearization out of it, just as RELAP-5, there is a single shot. It does the first in what would be a series of Newton iterations, and it's stops on the full equations set. This is just a local.

Now, here's an alternative way of doing things. And this is part of I think the assertion that things were incorrect.

The author of the letter went through an example where in effect he worked on an inverse problem, and in a very simple example, which was single phase ideal gas flow, and in the inverse problem, basically, okay, well, we had this equation of state. We're going to invert the equation of state eventually.

What I'm going to show you eventually is that for this equation of state, if I go through the linearization that I just showed in a generic sense, I get a matrix equation like this. This delta rho and delta rho E is another way of expressing that function F.

And I solve this matrix equation, and this is the answer I get, analytic solution to the problem for one iteration.

CHAIRMAN BANNERJEE: That's exact

DR. MAHAFFY: Well, it's an exact solution to my first iteration. It turns out because of some linear behaviors it is an exact solution.

MEMBER WALLIS: This delta rho E minus E delta rho is the same thing as rho delta E is it

DR. MAHAFFY: This guy here if you think about it, from basic calculus, all that is is rho delta E, yes.

MEMBER WALLIS: And you cancel the rhos, and you've got delta E over CV, which is a first law

DR. MAHAFFY: That's right. That's what it should be.

I'm leaving it in this term, because the author of the letter chose to think in terms of independent variables are the product of rho E and rho.

MEMBER WALLIS: Okay

DR. MAHAFFY: I'm trying to preserve a consistency here.

So now here is the analogy, in a simpler form I admit to what was going on in the letter. I've gone in now and I've inverted that equation of state now. And I've said, okay, my pressure is a function of density, and the product rho e is equal to this.

And I invert my temperature, and here is what it is, as a function of rho E and rho.

And now all I do is a Taylor series expansion here.

MEMBER WALLIS: And you get the same answer

DR. MAHAFFY: And I get the same answer.

MEMBER WALLIS: No surprise

DR. MAHAFFY: No surprise.

MEMBER WALLIS: What's the problem

DR. MAHAFFY: I don't know.

CHAIRMAN BANNERJEE: He comes up with a

form of - I just read it which is a bit different from what he gets using your form, right, your linearization?

DR. MAHAFFY: He's got a difference.

CHAIRMAN BANNERJEE: Yeah, he's got a difference. And why is that

DR. MAHAFFY: I did not - that's why I went - I treated the problem a little bit different with simpler algebra.

CHAIRMAN BANNERJEE: But if you did his problem?

MEMBER WALLIS: Did you demolish his point somehow?

CHAIRMAN BANNERJEE: If you did it his way, do you get the same result as he does

DR. MAHAFFY: I did not do it in exactly his way because I was -

MEMBER WALLIS: You have to sort of demolish his argument as well as reinforcing his own.

CHAIRMAN BANNERJEE: Yeah, I think more like if you could treat his argument - his final result is right, right? There is no question about that

If all he did was your stuff, he would be wrong.

DR. MAHAFFY: One of two things have happened. Either there was an error in his algebra - well, there are three possibilities, but the third I don't believe.

Either there was an error in his algebra, or there is a relationship between thermodynamic derivatives in his more complicated way of manipulating things was not folded in to get a collapsed resolve.

The third possibility I don't believe. There is an underlying assumption in the letter that has to do with the relationship between Jacobians and linearization made for Cs, in a system of equations and an inverse system equations.

I have a vague recollection fo seeing a theorem somewhere on that subject, but you'd have to have that in hand to actually complete the argument on the other side.

But my first response, without even going through this exercise I did here, is that we have done a classic Newton linearization of the problem as we have defined it. It is a correct Newton method.

MEMBER WALLIS: It's not a problem of equation of state. I mean is he questioning your Newton iteration of products? Or what is he questioning?

DR. MAHAFFY: That becomes a little vague.

CHAIRMAN BANNERJEE: I don't have a printout of the letter. I have it on my computer

DR. MAHAFFY: There is a general statement that what we've done is mathematically incorrect. If you read the three specific assertions, it says the approach in TRAC TRACE is not correct and cannot lead to a correct analytic solution.

He's right, okay. We are not getting an analytic solution. We are iteratively solving a nonlinear problem. We are not generating analytic solutions. There is no doubt about that. We are not trying.

The correct linearization by use of implicit function theory leads to the correct solution. Well, if you've got the inverted equations of state, I don't argue that point.

MEMBER WALLIS: But it says the approach in TRAC TRACE is not correct. Something specific which is not correct that he fingers

DR. MAHAFFY: No, there is an example with a specific instance for an ideal gas equation of state where some things don't -

CHAIRMAN BANNERJEE: I'm getting the letter up.

DR. MAHAFFY: What I've tried to do is go through the example that was presented in a cleaner format. I reduced the equations so they were easier to follow and produce in terms of Jacobian matrices and whatnot, and you've seen the answers I've got.

CHAIRMAN BANNERJEE: He seems to recognize what you are doing in this letter. Writes these derivatives down for an ideal gas.

Can we get a printout of this letter that we can distribute? I have it on my computer.

MR. CARUSO: Sure, I'll print it out.

CHAIRMAN BANNERJEE: Okay, why don't we take a break now, and we print it out, and we just return to it after that.

So let's take a 15-minute break until 20 to 5:00, and then we go over this letter.

(Whereupon at 4:24 p.m. the proceeding in the above-entitled matter went off the record to return on the record at 4:47 p.m.)

CHAIRMAN BANNERJEE: Okay back in session.

So John, have you got a copy of this as well?

DR. MAHAFFY: Yes, sir, I have a copy in front of me.

CHAIRMAN BANNERJEE: All right. We want to consider it some more and get your views on it.

Do you want some time to read it

DR. MAHAFFY: No, I've read it. And what happened, basically if you look at it, he's done the problem from two different directions and obtained on his bottom line two inconsistent answers.

My attitude was, while he's introduced some functional relationships here in setting the problem up here that were needlessly complicated. So to cut down the potential of error, what I did was, I cast the problem using the ideal gas equation to state specifically, in a simpler more direct form, which is what you see in the presentation.

And I went through, solved the problem, and got consistent answers.

CHAIRMAN BANNERJEE: Right. But then what has he done in order to get a different answer

DR. MAHAFFY: And once I had solved the problem from a slightly cleaner perspective, I decided not to go through a find a specific error in his -

CHAIRMAN BANNERJEE: If there is one

DR. MAHAFFY: I've solved the same problem.

I've just cast it in the form of variables that are a little easier and less prone to errors.

MEMBER WALLIS: Can you find in his document here a specific place where he says that something is erroneous, that something done one way gives one answer, and done another way gives another answer, and these are incompatible?

Is there some bottom like that

DR. MAHAFFY: What happens is that he gets expressions for delta P and delta T, okay. His equations 19 and 20, all right. And then he goes through and, using the inverted form of the equation of state, he comes up with another set of expressions for delta P and delta P that are 32 and 33, okay.

And then the idea is here, well, let's inspect these results.

MEMBER WALLIS: Does it matter what these equations are? I mean the method is still the same, isn't it?

DR. MAHAFFY: No, the approach here, it's a useful cross check in that his first approach is the total analogy to the TRACE approach. He is going to sit down and start with an equation fo state in which the independent variables are pressure and temperature, and he is going to derive, based on all the expressions present, some change in pressure and change in temperature.

MEMBER WALLIS: Right

DR. MAHAFFY: From the Newton type linearization, and that's the end point down there.

MEMBER WALLIS: But there is something very different here. In node 19 he's got this N + 1, which is the next step or something? Is that what delta P N plus 1 means is the change in pressure from now until the next time; is that what that means?

CHAIRMAN BANNERJEE: It comes from the solution of 12 and 13.

DR. MAHAFFY: No, it's the change in pressure, change in the Newton pressure estimate, all right. Starting with whatever my current -

MEMBER WALLIS: So N + 1 is the number of iterations, N, or something?

DR. MAHAFFY: N + 1 is the time step level.

MEMBER WALLIS: And delta P at time N + 1 ahead of now, is that what that means

DR. MAHAFFY: N + 1 designates where I am located in time in my discrete solution to the partial differential equations. And what he is saying here is that I had some guess at my Newton pressure. Whatever it may be, that was my guess that I used to generate certain numbers here, including the derivatives that are written, as I say, in a form that I consider to be overly complicated, so that all this delta P is given my last guess at Newton pressure here is how I change it to get a better value of Newton pressure consistent with the values of Newton density and Newton density times Newton energy that have come out of my stabilizer equations.

So again, the problem at hand is, I know the Newton density, I know the Newton product of density and energy, and I want to infer from that a Newton pressure and a Newton temperature.

MEMBER WALLIS: It would seem to be a trivial matter. Just by using thermodynamics.

CHAIRMAN BANNERJEE: No, I think what he is saying is, if you look at equations 19 and 20, then what you have in there in the square brackets is, let's say rho bar minus rho - whatever -

MEMBER WALLIS: That's the delta rho then?

CHAIRMAN BANNERJEE: No, that is the quantity you are trying to drive to zero, right. And rho E over bar minus rho E they are trying to drive that quantity to zero.

DR. MAHAFFY: Yes.

CHAIRMAN BANNERJEE: And what he is saying is, it's not entirely clear. If you look down a little further on page 412, he says the numerical values for the density and internal energy are most likely the values from the previous time step

DR. MAHAFFY: And that is incorrect.

CHAIRMAN BANNERJEE: Okay, so the same is true for all the other state properties on the right-hand side of the equations above. Thus as noted in previous notes, the Newton solution in the codes will depend on whatever is told in these locations in the codes.

No attempt is made to update these values even once. That is I think the crux

DR. MAHAFFY: Those statements are entirely incorrect, okay.

CHAIRMAN BANNERJEE: I guess that is the crux of his argument. If you do what he is saying there, that you don't update those values, then you will get the wrong answer.

DR. MAHAFFY: Well, it's not that you get the wrong answer.

CHAIRMAN BANNERJEE: Or you won't get as good an answer, whatever.

DR. MAHAFFY: What's really interesting is that really the only discrepancy between the two equations are his assumptions of the initial gas values, and the values at which certain derivatives are evaluated.

CHAIRMAN BANNERJEE: Well, I guess if you want to clear up this matter, you have to deal with those statements that he makes.

DR. MAHAFFY: Well, let's clear this up directly. Let's go in and look at equation 19, and let's compare that to equation 32, okay.

If you look at that term by term, you'll have to look at equation 18 to see what this D is, and you will see - or actually look at 17, that gives you a better direct comparison - if you look at 17, that compares directly to the denominator term in equation 32.

As you go through and you interpret each of these terms here, the only difference is between equation 19 and 32 are the assumed time levels at which the initial guesses are made.

CHAIRMAN BANNERJEE: Exactly.

MEMBER WALLIS: What is this delta rho bar then in 32? How does that relate to rho bar minus rho PT?

DR. MAHAFFY: That's what it is.

MEMBER WALLIS: So it's the same equation

DR. MAHAFFY: It's the same thing. If you look at these equations they are the same. It's just why they are written differently, I don't know. The only fundamental difference between them is -

MEMBER WALLIS: A plus sign instead of a minus sign?

DR. MAHAFFY: Well, I think there was a minus sign hidden out there that is hard to tell, at the very - right after the equals in 19, I believe there is a minus sign, that is blurred into the divide by in the one over D.

MEMBER WALLIS: But it still doesn't look quite - because inside the square brackets you've got two terms that add in one case and are subtracted in the other. Yet they looked at the same form. So I don't quite understand that.

CHAIRMAN BANNERJEE: I guess equations 32 and 33, which he claims are the correct linearized equations, have to be compared with 19 and 20.

MEMBER WALLIS: That's right.

CHAIRMAN BANNERJEE: Which he claims are the equations which are used in the quote. But then he has the further point he makes that the Newton solutions - I'm not quite sure what is meant by the previous times -

MEMBER WALLIS: Looks like the same equation.

DR. MAHAFFY: Let me clarify that for you.

First of all, if you kind of ignore old time/new time evaluation, equation 19 is equivalent to equation 32 -

MEMBER WALLIS: Except for the signs

DR. MAHAFFY: Well, if you dig deeper, Graham, what you will see is that that sign is made up for -

MEMBER WALLIS: Oh, I see, it's CP minus -

DR. MAHAFFY: Yeah, that wasn't written down in an orderly way.

MEMBER WALLIS: Okay, you're right. I take back what I said.

DR. MAHAFFY: Those things are consistent, and you'll see a similar consistency -

MEMBER WALLIS: Looks like the same equation, it's just that you are evaluating things at different times.

CHAIRMAN BANNERJEE: If you are

DR. MAHAFFY: And the crux of the matter is that he is making an assumption that to get the derivatives that are used in the Jacobian and the initial gas at pressure and temperature, we are backing all the way off to old time quantities.

And that particular - first of all, depending on your time step size as to how good, bad or indifferent that particular starting point is.

MEMBER WALLIS: He seems to be concerned about whether or not you are converging to the answer.

CHAIRMAN BANNERJEE: No, you know what he's saying, that delta rho E over bar and delta rho - whatever he calls over bar -

MEMBER WALLIS: Should be implicit.

CHAIRMAN BANNERJEE: - should be found at the new time step.

MEMBER WALLIS: It's implicit.

CHAIRMAN BANNERJEE: Yeah, it's found from the whole Jacobian.

MEMBER WALLIS: That's what he's saying.

CHAIRMAN BANNERJEE: It should be found.

MEMBER WALLIS: He's talking about how you converge.

CHAIRMAN BANNERJEE: So what you should be solving for is delta P N plus 1, delta -

MEMBER WALLIS: In terms of delta rho and delta rho E.

CHAIRMAN BANNERJEE: Rho bar and plus one, it should be delta rho bar E. That's what I don't know. Let's get it clear what he is alleging. All the quantities, delta P, delta B, delta rho bar, delta rho E, delta rho E bar, should all be found at N plus one from the Jacobian -

MEMBER WALLIS: This is simply thermodynamics. They should be related at the same time.

CHAIRMAN BANNERJEE: They should be found, and they should be plugged in, and you get everything correct.

MEMBER WALLIS: How does this differ from 19?

CHAIRMAN BANNERJEE: But what he's saying is that delta rho bar and delta rho E bar are not being found from the Jacobian and being updated; they are old time step values. That's how I read it.

That when you take your famous Jacobian, go back to your Jacobian -

DR. MAHAFFY: Which one do you want, this one?

CHAIRMAN BANNERJEE: No, not that one.

DR. MAHAFFY: This one?

MEMBER WALLIS: It doesn't help us until we say, where - when you evaluate -

DR. MAHAFFY: Yeah, it's not -

CHAIRMAN BANNERJEE: Let's get back to this one. Alpha doesn't end here.

Instead of delta P, delta TG and CL is just T he's got.

DR. MAHAFFY: All of these elements to the extent if we - let's go back even to look at our function, these quantities - this quantity, this quantity, this quantity, these are all evaluated in the initial guess using results for the new time void fraction, new time pressure, new time pressure obtained from the semi-implicit step.

So they are not simply old time quantities. They have been updated by evaluation of the semi-implicit equations in the SETS method already to something that is new time level.

MEMBER WALLIS: When you are driving something to zero, do you want to drive to zero the change from the old time to the new time, that's what you want to drive to zero, isn't it?

CHAIRMAN BANNERJEE: But if you go back to your Jacobian there you've got, now let's replace that delta alpha by a delta rho bar, and delta TG let's say by delta rho E bar, and let's call delta T delta T, delta P delta P.

Now you have got a vector of four. And

you have a square Jacobian of 16 elements.

He is saying you should be solving for that vector all the deltas altogether

DR. MAHAFFY: But he is saying more than that.

CHAIRMAN BANNERJEE: And he's saying you're not.

DR. MAHAFFY: Well, what I'm not doing is using an inverted equation of state, which would give me basically my pressure as a function of density, and internal energy per unit volume. And I'm not.

MEMBER WALLIS: Does that have anything to do with it? I think it's just a question of when you evaluate these delta rhos, isn't it?

CHAIRMAN BANNERJEE: He said that all of them must be found together at the new time stages

DR. MAHAFFY: They are all found together. If you look at this, this is a fully consistent Newton iteration.

MEMBER WALLIS: So your rho bar minus rho PT is the same thing as his delta rho bar N + 1

DR. MAHAFFY: No, no.

MEMBER WALLIS: Look at 19 versus 32. The only thing at issue is this delta rho bar N + 1 being equivalent to what you call rho bar minus rho P comma

T, and I have to know what you mean by rho bar minus -

CHAIRMAN BANNERJEE: At which status step.

MEMBER WALLIS: Yes, when you evaluate that

DR. MAHAFFY: Okay. The rho bar is the result - it's a new time result from a slightly modified set of flow equations, the stabilizer mass and energy equations. And that is evaluated at a time level N + 1.

CHAIRMAN BANNERJEE: And rho bar E - and rho E bar?

DR. MAHAFFY: Rho E bar is again stabilizer mass and energy equations. It's a number.

MEMBER WALLIS: And what is rho PT?

CHAIRMAN BANNERJEE: Those are just equations of state values.

DR. MAHAFFY: Those are evaluated -

MEMBER WALLIS: When is P and T evaluated

DR. MAHAFFY: They are evaluated also at a new time.

MEMBER WALLIS: So everything is at new time just the way his is?

DR. MAHAFFY: It's coming out of a slightly different way of evaluating the flow equations. So they are not going to be exactly the same values as you get once you've solved this coupled set of equations.

So although these are formally from a numerical standpoint at the new time level, because the equations that generated them were different, their values aren't going to be the final values of basically pressure, temperature, et cetera that are consistent -

CHAIRMAN BANNERJEE: Because they're just coming out of this predictive step; not the corrective step?

DR. MAHAFFY: Yes.

CHAIRMAN BANNERJEE: Okay, I think that resolves really the issue. What - and is there much of a difference between what these values would be coming out of the predictive step compared to out of the corrective step?

DR. MAHAFFY: If there are, we have time step control; it drops the time step size.

MEMBER WALLIS: Well, presumably they are converging to the same thing.

CHAIRMAN BANNERJEE: Yes, I guess that's his issue. And he, instead of saying it's coming out of the predictive step, he's saying it's coming out of the -

DR. MAHAFFY: He thinks it's coming out of the old time, and it's not.

CHAIRMAN BANNERJEE: So I think we should clarify exactly what we're doing here. This is really up to you guys to answer it. But if what we have said today is correct, then I think we should write that down, and we should show how much if any the error is. I mean the error could be very small.

In any case, if as you say if it doesn't converge, you would - the Newton-Raphson in one iteration doesn't converge anyway.

MEMBER WALLIS: It may overshoot and all kinds of things.

The thing that bothered me, though, is that you said these rho bars and rho PT came from something else.

DR. MAHAFFY: Think of it as a corrector step on a set of -

MEMBER WALLIS: On some other equations

DR. MAHAFFY: It's the same flow equations. What happens is, in one case, in a semi-implicit flow equations, let's look at the semi-implicit mass equation, the density is being fluxed in the divergence term, del dot rho V. The densities are evaluated at the old time level.

When you go to the stabilizer equation,

your corrector, that density now is evaluated at the new time level. Everything else is the same.

That's how you perturb the equations, and that's how you introduce stability into the system.

MEMBER WALLIS: Can you program a simple example of your equation and his, and show what difference it makes?

DR. MAHAFFY: I suppose we could.

MEMBER WALLIS: I suspect it's going to make almost no difference.

CHAIRMAN BANNERJEE: Here you solve the whole Jacobian together without splitting it in two. What you are doing is, you are splitting the Jacobian into two parts, right?

DR. MAHAFFY: The Jacobian is what it is for the system of equations as I've chosen to define it with my choice of variables.

The one point I want to make, it's right here on my last slide, okay, he has selected, it's cherry picking, he selected a problem for which he can get a quick answer.

He's picked a problem where it's single phase, ideal gas. He's saying I know my - his particular inverted form of the equation is state, so it's easy. I don't have to invert a matrix at all.

I just generate a matrix. I know what the coefficients are. I'm done.

The point I want to make -

MEMBER WALLIS: It's a different answer from what you would get to the same simple problem

DR. MAHAFFY: No, if you look at it, from the standpoint -

MEMBER WALLIS: Because you use the single phase gas the same way he does, you get the same answer?

DR. MAHAFFY: As long as we start with the same initial gas, as long as the perturbation is off the same base point, then we get the same answer. We won't.

CHAIRMAN BANNERJEE: Let me ask you a question. Instead of this four by four inversion to get each of these four quantities, at every point, is a fairly trivial exercise. I mean you are not doing the Jacobian of the whole system

DR. MAHAFFY: No, that's right, it's only a pointwise solution.

CHAIRMAN BANNERJEE: A pointwise solution. So why did you choose not to do that? Because was it just because -

DR. MAHAFFY: Yes, I'll give you the exact answer to that, to do a rigorous multi-iteration Newton scheme, what has to happen is that on each iteration after the first I have to go back and evaluate my entire equation of state to get my derivatives of pressure, temperature. I did that.

The equation of state, relatively speaking, is fairly expensive.

CHAIRMAN BANNERJEE: If you could do that at time step N that wouldn't matter. The elements of the Jacobian could be done at time step N; there is no issue with that.

DR. MAHAFFY: I normally call that Newton-Raphson.

CHAIRMAN BANNERJEE: Yes, the first iteration of the Newton Rafson.

DR. MAHAFFY: You can lock your Jacobian and proceed with your iteration.

CHAIRMAN BANNERJEE: Everybody loves that.

MEMBER WALLIS: It's a step forward

DR. MAHAFFY: But let me tell you the practical implications of that. That particular approach to nonlinear equation solution works well when you don't have a high degree of nonlinearity.

Unfortunately, we have this thing called the saturation line in two-phase load problems. And the equation of state is very, very nonlinear -

MEMBER WALLIS: When you cross that

DR. MAHAFFY: Yes, and as you know, it's one of our salvations in two-phase flow, things tend to ride the saturation line fairly closely. For a large percentage of the time in these practical problems that we solve, we are operating at a highly nonlinear area there.

CHAIRMAN BANNERJEE: The derivatives have to be taken along the saturation -

DR. MAHAFFY: And so, if I wanted to do it right -

CHAIRMAN BANNERJEE: You'd have to take the derivatives along the saturation line

DR. MAHAFFY: I'd want to do that. But -

MEMBER WALLIS: Well, I would like you to get an explanation which would satisfy the critic, convince him that you have done it right.

CHAIRMAN BANNERJEE: It may be that if you explain what you have done and why. Because I can see why, if there is an issue with the saturation line, then the old time step values at N of the Jacobian could be somewhat misleading.

MEMBER WALLIS: Could lead to some sort of oscillation, where you jump over and back across the saturation line.

DR. MAHAFFY: You can fix that.

CHAIRMAN BANNERJEE: You can fix that

DR. MAHAFFY: You can fix that. But the critic is in a fairly narrow perspective on this. His - if you look at it, the two different pairs of answers really only different in the evaluation point for the elements of the matrix. And -

MEMBER WALLIS: Then you should explain that.

DR. MAHAFFY: And I am going to concede that in the ideal gas form of this, what he is doing is formally, number one, faster, and number two, more accurate in some sense, okay.

Within the order of accuracy of everything, it's not significant in terms of what is going on in the linearization, but it's certainly faster; if I have access to an inverted form of the equation of state.

Here's the point I want to make up here.

CHAIRMAN BANNERJEE: Clearly you don't have access to that.

DR. MAHAFFY: I could get it, but it doesn't matter, and let me show you why.

Let's go to the full two-phase flow

problem. I am going to write my functions again, but I'm now going to write them as functions of void fraction, rho G, rho GEG, rho L, rho LEL. Okay?

And if you look at it, it's a whole lot simpler. I mean this guy here is a primary variable. I don't have to access the equation of state for it. This guy is fine. There is a combination there. I still have nonlinearities because I have a product of alpha and rho G, so it's not a simple linear equation that is just going to drop out.

But there is something more important. If you look at this expression here, look at the number of unknown variables. I've got one, two, three, four, five of them.

CHAIRMAN BANNERJEE: Don't you need T somewhere in that?

DR. MAHAFFY: No.

MEMBER WALLIS: Those are not all independent variables though, are they

DR. MAHAFFY: Yes.

MEMBER WALLIS: How do you now know rho L if you know P and T?

CHAIRMAN BANNERJEE: If you know P and T -

DR. MAHAFFY: Here's what's going on, okay, when I set up my Jacobian at the beginning, embedded in that is a primary assumption in TRACE, RELAB 5 and most other places, and that is, pressure of the liquid equal pressure of the gas. It was there. You didn't notice it go by, but it was there.

MEMBER WALLIS: So if I know P and T, don't I know rho G, rho L, or EG and EL?

CHAIRMAN BANNERJEE: Only along the saturation -

DR. MAHAFFY: It depends on how you arrange your equation of state.

MEMBER WALLIS: Well, if, along the saturation line, if I know P and T I still -

CHAIRMAN BANNERJEE: If you know P and T -

DR. MAHAFFY: It's just a question of, with an equation of state I am totally free to select my independent variables and my dependent variables. It's just a question of how I generate my tables.

MEMBER WALLIS: Is this all relevant? We are talking about simply the difference between 19 and 32, it seems to me, which would seem a very simple matter to resolve without getting complicated.

DR. MAHAFFY: Well, the differences there are trivial, and I will concede in that example -

MEMBER WALLIS: Well, the differences are trivial, but he says the approach doesn't begin to have any basis whatsoever in mathematics or engineering. Now that is a pretty strong statement.

DR. MAHAFFY: I'm trying to tell you in a quiet way that he's wrong.

CHAIRMAN BANNERJEE: You must write a reply

DR. MAHAFFY: If you would like, if the NRC would like me to contribute a letter, that's fine. This is my reply in Vugraphs.

MEMBER WALLIS: Isn't the cure to say that 19 and 32 essentially are the same thing

DR. MAHAFFY: That's right.

MEMBER WALLIS: There may be some slight difference, but the difference doesn't make any difference -

DR. MAHAFFY: That's correct.

MEMBER WALLIS: - to how you approach the answer. That's all you need to say. You don't need to get involved in a big complicated -

CHAIRMAN BANNERJEE: No, I think he's making a mistake in page 412 here in - which starts with, the numerical values are most likely the values from the previous time step. They're not.

MEMBER WALLIS: You are saying that that is not true.

DR. MAHAFFY: That is not true.

MEMBER WALLIS: Well, in that case you have found the answer then.

CHAIRMAN BANNERJEE: Well, then, I think you need to rebut this. Or if not you - you should supply - at least we would suggest that you supply the staff with what is actually being done and how this is factually incorrect.

DR. MAHAFFY: Well, it's more than that. And that's why I had this last slide -

MEMBER WALLIS: I also object to the ACRS being the vehicle for resolving this. I don't know why -

CHAIRMAN BANNERJEE: Because the letter came to you.

MEMBER WALLIS: I don't know why the letter doesn't go to the staff.

CHAIRMAN BANNERJEE: But it came to you, so it came to you. You're on the spot.

MEMBER WALLIS: Not necessarily. I just give it to the staff and say, tell me how to resolve this thing.

CHAIRMAN BANNERJEE: Well, that's what you did. So now we are trying to get it solved.

MEMBER WALLIS: The person to resolve it is the creator of it, who is presumably John.

CHAIRMAN BANNERJEE: He's going to

DR. MAHAFFY: That's the resolution.

CHAIRMAN BANNERJEE: Can we suggest a course of action here? One is, you may have something to add to it, but from an overall point of view, if we look at the crux of the matter, which is the statements following equation zero twenty, then if there are factually incorrect, that's the first thing that should be rebutted.

And then in brief the true procedure that is followed should be outlined in two or three sentences, and it could be I think shown that what you are doing is if not exactly the truth or what it should be, very close to it.

And if you have something more to show us and add, that's fine.

DR. MAHAFFY: Well, it's just this bottom line. Even if - go ahead, Tom.

MEMBER KRESS: What's missing is the statements you made that the values are determined from the predicted equation. And somewhere that's missing in here.

DR. MAHAFFY: In these presentations, I don't know if I put it in words or simply intended to say it, which I did say, so it's on the record, but this final point I want to make is simply, even if you can work from an inverted equation of state, it does not simplify the problem when you go to two-phase flow. I've still got a nonlinear set of equations I've got to solve through the production of a Jacobian and inverting a Jacobian matrix.

More than that, the matrix becomes five by five instead of four by four.

MEMBER WALLIS: Does this matter? If you could show that there is no essential difference between 19 as you use it and his 32? Then that would resolve it, wouldn't it? You don't need to broaden the conversation to this matter about inverted equations of state and stuff. That doesn't really matter, does it?

DR. MAHAFFY: Well, it does in that the author of the letter is suggesting an approach where you deal with an inverted equation of state, simplifying things.

CHAIRMAN BANNERJEE: Yes, but I don't think he is hanging his hat on it. I mean he is using that as an alterative. You can do it either way, but he offers that as one possibility. I don't think we have to rebut that.

DR. MAHAFFY: That's fine.

CHAIRMAN BANNERJEE: The main point though is that you do have to rebut the point that he's saying that it has no basis in mathematics or engineering

DR. MAHAFFY: This is my rebuttal up on the screen right now, okay. That is what a Newton iteration is about. It is mathematically well established. And that is what we used.

MEMBER WALLIS: I think he agrees with that, doesn't he?

CHAIRMAN BANNERJEE: I think he's more turning on the equation 19 and 20.

MEMBER WALLIS: Right. At what time do you evaluate - and how do you evaluate these changes in rho and in rho E.

MR. CARUSO: What's done in the code

DR. MAHAFFY: This, right here. This is what's done in the code.

MEMBER WALLIS: That's so general that it doesn't really - it's not debatable. It's an obvious statement.

CHAIRMAN BANNERJEE: His statement is that 19 and 20 are what is done -

MEMBER WALLIS: Claims they're not going to converge, I think. It's not the way to converge on an answer.

MR. CARUSO: I agree.

CHAIRMAN BANNERJEE: Who is going to take the responsibility for doing this? I mean we've had this meeting. We've had this discussion. This discussion has been -

MEMBER WALLIS: It's not us.

CHAIRMAN BANNERJEE: - is this now a matter of which we have anything more to do with?

MR. CARUSO: I don't think so. I think the staff understands what we've asked them to do.

DR. BAJOREK: It seems like what needs to be done here is, John needs to write this rebuttal, write some text around his notes, submit it to the staff. Unless somebody can suggest, or there are objections, they feel that there is a safety issue associated with this, or there is a larger uncertainty that we don't feel is there, then we would provide this to the ACRS, and that should close the issue.

CHAIRMAN BANNERJEE: And let's keep it short.

MEMBER WALLIS: So we are supposed to evaluate whether or not this is a sufficient rebuttal?

MR. CARUSO: We asked to hear their response, and then we'll decide what to do with that.

CHAIRMAN BANNERJEE: I think we may not have the wisdom to decide whether it's a sufficient rebuttal, but we can give our opinion.

The letter came to you. Then it was sent then to -

MEMBER WALLIS: To Rick Ransom. I'm not sure that I ever read it, simply an anonymous email that I passed on to the staff, because I don't read anonymous emails.

CHAIRMAN BANNERJEE: It was sent by Larkins then to the EDO.

MEMBER WALLIS: That's right.

CHAIRMAN BANNERJEE: The EDO then passed it on to the staff. And Larkins presumably asked for a response or something.

MR. CARUSO: The committee would like to be informed of your disposition.

CHAIRMAN BANNERJEE: So I mean you don't have to come back to us for an opinion.

MEMBER WALLIS: What bothers me is the lack of definitiveness about the rebuttal. The rebuttal should be so clear that it's obvious to anybody that it's the answer to the question.

MR. CARUSO: That is why it needs to be written down.

MEMBER WALLIS: It needs to be written down, absolutely clear, and it needs to avoid extraneous discussion. It seems to me the issue is whether or not 19 is in some way flawed compared with 32.

CHAIRMAN BANNERJEE: It doesn't seem to be. Maybe it's an approximate.

MEMBER WALLIS: It's 19 and the next one. There is a pair of these equations, 19 and 20.

CHAIRMAN BANNERJEE: I mean obviously this is a much more complicated situation than a perfect gas, so some approximation to 19 and 20 in terms of what time step is used would be perfectly acceptable, and I think that needs to be clarified.

MEMBER WALLIS: It seems to me that there is a basic question. If you are using a slightly different approximation, it would be useful to have sort of an example that shows that it doesn't matter.

CHAIRMAN BANNERJEE: That would really nail the coffin properly, if you could find your way to doing that.

DR. MAHAFFY: A day out of my life. Easy for you to say.

CHAIRMAN BANNERJEE: Day out of my life.

MEMBER WALLIS: What is the consequence, if you are wrong, what would be the consequence? He doesn't sort of tell us, if you do it your way you lead to nonconvergence or oscillations or instability. It doesn't in any way address the consequences of being what he calls wrong; it just simply says it's wrong. Is it wrong enough to make any difference?

DR. MAHAFFY: Let me tell you, this is related to a different discussion we had maybe a couple of meetings ago. One of the things that we have done with SETS over the years, on and off, I think the last time I did a thorough study of it was probably six years ago, because we have both a semi-implicit and an assessed numerical methodology, it's easy to take certain test problems -

CHAIRMAN BANNERJEE: If you would speak into the mike?

DR. MAHAFFY: Yes. We can take test problems in two-phase flow, let's say we're going to do an Edwards blowdown for a simple example, and I'm going to run an Edwards pipe blowdown experiment simulated with my SETS method turned on, including whatever I'm doing here with my void fraction at the end of the time step. And I can run the same calculation with a semi-implicit methodology.

MEMBER WALLIS: Which would be the way that this person suggests you do it

DR. MAHAFFY: It's just two different methods.

MEMBER WALLIS: Can you do it his way

DR. MAHAFFY: What I'm telling you is, I'm going to do one method that doesn't involve this approximation.

MEMBER WALLIS: Is that the way that he suggests you should do?

DR. MAHAFFY: No.

MEMBER WALLIS: Unless you address his contention, you haven't answered it.

CHAIRMAN BANNERJEE: I'm not suggesting you do a full calculation. I'm not suggesting that you take one time step or something, and take some subset of points.

I mean what he is asking is that the full Jacobian be solved altogether as opposed to being split into two parts, or whatever you are doing right now.

Can it be done just like that?

MEMBER WALLIS: Is that a true statement, that what he's asking is that the full Jacobian be solved rather than being split apart? Is that a true statement?

DR. MAHAFFY: The problem you've got in that situation, it goes to my last slide, what I'm telling you is, in a two-phase problem, if void fraction is even important, and therefore being generated by this step, his approach is a non-starter. You have to use this approach with a Jacobian that gets inverted -

CHAIRMAN BANNERJEE: I'm not saying to do an analytical inversion. But I'm saying, just invert the Jacobian, it's a five by five Jacobian.

MEMBER WALLIS: There's nothing about two-phase flow in his critique.

DR. MAHAFFY: That's right.

MEMBER WALLIS: So why complicate it by talking about rho G and rho L? He's using two equations, not four or five.

CHAIRMAN BANNERJEE: The approach he's taking depends on the fact -

MEMBER WALLIS: Just address the simple question he's asking. When you have these two equations, single phased flow, he's claiming that yours is not the appropriate approach. Just answer that question; that's all.

DR. MAHAFFY: I'll give you the direct answer to that. The direct answer to that is that if it is single phase flow, ideal gas or otherwise, and I work it in TRACE, this step isn't even done. Because this step is only used to predict void fractions that are not zero or one. There is no simple way to work in the scheme that he is doing things within the context of what's trying to be done in TRACE. It's not part of the problem.

MEMBER WALLIS: I think we may have contributed all we can to this, and it's really up to you to sort it out.

DR. MAHAFFY: That's fine.

CHAIRMAN BANNERJEE: Sorry, John.

We're a little over time.

MEMBER WALLIS: He's going to be quick, he said.

DR. di MARZO: This is an old problem that was left over of I guess a year ago.

CHAIRMAN BANNERJEE: You have a lot of slides.

DR. di MARZO: I'll chop off a bunch of them.

CHAIRMAN BANNERJEE: Ninety percent of them.

MEMBER WALLIS: You've got one minute according to the schedule.

PI GROUP RANGING

DR. di MARZO: The problem is the range of acceptable distortion of integral test facilities. The state of the art, or shall I say, the way we have been doing it in the past is to dismiss the whole issue with a very simple statement. We get to the Pi group, and we will accept whatever is smaller than two and larger than one half. That has been what has been done in the past.

And the question that arose is, what is the basis for that? And the answer is none.

So what we are trying to do here is to put some basis to some alternative method to clarify that issue.

And I'll do one example, because the other one is probably off time, but we'll see how it goes. I'm going to talk about AP600, but I'll make some reference also to a P1000 as I go along.

The first things that we do is we talk about the transients. We talk about the phase that we are going to select. And then we are going to talk about the figure of merit. Look at some results, and then look at the relationship between what's down here and what was done for a AP600, namely, the actual scaling report that Sanjoy and Marcus Ortiz and Larkin put together.

Now, the premise of all this is that you have to have a system that we would say behaves globally, in other words, where there are parameters such as for example pressure and inventory that controls the behavior of the system. That is the hypothesis at the basis of all this.

If you have a system where local phenomena control and override completely the behavior of the system, this cannot -

MEMBER WALLIS: How do you identify the most challenging trend here?

DR. di MARZO: I'm getting there.

So that's the first thing. You've got to have a globally -

MEMBER WALLIS: I have a problem right away. It may be that if the Pi groups aren't quite matched, that the CMTs will drain - at completely different times in this scenario. You've got a completely different scenario. The answer will be completely different.

DR. di MARZO: Yes.

MEMBER WALLIS: Now, this means the difference between a Pi group being 1.8 and 1.9 difference from what it should be makes all the difference in the world to the transient.

DR. di MARZO: Absolutely.

MEMBER WALLIS: How are you going to show that?

CHAIRMAN BANNERJEE: That is the issue he is trying to address.

DR. di MARZO: Let me proceed stepwise. The first thing that you do, looking at all the design basis that you have in front of you, all the transients that you have in front of you -

MEMBER WALLIS: How do you know that until you know the ones that are the most sensitive to scaling?

DR. di MARZO: Let's analyze the process. The first thing that you do here is that you have done a scaling of experimental facilities. You have run your tests. You now are in possession of internal test facility data. At this point of the process.

So there has been a PIRT. There has been a design of the facility. So at this point in the design basis space, the first question that you ask is, what is the most challenging plant.

CHAIRMAN BANNERJEE: Based on the experiments?

DR. di MARZO: Based on the experiments,

but you can also argue two points. Brake elevation is one of the characteristics that you are going to examine; and second is the relative position of the brake with respect to ECCS and vessel.

Okay, so -

CHAIRMAN BANNERJEE: What about size?

DR. di MARZO: First, this too - size is also -

MEMBER WALLIS: The code might predict a completely different most challenging transient.

DR. di MARZO: Yes, but the problem is this. You look at your data, and within the data you start asking the first question. Which transient has the brake at the lowest possible elevation?

Second question you ask: Is the brake in any way between the vessel and the EECS?

Those are the two characteristics you look at. You also look at all the data you have, and you look at the behavior, the physical behavior of the system, to determine if there are those issues that you are tracing, if there are specific phenomena that occur or do not occur.

Remember, at this point you are basing your information on integral facility, and you do not know exactly how good they are yet. But out of all this information -

CHAIRMAN BANNERJEE: Let's back up a little. Well, if you want you can go first.

MEMBER WALLIS: You are going to address Pi groups. I think you need to tell me what you are trying to decide here. I don't see what this has to do with deciding whether a Pi group has to be with half to two.

DR. di MARZO: We haven't gotten there yet.

MEMBER WALLIS: What question are you asking before you start?

DR. di MARZO: The objective of representation is to address that question, but we haven't got to the Pi group at all.

CHAIRMAN BANNERJEE: You are trying to pick the worst transient.

DR. di MARZO: Yes. Now I have in front of me a body of experiments that have been conducted -

CHAIRMAN BANNERJEE: But in reality this is not the way it was done.

DR. di MARZO: The way it's done is, you have done a scaling report. You have done a design of the facility. You have identified all the transients you want to run for the design basis. You have run all these tests.

CHAIRMAN BANNERJEE: You also have run the code.

DR. di MARZO: You have run the code.

CHAIRMAN BANNERJEE: For the full-scale system.

DR. di MARZO: And for all the facilities.

MEMBER WALLIS: Your scaling was used to design the facilities in the first place.

DR. di MARZO: That is correct. Now you are at this point -

CHAIRMAN BANNERJEE: So you can just say it's an iterative process.

DR. di MARZO: It's an iterative process, exactly. And at this point of the game you are. So once you have identified what is your most challenging transient, based on all this information, and you have identified within that transient which is the most critical portion of that transient, what is that?

It's typically when you are going for low pressure injection, for example, in the AP600, then therefore is when you achieve the minimum inventory, because that means core coolability or -- so you have identified that phase of the transient. At this point you make assumptions, and you basically do a one node representation of the system. It depends, if you can;

or two nodes; or whatever is called for.

CHAIRMAN BANNERJEE: The idea behind - if you are talking about top down scaling -

DR. di MARZO: Yes.

CHAIRMAN BANNERJEE: - the idea is to divide the system into some minimum number of nodes which are interconnected -

DR. di MARZO: Exactly.

CHAIRMAN BANNERJEE: - which capture the overall behavior.

DR. di MARZO: The overall behavior, that's exactly right. In this particular case one node is sufficient -

CHAIRMAN BANNERJEE: For this part of the transient.

DR. di MARZO: - for this part of the transient.

So I'm trying to do that, and this is irrelevant, because as you say it depends on what portion of the transient you are actually looking at. For this particular portion of the transient that we are looking at, which is the depressurization from opening of a ADS-1 to IRWST injection basically.

CHAIRMAN BANNERJEE: Through accumulators and everything?

DR. di MARZO: Accumulators and everything.

You are looking at a double-ended guillotine break on the DVI line. That is the transient that we are looking at.

CHAIRMAN BANNERJEE: I would say what you are doing should be split up a bit more. Right up to accumulator, then through accumulators to -

DR. di MARZO: You will see how it comes out that way.

CHAIRMAN BANNERJEE: It seems very broad brush.

DR. di MARZO: It's very broad, but it's basically the transient that goes from opening of ADS-4 to IRWST injection. That is what is down here.

You have conservation statement for mass and energy. Momentum is not considered.

MEMBER WALLIS: This is P over RT, it's a perfect gas or something.

DR. di MARZO: You use R, which is basically ZR, and you correct, essentially. It's a corrected equation of some intermediate values.

And then you basically - B is the volume of the system. W is the volume of the liquid. So that V minus B is basically the volume of the vapor space.

And then you add - so the mass in the vapor space and the mass in the liquid space.

What you have here is the flow rate of the areas discharge.

MEMBER WALLIS: Are you going to conserve momentum at some point here?

DR. di MARZO: No, there is no conservation of momentum.

MEMBER WALLIS: I hope not.

DR. di MARZO: There is the brake vessel side. There is the brake DVI side. And there is the flow incoming from the DVI intact side.

For conservation - so you are right, the conservation of mass, with all those terms, that I have highlighted there, which should come out like this.

And what you do is, for simplicity now, the problem is this, in order to make this thing transparent, and you will see later on what it means, you need to make it as simple as possible. So you start dropping all terms that are 10 percent or less. The change in mass of the vapor during that transient accounts for less than 10 percent of the total change in mass of the system, because you are basically emptying the system. So there is a lot of liquid that was there and is not there.

MEMBER WALLIS: The change in density doesn't matter, even though the pressure may change -

DR. di MARZO: Change of density is tremendous, but the amount of liquid that the system loses through that transient is ten times more than the change of mass that is in the vapor space.

MEMBER WALLIS: No, I'm thinking about the change of density in the liquid is quite significant when you drop in temperature.

DR. di MARZO: Also.

MEMBER WALLIS: But you don't seem to have that in there.

DR. di MARZO: You don't. You don't. You just take a constant density at the middle.

So you have a very simple conservation of mass.

MEMBER WALLIS: Well, less than 10 percent, you're talking about the vapor space.

DR. di MARZO: Vapor space.

MEMBER WALLIS: I'm talking about the change in rho L.

DR. di MARZO: There is also that one, but I didn't consider that.

MEMBER WALLIS: It doesn't matter? Because rho L is very different at 600 degrees than it is at 200.

DR. di MARZO: Yes, but you will see in the assumption that I make here, I'm assuming that the reference enthalpy and for all the reference density are for liquid at the average transient temperature. The temperature does change tremendously from beginning to end, but we take an average temperature, because the changing pressure is much more significant, you will see in a few slides what I am trying to say.

For conservation of energy, same thing. You write the conservation of energy from the liquid space accounting for the vapor generation. The vapor generation goes in the mass of vapor, and then goes out at the vessel side of the break, because that is totally vapor, we know from the data. It goes out at the ADS as totally vapor, and goes out at the DVI side of the brake with some -- which is about one third.

MEMBER WALLIS: The flow rate depends on the pressure.

DR. di MARZO: Yes.

MEMBER WALLIS: You're putting that in.

DR. di MARZO: Yes, absolutely.

So you write that. With this term written out from the equation of state this way.

So now you have conservation of mass and conservation -

CHAIRMAN BANNERJEE: Where is the heat going out with the vapor?

DR. di MARZO: This is the - heat is the vapor generation -

(Simultaneous voices)

DR. di MARZO: This is the vapor generation, and the vapor generation are three terms relative to the mass of vapor that's in the system. The vapor leaving on the brake side -

CHAIRMAN BANNERJEE: I mean for the energy part.

DR. di MARZO: This is the latent heat of vaporization applied to all these terms.

CHAIRMAN BANNERJEE: Going out of the brake, where is -

DR. di MARZO: The brake flow rate is in two parts. One vessel side, so VB lambda would be the energy going out of the brake from the vessel side, and then from the DVI side you have VB -- the flow rate from the broken DVI line times the quality of that flow.

CHAIRMAN BANNERJEE: Now, I'm saying the

top equation, where is the energy going out of the brake?

DR. di MARZO: Your writing the equation for this control void.

CHAIRMAN BANNERJEE: You're only writing for the liquid?

DR. di MARZO: Only writing for the liquid.

CHAIRMAN BANNERJEE: All right.

DR. di MARZO: All you have is the vapor generation here. The vapor generation -

CHAIRMAN BANNERJEE: I'm sorry, I thought you wrote for the whole thing. Conservation of energy only for the liquid?

DR. di MARZO: Only for the liquid.

The equivalent condition you write artificially this way. That's basically this number here, to give you a sense, in this particular transient is nine. So the changing temperature compared to the changing pressure is not significant. That's why I used an average temperature for the transient, which gives some distortion for the solution of that.

So conservation of mass, conservation of energy.

MEMBER WALLIS: So are you evaluating Pi

groups, or are you evaluating a very simplified model of blowdown?

DR. di MARZO: We are looking at a very simplified model of blowdown, but I will show that that fits exactly in the Pi group. But that's not the point. Once I have this tool, I can show you a lot of other things which turn out to be very clarifying with respect to --

CHAIRMAN BANNERJEE: This is some very simple model of blowdown.

DR. di MARZO: So you take this, you put it in here -

MEMBER WALLIS: So it's approximately exponential relaxation -

DR. di MARZO: Yes. The important thing is, this is the famous compliance of the system. You've got two parts to it. You've got the compliance on the vapor space, and then you've got the thermal compliance, basically the stored heat which figures in the heat capacity of the system.

This capacity is the heat capacity of the liquid, plus the heat capacity of the metal masses. So the metal masses are in here in how the system responds -

CHAIRMAN BANNERJEE: So there is some

approximation to this?

DR. di MARZO: Correct, but keep in mind this term.

Okay, so what we do from this is we eliminate the time, because time is not a problem. We just talk in terms of inventory and pressure. So when you do that, you come down in this form. I'll give you all these terms in a second.

But what is very important is that the rate of change of inventory with pressure is a bunch of terms here that's -

CHAIRMAN BANNERJEE: Can you hear him when he goes to the board?

DR. di MARZO: - that scales the system. And then there is this term, where the heat capacity of the system is embedded in here.

Now look at this portion of the equation. Here are all your potential distortion of the process. Here are all your scaling parameters, the size and so forth.

The terms in the order are the flow group, which basically is the injection from the intact DVI line; the brake vessel side; the brake DVI side; the ADS flow.

And then the energy associated with this group; the energy associated with this; the energy associated with that group; and finally the energy associated with that group.

MEMBER WALLIS: These are constants throughout the transient?

CHAIRMAN BANNERJEE: He's assumed them to be.

DR. di MARZO: Yes.

MEMBER WALLIS: He's assumed them to be constants throughout the transient.

CHAIRMAN BANNERJEE: Just to keep it simple.

DR. di MARZO: To keep it simple.

The power, in the power you have core power, and then you have PRHI, which has a significant role in this transient that you have to model in there.

MEMBER WALLIS: I'm not quite sure about this. You said the first one you've got the flow out the brake or something?

DR. di MARZO: This is the net inflow.

MEMBER WALLIS: Yes, but on the right hand side you have the flow out the brake?

DR. di MARZO: No, this is the flow from the DVI line, intact DVI line, this is the flow out of the brake -

MEMBER WALLIS: - that depends on the system pressure. It's not a constant thing.

DR. di MARZO: Sure, this thing would be modeled -

MEMBER WALLIS: They are all variable with time.

DR. di MARZO: These are all variables.

MEMBER WALLIS: So your Pi groups are all varying with time.

DR. di MARZO: We haven't gone to Pi groups yet. Let me - they all vary with time; that's absolutely correct. And I'll show you how they do that.

CHAIRMAN BANNERJEE: Well, with pressure in this case.

DR. di MARZO: Pressure. Time is eliminated. And the volume group, which is this group here - okay, so let's go back to this equation.

You've got the energy associated with the net inflow; the power group. You have the net inflow. You have the volume group.

CHAIRMAN BANNERJEE: But these groups can be functions of P and W.

DR. di MARZO: Absolutely.

I calculate, I show just for the sake of argument how this thing is with respect to data. And I plug it here, let's say normalize pressures against the data.

MEMBER WALLIS: How did you get - go back to the previous slide. How did you get these groups?

DR. di MARZO: No, I did this ratio -

MEMBER WALLIS: No, no, you go back to the other side.

DR. di MARZO: I wrote them all out, and that's what they came out analytically.

MEMBER WALLIS: Yeah, but now how did you pick all these values?

DR. di MARZO: How will I pick all these values in the actual calculation.

CHAIRMAN BANNERJEE: Okay, so you just - what are you doing in the next slide?

DR. di MARZO: Okay, I'm calculating this equation now. I have all the information. I have a starting point and I calculate everything.

(Simultaneous voices)

DR. di MARZO: If I want, there is a time. But then it is -- in other words, I can solve the equation one against the other.

The only time parameter is the PRHR at

this point. All the rest is pressure related. If I have the pressure I have the flow rate. So I can step forward in pressure and solve. Or I can use time and solve both equations.

CHAIRMAN BANNERJEE: You are showing us some slides, your slide -

DR. di MARZO: I can solve this set of equations. Let's look at in time, and then I can eliminate time to simplify my thing in the end.

MEMBER WALLIS: But now you have a curve showing -

DR. di MARZO: I have a curve to show you how it compares -

CHAIRMAN BANNERJEE: But to get that solid line you have to make some assumptions -

DR. di MARZO: Absolutely.

CHAIRMAN BANNERJEE: - regarding various things.

DR. di MARZO: Absolutely. I have to get the PRHR contribution. And I get that out of data.

CHAIRMAN BANNERJEE: So let's say you integrated the DW, DT and dp/dt.

DR. di MARZO: Yes.

CHAIRMAN BANNERJEE: Okay, so you integrated the DPDT. How do you get QC, QB, well CC you can get, the DW, DV -

DR. di MARZO: Okay, this I get from the data, PRHR. QC I get from my ANS curve.

The initial condition I know. These flows are a function of the pressure, so I get discharge flow basically.

CHAIRMAN BANNERJEE: How do you get that?

DR. di MARZO: If it's a single phase it's just a normal discharge flow, critical flow. If it's two phase I use homogeneous models.

CHAIRMAN BANNERJEE: So you use some combination of data and initial conditions.

DR. di MARZO: And initial conditions, then I use all the discharge flows -

CHAIRMAN BANNERJEE: All the curve shows is that your model of a single node is not bad.

DR. di MARZO: It's not that bad. That's beside the point.

Now let's see what we can do with this model; that's the important point. Which in the case can be a three-node model or whatever you come up with.

But once you have it, consider it, the first thing you ask is, who is important? Which of these groups are relevant to the trajectory or what the system does? That's the first question you ask.

MEMBER WALLIS: Rho V over rho L isn't very big.

DR. di MARZO: Rho V over L isn't very big. I don't even look at it. And VG is not very important. Okay, so that's a one over there.

The only one that matters are FG and then that sum.

Okay, so these are absolute values, okay. With - so let's examine the first term, which is that net inflow group, which is this black line here. It starts very high, because as soon as you open the brake, a lot of water leaves. It's negative initially meaning -

MEMBER WALLIS: It's the outflow group.

DR. di MARZO: - your outflow. This recoup which basically reduces a little bit your flow is the initiation of accumulator. This is ADS-2, ADS-3. At this point the accumulator flow is pretty large and manages to flip into positive -

MEMBER WALLIS: When it's less than one you are actually filling the system?

DR. di MARZO: Exactly. You are getting very close when it is positive, and more than one you refill. In other words the accumulator are about to refill at this point. Unfortunately, they drain. So as soon as the accumulator is empty, this thing goes back to negative. There is only the CMT, which is a much lower flow, able to replenish the system. And at this point IFWC activates.

CHAIRMAN BANNERJEE: The what?

DR. di MARZO: The IFWC, the low pressure - the red line is the power line, is the energy plus the power associated with this.

MEMBER WALLIS: It's a bit problematic to me. You've got a dw/dp.

DR. di MARZO: That would be -

MEMBER WALLIS: - think of how p is changing as well as how w is changing.

DR. di MARZO: Yes.

MEMBER WALLIS: If I had dw/dt I wouldn't have to do that. I could just think about how w is changing.

DR. di MARZO: Okay, pressure decreases, right? Inventory decreases. So we are looking at this dw/dp as a positive quantity. The larger it is, it means that the inventory is losing with respect to pressure. So if this is a large number, you are losing more inventory than pressure.

MEMBER WALLIS: Suppose I eventually get a state where the pressure is almost constant, and I am filling, then dw/dp isn't very useful.

DR. di MARZO: No, no, pressure drops.

MEMBER WALLIS: Dw/dp isn't useful to me -

CHAIRMAN BANNERJEE: Once it gets to saturation -

DR. di MARZO: Pressure drops continuously, because you keep on opening stuff.

MEMBER WALLIS: It keeps on dropping?

DR. di MARZO: It keeps on dropping until it reaches IFWC.

CHAIRMAN BANNERJEE: Forcing it to drop.

DR. di MARZO: I want it to drop. The problem is, I don't want to lose inventory while I drop pressure too much; otherwise I am cold, and am unable to cool. So it's a race between inventory and pressure that I am having here.

So I'm losing pressure but I'm also losing water.

MEMBER WALLIS: That's what the whole idea of ADS is is to depressurize without losing too much water.

DR. di MARZO: The question is, what makes this number large. Because if this number is large, I'm having a problem.

So at this particular point, the way this is set up -

CHAIRMAN BANNERJEE: So have you put the areas on top -

DR. di MARZO: This is gone. This is gone. And basically you can see -- this number is always negative, never makes it to one. This term here, as I said, goes positive and negative. When it's negative, it makes this term very large, which we don't like. When it's positive, it fights against one. Once it gets above one, you are refilling.

So in light of that, look at what's happening. Here you are about to refill essentially, but you lose the accumulator. The important thing is that the term in the denominator never make it to really much of it, they go from point one to one. So it's significant, but not enormous compared to what this is.

MEMBER WALLIS: Well, it's different, if they are close to one, you've got a denominator that is zero, and that's pretty important.

DR. di MARZO: No, it's a negative term, this thing is negative, always negative.

MEMBER WALLIS: EG plus PG minus one is in the denominator. When it gets to one you've got infinite -

CHAIRMAN BANNERJEE: No, go back to the -

MEMBER WALLIS: You're only in trouble if DP is zero.

DR. di MARZO: Exactly.

MEMBER WALLIS: But you see it gets up close to one.

DR. di MARZO: It gets up close to one here.

MEMBER WALLIS: Which is not very nice.

DR. di MARZO: It's not very nice.

MEMBER WALLIS: Because I'll have a big dw.

DR. di MARZO: You are correct. That is because of the PRHR and the core power.

Actually, the reason why it goes up is because there is a tremendous amount of heat released by the - the stored heat that is dumping.

MEMBER WALLIS: Now, all these saw-tooth things are due to various events happening?

DR. di MARZO: Yes, these ADS-1, accumulator starts, ADS-2, ADS-3, ADS-4, and then the end. These things are just changing of sign, the way it's plotted.

MEMBER WALLIS: Why does this help me rather than just plotting inventory and pressure and things like that?

(Simultaneous voices)

DR. di MARZO: This number here in all the times, is zero point zero something, so it's not important. This is not important.

CHAIRMAN BANNERJEE: I think, Marino, we know how to derive the Pi groups. The main thing is, what do they mean?

DR. di MARZO: Okay. Once we get up to this point, and we are only -- the important one. These are the parameters that affect that transient, according to the simple analysis.

These are the Pi that came out of your analysis.

MEMBER WALLIS: These Pis are varying with time.

CHAIRMAN BANNERJEE: These are what we got 10 years ago.

DR. di MARZO: Ten years ago.

CHAIRMAN BANNERJEE: All right.

DR. di MARZO: This is how they would be related to these things.

MEMBER WALLIS: Simply the ratio of flows makes a sensible thing.

DR. di MARZO: These are exactly the

definitions of the Pis as they were from your analysis compared to what I have here. There are only three Pis in this phase.

MEMBER WALLIS: They seem to be the ratio of heat transfers or flows; is that what they are?

DR. di MARZO: Yes.

MEMBER WALLIS: I could write them down at the beginning pretty well.

DR. di MARZO: Yes, sure. But the problem is this, once you get to this point, you have a tool that enables you to calculate what - the change in this parameter corresponds to the change in the figure of merit.

CHAIRMAN BANNERJEE: That's the new thing. What is the figure of merit?

DR. di MARZO: Okay, the figure of merit is the minimum vested inventory at the moment at which you inject. Now instead of saying, I'm going to put the range on the parameter, I'm going to put the range on the figure of merit, and I'm going to say, if the facility exhibits a figure of merit ten percent more than what it should be in the nominal case due to the distortion, I will call that acceptable yet nonconservative, because more water showing is nonconservative.

On the other case if it shows 20 percent less than what it should be at nominal case, I would consider it acceptable and conservative.

So let's look at how do this thing behave. On the one hand I have the 10 percent minus 20 percent, which is defined from the impact on the figure of merit.

On this scale I have the point five two, which was the original stipulation for this discharge. So you've got - of those parameters, you've got some parameters such as the brake flow and the accumulator flow that are clearly amplified. In other words if you go by point five oh two you are really wrong, way wrong, because these things are going to span the range of the affect immediately.

CHAIRMAN BANNERJEE: The way we took it, we integrated the accumulator flow so it doesn't really matter.

DR. di MARZO: It doesn't matter, but we said -

CHAIRMAN BANNERJEE: You can't be wrong on the total accumulator flow, because there is only so much water in the accumulator?

DR. di MARZO: No, this is the brake flow rate --

CHAIRMAN BANNERJEE: Brake flow is different, yeah.

DR. di MARZO: So you ave going from point five to two. That's what we used to do. And these two parameters would be completely inaccurate.

MEMBER WALLIS: So you are saying you need a much narrower range?

DR. di MARZO: Much narrower.

MEMBER WALLIS: I suppose it's to duplicate the transient. But in order to validate a code, it's a very different question.

DR. di MARZO: No, what I'm saying is at this point, if your facility happens to have a distortion on this parameter that is like -

MEMBER WALLIS: Point five.

DR. di MARZO: - point five, you're going for trouble.

MEMBER WALLIS: Well, then you will get a very much different minimum inventory than you will get -

DR. di MARZO: Yes, but this differentiates between your parameter. Before they were evenly consider, point five, two. Now I know which one are amplifying, which one are venting, for example, these two, you can do whatever you want. If your facility is distorted on these, you are not going to make much fo a different.

CHAIRMAN BANNERJEE: I thought CMT flow was very important.

DR. di MARZO: Exactly, that's the first thing that came about. In the DVI brake, it's a large brake; it's a four-inch brake. Accumulator saved the day. The CMT doesn't do much.

If you are going for a small brake like a two-inch brake or something like that, this would be completely different.

MEMBER WALLIS: So if you are not looking at the worst transient, CMT does help you.

DR. di MARZO: Exactly.

CHAIRMAN BANNERJEE: - to the conclusion that the CMT was very important.

DR. di MARZO: In the two-inch.

(Simultaneous voices)

MEMBER WALLIS: - when it drains. It drains at different times depending -

DR. di MARZO: Exactly.

(Simultaneous voices)

DR. di MARZO: But this transient is the fast transient, is the big guillotine break. So the first thing you learn is the converters are extremely important. CMT is not important in that.

CHAIRMAN BANNERJEE: The accumulator it has a fixed volume. So in a way if you scale the accumulator correctly -

DR. di MARZO: You have to scale it correctly, yes, sure.

CHAIRMAN BANNERJEE: It's part of the equation because it is going to lose all its water anyway.

DR. di MARZO: You have to have the orifice right. And so the brake.

There are quantities among those that have a mixed behavior, for example, the subcooling. The subcooling is distorted in that the facility has no subcooling you are going to have problems. If the facility has less subcooling you are okay, in the range stipulated.

CHAIRMAN BANNERJEE: What do you mean by subcooling? IRWST?

DR. di MARZO: The temperature of the accumulator for example in subcooling, or temperature of the injection. Brake flow quality, same thing.

There are parameters that -

MEMBER WALLIS: So you seem to be saying there is no magic number, point five to two.

DR. di MARZO: Absolutely.

MEMBER WALLIS: You should look at the sensitivity of things to the distortion.

DR. di MARZO: Exactly. And this is a tool that will help you get some ideas of who to look for and -

MEMBER WALLIS: So you are suggesting that the ACRS should never in the future accept arguments about Pi groups being between point five and two?

DR. di MARZO: Absolutely.

MEMBER KRESS: Well, I have a problem with that, and it goes like this. What the purpose of the integral experiments are is to validate the code. Now it's not to reproduce the results you might get in a real transient.

So I'm not sure this addresses the question, are you validating the code correctly.

CHAIRMAN BANNERJEE: It could be, but then it goes back to Graham's original question. If you got a distortion in the facility, a big distortion, in one parameter that is very important -

MEMBER KRESS: You may be on a different flow regime or something. That's where it would matter, I think.

DR. di MARZO: Or it could be a transient that is going completely in another direction.

CHAIRMAN BANNERJEE: Where it could matter for example is if you got IRWST coming in too early. So in that case -

MEMBER WALLIS: The whole scenario changes. You are not really validating the code.

MEMBER KRESS: But I don't see how we address those issues here.

CHAIRMAN BANNERJEE: I guess the real issue, Marino, I agree with Tom, is whether you get qualitatively different phenomena occurring.

MEMBER WALLIS: That's right.

DR. di MARZO: But gives you simply an indication of where to look.

MEMBER KRESS: Okay, I would agree that that is possible.

DR. di MARZO: It's nothing concluded, just an instrument to go and search.

MEMBER WALLIS: So there is nothing in your analysis that says that when you get a distortion of more than X percent, the CMTs drain at a completely different time, you get a completely different scenario, you can't do that. But if you run the code you can do that presumably.

DR. di MARZO: Right, absolutely. So you

can analyze now the facilities. For example -- analyze the facilities that were used during that process. And you find that these two are doing reasonably well, or -- for example, in the brake flow rate, was way distorted.

CHAIRMAN BANNERJEE: We knew that already.

DR. di MARZO: We knew that already.

MEMBER WALLIS: The only good experiment is in Italy.

CHAIRMAN BANNERJEE: And Japan.

DR. di MARZO: When you go for the accumulator again -

MEMBER WALLIS: Isn't SPES in Italy?

DR. di MARZO: SPES was in Italy.

MEMBER WALLIS: So I put them together, and said, you have a lot of flow on the accumulator and a lot of flow on the brake, so they kind of compensate.

Remember, there was an issue all the time. How do you know you are going to have -

CHAIRMAN BANNERJEE: You never emptied OSU properly because you had such a large accumulator.

DR. di MARZO: Yes, but the problem is, the combination of the two wasn't extremely bad. The only one you could rely exactly in the range as defined was ROSA, which is what we used.

MEMBER WALLIS: Well, look at all your conclusions. None of them is perfect for all cases.

DR. di MARZO: None of them.

MEMBER WALLIS: None of them.

DR. di MARZO: Okay? So that's basically what the tool does. It's a very simple tool. It's a back-of-the-envelope tool. But it points you - first of all, dispels the fact that you can use a point five two across the board. That cannot be done.

CHAIRMAN BANNERJEE: What did you come to, that you should be much more sensitive on brake flow and accumulator flow for this study?

DR. di MARZO: You can do it for more than one transient, clearly. You can do it for more than one phase within the transient. You can do it as much as you want. But the same methodology would apply as appropriate, and it will give you information.

MEMBER WALLIS: Tell us about TRACE. I thought we were here to evaluate TRACE.

DR. di MARZO: No, that's what I started with saying -

MEMBER WALLIS: So you are just a sideshow of some sort.

DR. di MARZO: It's a delayed show.

CHAIRMAN BANNERJEE: How many years after?

DR. di MARZO: One and a half, two maybe.

For SBWR, Gee did the analysis. So I'm reporting that analysis. But I want to show you a different way. They did something similar to this.

First of all let's look at the margin. You have AP600 margin of this kind. This is the elevation of the top of the active fuel, and this is the minimum vested inventory when IWSP injects.

If you look at an SBWR, this is top of active fuel, this is how much you have when -

MEMBER WALLIS: It never gets uncovered?

DR. di MARZO: No, when GDCS injects, that's basically where you are.

MEMBER WALLIS: We knew that already. At least we were told that by the vendor.

DR. di MARZO: So in doing the same approach, you can be much more lenient, because you have that kind of -- now I will show you the TRACEs for those two.

But before I do that, let me show you this. GIRAFFEE at a very high GDCS injection compared to Plank, GIST at a very low GDCS injection. This is another of the facilities that GE wanted to use to support their data, code validation.

CHAIRMAN BANNERJEE: Where is the facility?

DR. di MARZO: I think it's in Japan, too.

MR. CARUSO: San Jose.

CHAIRMAN BANNERJEE: Is it operational still?

(Simultaneous voices)

MEMBER WALLIS: So it's irrelevant?

DR. di MARZO: But they reported it. GIRAFFEE on the other end had a larger ADS flow, a very low brake flow. Okay?

Now if you have a large ADS flow, and you have a low brake flow, that means that you bias your discharge toward the vapor, which in terms of the trajectory equation means that you are losing more energy and less mass, which means that you end up with more water in the end.

This means on the other hand that you are going to recover, once you hit the minimum base of inventory faster in GIRAFFEE and slower in GIST compared to Plank.

MEMBER WALLIS: So what is the gist of all this?

CHAIRMAN BANNERJEE: The gist is better than GIRAFFEE.

DR. di MARZO: That's what you're getting.

MEMBER WALLIS: So what is the conclusion in terms of this?

DR. di MARZO: The coordinates are different.

MEMBER WALLIS: Yeah, and the minimum is different.

DR. di MARZO: And the minimum is different, because this one I said had that problem, is venting more vapor than liquid.

(Simultaneous voices)

MEMBER WALLIS: And what is the actual SBWR?

CHAIRMAN BANNERJEE: This thing, that's the calculated one.

DR. di MARZO: So that's totally irrelevant. This is top of active fuel down here.

MEMBER WALLIS: That's right. That's the whole message they try to convey is that it doesn't matter, you'll never get down there.

CHAIRMAN BANNERJEE: But in a way it does tell you -

DR. di MARZO: It gives you insights in what is going on, and that's basically what all this means. It's an extremely simple tool. It's a tool at the level of PIRT, if you wish.

CHAIRMAN BANNERJEE: Well, tell me what you have done which is more than what the simple scaling analysis, is this like a perturbation analysis on the scaling or what?

DR. di MARZO: No, if you do the scaling analysis, at the beginning of the scaling analysis you design your facility and so forth. When you come out of a scaling analysis, you come out -

CHAIRMAN BANNERJEE: The sort of scaling that leads to these Pi groups, which is a top down scaling.

DR. di MARZO: Right. When you come out of the scaling, you come out basically with values of the Pi group, but you do not come out with the impact of that Pi group on the figure of merit. In other words, you know that something is distorted by 10 percent, 20 percent, 100 percent, but you have no clue, no direct information as to how much that affects your minimum vessel limit.

CHAIRMAN BANNERJEE: So to put it in a nutshell, the contribution here would be that you show how perturbation of a Pi group may affect your figure of merit?

DR. di MARZO: Exactly.

CHAIRMAN BANNERJEE: And you have a relatively simple equation to do that. What you could do is, you could take the eventual results that came out of whatever scaling analysis it is -

DR. di MARZO: Exactly.

CHAIRMAN BANNERJEE: I'm trying to think of a methodology.

DR. di MARZO: It's one step beyond what you need. You had the other equations. I'm just using them as a transfer function -

CHAIRMAN BANNERJEE: You are doing the same thing for GE, and you can do the same to AP1000.

DR. di MARZO: But this is the key to solve the question, what is the acceptable range for the distortion is not a blanket -- you can't do that. You should do something like this.

CHAIRMAN BANNERJEE: You should write it up - my suggestion and we should get others - in a way where you make the relationship between - what you've really got is a reduced set of original equations. We had 19 Pi groups, whatever they were -

DR. di MARZO: On this page you had only three.

CHAIRMAN BANNERJEE: No, we eventually got them down to three.

DR. di MARZO: Yes.

CHAIRMAN BANNERJEE: And then you really

would like to show how these directly affect your figure of merit.

DR. di MARZO: Correct.

CHAIRMAN BANNERJEE: And get ea simplified equation for that. But you should write it in a generalized methodology that you can do it whichever system you're looking for.

DR. di MARZO: For example, I'll give you another corollary for this, AP1000, in AP1000 - first of all, there is a problem in general. When you look at this situation, people are scaling ADS-4 with power. Okay? Power turns out to be one of the major dominant parameter in this portion of the transient. So it is proper to scale ADS-4 with power for the long term portion of the transient.

MEMBER WALLIS: Well, initially it's just stored energy that is going out there.

DR. di MARZO: Correct. Let me finish the thought. If you scale power with ADS-4 you end up with an ADS-4 like an AP1000, much bigger than what you are going to need. Therefore you end up with a lot of entrainment which is what they'll end up with.

CHAIRMAN BANNERJEE: And you get your co-inventory down?

DR. di MARZO: Yes, and that's a selfinflicted injury that if that - if you look at this you understand.

The second thing which is even more significant, Graham, is that if you try to correct stored heat with power, which is something that always people do - remember the old facility they tried to correct - you are trying to change this term, when stored energy is in here.

So the only conceivable way in which you could do that is if you had power modulated on system pressure, so stored energy is released when pressure goes down and temperature goes down is not a fixed value that you correct with, because it doesn't make any sense.

Functionally one is a constant down here, and the other is a multiplier up here.

MEMBER WALLIS: It's not a constant though. The thing that concerns me with all of this is that these Pi groups are varying throughout the transient, so I'm not quite sure how you say that they are adequate or not.

CHAIRMAN BANNERJEE: They have to be calculated.

DR. di MARZO: They have to be calculated. In other words you would have to have a pressure senser or a temperature senser, and on that basis put in the stored heat.

But you cannot say I'm going to increase power -

MEMBER WALLIS: No, I'm saying, how can you say that ROSA has a Pi group which is unacceptable or something when it varies throughout the transient.

CHAIRMAN BANNERJEE: You have to look at each part of the transient.

MEMBER WALLIS: But how are you going to have say a point nine two for ROSA AP-1 when it varies throughout the transient.

MEMBER KRESS: Let me ask a similar question.

What I see he has here is a simplified replacement for the code itself.

DR. di MARZO: No, you can't compute with this.

MEMBER KRESS: You could compute the impact.

DR. di MARZO: Exactly.

MEMBER KRESS: With the code. I don't know why we don't do it that way.

DR. di MARZO: We are accused of vicious circle at that point.

MEMBER WALLIS: Where does point eight two come from, though? Is it at the beginning of the transient or the end?

DR. di MARZO: This is the scaling body.

MEMBER WALLIS: But where does it come from? You've got an FG or something, which varies throughout the transient.

DR. di MARZO: No, these numbers come from the scaling analysis that you have.

MEMBER WALLIS: But your Fgs and all those things varied throughout the transient.

DR. di MARZO: Absolutely, but these are the values that you get from the original scaling analysis.

MEMBER WALLIS: Which is something else altogether?

DR. di MARZO: Which is something else altogether. Then you modify a parameter with a multiplier for -

CHAIRMAN BANNERJEE: See, this is - for scaling analysis you use for these Pi groups should be readily accessible or somehow obvious for each phase of an accident.

DR. di MARZO: Yes.

CHAIRMAN BANNERJEE: With some very simple assumptions so you get a rough and ready answer. But you could, as Tom was saying, do the same thing using a code. I mean you would get better numbers -

DR. di MARZO: Better numbers, but the problem with the code is that they can say, wait a minute, you are using these to validate the code. So the code up to today you cannot use, because you haven't validated on the basis yet. Remember, we went into that predicament.

So the idea here is to generate a very simple clue if you should say methodology to guide you in deciding whether your facilities are good enough to then perform the assessment.

CHAIRMAN BANNERJEE: I think we were talking at a time when we had much less confidence in the code. Because we were getting all these oscillations which wouldn't give you - I mean the code was bombing out. All sorts of thing.

DR. di MARZO: If you have the code reasonably fast, and you are reasonably confident, that's a very good way to go too.

MR. CARUSO: How do you decide what the acceptable impact is?

DR. di MARZO: That's arbitrary. That's arbitrary, but it is definitely better than defining the other part of the deal. And the way I did that was to say if the facility ends with more water, it's not conservative, right. If the facility ends with less water is conservative.

Now you have to put some values, so I said, arbitrarily, 10 percent on this side, 20 percent on that side.

There anything can be, it depends on the match.

MEMBER WALLIS: Why is nonconservative ever acceptable? It's misleading. It makes you think -

MR. CARUSO: I seem to remember that there were points during the long-term cooling phase in AP600 where you had to get the number right. It wasn't a matter of conservative or nonconservative. It had to be right.

CHAIRMAN BANNERJEE: But remember, this is not for the purpose of computing anything, in the sense of predicting anything. This is simply to tell you which of the parameter you can range without too much -

DR. di MARZO: In a facility.

CHAIRMAN BANNERJEE: - in a facility, or how much distortion you can allow on that parameter, and which parameter on the other hand you should be extremely careful, because if they are off a little bit they can cause a lot of damage.

MEMBER KRESS: It would certainly be a good guide on how to design your experiment.

DR. di MARZO: Exactly. Then you go to the code and you say, I want to do a sensitivity analysis. Which parameter should I range? You know which parameter you should range - those two, the amplifying one. And then if you have time, the mixed one, and then if you still have time, you can range more.

But at least you start shooting -

CHAIRMAN BANNERJEE: Yeah, it narrows down the analysis that you need to do.

MEMBER KRESS: Could you do this for a new reactor design other than ESPWR or EPR? Could you do this for a gas reactor that you don't have any test data for?

DR. di MARZO: No, you have to have test data. Because remember the original hypothesis -- a system that is globally controlled. So for example if you look at ACR, that won't fit.

CHAIRMAN BANNERJEE: No, no, I did the scaling for ACR.

DR. di MARZO: Yeah, but the problem is, if when you run pipe you get something down there happening.

CHAIRMAN BANNERJEE: You can't do it with one volume.

DR. di MARZO: Exactly.

CHAIRMAN BANNERJEE: You have to do it with multiple -

DR. di MARZO: You have to figure out exactly how to handle that. Remember the discussion, top down, bottom up, right. If you can lead top down to bottom up, yes.

MR. CARUSO: So you don't use this to design the test facilities, you use this to evaluate them after they've been built.

DR. di MARZO: You'll make a mistake if you use this to design the facility, because what you put in here is this, and what you don't put in it -

CHAIRMAN BANNERJEE: No, but in the sense that Ralph is saying that if you want to have a certain range of flow rates and friction factors.

I'll tell you one of the things that was surprising that came out of the ACR was the resistance in the ECC lines became a very important factor you'd never think of before you do the scaling. Strange.

DR. di MARZO: But you can play with this kind of a methodology, you can essentially extract information that you can then use to guide you in then doing the proper analysis. But you don't shoot in the dark.

It's the same thing in AP600 for example. We killed ourself on the two-inch brake which turned out not to be the most dramatic brake.

CHAIRMAN BANNERJEE: We did DVI as well.

DR. di MARZO: It was very instructive, but it wasn't it.

CHAIRMAN BANNERJEE: We also did the DVI brake.

DR. di MARZO: The DVI brake was the real thing. So if you get something like this to start, you hit the most problematic one first, and then given enough time you hit everything else. But at least you go to the heart of the matter immediately as opposed to just chancing it out.

And this is back of the envelope; this doesn't take much.

CHAIRMAN BANNERJEE: I think you should write it up in some systematic way; show that it's applicable to different - not just a one-shot deal. Write the methodology out in a way that other people can use.

DR. di MARZO: For example, in AP1000 we

used this to tell you that entrainment was the issue. Because when we played entrainment here, it popped out immediately that that was enormous; that was a huge problem. Instead, change the quality of ADS because of entrainment, and those numbers flew out the window. So you know that entrainment is key.

MEMBER WALLIS: You have fellow, Banerjee, as a reference here. Why is that there?

DR. di MARZO: It was done a year ago, a year and a half ago.

MEMBER WALLIS: Is that there as an okay reference? Have you now supplanted this work? Is your work better than his in some ways, or what is it?

DR. di MARZO: I had the date in which -

(Simultaneous voices)

MEMBER WALLIS: But is your work simpler than his or better than his? How does your work compare with that work?

CHAIRMAN BANNERJEE: Well, I'm better, I would say.

DR. di MARZO: His was much more accurate.

CHAIRMAN BANNERJEE: Are we done for today? Or do you have more questions?

MEMBER WALLIS: We can go into a little discussion if you would like. But we can get off the record, though.

CHAIRMAN BANNERJEE: Can we go off the record.

(Whereupon at 6:21 p.m. the proceeding in the above entitled matter was adjourned)