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ORIGINAL

OFFICIAL TRANSCRIPT OF PROCEEDINGS

Agency: Nuclear Regulatory Commission
Advisory Committee on Reactor Safeguards

Title: Subcommittee Meeting on Thermal
Hydraulic Phenomena

Docket No.

LOCATION: Bethesda, Maryland

DATE: Wednesday, January 5, 1994 **PAGES:** 347 - 593

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

DATE: January 5, 1994

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UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

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SUBCOMMITTEE MEETING ON THERMAL HYDRAULIC PHENOMENA

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Nuclear Regulatory Commission
Conference Room P-110
7920 Norfolk Avenue
Bethesda, Maryland

Wednesday, January 5, 1994

The above-entitled proceedings commenced at 8:30 a.m., pursuant to notice, Ivan Catton, ACRS Subcommittee Chairman, presiding.

ACRS MEMBERS PRESENT:

I. CATTON, ACRS Subcommittee Chairman
E. WILKINS, JR., ACRS Member
P. DAVIS, ACRS Member
T. KRESS, ACRS Member
R. SEALE, ACRS Member
V.J. DHIR, ACRS Consultant
V. SCHROCK, ACRS Consultant
W. WULFF, ACRS Consultant
N. ZUBER, ACRS Consultant
P. BOEHNERT, Cognizant ACRS Staff Member
B. SHERON, NRC/RES
N. LAUBEN, NRC/RES
T. KING, NRC/RES
L. SHOTKIN, NRC/RES
J. KELLY, NRC/RES
R. BEELMAN, INEL
D. FLETCHER, INEL
G. JOHNSEN, INEL
W. WEAVER, INEL
L. HOCHREITER, Westinghouse

P R O C E E D I N G S

[8:30 a.m.]

MR. CATTON: The meeting will convene.

Gary, your topic is level tracking model?

MR. JOHNSEN: That's correct.

[Slide.]

MR. JOHNSEN: This morning I would like to talk about the level tracking model that we have just recently implemented in RELAP5.

[Slide.]

MR. JOHNSEN: I am going to cover these topics. First I am going to talk about level tracking, which is the model that we recently installed and contrasted with what we used to have in the code with is vertical stratification model, a vertical stratification model.

I am going to skip right ahead to some preliminary results to show you how the model behaves before I go into a detailed description of how it works, and then I will describe it and show you the assessment plans that we have.

[Slide.]

MR. JOHNSEN: The comments provided to us by the consultants, the ACRS consultants in part dealt with the model described in Volume 4 which is called a vertical stratification model. The discussion of this model characterized it as a level tracking model and it is not.

It is a model that was designed to sharpen the interface but not track the level per se.

We recognized that we needed to have a true level tracking model after we --

MR. WULFF: I would like to make a comment to this first statement that you made. I have here page number 359 from the last meeting in Idaho where I asked you whether the essential physics of the level tracking in response to a bulletin that you have in your viewgraphs were in Volume 4. Mr. Johnsen: Yes. Catton: Is that the report we got some month ago? Right. Wulff: But it is not physics. Johnsen: That is a broad statement, Wolfgang. Wulff: And I say it deliberately, I will repeat it and sign it if you would like me to. Catton: You are going to communicate that. Wulff: Yes, I will. Mr. Shotkin: Now what we would like is specific details.

Nowhere was it said at that time, don't consider that to be a level tracking model, so we were misled.

MR. JOHNSEN: I think that was your terminology and not mine. I think if you look in Volume 4 you won't find that terminology. That was your characterization of the model.

MR. WULFF: You had on your viewgraph level tracking that prompted this question.

MR. JOHNSEN: I don't recall exactly the viewgraph you are talking about.

MR. WULFF: But I do, and I do have the viewgraphs and I do have the record here. So I think it was always called a stratification model. That was your jargon and there is nothing wrong with that, but to say now that because there is a word different from another word that we reviewed the wrong is just rejectable. So I don't like to now be told that we reviewed the wrong thing.

MR. JOHNSEN: I didn't say you reviewed the wrong thing. What I am trying to say here is that the model that you reviewed in Volume 4 which, of course, was issued in 1990, is not a level tracking model. It is a vertical stratification model, and that in March of 1993 when we had the last meeting in Idaho Falls, we had in fact planned to implement a true level tracking model.

MR. WULFF: Then you should have told us that it is not in Volume 4.

MR. CATTON: I don't think this is leading anywhere, but Wolfgang is correct. Why don't you continue.

[Slide.]

MR. JOHNSEN: Let me just show some results first before we get into the description, and this has just recently been completed, so we are just beginning our testing of the model and we use this little test problem to verify the functionality of the model as it was coded or was intended to be coded.

This little test model consists of a 15-foot pipe, if you will, area of three square feet consisting of 12 control volumes, and initially it is filled part way into the second control volume, and the pressure you can see is 1,000 psi and saturated liquid. Then what we do is, we introduce saturated vapor at the bottom using a time dependent volume, a time dependent junction, and we gradually increase the flow of steam through the bottom to watch the behavior of the mixture level.

MR. CATTON: So how do you define level, some percent void fraction, 99 percent or something?

MR. JOHNSEN: I think you are going to see that when I describe the model itself, but basically it is a sharp discontinuity in the void fraction from a liquid continuous to a vapor continuous flow.

MR. CATTON: But in real life if you bubbled steam in the bottom that doesn't happen.

MR. JOHNSEN: What doesn't happen?

MR. CATTON: If your flow rate is anything meaningful with steam you don't have a sharp surface, you are throwing little droplets up in the air and all sorts of things.

MR. JOHNSEN: You mean if you begin to entrain the liquid.

MR. CATTON: Yes, so you usually pick something like .999 or something for void fraction. Go ahead.

MR. JOHNSEN: Okay.

MR. WULFF: Could you explain what the staircase vapor flow is supposed to represent?

MR. JOHNSEN: Basically what we are doing is bubbling steam through a stagnant column of water and then examining how the model predicts how the level swells as a function of the steam flow rate.

MR. WULFF: Where is the steam flow rate on the right shown on the left, is it TDV?

MR. JOHNSEN: Yes, it is entering right here.

MR. WULFF: And you are injecting it in this jump step pattern?

MR. JOHNSEN: We are injecting it in the bottom cell of this column of cells.

MR. WULFF: Okay, I understand.

MR. JOHNSEN: And it is flowing upward through the liquid.

MR. WULFF: I know that. The question is, why in this staircase pattern?

MR. JOHNSEN: Just to watch the behavior of the mix level as we gradually increase the steam flow.

MR. CATTON: And you run it long enough for it to settle out?

MR. JOHNSEN: YEs. We are letting it settle out at each steam flow, right, and then we are increasing the flow rate to the next level, watching it stabilize out, and so on as we continue to increase the steam flow.

MR. WILKINS: Do you expect to get different results that way than if you had done it at one level and then walked away from the whole experiment for two weeks and come back and then started at the higher level?

MR. JOHNSEN: Yes, we expect to get the same results. As long as we are not losing any liquid, it will stabilize.

MR. SCHROCK: What are the numbers on this flow rate scale?

MR. JOHNSEN: I am sorry, this is pounds per second, I left it off the viewgraph.

MR. SCHROCK: You keep the steam flow below the flooding limit, i presume?

MR. JOHNSEN: Actually what we did was, eventually we let this thing go until we drove all the water out. Eventually it blows all the water out.

[Slide.]

MR. JOHNSEN: At the steam flow rates that I showed you on the prior viewgraph, there is insufficient flow to entrain the liquid out, so the amount of liquid in a column remains constant at each of those levels I showed you and the code then predicts a level in the control volumes where the level is detected to be, and the level moves up the column. It starts out in the lowest most volume. Let me go back and put that up.

[Slide.]

MR. JOHNSEN: The level starts out in this control volume where the liquid starts out, and then it moves up through this control volume and into this control volume. You can see that those nodes are three feet in height each, which would be typical of what we might use in like the CMT, for example.

So you can see what happens is that below the entrainment limit, the mixture level swells up as we increase the steam flow. This is what we call the collapse level, that is the level that we start out with with no steam flow at all.

[Slide.]

MR. JOHNSEN: What the model does is, it calculates a void fraction above and below the level, and this is the plot that shows the calculated void fraction beneath the level in the cell in which the level is calculated to occur and it calculates a void fraction above. As you can see here for these flow rates that we chose here, the void fraction above the level remains at unity, which meaning that we are not entraining any liquid out at these lower flow rates. So that is why the liquid mass remains constant during this particular set of experiments, if you will.

MR. WILKINS: The difference between those values of alpha-plus and alpha-minus is what you referred to earlier as "sharp discontinuity" when you were talking to Ivan a few minutes ago?

MR. JOHNSEN: Right, exactly.

So let me then, after showing this little experiment, move on to the --

MR. WULFF: Could I ask a question, is there a time plot of the level versus time?

MR. JOHNSEN: Yes. No, that was versus steam flow.

MR. WULFF: This means you had steady-state asymptotic values plotted, and I think four values, one each at these levels, and you have these points.

MR. JOHNSEN: Right

MR. WULFF: How was it reached?

MR. JOHNSEN: I can show you. I have a back-up slide here to give you a better feeling for that.

[Slide.]

MR. JOHNSEN: This is actually a depiction of how we ran the flow up and then back down again to see if the mixture level would swell up and then come back down smoothly. So this is what the flow rate looked like as a function of time starting with no steam flow and then one pound a second, five, ten, up to 30, and then back down the same way.

MR. CATTON: I think the question that Wolfgang was asking you is the behavior as a function of time for one of the steps?

MR. JOHNSEN: Yes, that is what I was going to show you next. I was just showing you the steam flow that we used.

[Slide.]

MR. JOHNSEN: This is a plot of the level as a function of time for that prior steam flow profile I showed you. So we start out with the level, and these are the different volumes. This solid line is the volume that the liquid started out in, Volume 2; then there Volume 3 above that the dashed line; and then the almost dotted line is the fourth volume. So as you can see the levels --

MR. WULFF: What is on the left side, alpha?

MR. JOHNSEN: No, this is level feet. No, this is meters.

MR. WULFF: But then it goes through all the volumes, why do we have three different plots?

MR. JOHNSEN: Once the level moves out of the lower most cell, it shows up as being full, and then the level goes to the next cell up. So this is the bottom cell, and this is the level in meters. That is where it starts out. Then as you increase the floor --

MR. WULFF: No, that should be the level at the lowest flow rate, not at the cell. It had nothing to do with cell boundary.

MR. JOHNSEN: Yes, that is the lowest flow.

MR. WULFF: That belongs to the first flow rate, and the next rate --

MR. JOHNSEN: Hold on, Wolfgang, that is the zero flow rate. We start out at zero and we ended up at zero. So that is a zero flow.

MR. WULFF: So all these plateaus belong to a steady flow?

MR. JOHNSEN: That's right. It steadies out at a new level each time you increase the flow rate.

MR. WULFF: And it goes through all the cells?

MR. JOHNSEN: It goes through three cells, it passes through three cells. So you can see that we have a level moving up through the cells, and that we also have symmetry in the going up and coming back down case.

MR. WILKINS: And it starts above the bottom cell and never gets to the top cell?

MR. JOHNSEN: It never gets to the top.

MR. WILKINS: That is why it goes through three cells.

MR. JOHNSEN: Yes.

MR. WULFF: So what is the dashed curve and the dotted curve?

MR. JOHNSEN: This is the level in the cell above where the liquid originally started. So, in other words -- let me get that other diagram out.

MR. SCHROCK: Gary, the RELAP5 is supposed to be a transient two-phased code and there is an interesting density wave propagation problem here, and it is not possible to have a correct level coming out of a calculation that doesn't adequately treat these density waves propagating vertically in this channel. So I think you have to look at the development of the two-phased through the system the changes in the two-phase as you change the flow rate, as a function of time in order to test the fidelity of RELAP5 to give you the correct level. I mean you want more than just the correct level, you want the distribution of the void.

MR. JOHNSEN: Right.

MR. SCHROCK: But you are not going to get the right level if you, in a transient problem, if you do not have a reasonable representation of the transient void distribution. What you are showing us is not addressing transient void distribution.

MR. JOHNSEN: And I don't mean to imply that this particular test problem is a sufficient validation of the modelling, it is really just a first step to show how it behaves and that, in a steady-state condition we should be able to verify we get the correct results for the simple steady-state case.

MR. SCHROCK: But won't the conclusion be misleading if it only looks at steady-state conditions, and you are using very, very coarse noding which I don't think will ever follow the density waves. Then to say that the level tracking is okay, it isn't going to be okay in transient circumstances.

MR. JOHNSEN: But, Virgil, all I am saying right now is, this is an initial test of the model in a steady-state situation. Understanding the fact that it has to work in a transient fashion and that we have a lot more validation to do with this model, so I am really only showing you what our initial results are for simple case that we can use to verify that it is functioning the way we intended it to. So your point is well taken, and I am not saying that by virtue of running this test problem we are home free, we don't have anything else to do.

MR. SCHROCK: The heart of the problem is still to be addressed then, is that what you are saying?

MR. JOHNSEN: Yes.

MR. WILKINS: Gary has said that the objective here is to verify functionality.

MR. JOHNSEN: That's right.

MR. WILKINS: That is not the same as to validate.

MR. JOHNSEN: That remains to be accomplished. I forgot what I was going to say.

MR. CATTON: But you could compare this to some of the simple solutions like out of Graham Wallis'. You could compare the analytic result with your computed result?

MR. JOHNSEN: Yes, I could.

MR. CATTON: Have you done anything like that yet?

MR. JOHNSEN: Not yet, we just basically have this functioning in the code.

MR. ZUBER: When did you start on this development?

MR. JOHNSEN: I think about three months ago, something like that, three or four months ago. I am not positive about that.

MR. CATTON: I am still not clear how you define your surface.

MR. JOHNSEN: How I define what?

MR. ZUBER: He treats it as a shock.

MR. JOHNSEN: Basically it came out of shock.

MR. ZUBER: That expression really is in Ishii's book, I believe. I am almost sure it is.

MR. WULFF: We will come to that.

MR. WILKINS: There is another slide about to come in which he answers that question

[Slide.]

MR. JOHNSEN: So let's go to the description of the model now. Basically the purpose of this model is to recognize the existence and then account for the effects of a sharp void gradient representing a mixture level and a vertical component, and the principal features of the model are that there is detection logic to detect the presence of a level, and then there is the calculation of initial level parameters, and then the alteration of the convectant terms based on the fact that there is a sharp interface and alteration of the heat transfer, and I will discuss each one of these in turn.

[Slide.]

MR. JOHNSEN: Incidentally, I should preface this by saying that this model was taken from the TRAC BWR code where it was initially developed.

So first we have to determine what is an acceptable criteria for a mixture level and this is probably one of the most difficult things to do since there is always going to be some degree of question as to whether the detection logic is adequate.

What we have in the code right now is the following criteria for the detection of level. First of all, if we are looking at Volume J and testing Volume J as potentially having a level in it, we first confirm that there is no level above or below Volume J.

Secondly, we require that there be at least a void fraction differential between Volume J and the volume above it of 0.2, or that there be the same gradient between J and the volume below it.

Thirdly, we require that the volume above have a void fraction of .7 or greater.

MR. WILKINS: Let me just see if I understand that. The first one says, in order to determine whether there is a level in Volume J, I look first to J-plus-1 and then at J-minus-1 and discover that there isn't one?

MR. JOHNSEN: Right.

MR. WILKINS: How do I do that?

MR. CATTON: The void fraction in J-plus-1 is what?

MR. JOHNSEN: In other words, from the prior time step there was no level in either the volume above or the volume below.

MR. WILKINS: So this is a criteria for mixture level at time T1.

MR. JOHNSEN: Some T.

MR. WILKINS: And you have already gone through it at T1 minus delta T or whatever.

MR. JOHNSEN: Yes, right.

MR. WILKINS: And you got started at time zero and you knew exactly where the level was, it was in Volume 2, that is the collapse level.

MR. JOHNSEN: In that little test problem, that would have been the case, yes.

MR. KRESS: I don't understand why you use the J-minus-1 in that case to exclude the level in J because you have changed the time step, it might have moved from J-minus-1 to J.

MR. JOHNSEN: If it had, then there would be no level in J-minus-1 and we would satisfy the criteria.

MR. KRESS: Then I have the same question that Ernest had, how do you check to see if there was a level in there?

MR. JOHNSEN: I am not understanding your question.

MR. CATTON: Do you flag the cell that the level is in?

MR. JOHNSEN: Yes.

MR. CATTON: So your step number one is to look for the flag?

MR. JOHNSEN: Right, look for the flag, and each volume of the stack for which this model is turned on, we will be checking their logic.

MR. WILKINS: Let me try again, let's say you are at time T that you are looking at, at time T minus delta T, which is the previous step, you look to see if there is a level in J-plus-1 and J-minus-1?

MR. JOHNSEN: Right.

MR. WILKINS: And there had better not be any, otherwise there is no chance for there to be a level in Volume J at time T.

MR. JOHNSEN: Right.

MR. WILKINS: That is what I interpret this.

MR. JOHNSEN: Right.

MR. WILKINS: Now what Tom has said is, it might have moved from J-minus-1 to J during this time delta T, and so the level might well have been in J-minus-1 at time T minus delta T and it moved up to level J at time T. That is the problem we are having in understanding the first criteria.

MR. CATTON: Gary, you are about to get some help.

MR. WEAVER: Walt Weaver from INEL.

This criteria is only used if there is no level in any of the cells. You can start a problem out in a stack of cells and there is no level any place. We are trying to find out if during the evolution of the transient a level appears. Once a level appears, we move it from cell to cell in a deterministic way.

So these criteria are only for the appearance in a stack of cells for a level when previously none appeared any place.

MR. JOHNSEN: But it can disappear and reappear.

MR. WEAVER: It can disappear, and once the level disappears out of the stack, then you have to check to see whether it reappears in the stack. So these criteria are used only for the appearance of a level when none previously existed. If a level exists at the beginning of a time step, we move it with a level velocity from cell to cell or within a cell.

MR. CATTON: So what is your criterion for a level being in a cell?

MR. WEAVER: This is the criteria. We say the level is in cell J and we exclude levels in cells above and below because we don't want to have the problem that RELAP4 had which was levels in adjacent cells which is unphysical.

MR. CATTON: I guess I am still perplexed. I would have thought maybe you would have had a void fraction criterion of some kind.

MR. WILKINS: They do, that is numbers 2 and 3.

MR. CATTON: Yes, but .2 is awful big. I mean awful small.

MR. JOHNSEN: That is one of the difficulties.

is detecting the level involves a void criteria. The question is what number do you pick.

MR. CATTON: That's right. For example, if you were trying to develop a mixture level for reflood just for example, not that it is important or anything else, but, for example, if you don't carry it to 99 percent, you won't get the right results. So I don't know what you would use for that difference.

MR. SEALE: Aren't you really saying that a level exists in the cell where you have a change in void fraction greater than two-tenths with the proviso that above that level the void fraction has to be greater than seven-tenths?

MR. JOHNSEN: Right.

MR. SEALE: That is really all you are saying?

MR. JOHNSEN: That's right.

MR. WULFF: He makes a difference, he looks at differences in void fractions.

MR. SEALE: A change in void fraction of two-tenths.

MR. CATTON: I wasn't looking at the third one up there. He is saying that if the void fraction is greater than .7, the level is below. I can be pretty wet.

MR. JOHNSEN: This is the cell above.

MR. CATTON: That is what I was asking you earlier, what your cut-off value was, and that is it.

MR. JOHNSEN: .7, and the transition to, let's say, annular flow from bubbly slug flow is going to be in that range. If you have to pick a number, it is

MR. CATTON: You are probably going to have to fool around with these criterion a little bit.

MR. JOHNSEN: The validation is going to have to indicate whether or not this is adequate or not. You are right, we are going to have to test it and see if it is adequate, but I suspect that for example for the CMT that most of the time we will have a fairly sharp interface.

MR. CATTON: Well, I guess if the ADS valve is open like Larry said, 120 seconds or something, you are right.

MR. DHIR: This will be a good test also to check for interfacial drag correlations, see if you go on the one route and compare it with whatever is the level 2 --

MR. JOHNSEN: I can tell you that we do blow all the water out eventually.

MR. DHIR: But to compare at what velocity you blow it out.

MR. JOHNSEN: Compared to the flooding velocity, yes.

[Slide.]

MR. JOHNSEN: Once the level has been detected, then the model calculates several different parameters. It calculates void fractions above and below the level, and those are denoted as alpha-J-plus above the level and alpha-J-minus below the level. This would be the length of the cell delta Z, and a Z-sub-L is the position of the level, and we also calculate the velocity of the level which enables us to predict when the level will cross a cell boundary above or below.

[Slide.]

MR. JOHNSEN: The model assumes that below the mixture level alpha-J-minus is equal to the void fraction in the cell below, alpha-J-minus-1. For the void fraction above the level, the model checks to see if an entrainment criteria has been exceeded. This is a model from Rosen. If it has not, then the void fraction above alpha-J-plus is set equal to the void fraction in the cell above alpha-J-plus-1.

If the entrainment criteria has been exceeded, then it calculates the void fraction based on the amount of entrainment that is calculated to occur, and that involves a critical velocity which is basically a flooding type velocity.

MR. ZUBER: What is the V-sub-F, velocity of what?

MR. JOHNSEN: This is the velocity of the liquid.

MR. ZUBER: The droplets of both, or what liquid?

MR. JOHNSEN: The liquid flow above the level.

Let me see if I can get --

MR. CATTON: But in your stagnant tube isn't that zero?

MR. JOHNSEN: Yes, it is. In fact, I haven't confirmed this yet, but --

MR. CATTON: That will give your code a headache.

MR. JOHNSEN: I haven't confirmed this yet, but it appears that the inherent interfacial drag in the code will levitate the water before this criterion is achieved.

MR. WULFF: But G entrainment is the massed flow of the liquid above the level; is it not?

MR. JOHNSEN: Yes, it is the mass flux.

MR. WULFF: And so is Rho-FVF, if you say it is the velocity above. So that would be 1 and you would have zero for alpha-plus.

MR. JOHNSEN: I am looking for another slide that I have on this entrainment. Let me try to come back to that because I have another slide that explains this in more and more detail.

This is the initial criteria, but the bounding limits are that the void fraction below the level cannot be greater than the average void fraction in the cell where the level exists, and that the void fraction above the level can't be smaller than the average void fraction in that cell. Those are bounding limits in think in a physical sense.

[Slide.]

MR. JOHNSEN: The position and the velocity level is shown in this slide.

MR. WULFF: I have a question, why did these limits come up? Did the calculation come up with values alpha-plus and alpha-minus outside these bounds that you have to impose them?

MR. JOHNSEN: No. Again, this model was developed by GE under the TRAC code.

MR. WULFF: It seems to me that the calculation required to impose this boundary, and then something is wrong with the calculations

MR. JOHNSEN: I can't say precisely. Walt, do you know why those were included?

MR. WILKINS: Weaver from INEL.

Those are the physical limits. If the void fraction below is greater than the cell average void fraction, then the level had gone out the top of the cell.

MR. WILKINS: I don't think you have understood Mr. Wulff's question yet. He says, those limits are Mother Nature's limits and you shouldn't have to impose them. If your calculation is consistent with Mother Nature, they should follow. You should be able to look at your results and discover that the results you had are consistent with those limits. I believe that is what Mr. Wulff is saying.

MR. WULFF: Correct.

MR. WILKINS: And that if you in fact have to impose them or if, in some place in the calculation you have to impose them because otherwise you would have an alpha that would be outside those limits, then there is something unphysical about the calculation.

MR. CATTON: What do you do if those limits are violated, is that just an error check in the code?

MR. SEALE: Check your arithmetic.

MR. WULFF: No, it bounds it. It is saturating.

MR. CATTON: Yes, but your original comment was that they really don't need them. So if they do have them, what do they do with them? Probably use them as error check, if you get outside stop, something is wrong.

MR. WULFF: No, they don't stop. They continue with the limit.

MR. JOHNSEN: I think the coding goes back to that.

MR. WILKINS: Yes, that is what I would expect. If in an actual case you do reset them, then I think Mr. Wulff's comment is appropriate.

MR. WULFF: It violates Mother Nature.

MR. WILKINS: It is something that needs to be explained.

MR. KRESS: You always violate Mother Nature when you use a finite difference code, and you could end up with finite difference problems that will give you this situation.

MR. WULFF: I asked for an explanation or for the reasons why this was imposed.

MR. KRESS: I understand.

MR. JOHNSEN: We have not seen a violation. That was not the reason we included those limits. Those limits made physical sense to us and they were taken from the TRAC model. We haven't seen any violation.

MR. WULFF: Maybe your recommendation is to take them out and see what happens.

MR. JOHNSEN: Your recommendation is to take them out and see what happens.

This is the calculation of the levels. It is just based on conservation principles using the void fractions I indicated earlier for above and below the level and the average void fraction in the cell and differentiating that with respect to time gives us an expression for the movement of the level, the rate of change of the position of the level.

MR. WULFF: I think here is the answer to Virgil's question before. The first expression is the starting expression. It has no kinematics in it, no kinematics from the void propagation beneath. It has no kinematics in it from the gas velocity or the liquid velocity, and by differentiation you cannot recover the terms that were deleted in order to reach the steady-state expression at the top.

So I ask you, what is the justification for differentiating the steady-state expression on top in order to get the transient jump condition for the motion of the interface.

MR. JOHNSEN: Did you read my next slide?

MR. WULFF: No.

MR. ZUBER: Yes, I did, and that is wrong.

MR. JOHNSEN: And it is wrong?

MR. ZUBER: Yes.

MR. CATTON: Is this a quasi-steady approximation?

MR. JOHNSEN: Yes.

MR. CATTON: So you are ignoring accelerations?

MR. JOHNSEN: Right.

MR. CATTON: And your basis is the 120-second opening time?

MR. ZUBER: It has nothing to do with --

MR. JOHNSEN: No.

MR. WULFF: He ignores the kinematics of what happens.

MR. ZUBER: Actually, the answer is really on the next page. What Virgil said was absolutely correct, what you really have is a jump condition which is the one below Equation 7, and this is the one that Ishii has.

MR. JOHNSEN: That's right.

MR. ZUBER: There is no argument with that. What is inconsistent is this Equation 5, that is in error.

[Slide.]

MR. JOHNSEN: Let me go through this so that there is some continuity involved. When we get to Equation 5, we --

MR. ZUBER: Actually, if you use this in your argument, this is correct based on Equation 5, you have no argument because Equation 5 is incorrect.

MR. JOHNSEN: What I am trying to show is that this model will devolve to the jump condition.

MR. WULFF: Only if Equation 5 were right.

MR. JOHNSEN: Let's go through it then. Again, the expression for the level, differential of the level, and now we have to make an assumption that these substantial derivatives are equal to zero, which says basically that the void fraction below the level and the void fraction above the level are time invariant which is a quasi-steady assumption. When you do that, then the expression simplifies to this expression for the differential.

[Slide.]

MR. JOHNSEN: Now we go to Equation 5. Equation 5 basically says that the rate of change of void fraction in a cell is equal to the difference in the mass flux below and above the level times the area divided by the volume.

MR. ZUBER: Let me say what is wrong with that. What you have in two-phased flow, and actually we derived 20 years ago and I think you will find it in textbooks, is the void propagation equations, and you can use it at volume average and you would obtain it.

The point is, first, you don't have a source for generation of vapor or condensation and omega. If you look at the void propagation equation, it is at the alpha, the T-plus-C-sub-K, and C-sub-K is he velocity of the kinematic phase time the alpha. This is equal to an omega, plus you have flushing that can be minus, you have condensation.

Number one, you don't get that omega at all, and what you are really basing here on fluxes and what you should really have is on a kinematic base, but you cannot apply this to either condensation of evaporation because you don't have a source for vapor.

Look, really go back and look at the void propagation equations, how it is formulated, I think GE probably has it, Wolfgang has it, we had it 25 years ago.

MR. JOHNSEN: I did. I went back and looked.

MR. ZUBER: Then you don't have the source there.

MR. JOHNSEN: I am only trying to show in these two slides that the model we are using can be shown to be consistent with the jump condition, that is all I am trying to show. I am not showing you that this is the model.

MR. WULFF: You tried, but it didn't work, and it because of Equation 5 being wrong, and I think doing swelling the alpha-minus-DT cannot possibly be zero or small enough to be neglected because you are coming up with vapor catching up with the interface and percolating through, and that is a transient. So I think you are making a number of assumptions and then show that this Equation 2 has something to do with this jump condition.

MR. JOHNSEN: Right.

MR. WULFF: You tried, it didn't work.

MR. JOHNSEN: I disagree.

MR. ZUBER: The jump condition, the last equation, you should also have a term either for vapor evaporation at the interface or condensation.

MR. JOHNSEN: This is in the absence.

MR. ZUBER: In absence. I think this is what Ishii had. I have no qualms. So you could have started with this, but the Equation 5, if you apply it to a CMT or anywhere in the reactor, it is wrong.

MR. JOHNSEN: But I am not applying that. The model doesn't apply it. All I tried to do was to start with what the model has and show that --

MR. ZUBER: It should not have that, period.

MR. JOHNSEN: It could end up --

MR. ZUBER: No, it cannot.

MR. JOHNSEN: That is all I was trying to show. I am not saying the model incorporates that.

MR. CATTON: If you are bubbling the air through, this would be okay, wouldn't it?

MR. SCHROCK: No. It isn't right even for bubbling air through because the interface is moving, and so the alpha-plus/alpha-minus are changing with time due to the fact that the interface is moving through their mesh.

MR. JOHNSEN: They don't have to.

MR. SCHROCK: They have to if you are doing a transient calculation, and that is where you are going.

MR. WULFF: You open the valve.

MR. JOHNSEN: If I am moving with the mixture below the mixture level, the void fraction could look at time and variant, and yet the level is moving.

MR. SCHROCK: Not in your mesh it can't, not the way you calculated the void.

MR. WULFF: And certainly not in the experiment or the calculations you showed us at the beginning where you introduce step-wise steam that then arrived some time later at the interface and percolated through. So, at one time, you had one value of alpha, then you have changed the flow rate of steam and you have a different alpha.

MR. JOHNSEN: That's correct.

MR. WULFF: So there must have been a transient in which that alpha changed.

MR. JOHNSEN: That's correct.

MR. WULFF: Maybe above, if there was no entrainment, it was always 1. That is okay.

MR. JOHNSEN: It was always 1, that's right.

MR. WULFF: But below it cannot be said equal to zero, just from physics.

MR. JOHNSEN: I did show you that it is not, it does change.

MR. ZUBER: Gary, let me say this problem can be easily formulated in the correct way, and we should not really spend our time arguing.

MR. JOHNSEN: All right, let's go on.

MR. ZUBER: It is not a good formulation period.

MR. JOHNSEN: Again, Equation 5 is not in the code. It is only there to illustrate how we got --

MR. ZUBER: The point that Virgil brought is, you are going to use this code in transient conditions, so please listen to what we are saying. I think the approach you want to do it is good, I mean the philosophy is good, the tools you are developing. They way you are developing them are not good. It can be done easily and in a satisfactory way. I urge you really to do it because the only chance you have really to address the CMT and all these problems with moving interface for these will depend on how you model this.

MR. WULFF: Could you tell us, in RELAP the difficulties are in implementing what you have here as Equation 7 directly, or below Equation 7. Start with Ishii's jump condition and you have all the defenses behind you. Is there something in RELAP that --

MR. JOHNSEN: I don't think this is really inconsistent.

MR. WULFF: You were just convinced that it is. I am just asking, is there something in RELAP5. It is a different question now. Is there something in RELAP5 that prevents you? As far as I can tell, you have the alphas, you have the velocities of the vapor, you are computing that and you have been computing it for many years. Can you not just plug this in and calculate the time rate of change of the level, the level velocity?

MR. JOHNSEN: I can't answer your question. I don't know. I don't know off the top of my head.

[Slide.]

MR. JOHNSEN: Now I want to talk about what the model ends up affecting within the code itself. Without the model, RELAP normally would convect with what we call a self-centered average volume properties, so if the flow were upward, for example, over here, we would convect the average void in this cell to the next cell up.

With the level tracking model, we alter the void fraction of the conservation equations and this little chart shows how the void fraction above and below the mixture level is altered to recognize the existence of the level. I won't go through the whole table, I think most of it is self-explanatory.

[Slide.]

MR. JOHNSEN: The hydrostatic head term was also modified again to recognize the sharp interface. Normally without the level tracking model, we simply do a rho G delta Z calculation using the different densities in the cell above and below to formulate the gravitational head term for the momentum equation. With the level tracking model, we hit a more accurate differential pressure due to the location of the level.

[Slide.]

MR. JOHNSEN: Finally, we alter the heat transfer calculation to recognize the position of the level relative to a heat structure that is adjacent to that vertical cell, and this proportionality is performed based on the position of the level, so that the heat transfer to the vapor space, the continuous vapor space, is based on the location of the level in the cell, and likewise for the liquid.

MR. DHIR: How do you calculate these heat transfer coefficients?

MR. JOHNSEN: Pardon?

MR. DHIR: How would you calculate this heat transfer coefficients? Suppose you had a stagnant layer of vapor on top or gas and the liquid is at the bottom, a two-phased mixture?

MR. JOHNSEN: If it were stagnant?

MR. DHIR: Yes.

MR. JOHNSEN: First of all, we have a heat transfer package which, in essence, I could say is the boiling curve, but it also has condensation in it.

MR. DHIR: But the problem is, what lengths do you use in calculating the heat transfer coefficients?

MR. JOHNSEN: The length is input by the user for that particular heat structure. The user inputs the length, the length term that is appropriate for that particular heat structure. He doesn't assume it is necessarily --

MR. WULFF: If above the level there is condensation, you need to know the driving length above the level, that is from the level on up to the top of the wall?

MR. JOHNSEN: Yes.

MR. WULFF: How can the user put that in?

MR. JOHNSEN: That is one of the compromises you have to make. You have to pick a fixed level. We don't calculate -- we don't say, what is the distance between here and the top of the wall where the condensation might be taking place, so there is some imprecision in using some fraction of the length specified by the user.

MR. SCHROCK: Gary, this brings up one of the things that came out of the review of the documentation. I commented on it in my review and I saw that it was commented by others in their reviews. There are number of places in RELAP5 where there are very arbitrary choices made about the length scale for heat transfer coefficient evaluation. In some instances, the dimension of the computation itself is taken as a length scale for the evaluation of a heat transfer coefficient. That is unfounded, there is no basis on which to argue that an arbitrary choice made for a numerical computation has any influence on the physical problem of the heat transfer coefficient determination.

So this is an example of the kind of thing that is wrong in RELAP5, people have been telling you for years is there. You have heard it. Nothing happens about it, your sponsor doesn't think it is important, and so you don't do anything about it. How in the world can we evaluate a code that has this kind of nonsense in it when you won't recognize that the question has even been asked?

Is my point clear?

MR. JOHNSEN: Was it a point or a question?

MR. SCHROCK: It is a point and a question.

MR. JOHNSEN: Well, it has been changed in the code. It is no longer the diameter of the cell or the length of the cell, it is a user input number associated with the heat structure in the cell. So what is in the documentation from 1990 is obsolete at this point.

MR. CATTON: Let me see if I understand that, does that mean that if this cell, say, is five cells below the top I will have a condensation length scale and the user would input the total distance for this cell?

MR. JOHNSEN: The user would have to make --

MR. CATTON: I can understand where you might argue that in order to implement the code, and I don't know what this split is between the two here, what ZL is, I will just arbitrarily select the length scale to be from the top to the middle of that cell and let the air be what it may. What do you do, what does the user do?

MR. JOHNSEN: The user has to make a judgment as to what a reasonable length is in consideration of the --

MR. CATTON: That means the user has to be highly skilled in the world of transport phenomenon.

MR. JOHNSEN: Assuming, of course, that it makes a big difference.

MR. CATTON: In the case where it is, the Reynolds number, usually heat transfers are tied with a Reynolds number or something, and if I take one cell and compare that with three cells, I can be off a factor of two.

MR. DHIR: Also it is changing with time, so you can't prespecify.

MR. JOHNSEN: It is changing with time.

MR. DHIR: Every time it has to be calculated which is the proper length scale, it has to be checked.

MR. JOHNSEN: That's true.

MR. DHIR: So how can the user tell.

MR. JOHNSEN: Let me say that most of the application that we put the code to, this just is not all that important.

MR. CATTON: But we are in the CMT now where you, yourself, have discovered the condensation is important. So you almost need a length scale for force convection, a length scale for condensation, a length scale for this, that and the other.

MR. SEALE: Is the ultimate intent, recognizing that there is a lot of documentation that still has not appeared yet, but is the ultimate intent in the documentation to provide, among other things, instructions for the user to use in establishing what these various lengths should be as the user input so that the best judgment or the best intent of the developer can be reflected in the abuse or lack of abuse that the user uses on the code when they apply it? Is that part of the documentation that you plan to have?

MR. JOHNSEN: Yes. There is a volume that is a user guidelines, that is part of the code record.

MR. SEALE: And this kind of cheat sheet will be in it, that is something that will tell them how to scale those lengths for those calculations?

MR. JOHNSEN: I can't confirm.

MR. WULFF: It is impossible to do this because, as we had seen before, the condensation heat transfer coefficient starts up very high, then comes down to a value of one-third or so, and it really represents the thickness of the film that we have. While the level is way up throughout the top, that film is thin, a very high condensation. How can some user account for the drainage of the tank and condensation during that transient, it is impossible. It ought to be computed and it is easy to compute.

MR. JOHNSEN: It is not easy to compute, Wolfgang.

Don, do you want to comment on that?

MR. McELIGOT: Don McEligot, INEL.

I just wanted to reemphasize something that you said earlier, Gary, relating to the aspect that often this question is not important. I think looking from the outside, your experience has been in most applications you have had thick walls and if you had liquid film or liquid on the inside, typically you have high heat transfer coefficients. Let's say low thermal resistances compared to the thermal resistance of the wall. So in a case like that, these questions of length scale details become less important because it is really only an order of magnitude that one needs to have in order to show that it is something else that is driving the problem.

That aspect was pointed out by Gary yesterday when he used order of magnitude arguments in order to come up with an order of magnitude of the BO number, which is what you need here.

Now let me make one other comment, in the case where you happen to have turbulent natural convection, typically the length scale drops out of the relationship because you have Nusselt number versus Rayleigh number to the one third power, and the length scale you can pick anything you want, and there you could pick a -- it wouldn't make physical sense in the way that you say, but you could pick a unit cell length and it is still going to cancel out.

MR. DHIR: This code is also going to be used to validate it from the experimental data and the experiments may not have thick walls. So there your evaluation of condensation heat transfer coefficient becomes important. Also natural convection numbers he showed, I think they are in error by a factor of three or four at least.

MR. McELIGOT: Three or four is a drop in the bucket, I think. We are looking at order of magnitudes here, a factor of 10 I would begin to worry about.

MR. ZUBER: Let me say something, I have no qualms with order of magnitudes. I think this is good thinking. I am quite distressed when they bring something in a meeting, for example, two years ago, we brought the questions of condensation, we brought the questions of level tracking, it was pooh-poohed. Now we can see it is important. In the meantime, two years have passed and the things we bring up are in good faith, our concern is in order so that NRR has something to audit with that we can defend outside. I think take our comments with that sense, but don't try to dismiss it a priori and then the things faces up two years later, it is too late then to do anything.

MR. SCHROCK: There is another point, Gary, which is very simple, that is that you can argue that the heat transfer coefficient accuracy is not a very important fact in the accuracy of the overall code prediction for many situations, for many situations. The truth is, if you have garbage in the code to get the heat transfer coefficients, you are going to get garbage when it does become important.

You will never know from this code whether the heat transfer coefficient is important or unimportant if you continue to use garbage in the determination of the heat transfer coefficient.

I am not arguing, I don't think anybody here is arguing that you cannot show that there is an insensitivity to the accuracy of the heat transfer coefficient in a lot of situations. Maybe predominantly so, but that does not prove that there are not situations in which the reverse will be true and there is where you were in serious trouble.

MR. SHERON: I need to ask a question here, Virgil, if I could, because I have been listening to this, and I remember some time ago we were sort of admonished by the committee here for not using textbook correlations, literature-based correlations, and so forth. We tried to do that, and I am getting the impression most of these literature-based correlations have length scales in them, and now we are being admonished because we can't come up with some characteristic length when we apply it to these strange geometries that we didn't design but we have to evaluate.

What are you suggesting that we do, are we supposed to go back and build a CMT and run specific tests to get a heat transfer correlation for that particular geometry or what?

MR. SCHROCK: What I am suggesting is that there are more intelligent ways to estimate the heat transfer coefficient than to say, let's chose an arbitrary length scale which is based upon the totally unrelated issue of what the discretization of this system that we are analyzing has chosen to be.

MR. SHERON: He just said we are not doing that.

MR. JOHNSEN: I just told you we are not doing that.

MR. SCHROCK: I don't believe that. The code is filled with it.

MR. JOHNSEN: Then don't believe it.

MR. SCHROCK: Have you taken it all out now?

MR. JOHNSEN: I just told you that it is not that way in the code.

MR. WULFF: It is user input is what you --

MR. JOHNSEN: It is a user input, it doesn't assume the cell length, it doesn't assume the cell diameter. It is a user inputting number.

MR. SCHROCK: You are talking about the specific model that you are addressing here today, or are you talking about the code in general?

MR. JOHNSEN: The code in general.

MR. WULFF: But here you are taking the height, the full height of the tank, which is a simple geometry even though the level may be close to the top and you should use a small height.

If it is not important, why are we splitting within one cell the distance and then make this assignment? If it doesn't matter, you can just use your heat transfer coefficient, either one, either the lower one or the upper one for the whole cell and it wouldn't matter.

MR. JOHNSEN: If I recall correctly from the March meeting, this was a specific question as to whether or not we recognized the level in the cell with regard to the calculation of the heat transfer.

MR. WULFF: Yes.

MR. JOHNSEN: Now you are telling me you don't think it is important; is that right?

MR. WULFF: No, I am not saying it is unimportant. I am saying you are inconsistent by saying it is not important on the overall result, and yet you put great importance on splitting this. Somewhere it is either important or not important, but not both at the same time.

MR. CATTON: If it would be possible for us to get some documentation on this, we would appreciate it.

MR. JOHNSEN: Of what, documentation of this model?

MR. CATTON: Yes. The other thing is, if it is user input, in the CMT the level is moving.

MR. JOHNSEN: Yes.

MR. CATTON: If the length scale is user input, how do you do that, or what do you do, does he input the length scale relative to a level or something?

MR. JOHNSEN: In those cases where the length scale plays a role in the computation of the heat transfer coefficient and, of course, that depends on what heat transfer mode you are in, but to the extent that it plays a role, then the code would use the user input value for that particular heat structure, and each heat structure is independent.

MR. CATTON: So it might have a heat structure that is at the mid-level of the tank.

MR. JOHNSEN: Right. The user has to specify a length.

MR. DHIR: But it can't be specified, it is changing. So either you have to give some fixed evaluations and let it track the level and then calculate what the height should be.

MR. JOHNSEN: Take for example the condensation. I think I showed you yesterday that beyond a half a meter length, it is relatively insensitive to length for the preconvection.

MR. DHIR: Let's say it is three inches, you want to calculate three inches of your surface which is three inches tall and it is condensing. How did that user know if it is going to be three inches or six inches?

MR. JOHNSEN: Here again the user has to make a judgment as to what modes are going to occur and based on that pick a length scale that he thinks is going to give him a reasonable answer.

MR. SHOTKIN: Gary, can I just say something. We appreciate the ACRS' comments and we can take an action to go review our users' guidance document, update it as needed to include better guidance on the choosing of the length scales, and we will have that ready for a committee review when the documentation is ready. Is that okay?

MR. CATTON: That's fair enough. I think that is good.

MR. ZUBER: Can I just have a question on the previous slide, I didn't get a chance. H1, let's assume that we have a vapor above it.

MR. JOHNSEN: Right, vapor continuous.

MR. ZUBER: H2, we have liquid and bubbles?

MR. JOHNSEN: Right.

MR. ZUBER: Suppose I have 50 percent vapor.

MR. JOHNSEN: Below.

MR. ZUBER: Below. Can it be according to your --

MR. JOHNSEN: Sure.

MR. ZUBER: How do you calculate H2? I mean how do you calculate the heat transfer coefficient, H2?

MR. JOHNSEN: It would be based on what the wall temperature was relative to the two-phased mixture. It could be boiling or it could be condensation.

MR. ZUBER: Yes, but --

MR. JOHNSEN: The heat transfer subroutine checks --

MR. ZUBER: In your subroutines you split the heat transfer coefficient according to the void fraction and you say, for example, 50 percent would be from the wall to the vapor, and 50 percent from the wall to the liquid.

MR. JOHNSEN: It depends on what heat transfer mode it is in. If it is in boiling, it all goes to the liquid.

MR. ZUBER: For example, this is not boiling, but you are heating it.

MR. JOHNSEN: Let's say you are heating it, for example?

MR. ZUBER: Yes.

MR. JOHNSEN: Then it would go to the liquid. A 50 percent void fraction?

MR. ZUBER: Yes, 50 percent liquid, 50 percent vapor and it is a bubbly flow?

MR. JOHNSEN: Yes, it would all go to the liquid.

MR. ZUBER: Are you sure?

MR. JOHNSEN: I am going to cover that topic later on, by the way.

MR. ZUBER: Okay, good.

MR. JOHNSEN: There is a separate talk on that issue.

MR. CATTON: I think if you have concerns about any of this, the thing to do is to write me --

MR. ZUBER: I will, but since he was --

MR. CATTON: I understand. Because some people are going to miss their flights unless we pick up the pace.

[Slide.]

MR. JOHNSEN: This is my last slide, and I am indicating on here that as far as the level tracking model is concerned, we are going to look at several existing cases we have, GE level swell, THTF boiloff. We are in the process of doing those now. Of course, perhaps more importantly, we are going to be testing it using the AP600 related experiments that are indicated here.

MR. SCHROCK: Gary, I would like to suggest that you add to that a test against the analytical solution from simple drift flux modelling for the void propagation. In the bubbling, your simple thought experiment problem that you started with, bubbling the air through solved that as a transient problem, and compared it with predictions from the analytical solution that you will find in textbook references.

MR. JOHNSEN: That is a good idea.

MR. CATTON: I think Graham Wallis has that in his book, doesn't he, he even gives it as a homework problem, where is the level in the beer glass.

MR. ZUBER: They should also compare it when you have a aberration, a transient, because they should then tell you how much vapor it is generating under flashing.

MR. JOHNSEN: This was just a simple steady-state.

MR. WULFF: It could be a homework problem with an analytical solution with flashing.

MR. CATTON: The next speaker is Mr. Kelly on film condensation, and we are going to have to pick up the pace, so detailed questions maybe can be communicated to me and then I will forward them to RES. We are just now at 4:45 p.m. yesterday.

[Slide.]

MR. KELLY: What you have is Part 1 of the handouts, there are actually three parts. I am Joe Kelly from the NRC, and I will be talking about film condensation modelling.

[Slide.]

MR. KELLY: First, what is the objective of this work? It is to develop a modelling approach for film condensation within the framework of a two-fluid model, and it has a focus specifically to address concerns of the NRC consultants and also the ACRS with respect to these three issues which I will define a little bit better in a minute. Heat flux apportioning, that is how the heat flux was split between the liquid and the vapor, surface partitioning which was used in RELAP/MOD3, and that was determining how much of the surface was wet and dry, and also the effect of noncondensable gases upon condensation.

[Slide.]

MR. KELLY: Now I will give a little background on how the two-fluid model treats condensation. I give the concept of the model I am working on, and I do want to stress that this is a model that is under development so the work is not finished. Then I will go into some of the details of that. The first is film thickness, it is very important to correctly calculate the film thickness for condensation heat transfer. Then I will talk about the film side heat transfer resistances, then the effect of noncondensable gases, which would be the resistance on the vapor side, and an assessment plan.

[Slide.]

MR. KELLY: The first thing you have to do is ask yourself where is condensation potentially important in these new plans. So in the AP600, we have talked extensively about the CMT walls during the drain period, and also possibly the PRHR tubes during two-phases -- two different transients, during the ATWS and during the small break LOCA during first-stage ADS actuation. Condensation could be important there.

The steam generator tubes, reflux cooling, this is something that has been important in small break LOCAs in traditional plants, probably is not important here because of ADS, but it is something that I will have to look at later.

Horizontal pipes, the condensation exists but how important it is, I don't know yet. Containment response is very important for the AP600, and that is being modelled with the CONTAIN code, so we are not handling that within the context of RELAP5.

For the SBWR, obviously condensation is important in the isolation condensers and also the passive containment cooling system, that is what they are designed for. Also in the containment drywell, and of course we are using CONTAIN for that.

[Slide.]

MR. KELLY: Now that we have an idea of what components condensation might be important in, what is the regime of interest for those components. I did kind of a little miniature PIRT myself to give me an idea of what regimes I have to consider. I have a list of the components, the pressure in bar over the time period of which condensation may be important, vapor Reynolds number, film Reynolds number, and the noncondensable gas fraction.

The numbers for the SPWR I am fairly confident about because those come from the GE slide presented to the ACRS in an open meeting. These numbers --

MR. CATTON: But with the GE, we also felt that the bounds they put on the nitrogen concentration were too low.

MR. KELLY: Well, this would be in audit.

MR. CATTON: It should probably go all the way up to 80 or 90 percent, even one should be able to calculate starting from pure nitrogen in the tubes.

MR. KELLY: I agree. For the AP600, these numbers I calculated very crude hand calculations, and so note they are extremely approximate. But I wanted some idea of whether or not the film would be entirely laminar or whether some of it might be turbulent.

MR. SCHROCK: Excuse me, Joe. Are you using four gamma over nu or gamma over nu?

MR. KELLY: Four gamma.

MR. SCHROCK: Four gamma?

MR. KELLY: Yes.

MR. SCHROCK: Thank you.

MR. KELLY: That will be on one of my slides, but that is always a source of confusion.

[Slide.]

MR. KELLY: That finished the introduction, so now on the background part, it is broken up into what does this physically look like, what does it look like in a two-fluid code, and in the three areas, the heat flux apportioning, surface partitioning and the noncondensable gas effects.

[Slide.]

MR. KELLY: There are two pictures here, the top one is for a pure saturated vapor, the bottom one is the vapor with noncondensable gases, and these are reprinted from Butterworth & Hewitt without permission.

So in the saturated vapor case, all the vapor is at saturation. There is a small temperature drop at the liquid vapor interface, and the primary resistance heat transfer is across the liquid film. In the case of noncondensable gases, there is a temperature drop across the vapor gas mixture that can be comparable or even larger than the temperature drop across the liquid film.

[Slide.]

MR. KELLY: Now we will talk about the two-fluid model and how it treats heat transfer especially with respect to condensation. The wall heat transfer rate shown here can have up to three components. As you know, it normally only has one of these, but this would be the heat transfer from the wall to the liquid, wall to vapor, or something that I call the wall to interface heat transfer.

MR. ZUBER: Can you relate those to the pictures you had on the preceding slide?

MR. KELLY: Yes, and I will do that on the next slide, I will give you an example of what it looks like for film condensation.

MR. ZUBER: I just want to see where are these fluxes on the curve.

MR. KELLY: On this one, this is film condensation in a saturated steam environment. The only one of these parameters that is not zero is wall to liquid. The wall is not in contact with the vapor. The Q wall vapor should be zero, likewise Q wall interface.

MR. SCHROCK: What does Q wall interface mean?

MR. KELLY: I knew I was going to get what question.

MR. ZUBER: On that graph, can you show it again?

MR. KELLY: Yes.

MR. CATTON: Maybe just leave it up there.

MR. KELLY: I was going to get another back-up slide. I can only talk out of two sides of my mouth.

[Slide.]

MR. KELLY: The reason that QWI from wall to interface exists is because in the two-fluid model we only have -- this is an example of subcooled boiling which has nothing to do with condensation, but it explains why this term exists. The two-fluid model only has one liquid temperature per computational cell, and our computational cell volumes are large with respect to things like boundary layers. In the case of subcooled nuclear boiling, in the near wall region, there is a superheated layer and the nucleation occurs at the heater surface as, of course, you know.

The bulk liquid is subcooled. The bubbles in this region are condensing. So our bulk liquid temperature is subcooled, T liquid. So any heat from the wall to the liquid goes into sensible heat and not generating vapor. The only way we can generate vapor is if we force it by having a heat flux from the wall to the interface, and that is this term, and that goes directly into a mass transfer term.

MR. SCHROCK: But the code contains the interfacial heat and mass transfer, the physics are that the heat is transferred from the wall to the liquid, and then from the liquid to the interface driving heat and mass transfer at the interface.

So if the interfacial area treatment is correct and the interfacial heat transfer coefficient is correct, then you ought to get the heat flux at the interface correctly, the evaporation condensation effects averaged over the cells admittedly include averaging over zones in which different things are happening at different parts of the interface.

MR. KELLY: To do what you are suggesting, I would have to take one computational cell which may be out to here, break it into a layer next to the wall where the liquid is superheated and a layer outside the wall where the liquid is subcooled.

So what in effect I am doing is, I am using an empirical model to tell me what fraction of the heat flux from the wall is generating vapor in that near wall region. So I am not modelling this region exclusively, but I am forcing a vapor generation rate at the wall.

Then in the subcooled bulk, that vapor can condense, as you say, due to the interfacial heat transfer.

MR. SCHROCK: But the vapor generation is not at the wall, the vapor generation is at the interface and interface is distributed throughout the superheated boundary layer.

MR. WULFF: It is not at the wall.

MR. SCHROCK: It is not at the wall.

MR. KELLY: That is true, but we are talking in a very microcosm which we can't resolve within the context of the computational framework.

MR. DHIR: But the correlations also give you some kind of an average heat transfer coefficient at the wall.

MR. KELLY: Yes, and we have to take that heat flux and split it into what goes into sensible heat transport and what causes vapor generation.

MR. DHIR: How do you split it, you have to either know the details or it will be just a factor.

MR. KELLY: I agree. Actually at this point I am going to defer to Gary Johnsen because I am not talking about subcool and nuclear boiling, but if you want to wait until your talk on heat transfers, it is your choice.

MR. SCHROCK: The reason I questioned it is that this is something that has been questioned now for many years by consultants to the ACRS, there has been an insistence from Idaho that there is a physical basis on which this misconception makes some sense, but we never see a rational explanation of that physical basis.

Now you want to perpetuate the utilization of this fuzzy concept, but you want to defer to the originators of it to support its significance in the evaluation of these transient two-phased flows.

MR. KELLY: I will defend it on that standpoint, but what I don't know is the details of how they split the heat flux in RELAP5.

MR. JOHNSEN: For subcooled boiling, we use

Lahey's model for determining the point of net vapor generation.

MR. ZUBER: What did you use for bubbly?

MR. JOHNSEN: You mean for saturated?

MR. ZUBER: Yes.

MR. JOHNSEN: Then it all goes into vapor generation.

MR. KELLY: Or does it all go into the liquid, Gary?

MR. ZUBER: No, you just have liquid. You don't nucleate, you just have bubbly flow.

MR. JOHNSEN: Just saturated liquid boiling flow?

MR. ZUBER: Bubbly flow.

MR. JOHNSEN: Without heat transfer or with heat transfer?

MR. ZUBER: It can be heat transfer, but you are not boiling.

MR. JOHNSEN: I guess I don't understand.

MR. DHIR: Let's say he has a gas/liquid mixture flowing over the wall which is heated but no boiling on the wall.

MR. JOHNSEN: Okay.

MR. ZUBER: But you have void fraction, let's say .3-.4.

MR. JOHNSEN: In all likelihood, that would be using Dittus Boelter to the liquid, force convection to the liquid.

MR. ZUBER: In all likelihood. They way I look --

MR. JOHNSEN: The reason I said it is that you have specified a situation without regard to mentioning flow rates, degree of superheat on the wall, or anything, so I have to give you an answer that is indefinite.

MR. ZUBER: The thing is, if you partition that heat flux, the way you partition the heat flux is according to the void fraction in the cell and you say 40 percent goes to the vapor and 40 percent goes to the liquid. Although you may have no vapor in contact with the wall.

MR. JOHNSEN: In bubbly flow it all goes to the liquid. Let me just make a point about this, I am going to talk about this later, Virgil, but there was a problem, I think, with semantics here where we are getting a little hung up.

MR. CATTON: If you are going to talk about it later, let's let Joe continue. I am worried about time.

MR. KELLY: At any rate, for condensation this term is not used, although it is in the context of COBRA/TRAC or COBRA/NC which eventually became GOTHIC, and that is because there is analog between drop-wise condensation and subcooled nuclear boiling, and it is used to start the condensation when there is no liquid present in the cell. So there is no interfacial area initially, but that is not really important here.

For condensation -- actually, I didn't finish this slide. This is the wall heat transfer and this is what goes to the wall conduction solution. These two terms are the phasic heat transfer rates as the heat transfer coefficient and the driving the potential is wall temperature and the phasic temperature. This is what is input to the liquid energy equation, and this is what is input to the liquid vapor equation. The vapor generation due to this does have an input in both of the energy equations and, of course, in the mass equation.

[Slide.]

MR. KELLY: The condensation rate in the code is calculated as the sum of the interfacial heat transfer rates. That is liquid interface, vapor interface, over the light and heat. In RELAP5 this term does not exist for condensation.

The interfacial ones are calculated as a product of the heat transfer coefficient from the liquid interface, interfacial area and the driving potential is the bulk liquid temperature to the interface temperature or the bulk vapor temperature to the interface temperature.

MR. ZUBER: What is gamma sub-W?

MR. KELLY: That would be vapor generation due to the heat flux from the wall to the interface, the one that I had talked about for subcool and nuclear boiling. It is not used in condensation. It was included here for completeness, that's all.

MR. SCHROCK: How does the code know what is happening when there is condensation in part of the computational cell and the evaporation in other parts of the computational cell, how does it identify that this is a condensation problem and not some other problem in which gamma sub-W ought to be there based on total wall heat flux or what?

MR. KELLY: You are talking about splitting a computational cell into two pieces and calculating different ings for both halves of the computational cell, correct?

MR. SCHROCK: No. I am pointing out simply that there will exist in the physical world within a cross-section included in a computational cell evaporation in part of that volume and condensation in another part of the volume simultaneously in a given time period.

MR. KELLY: Is this the picture that you are talking about?

MR. SCHROCK: That is one of them.

MR. KELLY: In this one, the implicit solution in the code for the mass energy transfer would be condensation because the bulk liquid is subcooled.

MR. SCHROCK: I don't think you have gotten the gist of my question.

MR. KELLY: Probably not.

MR. SCHROCK: The gamma sub-W you have put your hand over and said, this isn't used in condensation, it is not in RELAP5 when there is condensation. What I have asked is, how does RELAP5 make a decision. I am now working on condensation instead of evaporation?

MR. KELLY: It depends on the temperature difference between the wall and the local saturation temperature. If a wall is subcooled with respect to that saturation temperature, it goes through and there is a logic for the heat transfer package but it depends on things like void fraction and wall temperature. So, for example, if wall temperature is above T-sat, it goes to look for a nuclear boiling type heat transfer correlations. If it is below T-sat, it looks for condensation type correlations.

Did you want to say something, Gary?

MR. JOHNSEN: I just wanted to indicate that, in fact, the code does allow you to have multiple situations within one control volume. You can have, for example, one heat structure that is cool with respect to the coolant, and another that is much warmer. So you conceivably could have boiling and condensation on two different heat structures connected to the same cell, and that would result in a summation of mass transfer terms that would result in net mass transfer based on which one of those mechanisms was stronger.

MR. SCHROCK: So there is a logic that is based on total wall heat flux?

MR. JOHNSEN: No, total vapor generation or vapor generation which could be positive or negative based on summing up those terms for each heat transfer surface. So that gamma W that he is showing you is the mass transfer associated with a particular heat structure within a control volume.

This capability then enables you to have boiling in volume that is subcooled where there is boiling near the wall and condensing in the bulk. Depending upon the magnitudes of those terms, the condensing versus the boiling, you either have net vapor generation or you don't.

MR. ZUBER: I don't understand it but anyway apply it now to condensation. You would not have this term in film condensation, is that correct?

MR. KELLY: That's correct. This is an example of film condensation in a pure steam environment. The interface temperature and the vapor temperature at T-sat, the heat transfer terms from the vapor to the interface, wall to vapor or wall to interface are all zero.

The wall heat flux is simply equal to the heat flux you take out of the liquid and that in turn is equal to the heat flux that you take from the saturated interface and that produces the condensation rate, just as you would expect.

MR. ZUBER: Okay, now can you move to the first equation for gamma? I have no problem with the first term. I think that is correct. I really don't understand the second and maybe we can talk later about it.

MR. KELLY: Okay, well, I'll hit it real quick. Let's go back to subcooled --

MR. ZUBER: Why?

MR. KELLY: Because that is the place where it is most important in the code. In that case the liquid temperature is less than the saturation temperature so this Qli is negative because we are condensing vapor in the bulk. This term we'll say is zero, so this first term is negative. We are condensing vapor but we want to generate -- this is subcooled nuclear boiling so we are generating vapor at the wall and allowing it to condense in the bulk. That is what Gary was saying.

This term is the vapor generation at the wall. It is the sum of these two that determines whether we have net vapor generation or net condensation but because we only have one liquid temperature in the code, the only interfacial heat transfer we can get when the liquid is subcooled is condensation so this term would be condensation for that case.

[Slide.]

MR. KELLY: One of the things that was criticized by both this committee and the NRC consultants was what I call heat flux apportioning. Now this is what was done in RELAP5, Mod 3, and this was about three years ago when it was released and I am only talking about during film condensation now, okay? The wall heat flux took a heat transfer coefficient to the condensation and the driving potential was T-wall minus T-sat or the wall subcooling.

This heat flux was taken out of the liquid phase as it should be. The other heat fluxes were set equal to zero, again as they should be, following the physical representation.

The condensation rate should then be that wall heat transfer divided by the latent heat, and that has to equal the liquid interfacial heat transfer and the difference between the bulk film temperature and saturation.

Now that is all fine, and it is as you expect but it caused a numerical problem and the reason is that the heat transfer from the wall to the liquid was using as a driving potential T-wall minus T-sat and not the liquid phasic temperature, so it decoupled the heat transfer from the liquid temperature. Consequently the liquid could be colder than the wall but the code would continue to remove heat from that liquid film, which obviously is not physical but it also caused a numerical problem.

It in effect caused the liquid to freeze and the code to bomb, have to back up and take small time steps.

[Slide.]

MR. KELLY: So this was fixed as part of the so-called "get well program" and this is how it was fixed. This is RELAP5, Mod 3.1.

Again calculating the wall heat transfer this same way, the condensation coefficient and the wall subcooling is the driving potential, and you do that because all of the coefficients in the literature are defined with respect to that but -- and here is what none of us were real happy about -- the wall heat flux was split in two parts, the wall to the liquid used the same heat transfer coefficient but now it took as the driving potential T-wall minus T-liq, which is good for the numerics, okay?

MR. DHIR: But not physically correct.

MR. KELLY: Right, because this number is less than this, so the extra heat transfer, because you wanted to get the right total, was taken out of the vapor phase and that does not, it is not physically correct because the vapor is not in touch with the wall, but anyway that is just to be complete about how the condensation rate was calculated.

MR. ZUBER: Pardon me. This is the first time I really hear a good criticism of the model which you were bringing it up and I think this is a good explanation why this was introduced. The first time I hear it after five years.

MR. KELLY: Okay, so this solved the numerical problem and got them past that point so they could compute, but as we have just discussed, it is not physically based because the vapor is not in contact with the wall.

Also it causes, it induces vapor subcooling in order to get that condensation right because we are taking heat out of the vapor.

Secondly, the liquid temperature, the film temperature if you will, now depends on this ratio of the heat transfer coefficient due to condensation here and what the interfacial heat transfer coefficient is.

These were derived in separate models. They are not consistent so what the film temperature is is not really being based on a physical model.

[Slide.]

MR. KELLY: This is one of the motivations for me doing the work that I am hoping that I'll get to present.

MR. ZUBER: When did you start?

MR. KELLY: I started working at the NRC in June.

I started working on the condensation model about the beginning of August and I get to spend roughly 40 percent of my time doing technical work.

MR. CATTON: Gee, that's pretty good.

MR. ZUBER: It's very good.

MR. CATTON: I hope you can maintain that.

MR. SHOTKIN: We try to protect him.

MR. KELLY: Exactly. My management has been very good about that.

MR. SCHROCK: You ought to go for 80 percent.

[Laughter.]

MR. WILKINS: You can't do that at a university.

MR. KELLY: The surface partitioning was done in RELAP5, Mod 3 and this is where they wanted to have some idea of how much of the surface was wet or dry when there was only a very small amount of liquid in the cell, and this is what they did.

They took a minimum film thickness, calculated a fraction of the surface wet, took film condensation on the wet part, single phase convection to the vapor on the dry.

In RELAP5, Mod 3.1.1, which is the new version released I think last November, there is no surface partitioning. If the wall is subcooled, then you use condensation heat transfer and then there is a ramp to single phase liquid as the void fraction gets low.

MR. ZUBER: You are correct.

[Slide.]

MR. KELLY: The next item was the noncondensible gas effects. RELAP5, Mod 3.0 there was a degradation factor used. However, as was stated in Volume 4, this model was of unknown origin and also has very limited assessment and so this is the model that you reviewed, so that was recognized as a deficiency in the code.

They went to correct it and this is what is in RELAP5, Mod 3.1.1.

They put in what is now called the Berkeley-Vierow-Schrock model.

On the film side, it uses a Nusselt formula with an empirical flow factor to take accounts of when the film is sheered.

On the gas side, there is an empirical degradation factor.

This worked reasonably well at low pressure, for example for the PCCS of an SBWR. However, it was found to significantly over-predict condensation rates at high pressure, for example the ICS conditions, so for this what was put in was on the film side the maximum of a Nusselt formula and a Shaw correlation and on the gas side, a Colburn-Hougen type model where there was no sensible heat transfer, and the Guilliland correlation was used for the mass transfer.

MR. DHIR: On the gas side, the degradation factor does it depend upon the composition?

MR. KELLY: Yes.

MR. DHIR: How does it relate to the compositions?

MR. KELLY: Pardon?

MR. DHIR: How does it relate to the composition of the mixture?

MR. KELLY: You are asking what is the form?

MR. DHIR: Yes.

MR. KELLY: I don't know.

MR. DHIR: There's a new version. I have not seen the write-up of RELAP.

MR. KELLY: Okay. On the Colburn-Hougen -- okay, now, this one you can actually ask Professor Schrock what the form is because he would know it much better than I do, and I don't have it on the top of my head.

MR. DHIR: Gas side --

MR. KELLY: But it is an empirical degradation factor based upon the bulk vapor partial pressure or bulk gas partial pressure, if you will. Okay?

MR. SCHROCK: Are you going to go into further detail in describing this --

MR. KELLY: Yes.

MR. SCHROCK: -- or is this it?

MR. KELLY: Well, that's all that I am going to give you on what is in RELAP today because that is not my mission.

What I am doing is trying to develop a new model that I think would be more applicable. That is what I am going to present today is the beginnings of that new model.

If that model is acceptable, it will be incorporated into RELAP5.

MR. SCHROCK: See, the reason I ask is that it isn't clear to me what is in Mod 3.1.1 from this description. I would like to know that.

MR. KELLY: Okay.

MR. SCHROCK: Am I going to learn that from a later presentation here today?

MR. KELLY: Probably not. What can be sent to you, they have software design implementation documents that they put out any time there is a change in the code. It's kind of a Q-A. That will describe the model and that can be sent to you.

MR. CATTON: That would be very helpful.

MR. LAUBEN: Could I make a point about this? I'm Norm Lauben.

Please understand that this is interim and from the schedules I showed yesterday we would like to begin 3.1.2 validation in the summer and what you would see in

3.1.1 would be -- it's not likely to be there for very long, but I'm sure we can give it to you anyway.

MR. SCHROCK: Well, whether it's there long or not, I am interested in knowing what is in the code attributed to my work.

MR. LAUBEN: Okay.

MR. SCHROCK: And one of the problems that we have in looking at the relationship between what is in these computer codes and what researchers have developed in their research efforts is that there is frequently a miscommunication.

The code contains something which is attributed, the Chen correlation is a shining example, when John Chen has told me he's horrified by what is being represented as his correlation. I know from my own experience that my correlation on forced convection boiling was misrepresented in the RELAP codes for years. It's misrepresented in textbooks as a consequence of that misrepresentation in RELAP codes over the years so I would like to simply make the point that there is a responsibility in the professional world to reveal what you are doing with somebody else's work where you are making reference to it.

MR. SHOTKIN: Mr. Chairman, I just have one comment that maybe you ought to look into.

There could be a conflict of interest of having a reviewer reviewing his own work on the subcommittee. You might want to consider that.

MR. CATTON: I will, but I think letting him take a look at what you did is a reasonable thing also.

MR. KELLY: I agree that when correlations are put in the code they need to be put into the code the way they were developed and used that way and that they should be well-documented to show that.

I cannot do that with respect to this because I did not do this work and so I am not familiar with the details.

What I can present to you is the work that I am doing now and I will give you details on that.

MR. CATTON: Okay.

MR. WILKINS: Lou, with respect to the remark you made, I don't see a conflict here. Virgil is not being asked to review his work. He's being asked to look at work other people did which they relied on some research work that he's published in the literature, and I think we're privileged to have him look at that because he knows more about that than anybody else.

I don't think there is a real conflict here, at least not on the basis of what has been said here this morning.

[Slide.]

MR. KELLY: The model that I am starting to develop has these objectives. I want to be able to correctly or at least as well as possible calculate the condensation rate but also the film subcooling. I also want this model to have a very large data base so it is applicable with as wide a range as possible.

In particular, it has to work for falling films and sheered films, and also laminar and turbulent films.

The constraints on this model -- it has to be physically based. It has to be compatible with the two-fluid framework, and I cannot introduce any new numerical problems.

The difficulties in doing this are, first, that condensation heat transfer coefficients are based on the saturation temperature as a subtemperature. I need to specify a wall heat transfer rate that uses the phasic liquid temperature as the sink temperature and then an interfacial heat transfer rate between that and the saturated interface.

The next part is how do you specify that liquid to interface heat transfer coefficient, because it is extremely difficult to measure.

MR. ZUBER: Let me ask you, maybe you could simplify it. The way you try to approach it you want to really kill all the condensation problems in one stroke, subcooled boiling and condensation on walls, film, flow. The question is really what are the conditions where subcooled boiling is important compared for example to condensation with films, because if you have condensation in bubbles collapsing it's not simple boiling, so I think you could probably simplify it by just looking at films.

MR. KELLY: That is all I am doing, yes. I am not handling direct contact condensation here.

MR. ZUBER: Okay.

MR. KELLY: The subcooled nuclear boiling only came up to try to explain what that Qwi term was.

MR. ZUBER: And it only comes for subcooled boiling.

MR. KELLY: Right.

MR. ZUBER: Okay.

MR. KELLY: That is a completely separate issue. I am not planning on mentioning it again.

MR. ZUBER: Okay, good, because I am trying to understand.

MR. KELLY: It gets confusing. That's why I gave the background was so we could try to discuss that and make sure that we were talking about the same things.

The last difficulty in doing this is of course the non-condensible gas effects. We need, we believe we need a better model, and it's one in which we need to calculate the interface temperature, which is a function of the partial pressure of the vapor at the interface and not the bulk partial pressure.

MR. DHIR: What are you trying to imply by this actual line? I don't follow.

MR. KELLY: Okay. Give me two slides and I'll show you where it is, okay?

[Slide.]

MR. KELLY: The idea is to calculate the wall film condensation as a series of heat transfer resistances. What I have shown here -- it is not a very good drawing -- but you see the wall, the liquid film, and the vapor region. I will identify four temperatures for you. The wall temperature. This is the bulk liquid temperature in the film, the temperature at the liquid vapor interface, and the temperature in the bulk.

Here I have shown the saturation at the bulk vapor partial pressure.

MR. ZUBER: Question. Question.

MR. KELLY: Yes?

MR. ZUBER: Why do you differentiate temperature of the interface? The interface is always in thermal-dynamically at saturation, so why do you have the I?

MR. KELLY: It is saturation, but it is saturation at the partial pressure of the vapor at the interface.

MR. ZUBER: Okay. So, you are considering --

MR. KELLY: Noncondensables --

MR. ZUBER: Okay. Go ahead.

MR. CATTON: You are going to use a Plesset-type heat transfer coefficient, based on the vapor pressure differences?

MR. KELLY: Basically, yes. Actually, I am going to follow -- that is the third part of this presentation. I am going to follow the development that was done at Berkeley by Peterson and Kajilami.

MR. DHIR: I don't follow. What are you trying to get? How can you get the RLI? There is no correlation which tell you RLI.

MR. KELLY: That is what I am going to tell you today. Okay?

MR. CATTON: There soon will be one?

MR. KELLY: I am going to infer one, yes. That is the point of this model.

[Slide.]

MR. KELLY: Okay. I am going to talk about the film side resistance now, and that has two parts: The resistance from the wall to the bulk liquid temperature, and at bulk liquid temperature to that interface -- the two resistances.

MR. SCHROCK: I would take exception to this concept. The Nusselt analysis treats the temperature distribution in the liquid film. There is a resistance to heat transfer across the liquid film and the temperature potential driving that is the difference between the interface temperature, which is the saturation temperature for the prevailing pressure, and the wall temperature. There is only an artificial thing that is created here by dealing with the bulk temperature of the liquid film.

MR. KELLY: I agree.

MR. SCHROCK: So, what is the -- what is the physics of the problem that gives rise to a resistance between the wall temperature and the bulk temperature and then another resistance between the bulk temperature and the interface temperature? That is artificial?

MR. KELLY: In a way, yes. And I am splitting it this way to make it fit within the construct of the two-fluid model. Okay.

[Slide.]

MR. KELLY: Let me put this back up. So, what you are saying --

MR. CATTON: What you are trying to do is to match the physics against the numerical algorithms.

MR. KELLY: The numerics. Exactly.

MR. CATTON: You are going to have to divide things up a little bit differently, but you want to maintain the physics; is that correct?

MR. KELLY: Exactly. So, what we know, if we have a condensation heat transfer coefficient, is the sum of these two resistances. We know the total resistance across the film, just as Professor Schrock said. This is the driving potential, T-Wall to the interface temperature. But, in the code numerically that doesn't work, as we discussed earlier. We need to use the phase liquid temperature for the heat flux from the wall to the liquid, otherwise, we get the numerical problem. So, my task is to find a way to approximate with some reasonability these two resistances so that the sum of them is correct and the ratio between the two is reasonable so that the film temperature that I get is reasonable.

MR. WULFF: Could I ask a question? The heat fluxes are the same in all of the points?

MR. KELLY: Yes. And, if it is steady state, yes.

MR. WULFF: Well, when you use Nusselt you imply that?

MR. KELLY: Yes, exactly. That's correct.

MR. WULFF: All right. Why do you need this -- this breaking up if you already know the fluxes? You could match your two-fluid concept, since you know that already. I am not clear why you have to introduce this artificial resistance.

MR. KELLY: Okay. The way it works numerically is you specify a heat transfer coefficient in an area between the wall and the liquid phase, and then you use the liquid phase temperature as the heat sink. Okay? That is what is stable numerically. So, that is why I have to split this up. But, I only get one wall heat flux.

MR. WULFF: But, in Nusselt, that is one-half times the sum of the wall temperature and the interface temperature because he has the linear temperature distribution already implied.

MR. KELLY: Yes. And, if you give me a chance, I will go through that. Actually, it is three-eighths times the wall temperature and five-eighths times the interface temperature, because you have to take into account the velocity distribution through the film.

MR. WULFF: That's correct.

MR. KELLY: That handles it for a laminar film with a smooth interface, so no rippling, and also the film isn't turbulent. But, that is what you start from. You are exactly right. That is where you start from and that is where I will go.

So, the idea is that, if I know a condensation heat transfer correlation, I know the sum of those two resistances, okay? For say a turbulent film, there are correlations for film heating and cooling where you only have sensible heat transport and not latent heat. So, then the driving potential is the wall temperature to the liquid phase temperature.

Based on that, I can approximate that resistance from the wall to the liquid film only as this -- where this is a heat transfer coefficient from a wall to a film with sensible heat transfer only. From that, I can then infer that liquid interface -- interfacial resistance simply by taking the difference of the two. So, what what I have done is I say I know this total. I have a pretty good guess at this part. I subtract out this part, and this gives me the interfacial part. I am doing that because this is so difficult to measure.

MR. WULFF: And what is H sub-heat?

MR. KELLY: This is based upon a heating or cooling correlation. So, you just have a liquid film falling down a surface with heat transfer to the film, changing the film's temperature, but not causing a phase change.

MR. WULFF: Yes, but you take a correlation. Can you tell us, would that be on the flat plate?

MR. KELLY: Pardon? Well, if it is laminar.

MR. WULFF: If you show, go on.

MR. KELLY: Yes. I am going to go through all of the gory details.

MR. SCHROCK: Wait. Wait, Joey.

MR. KELLY: Yes?

MR. SCHROCK: You have got a problem with this concept. It is the following. The temperature distribution for the film cooling, in the absence of phase change, or heat transfer at the surface of the film will be --

MR. KELLY: They are different.

MR. SCHROCK: -- will be quite different from the temperature profile in the case of condensation in which the heat flux is essentially uniform through the film thickness. And the kind of problem leading to the correlation that you want to use for this heat transfer coefficient, that is not true.

MR. KELLY: You are correct.

MR. SCHROCK: Okay. So, you won't get --

MR. KELLY: What I am trying to do --

MR. SCHROCK: -- the right heat transfer coefficient for your fictitious resistance from wall to bulk --

MR. KELLY: You are correct, but I --

MR. SCHROCK: -- for the condensation problem from a film --

MR. KELLY: -- will get a better guess at it than what we have now. That is the point of this.

MR. SCHROCK: I wouldn't agree at all. You can analyze the problem and get it without recourse to a correlation.

MR. KELLY: That is exactly what I am going to do for a laminar film is use the analysis.

[Slide.]

MR. KELLY: Going to the other side, which is supposed to be the difficult part, the vapor side resistance. In a pure steam environment it is negligible. And this just shows you what is in RELAP5. Basically, it is a very large number. It is large enough to drive the vapor towards saturation, but not so large as to cause numerical problems.

Now, when you have vapor with non-condensable gasses, this is different. What I am going to propose is to use the approach of a turbulent mass transfer coefficient, and then find the interface temperature.

MR. DHIR: What do you do if you have super-heated steam?

MR. KELLY: You superimpose the two -- the convective flux and you superimpose the two, so you calculate both.

MR. DHIR: You subtract? You are putting in heat one place and taking out heat.

MR. KELLY: Right. You superimpose the two, since one is positive and one is negative. You are right. You actually work within the confines of the two-fluid code.

[Slide.]

MR. KELLY: Okay. Now I am going to get to start getting to some of the details of the work I am doing. Basically all I have shown you up till now is the introduction. The first thing I am going to talk about is the film thickness. The reason for this is it is very important because it determines the resistances across the film. All of the resistances scale with respect to the film thickness. I am going to break it into two parts. I am going to talk about falling films, which we might expect in something like the CMT walls, and sheared films, which you see like in the ICS condenser and in the SBWR. It is going to be further broken down to a laminar smooth, where I am going to start with Nusselt, Laminar Wavy, turbulent, et cetera. And then for a sheared film, you have to talk about interfacial sheer and entrainment.

[Slide.]

MR. KELLY: As you have suggested, you can do this analytically for a smooth laminar film. This is a film thickness given by Nusselt. It is just a parabolic velocity profile integrated. M is the film thickness. M-star is the film thickness made non-dimensional with respect to the Nusselt film thickness parameter shown here.

The film Reynolds number is defined this way. It is four times the mass flow rate per unit wetted perimeter, divided by the viscosity. I will talk about this just briefly later. This is a correlation. Actually it is the result of the calculation by Kapitsa for a laminar film with surface ripples. It is only valid over a very small range of Reynolds number.

MR. ZUBER: I have a question. Where do you find it?

MR. KELLY: In Advances in Chemical Engineering there is an article by Fulford.

MR. ZUBER: The interesting thing about this -- he wrote two very good papers on film flow in '48. At that time he was black-listed by Stalin, and he was not allowed to work on anything else, so he did very beautiful work on films. After 50 years or 40 years, it is still useful. It is nice that you really found it.

MR. DHIR: There are two points here. One is what is the transition number where you go to laminar of every film? Second, I would say that there are better correlations available in the literature now. If you look at recent textbooks, they have a correlation which includes laminar, wavy interpreted. There is one correlation which will cover all of it.

MR. KELLY: Can you give me a reference on that?

MR. DHIR: Yes.

MR. KELLY: Okay. Because I have not found that one.

MR. DHIR: Yes, there are.

[Slide.]

MR. KELLY: But, what I did was I constructed a database of all of the film thickness data I was able to find. So, these are falling films, and it is 411 points from a wide variety of sources. Most -- let's see, all of this data -- most of this data is isothermal, so most of it is just water or a different fluid running down in either the inside or outside of the tube. I plotted the non-dimensional film thickness, M-star, versus the film Reynolds number. So, there are 411 points. Here this line is a Nusselt formula. You notice there is some -- a lot of the data falls a little bit below this. This is primarily the results of Chien & Ibele. I have that as a back-up slide; but I had too much detail, so I had to take it out.

In the turbulent regime, I looked at I think nine different film thickness correlations. None of them matched the high Reynolds number dependents correctly. So, I did a simple curve fit over the high Reynolds number region and came up with this value. What you see here are the two. This is the predicted over measured, so it give you an idea of the error, if you just take the maximum of those two formulas. You can improve that a little, if you wish, by putting in a transition region which you see here. And you will notice the transition region is very long. That is characteristic of liquid films because sublayers are large with respect to the film thickness. So, they interfere with each other, and it is a much longer transition region.

At the moment I am not proposing adding this extra complexity because you don't gain very much in accuracy. The data is just too widely spread.

[Slide.]

MR. KELLY: So, for falling films, what I am using at present is the following. This is the Nusselt for laminar. For turbulent, the curve fit that I showed, taking the maximum of the two. For a falling film, you can then take these non-dimensional film thicknesses and turn them into wall friction factors. That is what is shown here. Yes?

MR. DHIR: Where is your lens scale coming in and how do you relate this to the heat transfer coefficient?

MR. KELLY: Okay. The link scale and the Reynolds number?

MR. DHIR: Yes?

MR. KELLY: Okay. As I showed earlier, the Reynolds number is the standard one for films, so it is four times the flow rate, divided by the weight of perimeter.

MR. DHIR: I don't know that one.

MR. KELLY: Okay. When we talk about heat transfer I will say something about that. But, what I propose doing is using the local values calculated by the code. So, the local liquid velocity, the local liquid mass flux, calculated from the previous time step.

MR. DHIR: But, you are adding mass as condensation is going on. How do you know how much you are adding? Because that is driven by the temperature difference and how far you are from the leading edge of your condenser.

MR. KELLY: Well, I will let RELAP5 do the spacial and temporal integration. So, what you have done -- say you have a wall, okay, in a steam environment, and the wall is subcooled with respect to the steam. It will begin to start a film initially uniformly and then the film will start falling and get thicker as it goes down, okay? If the noding degradation is fine enough and you correctly specify the heat transfer resistance cross the film, then the code will do the spacial and temporal integration.

MR. DHIR: Heat transfer itself depends on M dot. It is an iterative process. You cannot do it. You have got to go back and recheck it. Okay?

MR. KELLY: Well, it depends on M dot because it affects the film thickness.

MR. DHIR: Yes. It affects the heat transfer.

MR. KELLY: Yes. Now, remember, we are talking about a computer code that is at the current time Currant time step-limited. So, the film thickness will change over time.

MR. DHIR: No, no, no. Even steady-state I am saying. Let's say steady state. Forget about the transient of how the film develops. Steady state you cannot calculate heat transfer coefficient at a given location just from this expression you have. You have to tie it to the old energy balance -- that how much energy I have taken out that gives me a condensate layer thickness. That, in turn, tells me what the heat transfer is at that location.

MR. KELLY: Okay.

MR. DHIR: The history affects it.

MR. KELLY: Yes. I agree. I am saying the history effects will be taken into account by the temporal integration of the partial differential equations in the code. So, will you give me that if at one point in time -- if at one instant in time, at one point in space I know the film thickness.

MR. DHIR: You have to do it. Okay.

MR. WULFF: No. He takes the last time he lags -- has some damping from -- that is the way I understand it.

MR. CATTON: So, essentially he is using the heat transfer coefficient derived from the previous step.

MR. KELLY: Exactly.

MR. CATTON: So, you are just slightly behind in the heat transfer coefficient?

MR. KELLY: Exactly. All of the heat transfer coefficients --

MR. CATTON: That is such an improvement over what is going n now. I think that we ought to cheer.

. SCHROCK: What about the interfacial shear effect? Your database here is simple falling film.

MR. KELLY: I will get to that.

MR. SCH Oh, you are coming to it?

MR. KELLY: Oh, yes.

MR. SCHROCK: Thank you.

[Slide.]

MR. KELLY: In fact, sheared films is the next topic. In RELA by solving the partial differential equations, the momentum and mass equations, we calculate a void fraction or, in effect, the liquid fraction, and hence, the film thickness as a function of space and time. So, we get that directly from the solution of the field equations with a big proviso -- you have to have the correct constitutive relations for that regime. In particular, the wall friction factor, which I have just showed, interfacial friction, entrainment, if the vapor velocity is high enough, and then the momentum transfer due to phase change. Now, this is normally quite small. It is only important for very low velocity vapor flows where the interfacial friction is not very high.

[Slide.]

MR. KELLY: Now, it is always best to start simple, where you know the answer. That is what I am going to do. I am going to go to a laminar smooth film and see what the effect the shear stress has. The linear shear stress distribution within the film, standard annular flow-type model, and you go through this.

Down at the bottom you see the film Reynolds number is expressed as a function of the film thickness and its quantity in brackets. This is the interfacial shear stress. This is two-thirds times the gravity -- the shear stress due to gravity on the film. Okay? I am going to introduce this as a characteristic shear stress on the next slide, and I am not the first person to do this. It was done by Professor Hanratty at the University of Illinois. As far as I know, that was the very first use of it.

[Slide.]

MR. KELLY: So, now I have taken -- from the equation I showed on the last slide, which I will put up here, I turned it around and I now have the film thickness, non-dimensionalized as a function of the film Reynolds number. So, all I did was take this and solve for M, and you get the expression over there. So, this is following Professor Hanratty's development. So, now I have a characteristic shear stress which is defined here. The point of this is this is only a function of the film Reynolds number. So, for a laminar smooth film, M-plus is the same, whether the film is sheared or free-falling -- or, excuse me, falling on the wall.

MR. SCHROCK: Wait, wait. You have got tell I in there -- it is a function of tell I.

MR. KELLY: Yes. You are exactly right. If --

MR. SCHROCK: I thought you just said it is only a function of the film Reynolds number.

MR. KELLY: M plus. M plus is only a function of film Reynolds number. The film thickness will now depend on that. But, this right-hand side is only the film Reynolds number. So, if I say the film Reynolds number is 500, then M plus non-dimensionalized using this characteristic shear stress is the same whether it is a sheared film or a falling film. Okay? So, for a laminar smooth film you can do it analytically.

Now, for a falling film, there is a direct relationship between this M plus and the M star that we used earlier. This is simply it. So, if I take the relation I had for M plus -- excuse me, for M star, I can get an M plus for a turbulent film. And the question is -- we showed M plus as a unique function of the film Reynolds number for a laminar film -- is it true for a turbulent film?

MR. SCHROCK: Could you back up just to the top of that and tell me once again how you established that first equation as M plus as -- the definition of M-plus is in the second line. The equation comes from what?

MR. KELLY: Okay. This equation came from integrating the shear stress distribution across the film, getting a local velocity distribution, integrating that to get the average velocity, et cetera.

MR. SCHROCK: I think you just answered the question. You said that it is only a function of Film Reynolds Number. But, you definition of Film Reynolds Number includes dependence on tell I. So, tell I is in that first line equation on the nest page.

MR. KELLY: Now, my definition of film Reynolds number is the four gamma over MU.

MR. SCHROCK: Well, what is the line that you just showed at the top?

MR. KELLY: This is the result of the equations coming down to this point. So, what you could write this as four gamma over MU is equal to this.

MR. SCHROCK: Okay. Then my question is what is the basis of this equation M plus is equal to point seven Reynolds film two the one-half power?

MR. KELLY: From this equation, solve for M, and use this definition of M plus.

MR. SCHROCK: That is not my question.

MR. KELLY: Okay. Sorry.

MR. SCHROCK: The line right above that says M plus equals point seven film Reynolds to the one-half power. What is the basis of that equation?

MR. KELLY: I derived that from this, using this definition of M plus. It is just simple algebra.

MR. SCHROCK: Then what is the basis of the assertion that M plus depends only on film Reynolds number where film Reynolds number is defined simply as four gamma over MU? You see, that says there is no interfacial shear dependence.

MR. KELLY: On M plus. So that -- so the -- if you will, the influence of the interfacial friction is within M plus.

MR. SEALE: That is M in that equation.

MR. KELLY: Okay.

Let me show you the results. And I can give you the references by Professor Hanratty, if you would like to see that.

MR. KELLY: This is data by Andreussi and Zanelli, so it's co-current downflow which is exactly the situation we're looking at. And it gets confusing with the two different nondimensional definitions of film thickness but I'll try to keep them straight.

This is the M plus, nondimensionalized with respect to the Nusselt film thickness parameter. This is the M plus which is the one nondimensionalized with the characteristic shear stress which involves the interfacial friction.

What you see is the film thickness versus Gas Reynolds Number with a parameterization on Film Reynolds Number.

Now basically these are different liquid flow rates. You see a range of Film Reynolds Number because out in this part entrainment begins to occur so the Film Reynolds Number goes down. So the high numbers are what the Film Reynolds Number was at the start.

Andreussi and Zanelli measured the film thickness and the local pressure gradient. Consequently you can calculate the interfacial shear stress.

Doing that, if you know the interfacial shear stress and the film thickness you can calculate by characteristic shear, then you can nondimensionalize the film thickness to get M plus.

When you do that, the points fall pretty close to this line. This is the Nusselt formula as I showed before and this is the one that I got for a turbulent falling film.

And so this is only sheared film data but it falls almost on the falling film, very close to it.

MR. DHIR: In between you have every film, don't you?

MR. KELLY: Yes. All bets are off. The uncertainties are just huge, you know, but you do the best you can.

MR. DHIR: And let's say you assume tau is zero?

MR. KELLY: No. The formula -- remember, M plus is only -- it's defined as the film thickness times the characteristic shear stress, but what I've shown is that's only a function of the Film Reynolds Number.

So this Nusselt relation is the same one for the falling film but on the data points were reduced using the measured interfacial shear stress to nondimensionalize it.

MR. CATTON: You've re-scaled it?

MR. KELLY: Right. And what you see here is a fairly large collection of falling film. It's all the data I showed before plus sheared film from Andreussi and Zanelli and two or three other data sources.

So all of it on one plot is M plus versus the Film Reynolds Number and the Nusselt relation and the turbulent relation. They're not perfect but given the uncertainties, it's not bad.

MR. SCHROCK: It looks excellent.

MR. KELLY: Thanks.

Okay. But I haven't said yet how I'm going get the interfacial friction, and that's one of the correlations that I need.

So the first thing you do is you look at the ones that are out in the literature and see if they're applicable to co-current downflow in the regime we're looking at.

While they're not terribly good, I've compared three: the traditional Wallis model; a model by Asali and Hanratty which included some downflow data but was primarily for co-current upflow; and model by Henstock and Hanratty which was primarily co-current upflow but had a little bit of downflow data.

And what I plotted is the predicted interfacial friction factor versus the measure. And what you're seeing here is only the data of Andreussi and Zanelli.

There are three other data sources that I've been able to find for co-current downflow and I've requested them from the library. I haven't gotten them yet.

And what you see is Wallis pretty consistently underpredicts interfacial friction. Asali-Hanratty, the trend's pretty good except for a couple of points, but it's a little low. Henstock-Hanratty, the magnitude's pretty good but there's a lot of points that are way off the curve.

MR. ZUBER: Let me ask you. Do they both have an interfacial shear? I mean the tau sub i in RELAP?

MR. KELLY: Yes. That's how you end up calculating the void factor.

MR. ZUBER: No, but I mean are you using the same one or if you use their tau sub i, how would you compare the data?

MR. KELLY: Okay. The model that's in RELAP is very close. It's based on the Wallis model, okay? And so it's very close to what you see here. And what I'm going to say is they need to make the interfacial friction package better.

For the case of co-current downflow they're going to need a correlation that is based on co-current downflow data, just as you should have a different model for co-current upflow and one for counterflow.

MR. ZUBER: Good.

MR. KELLY: I have not yet finished this because I don't have the data sources yet. I've requested them and am waiting on them, but this is what it's going to look like.

This is the interfacial shear stress, the interfacial friction factor and what I use for annular flow is the bulk vapor velocity minus 1-1/2 times the bulk liquid velocity.

You know it's somewhere between 1 and 2 and 1-1/2 fits for laminar flow and for turbulent flow it's not.

This would be the surface, velocity at the surface.

MR. SCHROCK: I don't follow that statement. It should be the interface liquid velocity.

MR. KELLY: Right. I'm approximating that interface velocity by 1-1/2 times the bulk velocity.

MR. SCHROCK: What's the basis of that?

MR. KELLY: For a laminar free falling film it is 1-1/2 times the bulk at the interface. For a sheared film it's a function of the shear stress and film thickness, so it varies. And of course, when you have a film that has waves on it, the waves travel at different velocities and you don't really know what it is.

MR. SCHROCK: So it's basically laminar film?

MR. KELLY: Yes.

MR. SCHROCK: No interfacial shear. See, the relationship between the surface and the velocity, that is, the velocity profile and the film is influenced by the interfacial shear. So that the most simple solution of Nusselt's hydrodynamic model is of course not really the best thing where there is shear present.

MR. KELLY: I agree completely. However, 1-1/2 is a better approximation than 1 and the uncertainties here --

MR. SCHROCK: but for the laminar part of the calculation which is the most important thing, these condensers rarely get into turbulent film flow and so it's indeed possible to get a good determination of the interfacial velocity from the hydrodynamic model including shear in the absence of interfacial disturbances.

MR. KELLY: Exactly.

MR. SCHROCK: But that would be much better than choosing the number 1-1/2 that comes from the theoretical profile based on no interfacial shear.

MR. KELLY: But what I will then say is that the interfacial disturbances are always going to exist. So a more accurate analytical result for that -- and what you'll see is all the uncertainty is going to be here. What you put here almost doesn't matter.

MR. SCHROCK: But my point is that you don't need to do something so simplistic as 1-1/2 times the average velocity of the film which comes from the zero interfacial shear hydrodynamic model. There is a very simple hydrodynamic model with the interfacial shear dependence. Why not use that instead? It will be a much better approximation than the one that you've proposed here.

MR. KELLY: Well, basically there's very large uncertainty aside from that.

MR. ZUBER: Why don't you give it to him.

MR. KELLY: I will. If you go back a couple of slides you'll see a relation for velocity profile and a film with interfacial shear stress. That's it. If I know the interfacial shear stress and the film thickness I know that velocity, just as Professor Schrock is saying.

MR. DHIR: But you could use Vi to calculate that.

MR. KELLY: Exactly. So I could, but it just makes this more complicated and all the uncertainty is here. Most of the models that you see in the literature for an interfacial friction either use 1 or 2 or 0. A lot of the co-current upflow ones ignore the liquid velocity, saying it's small relative to the vapor velocity.

Most of Professor Hanratty's work is done that way.

So all I'm doing is making it a little bit better than what's been used in the past.

For an interfacial friction factor, this F sub i, I'm going to base it upon an interfacial roughness and I'm going to use an explicit approximation to the Colebrook-White equation, an empirical formula to try and fit Colebrook-White in exquisite manner so you don't have to do it recursively.

So here you see a roughness factor. This is the interfacial roughness. From everything that I've looked at so far for co-current downflow, it appears to be a function of this nondimensional film thickness and the Vapor Reynolds Number. And this is what has to be determined.

I could give you something for Andreussi and Zanelli but the more I've looked at things the less I feel comfortable about only using one data set to predict anything.

Okay. That finishes the first part of the presentation.

MR. CATTON: I thought you were through with the presentation.

MR. KELLY: Unfortunately, not. You're going to have to sit through a little bit more unless you want to kick me off the stage.

MR. CATTON: No, no. What I would like to do would be to take a break and then maybe I could talk to Lou and Brian about the rest of the day and what we're going to do.

(Brief recess.)

MR. CATTON: Before you start, Jim, one second.

What I'm going to do is -- we're just going to march right through this stuff until we hit 3:30 and bingo, it's over. So what I'd like you to do is take a look at the Friday agenda and if you have any preferences for the five topics -- four topics, we should rearrange it right now.

MR. SHERON: I would just -- I've talked with Idaho and we've sort of decided that we would like to -- any presentations anyone would want to hear, we would like to give the full presentation rather than there shouldn't be any -- in other words, no abbreviation or do it fast.

MR. CATTON: I understand and had sort of come to that conclusion with Lou already. What I'm trying to figure out is the order so that whatever drops off the end is the least interest. And I think pretty much the order that it's in looks go, doesn't it? Interfacial heat transfer, critical flow and the momentum equation and closure and then RELAP 5 field equations.

And when we reach a point we'll just stop. How is that?

Okay. Let's do it.

Joe, it's yours again.

MR. KELLY: Okay. We've finished with the film thickness and now we're going to talk about the heat transfer resistance on the film side. So this is the heat transfer resistance across the film and the one that I have within the film.

Again, I'm going to break it down the same way into falling films and sheared films where the sheared films here is specific to co-current downflow.

And again, the same type of structure, starting with what we can calculate, which is the laminar smooth film, and proceeding on. And I'll give a summary.

So, again, you always start with a Nusselt analysis. The film thickness which we've shown before. And this is the local Nusselt number for condensation heat transfer. It's simply the inverse of that nondimensional film thickness. And it's shown here.

What people normally talk about when they talk about the Nusselt model is the integral form where it's been averaged over a heat transfer surface. And the only reason I'm introducing this is because a lot of -- almost all the laminar condensation data has been taken this way. And that will be denoted by these brackets.

I do not advocate using this in the code because of having to specify that distance. I much prefer using the local formulation using the calculate liquid Reynolds number.

Now, what I'm going to do -- remember, I want the resistance across the film to be correct but I need to distribute it between those two resistances, the wall to liquid and liquid to interface.

I'm going to start by doing it analytically with a Nusselt type analysis. So this is the heat transfer coefficient, conductivity over the film thickness where the film thickness is what became on the previous slide from Nusselt.

You can now calculate for this one specific case of a laminar smooth film, you can calculate the bulk fluid temperature using the parabolic velocity profile and the assumption that Nusselt made of a linear temperature distribution within the film.

This is it. So this is the bulk liquid temperature in the film. It's 5/8 times the interface temperature; 3/8 times the wall temperature.

From that, given that the heat flux is constant through the film, I can then calculate the resistance from the wall to the liquid. And it's 5/8 for the total resistance, which comes out to a Nusselt number from the wall to the liquid of 1.8.

On the other side of that going from the bulk film temperature to the interface, you do the same thing and so, of course, since the two resistances have to add up to 1 times the film resistance, it's 3/8. This is now the interfacial heat transfer coefficient. It's 8/3 times this. The Nusselt number is simply 8/3.

MR. WULFF: And when you take the local values, should there not be an integral from the beginning of the cell to the end of the cell or how do you approximate that?

MR. KELLY: That's a decision -- you're correct. That's a decision that we're going to have to make when we think about implementing it in RELAP 5. And you're correct if one wants to consider that because the discretization in RELAP 5 is relatively course.

Now, what RELAP 5 does calculate is the cell averaged volume fraction which you can convert that to a liquid fraction and hence a thickness.

MR. WULFF: If there are no dispersed droplets?

MR. KELLY: Correct. If there are dispersed droplets then you have to calculate what fraction of the liquid is in the entrained form.

MR. SCHROCK: That requires a great degree of accuracy on void fraction prediction. It's not really a very sound way to get at the film thickness and therefore the film resistance. That is, the computed void fraction in a cell done by RELAP 5. What I'm saying, it's a poor way to come to a determination of liquid film thickness. It will be very inaccurate.

MR. KELLY: Well, that's what I'll find out. What I'm doing is building a set of correlations of constituent relations for the specific circumstances for interfacial thickness, the wall shear stress, et cetera. If all those work correctly, then you have a good prediction of the film thickness within the constraint that the discretization is relatively course.

MR. SCHROCK: But I think you need to realize up front that that approach demands a very great precision on void fraction determination. And given that your determination is over a large computational cell, it's doomed to be inaccurate for the determination of the film thickness.

MR. KELLY: Well, I agree with what you're saying and it's going to have to be something I'm going to have to demonstrate that we can do. If we cannot do it, then I'll have to modify the approach.

MR. CATTON: How does --

MR. KELLY: Well, I've shown the analytical result and the first question is how does it compare with the data?

What I'm showing here is some points. This is falling film, so it's not sheared. It's on the outside of tubes. It's from a paper by Kutateladze and Gogonin. The first plot is water data. This Prandtle number should say approximately because all of this is averaged, so the panel number changes as the film temperature changes. And so this is a rough estimate of the Prandtle number.

So this is water data that Kutateladze compiled from a wide variety of data sources. This was from freon and these are the data sources. The solid line is the Nusselt prediction.

And what I'm showing is the Nusselt prediction averaged over the total heat surface transfer length, because this is how the older condensation data was taken. As I'm sure Professor Schrock can tell you, taking good quality condensation data is very difficult. So these are based on difficult energy balances.

MR. SCHROCK: But Joe, I think the older data where the focus was on getting local values was simply not there means that that older data isn't really a very good basis on which to build a new model. You ought to be looking at newer data where local values have been determined.

MR. KELLY: Yes. I have not yet been able to find any for a falling film. For sheared films, yes. For a simple falling film, no. I've only been able to find fairly old data, and all of it averaged, with the exception of if I look at the evaporation data or film heating data, then I have some local data and I'll show that to you.

MR. SCHROCK: See, part of the problem of the average is that each of these experimenters has done different vertical heights. Not even comparing things on a common basis very well.

MR. KELLY: I agree completely. And the point here is all of this data lies -- or almost all of it lies about Nusselt by a fairly significant fraction.

This is attributed to rippling or waviness of the liquid film.

So the question is what kind of behavior do we expect on a rippled film as opposed to a smooth laminar film? So I'm going to call this laminar wavy. You expect to see at least two types of waves. I'm going to break them just into two categories; ripple waves and disturbance waves.

These are roughly the film Reynolds numbers ranges over which you see those waves, but remember these are rough. Every experimenter gives different numbers. And also, those are for water.

For a ripple wave, and there are a fairly low amplitude periodic type waves, they reduce the film thickness because some of the ripples were able to be transported with the wave instead of the laminar substrate.

They also induce velocities normal to the interface. That's extremely important in mass transfer data. It's not terribly important for us.

There's also the possibility of recirculation under the wave crest, but in general, these waves are small enough amplitude that they don't recirculate.

Disturbance waves which occur at Films Reynolds Numbers approximately greater than 350, they're definitely kinematic. A very significant fraction of the liquid flow can be transported by the waves, which makes the substrate between the wave crest much thinner.

Also, these waves -- well, they're called disturbance waves so you can imagine they mix the film in the region of the wave crest. So it's like you have a developing length for each wavelength. So these are the ways the disturbance waves can enhance the heat transfer.

Looking through there's a lot of data on waves in mass transfer literature, but actually what I found here had to do with countercurrent flow. There's an experiment by Professor Dukler and we are looking at -- this is a probability density distribution of film thickness and I have it in two type waves, a ripple wave and a disturbance wave.

The point for me here is we're going to calculate our heat transfer coefficient based upon an average film thickness. That's shown here; the inverse of the average value. But what one should actually use is the average of the inverse film thickness. And that's shown here.

If you ratio these two, you get a relative enhancement of 8 percent for a ripple wave and 11 percent for a disturbance wave.

Now these probability distributions were built up over about 2,000 waves for each one. But again, these are rough numbers. What I want to do is get some kind of idea of how large the phenomena should be that we're looking at.

So what do I expect for this enhance due to rippling? Well, if I take the film thickness by Kapitsa and ratio it to the one from Nusselt, it's about 93 percent. So already the average film thickness is going to be a little bit lower, a little bit thinner.

You add to that the effect of averaging the inverse versus the inverse of the average and this is what you get, approximately a 16 percent enhancement with respect to Nusselt.

Now, it's not any hard fast great correlation, but it gives us an idea of what to expect.

And oddly enough, when you look in the low Reynolds Number region where you expect this to be applicable, the enhancement is roughly 15 to 20 percent.

There are a number of correlations that I found in the literature that I have in a backup slide. There's one by Kutateladze and some others, but when you get to higher film Reynolds Numbers, the enhancement grows from 15 percent up to 30, maybe even to 50. But once you get up to this part, you're beginning the transition to turbulent.

And I compiled a list. I think -- yes. It's 745 data points. And again, this is all averaged Nusselt number and I did a simple curve fit, saying that the wavy was equal to Nusselt times an enhancement factor. It's not perfect, but it represents the data reasonably well.

Well, I did that for condensation data that was average. I then converted that to a local correlation. It's the same form, just a slightly different coefficient which you will see later. And I'm going to compare it to evaporation data. And the reason is evaporation data was easier to take and there's more recent data where they have the local values.

And I've got the results of three different experimenters: Chun and Seban, Faghri and Seban and Fujita and Ueda. And I'm plotting the nondimensional Nusselt number, nondimensional as with respect to Nusselt film thickness versus Films Reynolds number.

So this is the turbulent regime and you see that it's a function of Prandtle number. There's not much laminar data, but you see it significantly enhanced relative to Nusselt and it's pretty close to the curve fit that I've proposed.

MR. CATTON: That is a really strong dependence on Prandtle number.

MR. KELLY: You're exactly right. You've got very good eyes.

MR. CATTON: The fixed Reynolds number of 5 times 10 to the 3 or so, right under the V of wavy laminar. And then you go from the bottom one which is at 5.7 up to 1.7.

MR. KELLY: It's Prandtle number to the .65 and I agree it's extremely high. And that's something we'll talk about when we get to the turbulent ones.

MR. CATTON: That look a little suspicious.

MR. KELLY: I was, too. It surprised me. And we'll talk about that, too. And also, maybe Professor Schrock can comment on it.

MR. SCHROCK: Right. I do think you ought to have a look at the Blangetti data also. You have that, don't you?

MR. KELLY: I have two examples of it.

MR. SCHROCK: Okay.

MR. KELLY: I don't have the original reference yet. I've requested it from the library but the NRC Library works somewhat slowly.

MR. DHIR: It's also how Prandtle number is defined. These are heated surfaces.

MR. KELLY: These Prandtle numbers are based on -- I believe they're based upon the saturation temperature.

MR. SCHROCK: There is the additional problem that is very hard to deal with, I think, that the Prandtle number variation within the liquid film in the domain of application for low pressure condensation is very important. There's a strong property dependence within the liquid film dynamics that is very difficult to deal with in terms of your simple correlations that you've put together.

MR. KELLY: You're correct. Fortunately, the Prandtle numbers that we'll be seeing most of the time lie between the high point 9's and 2.

So, it's -- I mean, it is if you're trying to get something within a couple percent, but when you look at all this data it's so scattered that if we're anywhere within plus or minus 30 percent, I'm going to be quite happy.

Okay. This was the good news. The one that wasn't quite as good is the one shown in the bottom by Fujita and Ueda. And so again, the same coordinates. This was a curve fit that I proposed but the dashed line here is a Nusselt number assuming laminar flow but heating to a film.

So it's like two parallel plates but you split it in between and say it's the film. And this is the asymptotic Nusselt number in that situation.

And this data falls in between those two. And my attempt at an explanation is that the inlet temperature of those data were subcooled, so part of this was subcooled versus evaporation.

MR. SCHROCK: Are these temperatures F or C?

MR. KELLY: Those are C. Excuse me. Sorry about that. I'm trying to use SI units wherever.

Okay. I'm going to switch gears slightly. Now that's all the data that I would have for evaporation and I just wanted to see if what I was proposing was reasonable.

Now what I'm going to switch and talk about is the heat transfer correlation to a film that's being heated as it falls down a wall.

I have two data sources. The classical one is by

Wilke, and I also have some data by Ueda and Tanaka. The Prandtle numbers are shown here. Again, they're approximate. Nusselt number versus Films Reynolds number, so here you see the turbulent effects and the effect of Prandtle number. And here you see the laminar behavior.

This solid line is a Nusselt number for a laminar film in heating with a constant temperature wall battery condition. It's basically the parallel plate solution looking at only one half with a line of symmetry down the center.

What this shows is that there's very little effect of surface rippling upon the heat transfer from the wall to the film when you're only doing sensible heat transfer.

So putting this together for the laminar wavy regime, that part is a simple local value of a Nusselt formula. this part is what I've proposed as a correct if you will for laminar wavy effects. So this is the heat transfer coefficient for condensation.

For wall to fluid heat transfer -- and remember this is what I need to specify in the code. And what I'm going to subtract from the condensation will give me the interfacial value.

I saw no effect of waves on the heating data. Consequently, I'm going to use the asymptotic Nusselt number for film heating. Actually, this should read 1.88.

MR. CATTON: What happened to the Prandtle number?

MR. KELLY: This is laminar.

MR. CATTON: Oh.

MR. KELLY: We're getting to turbulent.

MR. CATTON: The way those -- the Nusselt number relationship deviates from laminar looks like the turbulent characteristics or the initiation of the turbulent wave is a strong function of Prandtle number.

MR. KELLY: Yes. And these are extremely high Prandtle numbers for heat transfer data.

MR. CATTON: The bottom one is almost infinite.

MR. KELLY: Yes. It's starting to look more like mass transfer data.

MR. CATTON: That's right. Those Schmidt numbers are usually a few thousand.

MR. KELLY: So your thermal boundary number is usually very small compared to the fluid boundary layer.

MR. CATTON: Well, normally the way you get a high Prandtle number is on low thermal conductivity.

MR. KELLY: Right.

MR. CATTON: And low thermal conductivity, the turbulence has a much bigger impact on the changes on the Nusselt number. What I don't understand is why the initiation is so dependent on the Prandtle number anyway. It doesn't matter for RELAP 5.

MR. KELLY: Yes. I won't claim to understand everything.

MR. CATTON: If you have any thoughts on that I'd like to hear them.

MR. KELLY: Well, I've wondered about it some and haven't -- I mean, for me it seems strange to say that a film at a Reynolds number of 200 would be turbulent, but it departs from the laminar behavior there. Now, it may not really be turbulent.

MR. CATTON: But you see when you look at the Nusselt number/Reynolds number behavior, if you had roll waves, for example, which is really not turbulence, that would cause what you're seeing there because the convective contribution for a high Prandtle number fluid would be significant.

MR. KELLY: Okay.

MR. CATTON: I suspect that that's what the process is.

MR. KELLY: And also if you look at the Reynolds number dependence here, they're different than the wall's Prandtle number.

MR. CATTON: Yes. That implies maybe --

MR. KELLY: A transition regime.

Okay. So what I showed you was the condensation heat transfer coefficient and using the asymptotic Nusselt number for the heating part or from the wall to the liquid. So for my interfacial I subtract a 2 and there's no point in going through the algebra.

Now we're going to talk about turbulent films and what is the expected behavior or at least the behavior as I expected it.

You expect a transition region to begin somewhere between a Reynolds number of 1,000 to 2,000. Every investigator picks a different transition point. There's a very wide spread.

MR. SCHROCK: But Rohsenow's proposal was that it depends on interfacial shear rather strongly. Are you doing this first without the interfacial shear and then --

MR. KELLY: Yes. We're only talking about falling films then we're going to transition to sheared films.

I've read Professor Rohsenow's paper and I assumed that was the case. But I haven't seen it in the data, I'll be frank with you. And I'll show that later. I have not seen an early transition to turbulent behavior due to interfacial shear.

MR. CATTON: I think you're going to have trouble unless you can get a whole batch of data at the same Prandtle number because at the very high Prandtle numbers any kind of a disturbance in the film is going to significantly enhance the heat transfer.

MR. KELLY: I agree with you.

MR. CATTON: At the low Prandtle number, the initial disturbances aren't going to show up hardly at all. So I think what you're seeing is two processes occurring.

MR. KELLY: Well, you're completely correct. On one of my earlier slides when I introduced the concept there was a bullet I didn't go through and that was where after I introduced the process I said waht I'd like to do is use mass transfer data as a back check on what that interfacial heat transfer coefficient should be.

And exactly what you said. The Schmidt numbers are very high so the penetration depth is very small. And any surface disturbance overcomes everything. And it simply doesn't apply in our case because our Prandtle numbers are more close to 1. And everything we'll be looking at, they're based between .9 and 2.

So the point is very well taken and I'm trying now to develop a larger data base, especially for shear films where data exist of water steam data so that it will be directly applicable.

So I expect to see a transition somewhere in that regime and we're talking about falling films. Also, I expect that transition region to persist over a much wider range of Reynolds number than we're used to seeing for pipe flow. And that's because what I'll loosely call sublayers or buffer layers are large with respect to the film thickness.

So the film Reynolds number has to be get so the film can be thick enough that the buffer layer can be contained within the film before the film can act truly turbulent.

Also, I expect to see the primary resistance to heat transfer is going to be across what we'll call these laminar sublayers and there'll be one at the wall and one at the free surface because the free surface tends to damp out the turbulent eddies.

At high Reynolds number, just based on experience, you expect the Reynolds number dependence to go to .8. Simple Dittus Boelter type thing.

Now what about the Prandtle number dependence? On heating data it's tradition to use 1/3 or roughly 1/3. For cooling data, 0.4. But for interface mass transfer it's normally 1/2 and most of the mass transfer data uses Schmidt number to the 1/2. So I expected to see something between .4 and .5.

MR. DHIR: What is the boundary condition on this falling film? Is this a water film, a liquid film, falling off this heated surface? Is the interface supposed to steam or what?

MR. KELLY: If it's condensation or evaporation, it would be steam.

MR. DHIR: Right. You're talking about falling film which are heated?

MR. KELLY: Yes.

MR. DHIR: What is the boundary condition?

MR. KELLY: What is the boundary condition?

MR. DHIR: Yes.

MR. KELLY: You mean for shear?

MR. DHIR: No. If you shear or for massive transfer across or heat transfer across. You see condensation you have vapor that's condensing on the interface and then you have film evaporation that's taking place. You're talking about these heated films, the falling film heated case. What is that?

MR. KELLY: Okay. In that case, it an ambient air. It's an air on the outside. So it's rough -- it's approximately adiabatic. Pardon?

MR. DHIR: There will be evaporation while it's transfer.

MR. KELLY: No. Well, okay.

MR. SCHROCK: It depends on the volatility of the liquid and the experiments when it --

MR. KELLY: Correct. But that's the way the experiments were constructed, was to minimize transport due to latent heat.

[Slide.]

MR. KELLY: I looked at condensation heat transfer coefficient for falling films and there's a lot of them in the literature and they all contradict each other, in particular, if you look at the Reynolds number dependence or the Prandtle number dependence. Let's start with Prandtle number.

A number of the older correlations have no Prandtle number dependence in them, which we know is wrong. But then if you look at other ones, Colburn to one-third power, Soliman -- and this is actually for sheared films where you sat Tau i to zero. It degenerates to falling film with Prandtle number to the .65.

One by Blangetti. I don't know how he got four decimal places, but that was the value. And then the evaporation heat transfer data that you saw was fit by Chun and Seban to Prandtle number .65 and that greatly surprised me, but it really fits the evaporation data.

MR. SCHROCK: Have you looked at the range of Prandtle numbers and the database for each of these experimenters?

MR. KELLY: On the older experiments, they don't even quote the physical properties. It's extremely difficult to get anything out of those. The only ones that made any kind of systemic effort -- most people never even mentioned the Prandtle number dependence. It just all of a sudden appears.

However, that wasn't the case with Chun and Seban, and if you go back to the view graph, you'll see what the Prandtle number dependence was. I don't remember off the top of my head. But also Soliman. They had a large database for sheared films and then if you believe their model it had to have -- then they plotted it versus Prandtle number and saw what dependence would have to be for it to work and it came out to .65.

Basically, I'm going to have to end up doing the same thing when I get to the end of all of this and I haven't gotten that far yet. But again, fortunately, our Prandtle numbers are in the range of .9 to 2, so whether it's .5 or .65 is not critical. But, of course, like you I would like to know what is it really. But as you said, I suspect it's in this range.

What you see over here, which I don't remember if I put that on the handout or not, this is a Nusselt number with respect to the film thickness. So it's heat transfer coefficient times film thickness over conductivity. So these are the Reynolds number dependents. Remember, I told you high Reynolds numbers are expected to be the .8 power. Well, you can get any power you want here basically. It depends --

MR. CATTON: Well, between .7 and what?

MR. KELLY: And it basically depends on where the person took their data and how careful they were about fitting it, and I'll show you that in a minute.

[Slide.]

MR. KELLY: But what I -- the next thing I did was I plotted these up as a function of Reynolds number at two different Prandtle numbers. This is a Prandtle number more where we expect to be and this is for fairly cold water -- room temperature if I remember correctly. There is a wide spread in the data. Excuse me -- in the correlations.

[Slide.]

MR. KELLY: So then failing to find a great agreement between the correlations, I started looking at some of the data, and I showed you earlier some of the falling film data. This is some of that same data, but now I'm showing you the turbulent part and I'm plotting the Nusselt with respect to the film thickness.

So this is the data from Kutateladze for water to freon in a turbulent range, it appears, at something like Reynolds number to the two-thirds.

This is data from Grover's textbook for three different fluids -- water, diphenyl and diphenyl oxide. Prandtle numbers were really approximate in this data. But the water was roughly one and half, diphenyl was five, and for diphenyl oxide it wasn't even quoted. But this looks more like Reynolds number to the one power.

So in other words, I didn't learn very much, and this data is so scattered I can't get a Prandtle number dependence out of it either.

[Slide.]

MR. KELLY: So I fall back on the evaporation data, and this is from all three different investigators -- Chun and Seban, Faghri and Seban and Fujita and Uedei. I broke the data points into three different Prandtle number ranges. So the open triangles are 1.77 to 2. So that's here. This is a Prandtle number of about 2.9 and this is 5.1 to 5.7.

Now, I then nondimensionalized it with respect to film thickness and divided by Prandtle number to .65. Actually, I tried other Prandtle number dependencies and they didn't work. I had to go to the .65 as suggested by Chun and Seban. When you do that, they pretty much all fall in one line. This is the equation of that line, where this Reynolds number dependents I picked it and then plotted it and it seems to work. This corresponds to one of the correlations by Wilke for heating.

[Slide.]

MR. KELLY: I introduced Wilke's. I'll show you what those correlations are. So this is falling films.

Wilke presented an ensemble of correlations to cover a wide variety of Reynolds number ranges, going from laminar to fully turbulent with two different transition regions in between.

The number two only appears through very high Prandtle number fluids. Well, actually, it appears at the Prandtle numbers greater than two. Otherwise, you don't see it. If you put -- and this is the way he presented the correlations. If you put them in a nondimensional form with respect to the Nusselt film thickness parameter, and using the values of film thickness that I have, they come out this way.

[Slide.]

MR. KELLY: There are other correlations for film heating. Here are some of the ones I found. So you see, the Prandtle number dependence for heating is not a great controversy. Almost all of them are one-third or .34, and one by Gimbutis has a correction due to a hot wall effect.

This is the Reynolds number dependence for Nusselt number using the film thickness. So again, there's the same kind of spread that you saw in laminar. Excuse me -- that you saw with the condensation data.

[Slide.]

MR. KELLY: Now, if I plot these correlations against each other, there's not as wide a spread. First, I'll back up. In the laminar regime, this is the asymptotic Nusselt number for a constant wall temperature boundary condition and a constant heat flux boundary condition.

I've shown two different Prandtle numbers as I did for condensation. Because there the Prandtle number dependencies were about the same, there is not a big difference in these, except for one thing that I'll show you.

This is the transition region in the Wilke correlation for the higher Prandtle number fluids. And in this -- you'll see it both here and here -- is a short regime between a Reynolds number of 1,600 and 3,200, where the Reynolds number dependence is greater than one.

[Slide.]

MR. KELLY: I had a fairly good database for film heating. So I put all the points together, nondimensionalized them with respect to film thickness and Prandtle number to the one-third power, because this is heating data, in plotted them versus film Reynolds number.

In this regime I'm using the -- the correlation Number 2 is by Wilke. I think it's his Number 4. I'm sorry about the confusion in the numbers. So it's Reynolds number to the .93 and it fits the data quite well. But if I remember, it was only valid up to Reynolds number of about 11,500 -- was his database.

So at the high Reynolds numbers I simply assumed the Reynolds number dependence would be .8 and correlated it and just did a curve fit getting this coefficient, and they intersected at Reynolds number of about 13,000.

I also did a curve fit at what is a transition region for high Prandtle number fluids. I assumed that the Reynolds number dependence would be .5, which you normally have for a transition region between laminar and turbulent, and got this. But, fortunately, we don't have to worry about that because our Prandtle numbers are always going to be two or less. So I only needed this because there's a lot of high Prandtle number data on this slide.

This shows it as a predicted over measured so you can get an idea of what the error is. This band is plus or minus 15 percent. So that's what I'm going to use for heating.

[Slide.]

MR. KELLY: So that finishes falling films. But I have not yet found what I consider to be a satisfactory correlation for condensation in falling films.

MR. DHIR: I have one comment.

MR. KELLY: Yes.

MR. DHIR: There's a correlation by Chen and Tien and that is published in recent textbooks on heat transfer.

MR. KELLY: Yes. I have the paper now.

MR. DHIR: Then there's a review article by I think Fujita in recent conference on condensation. It was a foundation conference. He lists all kinds of sources for condensation heat transfer data.

MR. KELLY: Okay. What was the name again? I missed it.

MR. DHIR: I think it's Fujita or Chita. I forget. One of the two.

MR. KELLY: Okay, because I'll check that. I did just recently get the paper that you mentioned, and I will plot that one against these.

MR. SCHROCK: But it deals with the heat transfer coefficient for the whole film, that is the whole resistance, not this divided resistance that you're trying to create here for --

MR. KELLY: Right. But I need the whole resistance to make sure I get the right answer.

[Slide.]

MR. KELLY: So now I'm going to switch and talk about sheared films, and again we're going to start over with laminar smooth and go from something that we can calculate.

This was a nondimensional film thickness that I introduced before, M plus. Then you can just take that as thickness as a function of interfacial friction and get a

Nusselt number from that that's a function of interfacial friction. You notice that if the interfacial friction is very large it becomes proportional to the interfacial shear to the one-half, which is what's been seen in flat plate data.

For laminar wave -- now we're going to go to laminar wavy. If I look just at an enhancement due to interfacial shear as the interfacial shear is thinning the film. So this would be relative to Nusselts because the film is thinner than the Nusselt thickness.

Then the question I want to pose is, I have two effects -- the film being thinned by interfacial shear and ripples on the film surface. Can I simply add the two effects or do they interact in some way?

I expected them to interact because as you start to shear the film you're going to change the characteristics of the waves on the surface. Tip for co-current down flow typically the amplitudes go down and the wavelengths become shorter and you'd expect that to affect the heat transfer. Don't read the conclusion yet.

[Slide.]

MR. KELLY: This is the data mentioned earlier by Blangetti and Schlunder, nondimensional Nusselt number versus film Reynolds number for two different values of a nondimensional shear stress. So this is the interfacial shear stress nondimensionalized with the liquid density times gravity times the Nusselt film thickness parameter, which is only a function of properties, okay?

So values of 1.18 and 2.82. So these are slightly to moderately sheared films. Okay, the data is the symbols. This finely dashed line is a Nusselt formula for the local value. The dashed line is what you get if you calculate the film thickness, based on this value of the shear stress. You can calculate what the thickness would be, assuming laminar flow and the film, and assuming a linear temperature profile across that film.

The solid line is what I get if I take the enhancement due to waves that I introduced earlier and use this film thickness with it. So, in effect, it looks like the two effects are additive. This surprised me.

MR. CATTON: Why should that -- it shouldn't surprise you that they're additive. The question is, what are the characteristics as a function of the shearing? I think the additive part is clear.

MR. KELLY: Yes, you're correct. What I'm surprised is that I did not have to change my correlation for rippling to account for this.

MR. CATTON: Well, that's right. You would think there would be some affect of shear on that.

MR. KELLY: That's what I expected. Now, I admit it. This is a very small database.

MR. CATTON: On the other hand, if you're talking about if these are roll waves and the axis of the roll wave is along the wall, it uncouples from the shearing.

MR. DHIR: But the ripples are contributing only 10 to 15 percent. So it's not much. Basically, it depends on shear.

MR. CATTON: It depends on Prandtle number. If I take a high Prandtle number, the contribution is big.

MR. KELLY: This is water data, so the Prandtle number was on the order of two. I don't have that on the slide. Sorry. But what you see is the enhancement due to rippling is this much, and the enhancement due to the film thinning is only this much. But I have to superpose to get close to the right answer, and I thought that was pretty good.

MR. DHIR: I thought you calculated it to 16 percent or something?

MR. KELLY: Well, it's 16 percent at the low Reynolds numbers and gets close to 50 percent just before you transition the turbulence. At 50 percent, it's considerable.

[Slide.]

MR. KELLY: So that was the conclusion, that you do superpose the two, and so what I will have is a Nusselt number. It's a Nusselt number due to standard Nusselt analysis times the enhancement that I proposed to the rippling and then taking into account the actual film thickness. So that's what shown here, conductivity over film thickness times the enhancement due to rippling.

MR. SCHROCK: There is an aspect of the Blangetti experiment that might be responsible for these higher values and I'm not yet convinced that it is the explanation but their data are high. The nature of the experiment is that it's a short section. The film flow is introduced. It's not produced by condensation. And the change in the film thickness over the length of the experiment is small compared to the film thickness entering --

MR. KELLY: A lot of it's in the developing length.

MR. SCHROCK: So there is a development length here which is sort of a part of the experimental data and it's not been really quantitatively assessed. So it's possible that the higher values that they're showing come from an entrance effect which is a part of the experimental database.

MR. KELLY: Okay. Thank you. Those are the kind of things I need to know. Unfortunately, I do not yet have the original reference. I've requested it, but again, it takes a long time to get things.

MR. SCHROCK: It's a good experiment but it's only like a foot in height.

MR. KELLY: Yes. Okay, thanks.

[Slide.]

MR. KELLY: My conclusion was that I could simply superpose the two. Excuse me. Now, we're going to talk about turbulent sheared films. I'm just going to do a back of the envelope calculation to see what I expect, okay?

So I'm going to assume that the total heat transfer resistance is due to what we'll call laminar sublayers, one at the wall and one at the interface. For now, I'm not going to specify how thick these layers are. Nusselt number, I'm going to define with tc film thickness. So, in effect, it's going to be the film thickness divided by the thickness of the sublayers, which is going to be y(sl), and it's going to be both of the sublayers combined.

Nondimensionalize it as a standard using a friction velocity, and for the characteristic shear stress I'm going to use the one that I used before, which is interfacial plus two-thirds times Rho g film thickness.

So this is the definition of the nondimensional one, and you go through and you get a Nusselt number being a constant times Reynolds number to .85. Now, this was using my correlation for film thickness, the nondimensional one.

But the point is that if you use that

characteristic shear stress, you get only one function of Reynolds number. There's nothing on this right hand side that is a function of interfacial shear. The interfacial shear affects are captured through the film thickness.

[Slide.]

MR. KELLY: Well, does that work? Okay, it's a supposition.

So I'm going to first look at some condensation data, and this is condensation data where the interfacial friction is known. They measured the pressure drop and the film thickness so they could estimate the interfacial friction. All of these equal signs should be approximate, and there's a fair amount of error in this.

So there's a wide range of interfacial friction from 10 to 300. This is at nondimensional Nusselt number versus film Reynolds number. And you just see a wide scatter basically. But, in effect, as the shear stress gets higher, the Nusselt number gets higher, and there appears to be some affect with Reynolds number. But the scatter is so bad it's hard to see.

If I change and use a Nusselt number with respect to film thickness, if I look in the turbulent regime, most of this data has now collapsed to one line, you know, given that the data is very scattered. The slope I have is the .93 that I showed earlier.

MR. DHIR: How was the interfacial shear determined?

MR. KELLY: They measured pressure drop and also the film thickness.

MR. DHIR: I see. So you used their correlation?

MR. KELLY: Well, I used their measured values of Tau i. I didn't have to calculate it for myself. But these values are approximate and that's part of the reason this looks so scattered because it's not a parameter that you can control. So what, basically, all of their data that was any where near ten, they said it was equal to ten and that's part of the reason this is so scattered.

It's nice to see this but it's not very satisfying because the data is so scattered.

MR. ZUBER: I have a question. Your film Reynolds number is based on film thickness --

MR. KELLY: Well, it's --

MR. ZUBER: On the bottom.

MR. KELLY: Yes, I understand. It's the film -- okay. The definition I used does not explicitly have the film thickness in it. It's the film flow rate divided by the wedded perimeter.

MR. ZUBER: The same for the Nusselt number?

MR. KELLY: Yes.

MR. ZUBER: How did you determine that one? The same way?

MR. KELLY: Actually, to go from here to here I calculate the film thickness using these values of the interfacial shear, the Reynolds number that was reported in the data, and the correlations that I have that I presented earlier.

MR. ZUBER: Okay.

MR. KELLY: So the film thicknesses were not reported, so I calculated them. Thanks for asking that. I forgot to say it.

MR. ZUBER: I was afraid that you were plotting N versus M.

MR. KELLY: Yes.

[Laughter.]

MR. KELLY: It's a good trick, but I knew you wouldn't let me get away with it.

So then I went back and looked at Blangetti and Schlunder again, now with the emphasis on the turbulent regime. I do the same thing. So this is Nusselt number using the film thickness. Again, I calculated the film thickness because I don't have the measured values. Plotted it versus film Reynolds number, and pretty much the two data sets overlay each other when you do that.

This was the correlation for laminar wavy that I introduced and this is -- the solid line here is the

Reynolds number to the .9 and Prandtle to the .65. The coefficient I got just by fitting the data here. It's slightly different than the coefficients you've seen on the earlier slides. So there's not a complete consistency.

But what there is, is a consistency in that if you use the real film thickness for a sheared film, it looks like a falling film. What I've done here is just simply use these two correlations and combine them in a way that's somewhat standard for doing a transition. It's just an interpolation, basically, between them from anywhere to square to fifth power.

MR. CATTON: He has exercised this to a fairly -- well, this sort of approach.

MR. KELLY: Yes, I actually got it from Professor Hanrarty, but you know, it's the same type thing.

[Slide.]

MR. KELLY: Well, that was condensation data. I then went and looked at the heating data for sheared films and I have some by Ueda and Tanaka. So it's the same type thing where they measured the film thickness and the pressure gradient and calculated the interfacial shear stress. But I only have two values, 10 and 30, and again, they're spread in this and these should say approximate. I plot it as the nondimensional Nusselt number -- the Nusselt number star here, and what you see is as you go from a falling film to a sheared film the Nusselt number increases, as you expect.

If you then define the Nusselt number using film thickness, it collapses to one line, and what I plotted here are the correlations of Wilke for falling films. It's correlations Number 3 and 4, and they fall very well right on top of each other. I'm actually surprised it works out this well.

So that allows me to get to the summary on film side heat transfer.

[Slide.]

MR. KELLY: For laminar falling film, we've seen in condensation heat transfer is significantly enhanced by waves on the surface. That the curve fit that I've proposed for that. Wall to fluid heat transfer, conversely, is relatively unaffected by these surface waves, and I'm going to use the asymptotic Nusselt number for that.

For turbulent falling film I don't have so good a conclusion at the moment. The condensation data that I have are simply too scattered to with any degree of confidence determine the Reynolds number and Prandtle number dependents. So I'm going to have to use something like the evaporation data or infer it from sheared film data.

This is what the evaporation heat transfer data in the turbulent regime was fit by.

[Slide.]

MR. KELLY: For a turbulent falling film, pretty much is standard of the correlations by Wilke. So that's the laminar. Number 2 is the one that applies for higher Prandtle numbers so we don't really need it. So we won't have to use that one.

This is only for this relatively small Reynolds number region. Unfortunately, it's the Reynolds number regime that's fairly important to us because for most of our applications -- for example, in the SBWR the tubes are simply not long enough to get to high film Reynolds numbers. So we're just moving into the turbulent regime and quite a bit of it is over the laminar regime. So I will be using these two.

But then at high Reynolds number -- and you can in something like a PRHR, if you assume you have single phase steam coming into it because the tubes are much longer, you can go to high film Reynolds numbers, and so for that case, this was the curve fit I had, assuming that the Reynolds number dependence was Point A.

[Slide.]

MR. KELLY: Now for sheared films. I made the supposition that most of the important effects were captured in the film thickness and I believe that I have sufficiently demonstrated that. I need, of course, to assess it against the much wider data set before I'll even believe it, but that's what I'm assuming for the moment.

So what that says is that falling film heat transfer correlations are applicable to the sheared film case if you use a Nusselt number based upon film thickness. And that's what I show here -- Nusselt number and it's equal to the film thickness divided by conductivity times a function of Reynolds and Prandtle number, where this function will now be the same whether the film is sheared or falling.

That ends Part II.

MR. SCHROCK: I'd like to say that you're to be congratulated on a very fine piece of work, Joe.

MR. KELLY: Thank you.

MR. SCHROCK: I don't agree with every aspect of it as I've already indicated. I wish I could persuade you that the breaking up of the film resistance into two parts is a sort of unnecessary and not very desirable exercise, but maybe you can convince me that it is that.

I think that you've brought forward a tremendous amount of experimental data to make your points, but in relationship to this one point of whether there is a solid relevance of the film heating data in the absence of mass transfer, I don't think you've really thought through real enough yet the fact that those temperature profiles are very unlike the temperature profiles that exist in the condensation problem.

In the condensation problem, there is very little deviation from linearity of temperature distribution across the film thickness and, for that reason, the determination of the bulk temperature which you're getting is very different than the determination of bulk temperature in these heated film empirical correlations.

So you have to look at what does bulk temperature mean in those heated film experiments. It means something quite different in your condensation application because the temperature profiles are very different.

MR. KELLY: Yes, I agree with what you're saying and I'm doing the best I can within the constraints that I need to use. Now, in one response to that, if I go to laminar film where the temperature distribution is linear, almost linear, the Nusselt number for condensation was obviously one, if you make it based on film thickness.

If I did that, if I get my resistance of a Nusselt number between the wall and the bulk liquid temperature, if you remember, it was 1.8, okay? Just simply integrating it with Nusselt but only going to the bulk temperature. The heating data is well correlated by a Nusselt number of 1.9. So at least for a laminar film it's not that big a difference.

But you're exactly correct that the temperature profiles are different.

MR. DHIR: And they will depend on the distance from the leading edge. The shorter the distance, the farther you will be from 1.8.

MR. SCHROCK: What they're correlating is a long enough section, so it's similar to things like Dittus Boelter where the correlation represents an average over the length, including an entry region. You must specify a minimum l over d for the correlation. There has to be a minimum l over d in Wilke's correlations because the film thickness is so small compared to the distances of interests.

MR. KELLY: Wilke's values were local. So they were fully developed.

MR. SCHROCK: But fully developed. That's the point, fully developed profile.

MR. KELLY: So that they expect him to under predict the values near an entrance. That's correct. Now, I'll go on to Part III. That's noncondensable gases.

MR. SEALE: I have another question. You can answer it perhaps while you're just distributing that. You mentioned several times difficulties with getting materials from the library.

MR. KELLY: Yes.

MR. SEALE: How long are you talking about?

MR. KELLY: It depends. A lot of the things I've had to go Xerox myself if it's something that the library has. Unfortunately, the NRC library is horribly understaffed. The people there are very responsive and they've been very helpful. You know, they've made a great effort to try to help me. But there's only three of them for the whole agency.

MR. CATTON: Is that the library that's in this building?

MR. KELLY: Yes. So if it's something that they have to get on interlibrary loan, it takes months.

MR. ZUBER: My experience was they're very, very helpful and very proficient and efficient but they're understaffed. But they're most, most helpful.

MR. CATTON: You have the National Bureau Standards library, though, don't you? It's probably about as close.

MR. KELLY: What I've decided is I have to go talk to the librarians here and see if they can direct somewhere else and I guess that's the one.

MR. ZUBER: Or the Library of Congress.

MR. KELLY: It's just difficult for me to take time out from the office to go get an article. I'm used to being able to send a requisition to the library and have it come back.

MR. CATTON: And I guess you don't a student to send.

MR. KELLY: If you one --

[Laughter.]

MR. JOHNSEN: Joe, you can always call us to at the IML.

MR. KELLY: Okay, thanks, Gary.

MR. JOHNSEN: Don't hesitate. Pick up the phone and call.

MR. KELLY: Thank you. I will take you up on that. But you know that at the NRC we can't do E-mail outside of the agency.

MR. BOEHNERT: Yes, you can.

MR. KELLY: Well, if you have a hook-up to INTERNET, which I don't. I don't think there's one in my building, which is Nicholson Lane North.

MR. BOEHNERT: I'm surprised. I thought most of the agency was getting it.

MR. KELLY: Eventually, yes.

[Slide.]

MR. KELLY: Now, we're going to go to noncondensable gases. Here, I have not done the work that you saw on the other. All I'm going to do is lay out the approach that I'm intending to follow and, basically, I'm going to follow the method proposed -- used by Peterson et alibi and Kageyama et alibi. So these are basically the models developed at Berkeley.

What they do is they take an energy balance at the interface. So this is the wall condensation. The heat flux through the wall condensation is split into two parts, a flux to the latent heat and a flux of sensible heat. I should go back and briefly remind you of the physical situation.

We're talking about the picture on the bottom now where there's a significant temperature drop between the bulk temperature and the interface temperature. In our brief audit we do not know that interface temperature and this is because there is a distribution of the mole fractions of the vapor and the noncondensable gases across the boundary layer. Okay?

But, of course, this is the number we want, is the wall condensation. Now, you can express that, as shown here, the heat transfer coefficient. The driving potential is the interface temperature, the saturated value at the local vapor partial pressure. Then the two heat transfer coefficients, one due to the light flux -- flux of light and heat, one due to sensible heat, and you see the driving potential. This is the bulk saturation of the vapor and the interface saturation temperature.

[Slide.]

MR. KELLY: Now, we're going to follow the development done at Berkeley. The first assumption we make is the one that everyone makes, and that's that you can treat the vapor gas mixture as a mixture of perfect gases. That allows you to take the mole concentrations and express them as partial pressures.

The next step that was made is using the Clausius-Clapeyron relation to take the partial pressures and express them as a temperature difference, and this is the temperature difference I want between the vapor bulk and the saturation value at the interface.

In doing this, we introduced a vapor liquid specific volume. That's approximated this way: sets the gas constant an average across the boundary layer and the gas, and this X is the mole fraction of vapor, the wall gradient mean value across that boundary layer.

Then the diffusion coefficient, the diffusivity, is pretty much proportional to T squared over P. The relationship we're going to derive, there is this value of the diffusivity times P over T squared. That's approximately a constant. So the assumption has been made you can evaluate as a constant at as a reference value and then you don't have to worry about the dependents.

[Slide.]

MR. KELLY: So then from this, from those assumptions, you can formulate a model. So what you have to do is assume an interface temperature and the mole fraction of noncondensable gas or vapor at that interface.

Then they have defined a condensation conductivity and it's shown here. Everything in these brackets is a constant. It can be evaluated once. This is the latent heat. This Phi is defined here. It's the ratio of the log mean values of the gas and the vapor mole fractions across that boundary layer, and it's shown here in terms of the bulk value and the value at the interface of the gas.

T ave is an average of the temperature at the interface and the temperature in the bulk. So you make these two assumptions. You can evaluate Phi and you can evaluate this condensation conductivity.

From that and the Sherwood number you can get a heat transfer coefficient due to condensation. This is on the vapor side, okay? The small case "d" is a diameter. From this you can calculate -- from this and continuity of heat flux through the film and through this boundary layer, you can calculate directly what that interface temperature is.

Then you have to check this against what you assumed and iterate until you converge. When you've done that, you can then calculate what the wall heat flux is, which, of course, is what we wanted.

[Slide.]

MR. KELLY: Now, this model -- and again, it's not by model. But it has one very strong advantage. That's that this iteration parameter, Phi, is much less sensitive than the partial pressures that you see in some like a Colburn Hougen model. Consequently, you can get a better initial guess and few iterations. I like this approach very much, which is why I selected it.

Also, what we might be able to do is make one further assumption, but this is tentative. It's something that I have to check out when I actually get the model up and running. And what I'm going to suppose is that when noncondensable gas effects are important then the bulk value of the gas mole concentration is not a small number. Okay, it's not infinitesimal.

Also, due to the noncondensable gas effect the heat flux is not terribly high. This means that the difference between the interface and the bulk concentration is not a large number and the denominator here is not very small.

So maybe this value isn't very large. If this is not large, then Phi can simply be expressed as the ration of one minus the bulk gas concentration over the bulk concentration. If that's true, you don't need to iterate.

Now, this is totally supposition.

MR. SCHROCK: I don't think that's a good assumption to make.

MR. KELLY: You're probably right. I'll show you what the form of that is.

MR. SCHROCK: Based on experimental data that is.

MR. KELLY: I haven't closely looked at that yet. I will be. What I can do is use this as a first guess.

[Slide.]

MR. KELLY: At the very least, I can use it as a first guess if I don't have a better value.

What I've plotted is the actual minus the approximate value divided by the actual value. So that would be the error versus that ratio I just talked about. This is the difference between the interface value and the bulk divided by the bulk. If that number is less than about ten percent, the error is on the order of five or so percent and it's not terribly large.

For small values it's a very good approximation. But as the values become large, you see it falls apart, as expected. So it's just something I'm going to check to try to make the computational effort easier if possible.

[Slide.]

MR. KELLY: To use this model you need several constitutive relations. You need a convected heat transfer coefficient, h of s. We also need to know the ratio of a Sherwood to the Nusselt number because I used the Sherwood number earlier.

For the convected heat transfer coefficient, there are at least four effects that we need to consider -- entrance links affects, boundary layer suction. Because we're condensing, it looks like you're sucking the boundary layer closer to the liquid film and that enhances the heat transfer. Roughness due to interfacial waves and also the possibility of mist formation in the vapor gas mixture.

All four of those act to enhance the heat transfer, and that's what I talk about here. Entrance effects are possibly important for the PCCS and the ICS because roughly the l/d for the entire condensers on the order of 50. So definitely you see these entrance effects in the experimental data.

The boundary layer suction is probably small from the things I've seen in papers. The roughness effect is not small, and what I've plotted here -- I didn't plot it. This is reprinted from Rohsenow and Choi's textbook. It's a Nusselt number over a smooth tube Nusselt number versus the friction factor over the smooth tube friction factor. So this is error data turbulent force convection and you see enhancements between 1 and 3 over this roughness range.

For interfacial the friction factors go on out close to 100. What I'm planning to approximate this as the heat due to surface roughness divided by the smooth is a square root of the ratio of interfacial friction to smooth tube friction. That's basically assuming that things like subway or scales is the square root of a shear stress.

There are some correlations for this, but again, there all in contradiction with each other, and for me all the uncertainty is in the value of f of i.

Mist formation -- I haven't researched this yet but in the paper by Peterson et alibi they quote the data of Mori and Hijikata where it was found that the heat transfer coefficient for sensible heat transport was approximately a factor of two greater than the convective, and in Peterson's work they used a value of seven, and that's something that I'll have to look at.

[Slide.]

MR. KELLY: The other thing I need is the ratio of the Sherwood to the Nusselt number and it's traditionally of Schmidt number to Prandtle to some exponent n. Again, what's the exponent? Dittus-Buelter for cooling, .4, mass transfer gas absorption, .5, the Gilliland and Sherwood -- and this was evaporation of a liquid film into an air environment. They correlated their data with a ratio -- with an exponent .44. In a paper by Kageyama et alibi, they use an exponent of .6, and that's probably what I'll start with because that was based upon condensation data.

[Slide.]

MR. KELLY: My last slide -- I actually skipped the summary because I didn't want to tell you all this over again, but this is the assessment plan. Once I get the model finished and into a code and start checking it out, this is how I'm going to do it.

For forced convection, I found it hard to find pure steam data. There is a large database done by Goodykoontz and Dorsch. I believe this is NASA data, and that's what I've requested from the library. We don't have it yet. There are four cases that are in "Multiphase Science and Technology series. So at the moment I have four cases there.

I have two cases from Babcock and Wilcox, which is at high pressure, and from the MIT tests there are eight of them that are pure steam.

For the effect of noncondensables, I have the data from MIT from two different experimenters, Siddique and Husanein. Now, this is what I call developmental assessment. This is the data I can use in developing the model and adjusting these correlations to make sure they all work together.

Now, the other term --

MR. SCHROCK: You don't have our data you're saying?

MR. KELLY: Well, if you remember, I did write to you and you were kind enough to reply and you told me that the data was propriety and that I would have to ask --

MR. SCHROCK: No. I didn't say that. I said that it had to be requested from General Electric and I assumed that you would do that and have it. Have you not done that? I mean the data have been supplied to NRC in the regulatory process so I don't understand why it wouldn't be available to you.

MR. KELLY: Okay. I misunderstood then. I thought the data was proprietary, in which case I could not use if for developmental assessment. You'll notice it's down here under applications.

MR. SCHROCK: Well, some of it is published already and at the International Multiphase Conference Mr. Cooper --

MR. KELLY: Yes, you did send me the papers. But I needed the raw data and I will check --

MR. SCHROCK: The thesis were requested. I mean you requested the thesis.

MR. KELLY: Correct.

MR. SCHROCK: They have been supplied to NRC in relationship to the certification program, and what I replied in my letter to you is that I should not be the person that you solicit this from in view of its role in the certification program. That's the message I intended to convey. You have to get it from GE because I think they're quite -- they have already given it to the people in Regs so I don't understand why you don't have it.

MR. KELLY: Yes, I'll check and find out because I --

MR. CATTON: Well, the thesis isn't proprietary, is it Richard?

MR. SCHROCK: Of course not.

MR. CATTON: He can get the thesis.

MR. KELLY: Yes, that's what I had asked for and was told I needed to get it from GE and by that response I misunderstood and thought that it was proprietary, and if it was proprietary I cannot use it in developmental assessment but I can use it in applications assessment. What that means is that I can't adjust a model based on it but I can see how good the model is.

MR. SCHROCK: Well, GE puts no limit on us. We don not accept research contracts that do not permit us to publish our results. On the other hand, we do respect the sponsors right to deal in the regulatory arena in this way and so you have the data in the Agency or other data which have yet to be fully documented for GE which will shortly be available. But I don't think that you should represent this as Berkeley data as unavailable for your assessment. It is certainly available.

MR. KELLY: Good. I'm glad to hear that because, frankly, I can use all the data I can get and I intend to do an extremely large amount of assessment, not using RELAP 5, but I have a small two-foot study state code that I use for correlation developmental. I'm having them modify it for condensation, but that's what I'll do. So I'll talk to NRR first and see if they have the data.

MR. CATTON: Well, they have the two -- were they masters or Ph.D.'s, Richard? Because we got them.

MR. KELLY: Good. That's good news. Thank you.

So then, this will be moved up to developmental assessment as well and I will then talk to you later about this, Professor Schrock, to see which ones are the best ones to run because the cleaner the boundary conditions, for me the better.

For what I'll call applications assessment, I will look at the CMT tests because I need to worry about falling films on the inside wall of the CMT. Now, the CMT tests -- these are the ones done by Westinghouse. They'll be two sets. One is pure steam and one with steam and a noncondensable gas, and three types of tests. One is where the tank is empty and there is just pure wall condensation and they drain off the condensate at the bottom. So it is a transient. But it's only wall condensation.

Then there will be the series of tests where they maintain a stable level in the CMT so they get both wall and pool condensation. But they have that at different levels, and the idea is that way you have different wall condensation amounts and you can subtract it to get an estimate of what the condensation on the pool surface is.

So admittedly the aspect ratio isn't the same as the plant but it's a whole lot better than no data.

MR. CATTON: Well, if it a stable level then it will behave almost like an air function. The condensation on the water interface will die out pretty quickly. It's almost like air function.

MR. KELLY: These tests will be done with RELAP 5. So it will be done on a transient mode.

MR. CATTON: The drain test will be the interesting one.

MR. KELLY: Right. And I will plan on doing all of those test with RELAP 5.

MR. ZUBER: What is scheduled when?

MR. KELLY: Well, for getting a model into the code, that's scheduled to be done by June. So the developmental assessment will be done by then. Then the applications assessment will be done after that. That's my goal.

Then finally, we have the PANTHERS experiment for SBWR. So it's full scale, typical, you know, prototypical conditions for both the ICS and the PCCS. So believe it or not, I'm finished.

MR. CATTON: Well, I would first like to compliment Joe. I think you've done a very good job and you carried this through from the beginning to the end in what is a very scrutable presentation. It's fairly easy to follow and I think you're going to be successful with it and I thank you for the presentation.

MR. KELLY: Thank you.

MR. CATTON: Novak?

MR. ZUBER: I'd like to make five comments.

MR. CATTON: Five?

MR. ZUBER: Five. Number one, I would really like to complement you. It was a very, very good technique of presentation. It was a good technique of presentation. It was candid and it was really pleasurable to listen to you. I'm sure this was one of the best I have heard.

MR. KELLY: Thank you.

MR. ZUBER: Two, I have a concern I have to adjoin here with Joe. What you really found is very interesting and very appealing but everything is dominated by the film thickness. I think this is a nice piece of work. I see, however, a problem because I don't think -- I'm afraid that data won't be really accurate to predict alpha and that all these refinements which you went through -- and they're really good and sound -- will be automatically completely overshadowed by the inaccuracy of calculating the liquid under those conditions.

Three, I would really urge you to write a report as soon as possible. I think this will give credit to the work this agency is doing. Four, I was really pleased to see the that the concerns which this Committee has voiced over the past three years have been addressed in this model. Finally, we are getting somewhere.

Five, it is really too bad that this was not done two or three years ago. We could have avoided many unpleasant discussions, and more than that I think the problem would have much ahead, the schedule would have been much better.

So, altogether, I think it was a pleasurable thing to sit and listen to you. Thank you.

MR. SEALE: One other thing. I think the management, that Joe's supervision, ought to be congratulated also for providing the opportunity for him to do this. This was a very nice piece of work.

MR. LAUBEN: Can I say something? I'm his immediate supervisor and I don't won't to take very much credit.

MR. CATTON: Why not?

MR. LAUBEN: Because he's had to work a lot on his own at home with this stuff and that's a real credit to Joe and at the same time trying to satisfy his Ph.D. supervisor at Berkeley and do the things we've asked --

MR. KELLY: At MIT.

MR. LAUBEN: Sorry. At MIT.

MR. KELLY: Unfortunately, this is not my Ph.D. work. I wish it were.

MR. LAUBEN: Right. He's in essence doing virtually three jobs -- this job, the things we ask him to do, and completing his Ph.D. requirements. So I think that's quite an effort. I want to give him all the credit for it.

MR. SEALE: We urge you to keep this in mind though.

MR. WULFF: In view of what I said about the level tracking this morning, I also like to join Novak in what he had said. I think this was a good example of how we can find out what is being done and it was very good work. That was a good example of a presentation.

MR. ZUBER: May I add one more thing? I hope that this agency will take work like this as a standard. I think our work with you would be much easier to do and it would be much easier than to confront any concerned scientists or whatever. I think this should be a standard for the work and it's an example it can be done. It depends on the quality of their people and the vision of the management.

MR. CATTON: I agree. I agree. I think what I'd like to do is to -- if people will accept my suggestion -- is take just a 30 minute break for lunch, come back at 1:00 and then we'll just work our way through these topics until we reach 3:30 and end it at that point. Okay? Let's do it.

[Whereupon, at 12:33 p.m, the meeting in the above-entitled matter was recessed, to reconvene at 1:00 p.m. this same day.]

AFTERNOON SESSION

[1:00 p.m.]

MR. SHOTKIN: The next topic is critical flow.

MR. SHERON: Ivan, we were wondering if we could do the RELAP5 field equation since Professor Ransom is here.

MR. CATTON: All right.

MR. SHERON: Trying to get him and the right people in the same room at the same time is kind of tough. If we could start with Walt Weaver.

MR. CATTON: He's the speaker for the rest of the day.

MR. SHERON: Except for Gary, on the interfacial heat transfer.

MR. CATTON: That's right. Why don't we do that, do the field equations and then back to interfacial heat transfer.

[Slide]

MR. WEAVER: Good afternoon. My name is Walt Weaver, from Idaho National Engineering Laboratory. I will be making a presentation on the RELAP5 field equations. The presentation is divided into two pieces. I will make some general comments about the documentation of the field equations since that seems to be a concern of the Advisory Committee, and we believe that the totality of the documentation is rather complete but it was not all contained in Volume 4 of the manual which was what the Committee reviewed. We are going to rectify that situation, and I will speak to that in detail.

Then, there was a specific comment -- that was a general comment -- there was a specific comment made on the form of the momentum equation; that it was in the non-conservative form, so I will discuss that at some length. First, regarding the documentation of the field equations.

[Slide]

MR. WEAVER: Most of the documentation that we prepared was directed toward the code user, so that the user could tell what was actually in the code. The documentation was not prepared with the idea of defending the particular form of the equations as they finally appeared.

Volume 4 which was originally prepared as a stand-alone document in support of the CSAU effort was intended to show the context in which the constitutive relations appeared in the equations, so that it would motivate the discussion of the source and applicability of the individual constitutive relations. That's repeated in the last bullet.

The section on the field equations was to show where the factors fit within the calculational scheme rather than to derive the equations from first principles.

[Slide]

MR. WEAVER: We are proposing to issue a new RELAP volume based on Vic Ransom's internal of the work that he did and presented at this summer school in France in 1989. We are proposing to take that report and put it through tech editing and issue it as a NUREG report. This report starts from the continuum equations of two inter-penetrating media and does all the kinds of mathematical manipulations that you need to get down to the partial differential equations.

I must say that we had not issued this before because Vic was thinking of using this as a basis of a book. He has since decided not to do that because of the press of being the Chairman of the Department at Purdue, and has given us permission to reissue this report as a NUREG.

We will take the equations as derived in this new RELAP volume as partial differential equations and develop in more complete detail the difference equations that are actually coded and solved in the RELAP5 program, so that you can see the tie between the partial differential equations as derived in this new volume and the difference equations that are actually solved by the computer program.

Finally, we will take out of Volume 4, the material on the derivation of the equations which was admittedly sketchy, and reference the final equations in Volume 1 as the starting point for Volume 4 for showing where the constitutive relations fit within the framework of the calculational procedure.

To facilitate doing that, for converting our documentation into an electronic form so that we can keep the documentation much more up to date, we are converting it to a desktop publishing program called Framemaker which has a very good equation editor so that the engineers themselves can get the equations in the manual and get them correct. The procedure at the laboratory prior to the preparation of the manual in 1990 was very cumbersome, where we had to iterate back and forth between the word processing people. They did not have what you call a WYSIWYG way of looking at the equations. They had to print them out to see what they would really look like. Every time we iterated back with them they would correct one mistake in an equation and introduce another one. It was quite a cumbersome process.

With this new desktop publishing system and all the engineers having it available on their own workstations we will be able to maintain living documents in that, when an engineer makes a change to the code not only does he submit changes in the FORTRAN coding into the code architect but he also must submit changes to the manual. The manual will always be up to date with the current version of the code, and it will alleviate some of the problems we have had with us saying that's not really what the code looks like now. This has become available as we have been able to get workstations and this high quality publishing type software.

That is all I wanted to say about the documentation of the field equations. Now, I would like to address the particular comment that was made by Dr. Schrock, that the momentum equations were written in non-conservative form. He didn't understand why it was done and where the justification was.

MR. ZUBER: Let me ask you, these Volumes, are they going to be available in September?

MR. WEAVER: Yes.

MR. ZUBER: All of them?

MR. WEAVER: Yes.

MR. ZUBER: Okay.

MR. WEAVER: They are planned to come out with the code so that everything will be documented and will all be current with the version of the code, and we won't have to send out interim internal documentation to describe the model. When the code comes out the documentation will automatically become available.

MR. CATTON: If you put it on a CD ROM and you put and use Framemaker's indexing program, you can do a search of whatever you want.

MR. WEAVER: That's right. Framemaker has a very strong what they call cross reference feature, so that you can automatically generate nomenclature and make sure that the mass transfer, kilograms per second in a cell, is always gamma rather than M dot AB as is currently in some of the documentation. It ensures consistency.

You can also generate an index as well by doing key word searches. It's a very, very nice program. Because it's so versatile, of course, it takes some getting used to, just like any of the other ones would. It's intended for the assembly of multi-volume books as you will, where various sections are prepared by different people. There are automatically numbers from one section to the next, so the numbers are all consistent.

When you have an equation number you can cross reference it in the text. If you put another one in between it, it changes the cross reference in the text. It goes a long way to automating a lot of the problems that you see when you try to edit things by hand and then change. When you add a reference, for example, it slides all the ones down and changes all the reference numbers in the text. It will go a long way, to alleviating some of the problems we have had with the documentation.

The third thing is that, maintaining the living manuals will help alleviate losing the institutional memory. So that, when a model is put into the code the manual will be under document conversion control just like the code is. Are there any questions regarding what we are planning to do for the documentation? This is really the first section of my presentation. It kind of stands by itself.

[No response.]

[Slide]

MR. WEAVER: Let's go on to the momentum equation. The observation was made that the momentum equation was written in non-conservative form, and the manual didn't discuss why it was in non-conservative form versus conservative form.

What I have done is extract some of the design principles for the numerical procedure which were contained in Volume 1 of the manual. These were the principles that were used to derive the particular form of the difference equations that were solved in the code. Mass and energy conservation were considered paramount, and a greater degree of approximation to the momentum conservation was considered acceptable at that time. This is historical now. I am talking about where the equations came from and why they are the way that they are in the code.

Implicit evaluation was used only in the terms needed to exceed the sonic Courant limit because that was the problem at that time, trying to run at greater than the sonic Courant limit. The time level of the various pieces of the terms, whenever you had a product of void fraction, density, times of velocity for example, the time levels of those pieces were adjusted to make the implicit terms linear.

The mass flux term in the continuity equation, the fluid property which was void fraction times density, was evaluated at the beginning of the time step. The velocity was evaluated at the end of the time step. So, the velocity was implicit in that term. The fluid properties were explicit, but that makes that term linear.

I would like to say that the non-conservative form has errors of the same order as the conservative form. That comes from the differencing of the spacial gradient operators. If you difference the gradients in first order differencing, whether it's conservative form or non-conservative form, the method is still first order. It may converge faster for a conservative form because the coefficient in the leading error term may be smaller, but it's still only first order.

[Slide]

MR. WEAVER: Using the design principles as stated in the first bullet on the previous slide --

MR. CATTON: The energy equation, is it written in conservative form?

MR. WEAVER: Strictly speaking, the expanded -- what is the so-called expanded form of the equation -- none of them are conservative form because you have expanded the products. The definition of conservative form from the textbook states that it's in conservative form if all the coefficient of the derivative terms are constants or, if they are not constant, they do not appear within the derivative terms.

When you expand for example the product of Alpha Rho in the time derivative of the continuity equation and use the expanded form that makes that particular part non-conservative, according to the strict definition of conservative form.

MR. CATTON: This definition means that the convective terms are written as Del dot, Rho V dot.

MR. WEAVER: Right. That, too.

MR. CATTON: Del dot Rho, whatever.

MR. WEAVER: The same thing happens when you split that Del dot of whatever product is into the coefficient times the divergence of the individual and expand it all out, that also destroys the strict definition of conservative. There is non-conservative in the strict sense in both the time derivative and in the spacial derivative.

The reason that the non-conservative form was used is that when you combine the non-conservative form with these design principles where we have adjusted the time levels of the properties of some of the primitive variables and not others you get a Poisson equation in pressure, which means that the system matrix that you have to solve, instead of being a six N by six N where N is the number of cells for example, it comes down to an N by N and then you back substitute for the other five variables.

When the numerical procedure was first developed -- how many years ago, 77 -- computer capacity was not what it is today. That was a great advantage to reducing the amount of time spent in the matrix solution. Another part of this is, RELAP does not iterate. It takes a single advance, controls the truncation error based on the time step. You may be able to take larger time steps but then you are balancing off the time required to re-evaluate the matrix coefficients and solve the matrix against the series of smaller time steps without iteration.

I believe that when the algorithm was first put together they looked at iteration and decided that a single step method was more efficient computationally.

MR. CATTON: Virgil, this is your question.

MR. SCHROCK: I am sorry I am late.

MR. CATTON: You got here just in time.

MR. WEAVER: One of things that I think is the major concern is the modeling of the momentum flux term in the momentum equation, because those are the ones that you normally see big errors in and you can see errors in your solution as a result of the modeling of the momentum flux terms. I am going to concentrate on the momentum flux terms now.

[Slide]

MR. WEAVER: What are the consequences of inaccuracies of modeling the momentum flux terms. You get artificial pressure losses in your system which result in lower flow rates for a given momentum source, if you have a pump for example, running at a certain speed and putting out a certain head. If you have numerical losses of momentum in addition to your physical losses of momentum, physical losses being wall friction, interfacial friction, you will get in a steady state a lower flow rate than you should. This also shows up as the incorrect pressure drop or recovery through area changes.

To look at this problem I have come up with a simple problem to look at the accuracy of the momentum flux formulation in RELAP.

MR. WULFF: Should there not also be among these two a third one, referring to the numerical damping when you have oscillatory flow that the amplitudes of your flow oscillations --

MR. WEAVER: That's right.

MR. WULFF: Are smaller. Is there an assessment of the magnitude of this damping, not to the friction but to the numerical damping?

MR. WEAVER: We have run oscillatory problems like the manometer which is a well known thought problem, to investigate for oscillatory problems, whether we do get numerical damping. That's shown in Volume 3 of the manual, I believe. Yes, you are right.

MR. WULFF: What frequency is that?

MR. WEAVER: What frequency? I believe it's about 2.8 hertz, the magnitude of the manometer, the frequency of the period of manometer that we set up.

MR. WULFF: Two oscillations per second?

MR. WEAVER: That's not true.

MR. WULFF: That's what I thought.

MR. WEAVER: The period is 2.8 seconds.

MR. WULFF: Right.

MR. WEAVER: I don't want to put words in your mouth, but I believe your concern might come out of stability, where we know that these finite difference methods have finite damping associated with them due to the upwind differencing in the --

MR. WULFF: Whatever numerical damping there is, it can perhaps be easily assessed for oscillatory flow. But there is a relation between the damping of oscillatory flow and non-oscillatory flow, and you have a distortion in the times and time constants. You have indicated that.

Is there a quantification of this artificial pressure loss? Do you have a ratio between --

MR. WEAVER: I am going to show you, based on these little simple thought problems, what the numerical loss is. The problem consists of a closed loop laying in the horizontal plane, so there's no gravity head. It has area changes in it, so that we look to see whether we are getting the correct pressure drops and losses through contractions. We turn off the wall friction. RELAP has a user flag that you can run frictionless. It was put in to do some of these kinds of things. Running single phase, so we don't have any interfacial frictions.

The problem has no momentum sources. If I start the loop up it should go forever, if I am conserving momentum.

[Slide]

MR. WEAVER: This is just a little diagram. I said before it's a horizontal pipe that closes back on itself. There's a 50 percent area of deduction here. We are running either single phase liquid or single phase vapor. The guy that ran this for me picked a pressure of 50 bars. To be more extensive we should run more pressures.

The liquid velocities and the vapor velocities he picked were based on a constant mass flux. RELAP has an input option of specifying the mass flux at all the junctions, and it automatically adjusts the velocities for you. This is the conditions that were run.

[Slide]

MR. WEAVER: We ran two cases for single phase liquid, where the irreversible or physical losses at the contraction and at the expansion were both user set to zero by the user. You have the opportunity of putting in a loss coefficient to represent the irreversible loss through a fitting. Then, we ran it again, where the irreversible loss of the contraction was 0.5, and for the expansion was 1.0. We are going to look at what the velocity does in this, whether it keeps circulating infinitely or not.

[Slide]

MR. WEAVER: Now, for single phase liquid with no losses which is this case, it looks like it's constant. I have another expanded view of that to show you that I am not trying to pull the wool over your eyes.

MR. CATTON: How many cycles was that?

MR. WEAVER: That's 2,000 cycles. This is with the physical losses. For this case it looks like you have no losses at all.

[Slide]

MR. WEAVER: When you expand that and plot it on a much larger expanded scale, you will notice that now these numbers are varying in the sixth significant figure. You really do have a small numerical loss. This is because liquid is not incompressible. If you look at the equations you will see that it's a velocity equation for the divergence. It has area ratios in it but it does not have density ratios.

Looking at it, I first thought that it would be correct for an incompressible fluid. For this case liquid is basically incompressible, and you can see that that's been borne out. Once again, that's 2,000 cycles. Zero sources, only the numerics. Since we are solving the matrix equation for the pressure solution with a numerical procedure, some of this might be round off in the matrix solution. You can't tell. It's hard to tell when it's this small.

Wolfgang, you look perplexed.

MR. WULFF: No. I don't recognize what I asked before. It's the relative importance of this. You simply picked some loss coefficients.

MR. WEAVER: Right.

MR. WULFF: I am not sure the coefficient of 0.5 is for a tube going into an infinite tank, perhaps. I don't know where you picked this from and how relevant they are for the entrance in the core or for the space or grids. Are we dealing in this range?

MR. WEAVER: Yes. The loss coefficients for fittings and grid spacers are in the range of one to ten.

MR. WULFF: One to ten.

MR. WEAVER: Yes. You might argue that the "K" for the contractions should be .25 rather than 0.5. The guide picked 0.5 based on the 50 percent contraction.

MR. WULFF: When you have a two-phase multiplier with two-phase flow, what is the assessment of the numerical damping then?

MR. WEAVER: I have not run two-phase flow cases. I am going to show you on the next slide that the magnitude of the loss does depend on the density gradient around the loop. Again, it is small, compared to the physical losses. If you have two-phase flow with multipliers the physical loss only goes up. It doesn't go down, it goes up. It goes down, relative to single phase flow.

I would have liked to have been able to -- for this one, if I put in a momentum source so that it was absolutely flat to 13 significant figures, it would have been very, very, very, very small. Whether I have four varies on there or five varies on there, I can't tell you.

MR. WULFF: But we have ten seconds and not ten hours or ten minutes.

MR. WEAVER: That's right. But in a real plant you have momentum sources, you don't have sourceless flow. You have real friction which is a sink, and you have real gravity head which is a source. I am just trying to get the magnitude for frictionless here. I am trying to put it in perspective, what the numerical loss is for this frictionless case.

[Slide]

MR. WEAVER: If I do the same thing for single phase vapor you can see that there is a small loss. I say small, relative to losses associated with real physical losses in a real system.

MR. SCHROCK: Have you compared the K --

MR. WEAVER: I have been trying to do that, and I was going to speak to that, Virgil.

MR. SCHROCK: I don't think you heard my question yet.

MR. WEAVER: Okay.

MR. SCHROCK: Have you compared the K with the FL over D for the loop that you simulated?

MR. WEAVER: No, I haven't. F is zero, for these cases.

MR. SCHROCK: The way you have calculated F is zero. You have a geometry and you know what the friction is in that geometry, have you compared the FL over D for that loop that you have calculated with the K's.

MR. WEAVER: I have not calculated the FL over D, if I allowed F to be greater than zero. I ran these frictionless, so by definition F is zero.

MR. SCHROCK: I know, but your conclusion is that friction is represented by K's. I am asking you to tell us if you know, how FL over D for the loop that you have calculated compares with the K's that you have used in your computation.

MR. WEAVER: No, I do not know.

MR. SCHROCK: That ought to be compared.

MR. WEAVER: If you put in real friction this would only go down faster, because you are putting in more momentum sink.

MR. WULFF: I think Virgil's point is that you dominate with form losses which are always greater. But if you have geometries that have only the small L over D, then you have a much smaller decay due to physical reasons.

MR. WEAVER: Yes, I would agree with that.

MR. WULFF: What you have here, I think you can really -- instead of making these calculations look at the truncation error and normalize it with the friction, and you can give an expression for the ratio of these two forces of diffusion types, one through numerics and one through wall shear.

MR. WEAVER: In the single phase incompressible case, the ratio will be zero, incompressible. You will have no numerical loss that will be exact.

MR. CATTON: I really find that a little bit difficult to believe. It may be true, but I still have difficulty with it. What did you use for V, Delta T over Delta X.

MR. WEAVER: Courant number, I did not run these calculations myself. This was run with a fixed one millisecond time step, so I suspect that you are fairly low below the Courant number of one. As I said, I did not run these myself.

MR. WULFF: You do have these extra terms, VISC F and VISC G. How large are they compared to the friction term?

MR. WEAVER: The combination of the difference of the cell centered velocity squared and the VISC term gives you a conservative form for the velocity diversions, in that the momentum cell velocity is convected by the cell centered velocity which is at its edge. That VISC term has an area ratio in it, which is why when I looked at it I suspected that I would get these nice good answers when I was incompressible.

[Slide]

MR. WEAVER: But because it did not contain a density ratio, I also suspected that I would have a numerical loss when I went to compressible flow, which is represented by this:

[Slide]

MR. WEAVER: This confirmed my suspicions. This is an area where we need to look some more.

MR. CATTON: Is this also for 2,000 steps?

MR. WEAVER: No. It's 1,000 steps, I think.

MR. CATTON: That's still a lot.

MR. DHIR: Considering the time is only one second, that's large.

MR. WEAVER: I have to quantify the numerical K somehow. Since RELAP, if I put a pump in here and change the pump speed to make up the numerical pressure loss so I get an equivalent K, because of the heating of the fluid due to the pump I never get to steady state. The code will not allow me to put a negative K someplace to make up this loss, to get an idea of it. I have to make some changes to the code, adjust the K, so this is flat. That will give me an idea of the relative magnitude of the numerical loss versus losses which are more physical for real systems.

I also think I know how to correct this, by putting density ratios into this term so that this will be flat.

MR. WULFF: The original purpose for these VISC F and VISC G terms was to stabilize numerical oscillations; was it not? Is there a second stabilizing step? What we are really interested in is the combination of the numerical damping that comes from this source and any other source in this case on the momentum balance.

I think you have shown us some examples, but they are selective. I am not prepared to generalize these selective answers.

MR. WEAVER: The momentum flux term is written as the difference or the squares of the cell centered velocities plus the so-called VISC term, which maybe is misnamed but that's historically what it has been called. When you do the algebra you discover that the combination of the difference of the cell centered velocities and the VISC terms gives you a "conservative form in velocity" for the velocity diversions, in that the momentum cell quantity which is the junction velocity is convected to the next momentum cell with the cell centered velocity between them. What leaves one momentum cell enters the same face on the other side.

I don't believe that the VISC term was added after the design of the numerical procedure. It was put in that way from the beginning, but I can't confirm that. I wasn't around 15 or 20 years ago.

You can show algebraically, that the difference of the cell centered velocity squared and the particular form of the VISC terms does lead to a so-called conservative form in velocity, not in momentum. The areas are in there but the densities are not in there, which is why we see this.

MR. WULFF: Is that documented?

MR. WEAVER: It will be. As we improve the manual through this process we will put that in and t[illegible] l be part of the expansion of Volume 1, so that you will see exactly where all these terms come from.

MR. RANSOM: May I address the Chair?

MR. CATTON: Sure.

MR. RANSOM: I would just like to add a little clarification to this. What you are talking about is work that John and I did years ago, and maybe I can add some clarification.

We purposely formulated the momentum equation to be in divergence form in terms of the velocity squared, such that it would degenerate to Euler's equation in cases of variable area ducts and steady flows. When we did that the usual approach was to use upwind differencing for the momentum flux term, which in a variable area duct leads to losses. We felt that was unacceptable.

What we did is, we formulated the VISC term which would normally arise as a result of upwind differing, in such a way that for incompressible flow and variable duct flow would lead to zero viscosity. Flows like you have just examined here, indeed, compressibility is the only effect which would lead to some loss, just as Walt has discovered. That's completely consistent.

The only type of way which is actually damped by that upwind differencing or equivalent artificial viscosity are compression waves, which are propagated through the flow. There will be some damping associated with that type of thing. Oscillatory flows like a manometer for example that don't involve compressibility significantly, will not have any damping, and that's what the test problem showed for varying and significant damping.

I would just like to add one more thing. I have talked with these people repeatedly. This word, conservation form I think Walt has corrected pointed out, this is something that came from the literature about 10 or 15 years ago having to do with hydro codes and calculational schemes. I think like it was correctly pointed out, it means divergence form, when the convective term can be put into divergence form.

Really, in RELAP5, both the mass and energy equation are in what you would call conservational or divergence form. Even though the time derivatives have expanded, that's not considered a non-conservational form. The thing you have to realize is, whenever you deal with partial differential equations if you want to use method of characteristics or other schemes to examine how boundary conditions and things like that should be formulated, it's always necessary to look at them in the primitive variable form.

This idea of conservational form came about as a result of difference equation formulations, in which presumably the things in the divergence would be preserved across discontinuous changes such as shocks. When we originally worked on these codes it was never envisioned that these would be applied to high speed flows. Indeed, there still today, is not any application where that's the case, where compressibility is of major importance.

Although not shown here, we have run nozzle problems with super sonic flow, where actually the normal shock that occurs in the super sonic flow is well captured by even these methods. That would be an interesting case maybe for you to look at, to get some idea of what kind of damping really is inherent in the schemes.

Conservational form does not mean that momentum is not conserved. The differential equations that are being used are just as valid as any form of the equations you would want to use. When you get to the finite difference form, obviously, there is going to be truncation involved there and some differences from that point of view. Hopefully, just a little clarification.

MR. CATTON: Meteorologists use the conservative form. I guess it was first developed by Arakawa at UCLA. The reason they give is large scale, large nodes, long times, not necessary to be in conservative form.

MR. RANSOM: I guess I am not familiar with that. There's nothing wrong with using conservation variables. That's Rho V, Rho V and Rho U, the energy variable. There's nothing wrong with that. That's just as good. It's just that here, when we talk about velocity, that's considered one of the primitive variables which you really would unfold anyway.

MR. CATTON: I understand the meaning of primitive variable.

MR. RANSOM: One point that I would like to make that I don't think came up yesterday, if you look at the conservation variable that should be preserved for a material interface or contact surface, it's velocity. Velocity is a conservation variable. It seems that we are dealing more with contact surfaces and material interfaces than we are with compressibility.

Really, velocity as a variable in divergence form, is the more appropriate variable for most of our cases. That's an interesting fact.

MR. SCHROCK: I guess I am a little surprised that you view the problems addressed by RELAP5 as not involving compressible flows. Surely, the flashing in the CMT is something that has to be described in terms of a compressible fluid phenomena.

MR. RANSOM: When I say compressibility, again

Virgil, what I mean is relative to the mach number that the velocity represents and generally accepted critical flow points. That's the only place we approach a mach number one.

Normally, we are well below mach numbers of one in most of the internal system. That's the sense that I am using compressibility here, in a sense that divergence of the velocity is basically near zero.

MR. SCHROCK: In terms of a two-phase sound speed, I am not all together sure that you really always are at such low mach number.

MR. RANSOM: Not always. That's true. We can choke internally, as the code will predict at times because of this low velocity of the sound speed. It's not 100 percent, that's true. If you would like to see how it responds to compressible flows, this nozzle problem that I am talking about is a good example, and actually does surprisingly well.

MR. WULFF: Can I ask a question?

MR. WEAVER: I want to follow up Virgil. I believe you received a reading list of papers that we were going -- should have received a reading list.

MR. CATTON: When?

MR. WEAVER: We gave them to NRC quite a long time ago.

MR. CATTON: We haven't received any.

MR. WEAVER: A list of all the references we --

MR. SHOTKIN: We sent them to Mr. Boehnert, the list.

MR. WEAVER: The paper that Vic talks about, where this nozzle problem with shocks, is on that list. If you want to look at it you can.

MR. SHOTKIN: We could send it again.

MR. WULFF: The smallness of the slope here at the top curve is due to the fact that you have almost equal velocities across the cells. That is, one should look at --

MR. WEAVER: I can change the velocities --

MR. WULFF: Let me suggest that this should be checked if you have strong dilatation in the cells. That is, if you have either large evaporation in the cell or condensation, contraction. Then, determine what the relative significance of the numerical diffusion is relative to the wall shear.

MR. WEAVER: I can do that.

MR. RANSOM: I might caution you with that. The one thing you are going to find is, it's very hard to find a steady state problem that involves that kind of problem. There are normally propagating type problems, so the information is swept out of the system and you can't really find that. That's an aside.

I also should point out one other thing. On this diagram that you drew, Walt, you really should have shown those abrupt area changes as smooth. The way you are modeling that, I presume is, they are smooth.

MR. WEAVER: Smooth, with --

MR. RANSOM: The toughest test for this is actually a variable area duct, if you really want to see whether or not Bournolli's equation is satisfied. That's exactly the artificial viscosity -- if you want to call it that -- was formulated, so that it does affect recovering Bournolli's equation for frictionless incompressible flow under steady state.

That's in contrast to a lot of the other hydro codes. They will actually have sources and sinks as a result of the upwind differencing. We purposely tried to avoid that.

MR. ZUBER: I hope all this will be explained in your Volume 1.

MR. WEAVER: Yes.

MR. ZUBER: Good.

[Slide]

MR. WEAVER: I was trying to respond to a question about the magnitude of this loss versus what I call physical losses. To do that I will have to change the code, to put an explicit momentum source as a negative K into the code. The code does not presently allow me to do this. These were done about last Thursday.

I am going to pursue this, to try to quantify the numerical loss as a K versus the physical losses that you have in real systems, so that we can get a handle on the relative magnitudes of this.

MR. CATTON: Shouldn't you also be doing these kinds of tests with a Courant number that is close to what you run them at in an actual plant calculation?

MR. WEAVER: In a real plant calculation the Courant number runs the gambit, from one all the way down to 0.1. The velocities are all different in various places, and the lengths of the cells. The only thing I can say when I look at a code calculation is, one of the cells in the code is running close to Courant number of 0.1. I can say nothing on the printout about the Courant numbers of any of the other cells or the spectrum of Courant numbers, or things like that.

That's why we go to these little simple problems where you have equal length cells and where you have constant velocities. There is actually two Courant numbers in there. There's one for the big area and one for the small area. There's just two Courant numbers in that problem. When you go to real decks it gets real messy, very fast.

MR. CATTON: I understand.

MR. ZUBER: Just one other point. I agree with Wolfgang, to put this literally to rest you really should consider a large change of density. You show a density right here which is relatively small. Since you are really working with compressible fluids, very much compressible, I think you should really address that problem and put it to rest, one way or the other.

MR. WEAVER: The code will not currently let me run frictionless in the two-phase case. I can turn off wall friction in the two-phase case but I cannot turn off interfacial friction. Interfacial friction is a damping mechanism, a real damping mechanism, that we have in the code.

MR. WULFF: But you can add it up for cell and get the sum, and use that as your basis.

MR. WEAVER: But I can't get a steady state with two-phase without a momentum source to make up the numerical loss. Somehow I have to get a steady state so that I can do all this adding up, so the transient effects don't affect the answer. I agree with you.

MR. WULFF: Then, you should use a simple analytical solution.

MR. CATTON: I would like to make a suggestion.

If you have any recommendations for how they should do this why don't you communicate them to me, and I would like to move on.

Are you about finished?

MR. WEAVER: Yes. All I have is a summary of those four test cases.

[Slide]

MR. WEAVER: I make a rather bold statement here on the last slide. Through a variety of thought problems and separate effects tests and integral tests we have never seen anything that we can attribute to the non-conservative form of the momentum equation. I believe your concern was for natural circulation flows where the driving heads are very small. I reference some simulations that we have done with natural circulation. These two are in the manual, Volume 3 of the manual, which I believe you have.

There is a compendium of natural circulation experiments and simulations as an NRC NUREG. These BETHSY tests, natural circulation tests that were done in France, compared the data and the same semiscale natural circulation tests that were done in semiscale. These appear in the same place.

MR. ZUBER: Who did the BETHSY calculations; you did?

MR. WEAVER: One of our analysts, as part of the standard problem. I don't know what the standard problem number was. It was Paul Roth, at Idaho. That's the end of that one.

MR. CATTON: Thank you. I think we would like to hear next, the interfacial heat transfer.

[Slide]

MR. JOHNSEN: My presentation this afternoon on interfacial heat transfer modeling was specifically motivated by comments that were forwarded to us from the consultants.

[Slide]

MR. JOHNSEN: I am going to address three specific comments that were conveyed. Professor Schrock made the observation that the modeling of the interfacial area is simplistic; that the heat transfer coefficients in the interfacial heat transfer model are ad hoc; and, I think both Dr. Wulff and Professor Schrock had problems with the mass transfer at the wall terminology. I want to address each one of those areas.

[Slide]

MR. JOHNSEN: I am going to start by providing sort of a summary response, and acknowledge to you that the models for interfacial heat and mass transfer are based on idealizations of the various flow regimes and to the extent possible augmented with literature based coefficients, again, where that's possible.

We believe the models are faithful to the philosophy of --

MR. ZUBER: This slide is missing.

MR. JOHNSEN: The one that says summary response is missing? I will try to get a copy to you before the end of the day.

We believe the models are faithful to the philosophy of using a simple but plausible model, and then validating it using relevant data. Ultimately, the level of sophistication in the model is based on the principle of providing an adequate simulation for the intended purpose to which the code will be placed. That's a bit of philosophy, I guess, more than anything else.

[Slide]

MR. JOHNSEN: I am listing here some of the idealizations of the flow regime interfacial area that we use in the code. Bubbly, assumes spherical bubbles and a size distribution of bubbles. I mentioned yesterday, the maximum bubble size is based on a Weber number criterion. The interfacial heat transfer coefficients are in fact based on the idea of spherical bubbles, so there's consistency between the interfacial area assumption and the coefficients that were being used.

So on down the line, you can see for yourself what the idealization is that is being made.

[Slide]

MR. JOHNSEN: One of the consequences of adopting the two fluid concept is that it requires you to specify a lot of coefficients that you cannot derive from direct measurement, simply because there's no feasible way of doing it. In fact, the combination of possible thermodynamic states and flow regimes in RELAP requires us to specify 48 phases of heat transfer coefficients. These are the liquid side and vapor side heat transfer coefficients that connect at the interface.

Of those 48 coefficients, 24 of them apply to metastable states. For the most part, the ones that apply to the non-metastable states -- in fact, in 18 out of 24 cases -- the heat transfer coefficients are from the literature.

MR. DHIR: Excuse me. What do you mean by non-metastable.

MR. JOHNSEN: I mean in other words, conditions that are plausible, super heated liquid for example in contact with the subcooled liquid, that's a non-metastable state. Super cooled vapor is metastable. Super heated liquid, metastable. That's what I mean by metastable.

MR. DHIR: No, you are saying non-metastable. Suppose I have wall, that vapor is condensing on it; what is it?

MR. JOHNSEN: You have super heated vapor in contact with a cold wall.

MR. DHIR: Cold wall.

MR. JOHNSEN: Yes, that would be a stable state, not metastable. Subcooled vapor is metastable. Superheated liquid is metastable.

MR. DHIR: You talk about it, that you have something sitting at a neutral equilibrium and it can go either way.

MR. JOHNSEN: Yes, stable. Stable, as opposed to non-stable. The coefficients that are used for the metastable states are in fact designed to ensure that these states are transitory. They are not stable, so they should be not long lasting.

I did point out at the bottom of the slide that there is an error in the Volume 4 relative to bubbly flow with super heated vapor -- that's what that stands for. The Volume says that the NUSSELT number is equal to ten to the fourth, and that's not correct. What it should say is, the heat transfer coefficient on the vapor side is set to ten to the fourth watts per squared meter degree Calvin, which was suggested by this reference here.

MR. SCHROCK: Gary, I have the statement in front of me that I wrote on this topic. What it says is, the heat transfer coefficient in most cases is just arbitrated and motivated only by what the developer deems a desirable magnitude. An example is the coefficient HIG. I quote from the documentation on page 4.1-15, following as the quotation.

"The volumetric heat transfer coefficient HIG for bubbly SHG is not based on theoretical empirical correlation. The NUSSELT number, NUib equals ten to the four is chosen to be large in order to bring the gas temperature rapidly towards the saturation temperature. Function F6 clearly enhances this tendency, especially as Delta T SG increases magnitude. Function F7 apparently improves numerical stability for low void fractions. The determination of volumetric interfacial area is discussed in Section 4.1.1.1.

Clearly, there is room for improving the determination of HIG for this case, although to the best of our knowledge this might require further experimental work."

That's what I said. I have added my own comment on that. "This statement is typical of those following 14 various coefficients presented in this section. It is evident that the interfacial heat transfer calculation has little engineering basis. By their nature these ad hoc equations cannot be compared directly with experimental data to assess their value. The interfacial heat transfer package is weak."

Those were my comments. I think you have misrepresented them in the way you have presented the comment that you are trying to address here. Further, there is a numerical discrepancy between your assessment of 18 of 24 or from the literature, whereas I have stated here in my written report that I counted 14 in the RELAP5 documentation.

MR. JOHNSEN: The following two slides in your packet should summarize all 48 coefficients. You can count them for yourself. Where they come from literature, that's so indicated. I wasn't going to go through them in this presentation but they are in your packet for completeness.

There is, regrettably, some mistakes in Volume 4. It went out as a draft. I would apologize to you for those areas in the documentation where there are mistakes, such as the one I have indicated here.

MR. SCHROCK: You are saying that the document that I reviewed was inaccurate, not that my count is inaccurate. What are you saying? Did you look at the documentation to verify that what I said is incorrect?

MR. JOHNSEN: I was not able to establish that there were 14 coefficients that would be considered ad hoc, that is to say, having no basis in some form from the literature.

MR. SCHROCK: There's a considerable difference between 14 and six. I think what you are doing is kissing this off as something that is kind of irrelevant. It's not irrelevant.

MR. JOHNSEN: I am not kissing it off at all, Virgil. I am trying to be forthright, and explain that in those instances where there was a way to attach a literature based model to the code we have done so. Not in every case can that be done. In those instances where you cannot do it, then you try to pick a reasonable --

MR. SCHROCK: Here's a very nice example of the fact that it's recognized by somebody writing this documentation, that this might require further experimental work.

MR. JOHNSEN: Again, that was a value judgment.

MR. SCHROCK: Is this on the list of things at INEL that need to be done? Have you advised your sponsor that there ought to be some experimental work done to provide a basis for the coefficient disputed in RELAP5.

MR. JOHNSEN: No. No, not for the bubbly flow. We see no need whatsoever at this point to require any further information.

MR. SCHROCK: That was the thrust of my comments here.

MR. JOHNSEN: I think what I want to try to convey today is that the code does embody assumptions, and that they can be looked at and criticized. We could argue all day about some of these things. In the end what we try to do is, come up with a plausible approach to modeling something and then testing it against what we think is relevant data, that being data that can be associated with the ultimate application of the code.

What I am trying to do today is not so much defend every coefficient that is in the code -- I don't think that's very productive. What I would rather do is show you a few examples of the kinds of experiments we have used to either find out our model was adequate or was inadequate, irrespective of its simplicity or how arbitrary it might seem.

[Slide]

MR. JOHNSEN: I have given you here, a list of the experimental problems we have used to specifically test, validate and --

MR. WULFF: Excuse me. You are not explaining the tables, telling us what --

MR. JOHNSEN: I am not going to spend the time to go through the tables.

MR. WULFF: I don't mean to go through the tables. I think you need to know what the "F's" are, and you have to give us dimensions. This table is really not readable.

What is three times ten to the sixth?

MR. JOHNSEN: If we start into the table I am afraid we are going to be here all day long. If you have a specific question, I promise to give you an answer to it.

MR. CATTON: That sounded like a specific question. What is three time ten to the sixth?

MR. JOHNSEN: Which one are you referring to?

MR. CATTON: Liquid film, under hif,SHL.

MR. WULFF: Is that 3.6 by Theofanous, or how do I interpret this table? Where does it come from?

MR. JOHNSEN: You are referring to --

MR. WULFF: Table one.

MR. JOHNSEN: This number right here; is that correct?

MR. WULFF: That's one place, yes.

MR. JOHNSEN: This is the liquid side heat transfer coefficient where there is super heated liquid. What this says is that the coefficient on the liquid side is set to be very high.

MR. WULFF: Is it in watts per meter squared in degree?

MR. JOHNSEN: The specific units there are watts per square meter degree, Calvin. This is one of the metastable conditions, super heated liquid.

MR. ZUBER: I have a short question. In annular flow you have a liquid. How do you calculate heat transfer at the wall.

MR. JOHNSEN: That depends on the wall condition, whether it's --

MR. ZUBER: Suppose you don't boil.

MR. JOHNSEN: Suppose it's condensation?

MR. ZUBER: You have annular flow through a pipe which may be heated, you are routing energy to it but not burning. How do you calculate the heat transfer.

MR. JOHNSEN: In that case, assuming that the velocities were high enough, it would be forced convection to the liquid phase.

MR. ZUBER: Only liquid.

MR. JOHNSEN: Yes.

[Slide]

MR. JOHNSEN: I don't want to spend time discussing each one of these cases. They are in your handout. In each case what I am describing is the experiment that we used and the specific objective in terms of assessing the code. What I am going to do is show you several that relate directly to the interface heat transfer modeling for various flow regimes.

[Slide]

MR. JOHNSEN: The first one I am going to show you is this Christensen Subcooled boiling experiment. This experiment involves a heated tube in which we introduce subcooled liquid at the bottom, subcooling of about 12 degrees Calvin. The experiment was run up at five and one-half mega-pascals. The skin heater power is 70 kilowatts, a little over a meter per second.

This experiment was used to assess the interface mass transfer and subcooled nucleate boiling models. This is the example that Joe Kelly was actually talking about this morning.

MR. DHIR: Here, I have question. How do you calculate first the subcooled nucleate boiling heat transfer coefficient?

MR. JOHNSEN: Using the Chen correlation.

MR. DHIR: Is it valid?

MR. JOHNSEN: Rex, do you want to comment on that, the applicability of Chen and subcooled boiling?

MR. SHUMWAY: This correlation is referred to as applicable for subcooled boiling when you use T-wall minus T-liquid on the convection part of the correlation, T-wall minus T-sat on the boiling part of the correlation, Zuber.

MR. JOHNSEN: From his original reference, you mean?

MR. SHUMWAY: No. He originally, just based upon saturated liquids. Other people later on have said it's also applicable for subcooled if you use the liquid reference temperature on the Dittus Buelter part of the correlation.

MR. DHIR: Even if you go back and look at your documentation in Volume 4, clearly, there is a comparison of heat flux as a function of super heat. Chen's correlation and Tom's correlation, see how much difference you get with wall super heat itself. Chen's correlation gives you very weak dependence on wall super heat. In reality, there's a very strong dependence on wall super heat, Delta T to almost cubed. Chen's correlation gives you 148 or something. It's a misuse of Chen's correlation to subcooled boiling.

MR. JOHNSEN: As Rex said, I think other investigators have --

MR. DHIR: Why don't you give reference to who has used it?

MR. JOHNSEN: Rex, the other investigators that you mentioned, are they contained in the references we have provided for Lou Shotkin?

MR. SHUMWAY: No, I don't think so.

MR. JOHNSEN: Could we come up with those?

MR. SHUMWAY: Yes.

MR. JOHNSEN: We will provide those to you through the NRC. I am talking about the specific references that claim that the model is applicable to the subcooled boiling.

[Slide]

MR. JOHNSEN: Here is a plot of the result using MOD3. What I am showing you here is the void fraction as a function of the elevation of the tube in meters. On the left hand scale the void fraction, this is the experimental data in the RELAP calculation. In the experiment they did not measure the subcooling, so I am just showing you the RELAP prediction of what the subcooling is as a function of the elevation.

This is a good example of where the bubbly flow is predominant and where you have subcooled boiling going on, resulting in a combination of generation and condensing in the bulk.

MR. WULFF: This is a combination of heat transfer and vapor generation rates. It is not an assessment, an isolation and then an assessment of heat transfer.

MR. JOHNSEN: That's correct. It's not heat transfer alone. As a matter of fact, it involves also the interfacial as well. There's a combination of things going on here. I am not going to say we are isolating just one thing. You can see that the overall capacity to model this kind of situation appears to be rather good.

MR. ZUBER: What criterion do you have to stop that void curve?

MR. JOHNSEN: In the code?

MR. ZUBER: Yes.

MR. JOHNSEN: The code uses a nucleation criteria that spans one degree of superheat, such that at one degree of superheat you begin nucleation with a minimum void fraction of ten to the minus five.

MR. ZUBER: I am familiar with some of the Christensen data.

MR. JOHNSEN: I may have misunderstood your question. We are using Leahy's model for the point of net vapor generation.

MR. ZUBER: What would have been probably better is if you had started where he starts boiling, not from the entrance but somewhere down that pipe, then you can assess whether you really predict that point correctly and calculate correctly and then you predict the curve.

MR. JOHNSEN: You mean, extend this back in this direction?

MR. ZUBER: That's right. I don't know whether this is the entrance of the pipe or what, the zero.

MR. JOHNSEN: This is the end of the heated section. I believe there was a lead in section that was unheated.

MR. ZUBER: Then you have a portion which is subcooled at the superheated region at the wall, and then you start taking off.

MR. JOHNSEN: What I am showing you is, the bulk fluid temperature was subcooled all the way down the channel. All the way down the channel it remains subcooled, according to the code calculation. The code says it was subcooled all the way down the channel.

MR. ZUBER: There are data where you start with subcooling and then you develop superheated point somewhere down the pipe, and then you develop --

MR. JOHNSEN: Then you start getting voids, pulling you away from the wall.

MR. ZUBER: The "S" shaped curve, I think that would be a better test. Here, you start immediately developing the voids from the very entrance.

MR. JOHNSEN: I know what you are saying. We have run those tests specifically for Savannah River because they were concerned about --

MR. ZUBER: Those, we didn't see. I think I see -- it's better for you to provide us with something -

MR. SCHROCK: I wonder if you are correctly representing Christensen's data. My recollection of it is, there is a substantial region upstream, more than a couple of centimeters as this illustrates, before you get in that void generation.

MR. JOHNSEN: Yes. I think this zero was the beginning of the heated section.

MR. SCHROCK: I don't think so, that's what I am saying.

MR. ZUBER: It doesn't mean that you start boiling at the entrance.

MR. JOHNSEN: I understand that.

MR. ZUBER: You have to build up a superheat at the wall, and then you start boiling.

MR. JOHNSEN: I understand.

MR. ZUBER: I think that would be a better test for your capability to calculate subcooled boiling than this curve here.

MR. SCHROCK: Can we get a comment from the person that did that; is this really the correct representation of Christensen data? I don't think it is. I think the Christensen data shows a non-boiling temperature preceding that vapor generation.

MR. JOHNSEN: You are really just quibbling about what this zero really means.

MR. ZUBER: It's not quibbling.

MR. SCHROCK: I am not quibbling. I am asking you, are you plotting this data correctly.

MR. JOHNSEN: It's within the latitude to decide to put zero where the heating begins or put it somewhere else. I don't remember in the reference exactly where the zero point is.

MR. SCHROCK: In the Christensen experiment the heating began at the beginning. But with a high level of subcooling at the entrance, there's a substantial non-boiling or at least zero net void generation distance before you begin this S-shaped curve that Novak is talking about.

MR. CATTON: Beyond the edge of the heater. If I extrapolate your black line -- there is a void fraction at zero. Somebody didn't plot it very well.

MR. JOHNSEN: I know what you are saying.

MR. ZUBER: Gary, I am not sure whether subcooled boiling is very important for AP600. This has to be thought about. The thing is, if you want to apply this code to Savannah that was a critical question, how well you predict it.

MR. JOHNSEN: Right.

MR. ZUBER: Had you done these calculations you would have seen that there is a region of subcooled flow without any voids. Then, you start developing the curve somewhere downstream. That is a test of your capability.

MR. JOHNSEN: Significant void and then net vapor generation that precede.

MR. WULFF: Your 12 degrees subcooling are representative of entrance subcooling in a boiling water reactor under normal conditions and has tremendous heat flux, more than in this experiment most likely. It takes on the order of 30 centimeters before net vapor generation begins. That's where your zero should take off in the void fraction.

MR. ZUBER: Again, this may not be important for this reactor.

MR. JOHNSEN: The main reason for these to be in this presentation was to try to illustrate the philosophy of formulating a model and then testing it against relevant experiments. This is not AP600-specific.

MR. ZUBER: I am not convinced that you really have the capability until you show the other parts.

MR. JOHNSEN: Okay.

[Slide]

MR. JOHNSEN: The Oak Ridge heat transfer test facility void profile test is another one we have used. We have a bundle, eight by eight electrically heated bundle, four and one half mega-pascals. You see the heat flux, mass flux. Again, inlet subcooling that was quite substantial.

MR. CATTON: Is this one of the THTF tests, where they actually measure the void fraction?

MR. JOHNSEN: Yes.

MR. CATTON: Good.

[Slide]

MR. JOHNSEN: This is the predicted of an actual void fraction in the experiment. The data is the dash line. RELAP is the solid line. The comparison is reasonably good.

I believe that these were the grid spacer affects in the experiment that couldn't be captured in the calculation.

MR. ZUBER: Do you know for these experiments where you have equilibrium conditions, cross sectional, for this set of data?

MR. JOHNSEN: Not off hand, I don't know where the point of equilibrium was reached. Right off hand, I don't know.

MR. CATTON: They get super heated steam up on the top when the void fraction is very high.

[Slide]

MR. JOHNSEN: The next one I am showing you is the Bankoff stratified flow case. Our objective here was to look at the condensation for stratified flow. This is an experiment that was run down at atmospheric pressure. It involves a rectangular duct, roughly six by 30 by 160 centimeters in dimensions. You have liquid and steam flow at the entrance -- at one end of the test section -- wherein, the liquid is coming in basically at close to room temperature and the steam is coming in above the boiling point at atmospheric pressure, somewhat superheated steam.

The idea is to examine the condensation of the steam on the liquid as it flows down the test section. This is the RELAP5 diagram, if you will of this experiment, where we are introducing both steam and liquid at one end of the test section and using an atmospheric sink, if you will, on the other end. The flow of the liquid and the steam is low enough that you get a stratified condition running down this channel.

What Bankoff did was to measure the flow of the steam at various locations down the duct as a means of inferring the contact condensation of the steam on the liquid surface. This is the first run. There were two different runs as I indicated on there. One is considered the low flow and one is a little higher flow case.

MR. DHIR: Excuse me. What heat transfer coefficient do you use, what correlation?

MR. JOHNSEN: For stratified flow?

MR. DHIR: Yes, for this situation. Steam is flowing over cold water.

MR. JOHNSEN: Right. Can you help me with that one, Rich? It's in the Dittus table.

MR. DHIR: Stabilized heat, Dittus Boelter correlation. Is that valid?

MR. JOHNSEN: Let me go back and look at the table.

MR. CATTON: That's what it says.

MR. JOHNSEN: Yes, that's what we use. On the liquid side it's Dittus Boelter.

MR. DHIR: On the vapor side it's also Dittus.

MR. JOHNSEN: That's correct.

MR. DHIR: Is that correct way of doing it?

MR. JOHNSEN: In this case with the stratified condition, it's an assumption that we can apply a pipe-like forced convection correlation.

MR. DHIR: You have to justify the assumption before you can apply it.

MR. JOHNSEN: Don.

MR. MCELIGOT: Back in the late 1960's there were some studies done on non-compressed gases, so they would apply for either the liquid side or the saturated vapor side for square and for triangular tubes over the range from laminar where there is a slight difference in the coefficient you would use up through turbulent flow. I suspect maximum Reynolds numbers were around 60 to 100,000 or so, based on hydraulic diameter.

Essentially, these results agreed with the Dittus Boelter equation as presented by McAdams for this particular case, the vapor, where it would be a coefficient [illegible] 21. There is some justification for using it in the case where you have a rectangular cross section.

MR. DHIR: No, that's not the question. You have a developing flow from the liquid side as well as from the vapor side. You have to calculate first the number for the vapor and the liquid and see how long that length would be.

Then, you have to go back and correct your heat transfer coefficient on both sides. What was the number for the liquid for example?

MR. JOHNSEN: I don't know, off hand.

MR. DHIR: Is it laminate or what?

MR. JOHNSEN: Obviously, they are not a high flow, otherwise they wouldn't be stratified --

MR. DHIR: Then you have very long entry length.

MR. CATTON: Looking at the channel most of it was probably entry length.

MR. MCELIGOT: That would depend on the Reynolds number. It was not, but if it had been a circular tube a Reynolds number of say 2,000, in order to get the NUSSELT number within about five percent it would take 100 diameters. That entry length varies linearly essentially, with the Reynolds number. A Reynolds number of say 100 the necessary entry length would be the order of ten diameters.

That's just giving you order of magnitude estimates.

MR. DHIR: My point is, one has to assess first before just blindly applying a correlation.

MR. WULFF: My problem is that Bankoff's experiments were on a flat bed of liquid with vapor on top of it. Dittus Boelter was internal duct flow, whether that is rectangular or circular, it's still internal flow. Why, with this difference in geometry, Dittus Boelter apply here.

MR. JOHNSEN: Under a stratified condition what the vapor essentially sees is the liquid as being a wall surface and vice versa. That's the analogy.

MR. SCHROCK: I could contrast that to the case of flow in a rectangular cross section heated on one side only. Of course, there's a lot of data in the literature, much of analytical, to show you that the heat transfer coefficient is not the same for that case when you heat from one side only.

This liquid layer is being heated only at the interface.

MR. JOHNSEN: That's correct.

MR. SCHROCK: It's being heated on one side only. Consequently, Dittus Boelter equation should not be expected to apply.

MR. JOHNSEN: What would you say would be the quantifiable -- how would you quantify the distortion?

MR. SCHROCK: As rather significant.

MR. JOHNSEN: Can you give me a number?

MR. SCHROCK: It's one of the early papers, about 1951, in which it shows the determination of heat transfer coefficients for flow in that kind of channel heated from one side only. The important difference here is that the temperature profile is essentially only one-half of the kind of temperature profile that you are looking at in the full symmetrically heated tube. It goes to an adiabatic wall on the other side.

When you calculate both temperature you get a very different number there. The heat transfer coefficient has a dependency on that temperature profile. That's what it's really representing, is a lump parameter characterization of the dimensionless temperature profile. That's what it means.

MR. DHIR: Many textbooks have tables showing these geometries, one side heated, both side heated, rectangular, circular, whatever it is, you can get for steady state or fully developed flow. Still, you have to account for the development of the length.

MR. JOHNSEN: I guess this is another -- Vic, do you want to comment?

MR. RANSOM: Can I make a remark?

MR. CATTON: Sure.

MR. RANSOM: I have listened to this presentation, and the thing that seems to be constantly happen here as we look at the details of the physical process that's going on -- while I think that's good from an insight point of view and the developer should do that -- it seems worth remembering that this thing is a systems code. Actually from the start we have sort of assumed fully developed flow everywhere. It's only a special case, if you really have to depart from that.

So often times the attempt has been to use a fully developed flow approximation for friction coefficients and heat transfer coefficients. In this case for example, friction has been calculated around a friction factor correlation and a relative velocity. If you simply use the Reynolds analogy for the heat transfer you would get a Dittus Boelter type of heat transfer correlation.

I don't think they are out to lunch in a case like this. You can argue with the details but the actual fact of the matter is, I don't think any heat transfer coefficients have been measured. They are very difficult to do between an interface and two fluids, like you are talking about here.

Really, some of these rectangular duct condensing flow type experiments are about the only cases where there's data that you can kind of assess are you in the ball park or not. I guess what I would look for is, are you qualitatively predicting the phenomena that go on. That's the first criteria. If you are not, clearly, you are out to lunch. I mean, you have to do something.

If you disagree with the data but you are qualitatively correct, then it's a matter of assessment of how important is this phenomena and perhaps in the end what does it mean as far as reactor safety is concerned. If it's big, obviously there may be some concern. I think you have to make that connection.

MR. ZUBER: May I reply to this?

MR. CATTON: After I do.

MR. ZUBER: Since you are the Chairman.

MR. CATTON: I will take the Chairman's prerogative. I don't really understand the remarks that you just made. You pick a problem, where it looks like the entry length effects are probably pretty strong. Where that steam first sees the water the gradients are very steep and you compare with a Dittus Boelter.

I think you have to ask yourself the question as to why it looks so good. You have to ask the reverse question as well, because your correlation should not have done well for this. Why did it.

MR. RANSOM: I would argue that actually it did reasonably well, because the heat transfer coefficient which they calculated was approximately what is actually occurring. I think Bankoff measured the temperatures and the steam, the liquid flow rates, all the data were there. I don't think they got the right answer for the wrong reason.

I think the disagreement that you see -- there is some trend, but it's not a strong effect.

MR. CATTON: I wouldn't look at this as disagreement. I certainly would ask as to why the agreement is so good. Novak.

MR. ZUBER: I would like to agree with Vic in one respect. I don't think that they can predict everything perfectly. I think what we have to do in the system code is to predict reasonably well, important phenomena. In this sense, I agree with you. I don't exactly know how important subcooling boiling is for AP600.

This aside, what bothers me is the ROSA which has been going for years, at least for three years. Whenever this Committee brings up a question it's been dismissed, is it important, is it relevant to safety and so on and so forth. Two years later, three years later, we find that yes it's important, and they have a crash program to put better models. Which puts during that time we have a confrontational discussion, criticism and defense. When we start resolving the problem we don't have enough time and people around.

I think what is really needed is a change of attitudes. We are not really criticism, at least I don't think we are. On the other hand, if something is important one should have open eyes and open mind to accept it and address it. I agree with you, we cannot predict everything to the last digit. But, we have to be sure that what is important is correctly addressed.

My experience during the last three years, we have been washing all this and all has been put under the rug. A given example is the condensation, thermal layers and now it's important. We brought this program three years ago.

MR. CATTON: I think that's a fair criticism.

MR. RANSOM: I guess I can't really address that issue since I haven't been involved in it that much. I think there are some legitimate concerns there. I would agree that today -- back in 1974 when we first developed this code we thought interfaces would be an important part of the problem. I, personally, worked on that for about a year and one-half. After getting nowhere basically because it is a tough problem, we decided it was more important to move ahead because we weren't seeing a lot of interfacial type of problems at that time. These were large break blowdowns, small breaks, et cetera.

Today, I think you are seeing the importance of that kind of phenomena in the kinds of systems we are dealing with today and the kinds of accidents. Clearly, there is a reason to go back and address that issue. I would agree with that and support that. I think this Committee's concerns should be important.

MR. CATTON: Gary, it's back to you.

MR. SHOTKIN: The only thing I would like to point out is, I agree with Dr. Zuber. But I think we have agreed with him for three years. I think we have pointed out in 1990 or 1991 that we needed improvements in condensation modeling, we needed improvements in level tracking, thermal stratification, the list that you see that's being worked on. I don't disagree.

I just agree, that you are not giving us any credit for having identified it.

MR. SCHROCK: I would like to comment on that. I made the point at the conclusion of my review of the documentation that the interfacial heat transfer package is weak. I am convinced, the interfacial heat transfer package is weak, and it would not be hard to make substantial improvements. If we go away from here with that view having been suppressed and NRC management contention that it is a phenomena in which the integral performance of the system has low sensitivity to the heat transfer coefficients, we are going to go on with the same low quality interfacial heat transfer package.

I don't think that's in the agency interest. I think you ought to be looking at it realistically and recognizing that it's not hard to fix most of what is weak in the interfacial heat transfer package and it ought to be done.

There may in fact arise situations in which there is a significant dependence upon interfacial heat transfer package that you do not recognize today, that nobody in this room recognizes today. You won't be prepared to cope with it if you go on with this weak interfacial heat transfer package.

MR. SHERON: I feel compelled to respond. As someone who has to sit here and make decisions on how to allocate finite resources in the face of declining budgets, I just got hit with a $3.7 million budget cut. I have to figure out where to take it. I was just told that certain other areas in my division are high priority on the agency's agenda as well as thermal hydraulics.

I have asked repeatedly of the Subcommittee, if they could please tell me what the safety significance is of all of these things that you desire us to do, to make this code academically pure, if you don't mind me using that term. All I have heard is that this could bite you in the rear end sometime in the future. It may be a problem, we don't know. It's your responsibility.

That sort of says that we have to do everything and prove it out before anybody's happy. I have said before, we have to make engineering judgments a lot of times on a lot of this stuff in order to get the job done. I get bothered by the fact that somehow engineering judgment is not acceptable.

I have asked the Committee, tell me what are the priorities within the Office of Research that you think we should be spending money on. Should I be putting unlimited funds to solve all of these problems on the RELAP code when in fact I can show you that the risk is not even dominated by this area. I have a hard time with that.

As a consequence, I have to make a decision that maybe we are not going to be putting the resources on that, and then I have to come down and continuously get criticism because we get accused of not addressing something.

MR. CATTON: I think you are missing a point, Brian. I think in this particular case what Virgil says is true. If you would just say look, I understand and I believe and Virgil is right, but I don't have the money to spend on it. Finished. To me, that's okay.

What gets perplexing is, when these things are pointed out and then huge arguments ensue as to whether or not this thing is weak or not. If you take a look at the table it's filled with ten to the four. To me, I agree with Virgil. That says that a lot of the basis for this is weak. If you decide a lot of this is weak but we don't think it's very important furthermore, I agree it's weak but I am not going to spend the money because I don't have it.

I think that's a position that you can take because you are the one who is spending the money. Don't tell us that it's not weak when it is. That's where we get into this kind of a back and forth, is when they hold up the ten to the four and say that's fine, it's plenty robust.

MR. SHERON: There is just a difference of opinion then, okay, if that's the case.

MR. CATTON: I don't know how to deal with that.

MR. ZUBER: Let me add something else. At least two years ago the problem of thermal stratification, level tracking and condensation was brought up. You made the same argument, tell me what is important. Two years later we find out that yes, it is important and now trying to do a good engineering job in six months, which is very hard.

MR. SHOTKIN: Novak, those have been in our plans for two years.

MR. ZUBER: I shall go through --

MR. SHOTKIN: I will comment after you are done.

MR. ZUBER: I shall go through the minutes of the meetings and report that these comments were dismissed by the staff, and they are on the same grounds. It is being shown, and we have to address it two years later.

MR. SHOTKIN: I will show you our plans, Novak. Those models were in our plans for at least two years that I know of.

MR. CATTON: Why don't you continue, Gary.

MR. JOHNSEN: Okay.

MR. SHOTKIN: Gary, could you comment on the ten to the fourth that Professor Catton brought up. Would you explain that.

MR. CATTON: It's not one, it's eight of them.

MR. SHOTKIN: Yes. Could you explain what that is.

MR. JOHNSEN: You will notice that most of them cover the metastable conditions. I mentioned in the preceding slide that of the 24 that cover the stable conditions, 18 of them are at the literature and six are engineering judgment numbers.

MR. CATTON: Fourteen, I guess, if I include the three times ten to the six.

[Slide]

MR. JOHNSEN: This is the low flow case. This is the higher flow case. You can see that the match is pretty good, despite the fact that some would believe that the Dittus Boelter is not applicable.

MR. WULFF: But the fact is that this is a combination of coefficients that you present here, and we cannot decide whether Dittus Boelter is applicable or not on the basis of the agreement on these. I have said this before, and you keep repeating that this tells you that Dittus Boelter is applicable.

MR. JOHNSEN: What I am saying is that the correlation is producing a heat transfer coefficient that is obviously leading to a reasonable result. That's what I am saying.

MR. WULFF: Along with other things you cannot -- compensating errors.

MR. JOHNSEN: Is this the right answer for the wrong reason again?

MR. CATTON: We don't know.

[Slide]

MR. JOHNSEN: Bankoff also measured the liquid temperature exited of the rectangular duct. This is a comparison of the liquid temperature at the end, after having condensed most of the steam. The temperature comparison is not bad, it's reasonable.

MR. CATTON: Is this counterflow?

MR. JOHNSEN: Cocurrent, stratified flow.

[Slide]

MR. JOHNSEN: That's the stratified experiment. This is the completely opposite kind of experiment. This one is by Aoki, looking at steam being injected at one end of a test section and a spray of liquid in the middle of the test section, and watching the condensation of the steam on the droplets. Again, this is a rectangular/horizontal test section run at atmospheric temperature.

The steam coming in at 100 degrees C just saturated at atmosphere, the liquid flow at the center of the test section being introduced as a spray, and the water being pretty much at room temperature. So now, we are going to have dispersed flow rather than stratified flow, droplets.

[Slide]

MR. JOHNSEN: This is the diagram of the experiment, steam coming in at one end from the time dependent volume, liquid being introduced toward the center of the test section, again, on a spray form, and atmosphere on the other end.

MR. SEALE: That arrow has to be wrong.

MR. JOHNSEN: Yes, it should be pointing the other way.

MR. SEALE: Okay.

[Slide]

MR. JOHNSEN: What Aoki did was to measure the pressure along the test section, and you can see that what occurs is a pressure depression in the vicinity of the injection point that the code qualitatively matches but doesn't quantitatively agree real well, let's put it that way.

The characteristic of this pressure suppression and recovery downstream is captured. There are some three dimensional effects going on here that the 1-D code just can't capture. The basic trend is --

MR. SCHROCK: Is this test section horizontal or vertical?

MR. JOHNSEN: Horizontal. Now, I would like to turn to the last item on --

MR. WULFF: I have a question. You say on this previous viewgraph, assess condensation model for this flow. Would you show us the -- it shows pressure. Why is pressure the thing that you show us. I don't think it's very sensitive to vapor generation rate. How sensitive is it. What I would really like to see is the objective that you list here, plotted versus experiments.

MR. JOHNSEN: We would like to have done that too, but this is the measurement that was made. It is an indirect indication of condensation in this particular situation, where you have droplets being entrained in saturated steam.

MR. CATTON: All they measured was the pressure distribution?

MR. JOHNSEN: That's all they measured.

MR. WULFF: Then, it was inadequate experiment to use for the purpose.

MR. JOHNSEN: These kinds of experiments are quite difficult to find.

MR. SCHROCK: Why should there be a discrepancy upstream of the injection point.

MR. JOHNSEN: The local pressure suppression in the calculation is causing the flow upstream to be greater in the code, therefore, the pressure dropped from the entrance to the over stated.

MR. SCHROCK: What does Aoki control in his experiment, upstream pressure or inlet flow rate.

MR. JOHNSEN: The inlet flow rate of both steam and the liquid.

MR. CATTON: So, at zero which is where the injection point is, there's a lot of multi-dimensional effect?

MR. JOHNSEN: Yes. There are also multi-dimensional effects that we can't capture.

MR. CATTON: So, it's probably causing a barrier and effective constriction and that gives them the larger pressure drop and you don't calculate that. I understand.

MR. SEALE: How long was Aoki's section?

MR. JOHNSEN: I think this is 60 centimeters

MR. SEALE: It must be a misprint on the --

MR. JOHNSEN: It says 2.86 centimeters by 60.

MR. SEALE: That's centimeters, again.

MR. JOHNSEN: Centimeters. It's a mistake on that first slide. It says meters, and it should have said centimeters.

MR. CATTON: Sixty meters would be pretty --

MR. SEALE: Yes. I guess it also depends on how directed the spray really is.

MR. JOHNSEN: Right.

[Slide]

MR. JOHNSEN: I want to turn to the last item I had on my outline slide, which is a discussion of the mass transfer of model, in particular an explanation of the term "gamma W" which was questioned in the comments as being confusing, and that there was no such thing as vapor generation at the wall. That observation is absolutely correct.

The total vapor generation model in RELAP consists of two parts, vapor generation the bulk, the first term here, and vapor generation in the vicinity of the wall. In the bulk this vapor generation term is given by this expression here. These, again, are the interfacial heat transfer coefficients. This is the jump condition which depends on the thermodynamic states of the phases.

These are discussed in Volume 1 and in Volume 4. I think most of the confusion arose from reading about gamma W in the section that had the conservation equations, where it was not completely described as it was elsewhere in the documentation. So, again, gamma Wj then stands for the mass transfer at the liquid vapor interface that is near the wall of a heat structure J, attached to the volume. For every heat structure that is attached to a control volume we have the potential for a term of vapor generation.

This goes back to what I said earlier, that it is conceivable to have a control volume that has a cold heat structure in it and a hot one, and you could have boiling at one and condensation on the other. That would be reflected in the sum of these terms.

The reason this approach was used is because as Joe Kelly pointed out earlier this morning, RELAP has only one liquid temperature. It does not calculate the fine details involving thermal gradings near the wall and some other approach must be used, and that's why we used this approach. A perfect example of where this is important is, again, subcooled boiling, where the bulk liquid can be subcooled but there is vapor generation near the wall which may or may not persist out into the bulk.

[Slide]

MR. JOHNSEN: This is the diagram that Joe showed this morning. He clarified it better than I did, by drawing a line down here to indicate a superheated layer next to the wall. The idea behind the diagram was to indicate that the overall vapor generation rate is the sum of what goes on in the bulk plus what occurs at the wall.

In this particular case as would be the case in the Christensen experiment, we have subcooled liquid. Therefore, in the bulk there's condensation going on. Therefore, this is negative, the vapor generation near the wall. This is positive. In the case of subcooled boiling the sum of those two is greater than zero, so we do get a net generation of vapor.

MR. DHIR: For calculating gamma Wj, do you just use the Chen correlation and convert that into --

MR. JOHNSEN: No. In the case of boiling that is the so-called Gamma Wj, is where that's going.

MR. DHIR: Near the wall, gamma Wj.

MR. JOHNSEN: That's right. That's partitioned. Some of it goes into vapor generation and some of it goes into the --

MR. DHIR: What partition, on what basis?

MR. JOHNSEN: That's Leahy's subcooled boiling model, for the prediction of vapor generation and subcooled boiling.

MR. DHIR: That is combined with Chen's correlation then.

MR. JOHNSEN: That is correct.

MR. SCHROCK: This goes back historically to the work done at -- I am trying to remember the fellow's name -- on the subcooled boiling. The modeling was not in the context of two-phased multi-fluid models at that time. The reference that you are making precedes this form of two-phased modeling.

What you have done is to borrow ideas out of the older modeling in which there was a vapor generation in the bubbly layer close to the wall. That was not identified as vapor issuing from the wall. There is a vapor generation within the liquid near the wall.

Now, you have come along and introduced the multi-fluid formulations. You have an interfacial heat transfer, you have an interfacial area description, but you choose to isolate some unidentified part of the interfacial area and say we are going to call this a wall vapor source. That's basically what you have done. You don't clearly identify what is the amount of the interfacial area per unit volume in your computational cell. Instead, you are treating it as though it is literally vapor issuing away from the wall.

That puzzles you, I can see. But I think this is the source of your difficulty in describing it. It's true, you have used a correlation from Leahy subcooled boiling model, but that subcooled boiling model is not structured in the form of your two fluid model used in RELAP5. You have somehow made a determination that that particular part of it can be adapted for your two fluid model. It's not clear how you accomplish that adaptation. This is the difficulty that you are having, I think.

MR. JOHNSEN: I think the adaptation simply says that a portion of the heat transfer is going into vapor generation, and a portion is going into heating a liquid.

MR. SCHROCK: But you have interface on all the bubbles that are present. There are some bubbles that are growing and there --

MR. JOHNSEN: Once they are generated, yes. Once they are generated, then they are --

MR. SCHROCK: Once they are nucleated.

MR. JOHNSEN: Yes.

MR. SCHROCK: Once they are nucleated. They grow, as a consequence of energy flow to the interface out of the liquid.

MR. JOHNSEN: Right.

MR. SCHROCK: The physics of that is not being described, at least in the description of the documentation that you are using. You have the problem of how do you average over computational cell, interface which is involved with condensation and interface which is involved in evaporation. That's what you need to be partitioning somehow. I don't think you know how to do it.

It isn't correctly determined simply by taking wall heat flux and heated vaporization to determine a vapor source. It ends up being per unit volume of computational cell in a totally artificial way.

MR. JOHNSEN: Again, this is a situation where you are trying to take the two fluid model and stretch it into a regime that is indeed more complex than the basic model to begin with. We think this is a reasonable approach to the situation of subcooled boiling. The results we have achieved seem to bear that out.

Are you suggesting that we somehow within the context of the two fluid model, take the position that there's a certain amount of interfacial area associated near the wall and a separate --

MR. SCHROCK: I said that there is interfacial area in a computational cell, some of which is associated with evaporation process simultaneously some of which is associated with the condensation process.

MR. JOHNSEN: I agree with that.

MR. SCHROCK: The change in the vapor content is a result of the net process in that computational cell.

MR. JOHNSEN: Yes, I agree.

MR. SCHROCK: It's not a source of vapor at the wall.

MR. RANSOM: I think Virgil's view is absolutely right, as a matter of fact. It would be better if they talked about it in that same way. The way you are trying to do it is model the vapor which is produced in the wall layer in the subcooled boiling situation. I think the only part of the idealization that missing is when they create vapor it's the net vapor coming out of that layer. They are not trying to model the bubbles that exist within that layer.

Within the context of the structure it immediately begins trying to condense those bubbles. It interfaces then with the bulk mixture. Really, it's like talking about the vapor which is now, after net vapor generation, is being transmitted from the wall layer to the bulk. It's the heat exchange mechanism that carries the energy from the wall into the bulk.

Obviously, they could do better in terms of documenting that and clarifying that, and I think that needs to be done. It's that kind of view that was the basis of that model.

I will make one other comment with regard to these heat transfer coefficients. I think this Committee was a member -- many of the people were -- remember MOD 0 and MOD 1, we only used one energy equation. In actuality I think I can take credit for that approximation. From early days I thought if you look at situations it's very hard to find a situation where both phases are away from saturation.

Generally, the Committee agreed that it was a good approximation. The problem came that we couldn't model subcooled boiling, we couldn't model the post-CHF heat transfer process,because those are two examples where each phase exists away from the saturation point. The logic just became untenable.

When you look at these large coefficients they have in the table what those really are is, they are saying from an engineering approximation point of view you have assumed that that phase is essentially saturated. I don't believe the database exists actually, to do a whole lot better than that.

They have in a couple of cases in subcooled boiling post-CHF heat transfer. You do find mechanistic modeling of the heat transfer from both phases to the interface, but many of these other cases is really a problem. What these documents should be saying is, they have essentially from an engineering judgment point of view assume that in that regime that phase exists near the saturation point. It also exists in a metastable state, so it's tending to relax to the stable state.

You are always moving -- the physics are moving you in the correct direction. Whether at the correct rate or not, it's difficult to tell.

MR. SEALE: Could I make a comment?

MR. CATTON: Yes.

MR. SEALE: Having not been a party to those other decisions and so on, I would also observe that when one made a model which was based on a single energy equation, that model clearly had limitations and they were understood reasonably well by all.

When one finds that one can no longer use a single energy model and you begin to have to model things in more detail, I think it's important that we still recognize that some of the models we evolve are better in quality than some of the other models. Furthermore, from a practical point of view, we should recognize that some things need to be modeled better than other things do. Indeed, that's the decision that the management people have to make, where you spend your bucks.

The problem is, there seems to be a tendency to want to defend the validity of all models with equal tenacity. That's clearly not the case. The problem is that when you do that and you find yourself five years down the road having to look at the situation again because now the situation is a little bit different than it was the last time you fixed it, you have damned the people that have that job five years from now to go through all of the detail again instead of giving them a list that says this is a pretty good model. This one isn't, but it's as good as it needs to be.

Do you see what I am suggesting?

MR. CATTON: I think that's absolutely right.

MR. RANSOM: I can't disagree with that. I think the thing that tends to be missing is documentation.

MR. CATTON: There is that too, but the defense of poor models leads to lively discussion and ill feelings. I think it would be better just to lay it out. As long as you are mentioning history and you referred to this Committee, this Committee made strong recommendations about the need for an experimental database for two fluid modeling and that never came about. That's why you don't have the data that you are looking for.

Any time that you decide that you are going to take something and model it in more detail you have to ask yourself if you have the information to do it.

MR. RANSOM: That's the message that I basically get, there's plenty of blame to go around.

MR. CATTON: You bet. I am just giving a little of it back. Gary.

[Slide]

MR. JOHNSEN: I am just about done here.

MR. CATTON: We are going to finish at 3:30. That's when these guys said they had to run.

MR. JOHNSEN: I am on my last slide.

MR. CATTON: Maybe we are going to finish at 3:15.

MR. JOHNSEN: There are some other presentations that we haven't gotten to.

MR. CATTON: I understand that. We had decided that we would go until we got to 3:30 and quit, because I lose some of the people.

MR. JOHNSEN: Again, I just wanted to summarize this particular presentation by acknowledging that we do have simplifications in the code, idealizations, engineering approximations, which are subject to question. I think it was correctly observed by Professor Seale that these are situational judgments, what's good yesterday might not be good tomorrow.

I showed you yesterday that in the CMT behavior that we saw that there were a number of instances where we have work to do. The current code is not going to do the job for that situation. Whereas, we didn't have a similar situation with the current generation reactor. These judgments are situational in nature.

That's why you have to go back to the code applicability question every time you have a new design or a new transient that you have never done before. The philosophy is that once you have identified the design and the transient state that you want to run and you test your code, test your assumptions, you test your approximations using relevant data, until you reach a level of acceptability in the results. That's the basic philosophy.

I think it goes to the question of how good is good enough. I think the answer to how good is good enough is that there's a situational decision that has to be made.

MR. CATTON: I would agree with that.

MR. JOHNSEN: Condensation was not very important in the last generation of reactors.

MR. ZUBER: Absolutely right.

MR. JOHNSEN: It is very important in the current generation.

MR. ZUBER: Absolutely right. That was pointed two years ago or three years ago. The same thing with the certification, the same thing with the tracking. These were really not important for the large break LOCA and we didn't address them in detail, and they were successful.

If you can model them correctly we are scot free. If not, we are open to questioning. The point is, this was not known in August and this work on condensation started. It was known two and three years ago. Whether it went to the large code previous generation, you knew the shortcomings of this codes. They were not able to address the problems of tomorrow or of today.

MR. JOHNSEN: The planning on the code development was begun in 1992. We didn't begin working on AP600 or SBWR until September, 1992. At the time that we had the last meeting in Idaho Falls, we had been working on the advanced reactors for less than six months. There was more work that went back earlier than that, but not code development.

MR. CATTON: I have INEL reports that are 1991.

MR. JOHNSEN: I am talking about code development work now.

MR. ZUBER: Let me just say, I don't know when you started. I just ask questions on the thing that started in August and I think on the thermal stratification a month ago. Those are really the essential problems to be addressed for this reactor, and they are addressed within the last six months.

MR. LAUBEN: Schedule, it looks like we were pretty late. I don't think there was any intention of dragging our heels. I remember the first time I got involved and submitted a presentation to the Committee was March of 1992. That was a year ago, before the previous March. We discussed these things.

Not only did we know these things were a problem but we ran into them as problems when AP600 was first analyzed in early 1992 and late 1991.

You may criticize us for going slowly, but I don't think we were trying to sweep those things under the rug, nor did we not recognize that they were important. In fact, this is now the third iteration on condensation heat transfer. Maybe we didn't apply a sufficient enough effort but we certainly were attempting to address it within the context of the funding and resources that we had.

MR. CATTON: I think with that, I would like to -- we only have 15 more minutes before these other guys leave. I think we will just hold the three that we missed, critical flow -- unless critical flow can be done in ten minutes which I don't think so, not with Virgil.

MR. ZUBER: I think this is something that should be done carefully.

MR. CATTON: I understand that. What we will have to do is -- Norm, you said something about the results of PIRT, and that would be something that would be a good topic for us to start digging into.

MR. LAUBEN: Yes.

MR. CATTON: It could be, if we do that reasonably soon we could couple these other two that we skipped, critical flow and -- really, it's just critical flow.

MR. ZUBER: Momentum.

MR. CATTON: We kind of talked about momentum equations. We could bring those two in with the discussion of the PIRT results. I think you indicated that they would be available in June. I don't want to wait for the final report. I would like us to interact before the final report.

MR. LAUBEN: Gary, did you want to --

MR. WILSON: Yes. I would like to respond to that. By June, we would be willing to come in -- I think

BNL with the SBWR concurs with this -- we would be willing to come in for both the AP600 and the SBWR, and show you the approach and the plans to achieve the PIRT. By June, I do not believe we will be in a position to show you significant PIRT results because they will be interim results.

It will be more in the order of September or October when we would be able to come in and show you the results of the PIRT. I am perfectly happy to come in June or even maybe in May and show you the approach and show you the scheduling, but before September I don't think we will be in a position, either BNL or ourselves, to talk about PIRT results.

MR. CATTON: Norm, what do you think? There's a remote possibility that we could be helpful to you.

MR. LAUBEN: That's right. Listening to what Gary says, I think there's some interesting things to hear about the approach, understanding that this is work in progress and you would be looking at interim results. I would kind of leave it up to you. It may be fine to do this in the summer.

MR. ZUBER: I would really advise to do it as soon as possible. What is really distressing is that something which should have come at the very beginning of this program to identify what we have to do is coming almost at the same time the code is already available. I don't know who approves what.

MR. CATTON: As long as it's iterative, I guess.

MR. ZUBER: The point being, the sooner we hear we can have a better dialogue. One more thing. I think what is also important is to address these two questions which remain. Also, establish a dialogue where we do not dismiss things which are not important as you prove this is important, prove this is important for safety, or us criticizing something which may not be important for the reactor. I think we have to establish a better working relationship. I think maybe this new meeting as soon as possible would help.

MR. CATTON: We can keep it on a technical --

MR. WILSON: You heard a very strong pitch yesterday from Ron Beelman about welcoming you to come on an informal basis whenever you choose, and we would love you to come in the office and see what's going on and talk to us on an informal basis. That same invitation exists for the PIRT work and for the AP600 PIRT work, and I believe Kumar would make the same offer.

Any of your consultants, any of the Subcommittee members that would like to come in at any time, we would love to have you. I think we could --

MR. ZUBER: There is a safety concern that I am concerned with here. This is the isometrical behavior. The question is, are the conditions during a transient whether we lose a sink. I think it will be really worthwhile to address this questions early in the game, not to leave it for October or later on.

MR. CATTON: I agree with you.

MR. ZUBER: I think this could also be combined with PIRT, and do it at a very early stage so that we can at least discuss it and see what experiments we can use and how we can address that problem. I think this may be a safety issue. If not, we should put it --

MR. CATTON: We have both AP600 and SBWR. The question is already asked. This is getting to be pretty free wheeling. I think we can end the recording.

[Whereupon, at 3:20 p.m., the transcribed portion of the meeting concluded.]

REPORTER'S CERTIFICATE

This is to certify that the attached proceedings before the United States Nuclear Regulatory Commission in the matter of:

NAME OF PROCEEDING: ACRS Thermal Hydraulic Phenomena

DOCKET NUMBER:

PLACE OF PROCEEDING: Bethesda, MD

were held as herein appears, and that this is the original transcript thereof for the file of the United States Nuclear Regulatory Commission taken by me and thereafter reduced to typewriting by me or under the direction of the court reporting company, and that the transcript is a true and accurate record of the foregoing proceedings.

Marilynn Estep

Official Reporter
Ann Riley & Associates, Ltd.

Idaho
National
Engineering
Laboratory

RELAP5 Field Equations

Presented by

Walter L. Weaver III

ACRS Meeting

January 4-5, 1994

Summary of Presentation

- Documentation of Field Equations
  - -Totality of documentation is rather complete but contained in several volumes and reports

- Momentum Equation Differencing
  - -Use of the "non-conservative" form of momentum equation acceptable because the numerical momentum loss is small compared to physical losses

Documentation Of Field Equations

- Most of RELAP5 documentation was directed towards code users.
- Intent of documentation was to state what was in code rather than show complete derivation.
- Volume IV was never intended as a complete derivation of the field equations but was intended to document the sources and details of the constitutive relations for interphase heat/mass transfer, wall heat transfer, interphase drag, and wall friction.
- Section in Volume IV on field equations was to show factors and terms that required closure to complete solution.

Proposed Improvements To RELAP5 Documentation

- New RELAP5 volume.

  An internal report, V. H. Ransom, "Course A-Numerical Modeling Of Two Phase Flows For Presentation At Ecole d'Ete d'Analyse Numerique," EGG-EAST-8546 (May 1989) develops the field equations used in Relap5 from basic conservation principles. (available from NTIS as CONF-8906249-1. Also available at OSTI-Oak Ridge)

  This report will be retitled and issued as an NUREG report.

- Volume I

  Hydrodynamic section would start from equations developed in Ransom report and show more complete derivation of numerically convenient set of difference equations.

  Increased discussions on numerical approximations.

- Volume IV

  Material on derivation of field equations will be removed and Vol IV will repeat the final equations derived in Vol I

Planned Improvements To RELAP5 Documentation

- Existing RELAP5 documentation is being converted to electronic form using document publishing software for technical material (Framemaker)

- Input requirements and some other text and equations have been converted.

- Intend to maintain ‘living manuals”; that is manuals will be updated as code source is updated.

Basis of Momentum Equation

- Momentum equations are written in "non-conservative" form for numerical convenience.
    1. Mass and energy conservation were considered paramount. A greater degree of approximation to momentum conservation considered acceptable
    2. Implicit evaluation used only in terms needed to exceed the sonic Courant limit
    3. Time level evaluations adjusted to make implicit terms linear

- A "conservative" form has errors of the same order as a "non-conservative" form

Basis of Momentum Equation(cont.)

- Use of “non-conservative” momentum equation along with design principles reduces the system of field equations to a Poisson equation in pressure reducing the computation time spent in matrix solution.

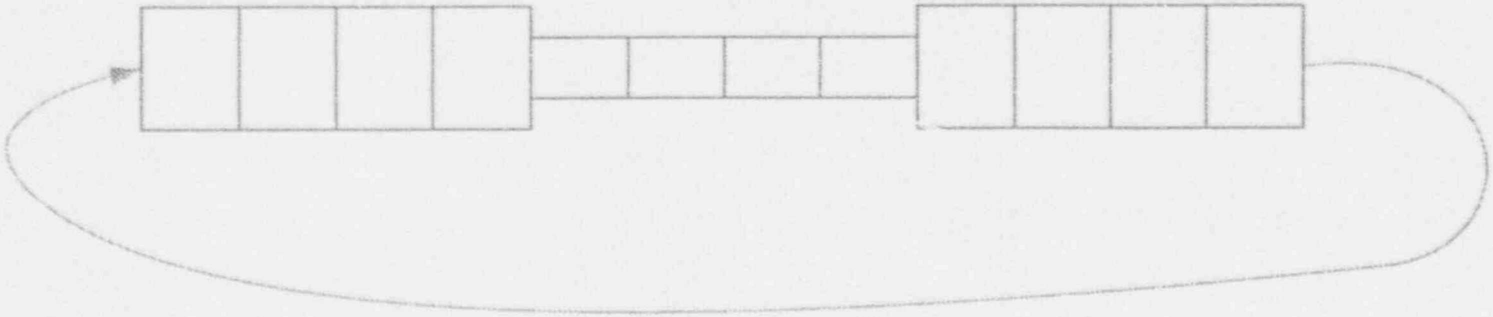
- Major concern is the modeling of the momentum flux terms in the momentum equation

Momentum Flux Modeling

- Consequences of inaccuracies in modeling of momentum flux term
  - artificial pressure losses in equations resulting in lower flow rates for a given momentum source (i.e. pump work)
  - incorrect pressure losses or recovery through area changes

- Several simple problems used to assess accuracy of momentum flux term modeling
  - frictionless loop with area changes and no momentum sources

Loop Test Case Nodalization



Horizontal Pipe

50% Area Reduction

Single Phase Liquid or Vapor

Pressure = 50 bar

Liquid Velocity = ~3 m/s

Vapor Velocity = ~100 m/s

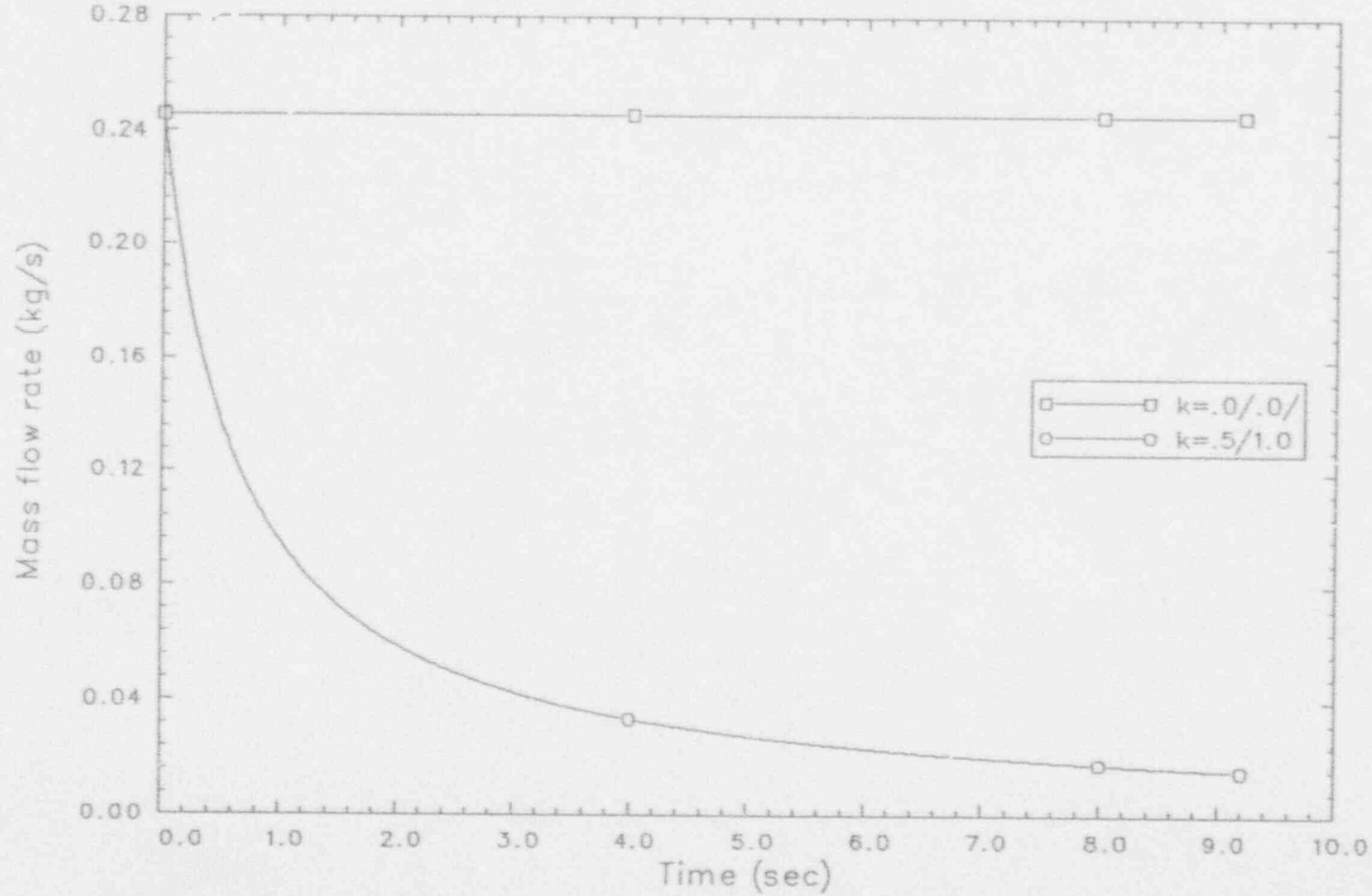
Momentum Flux Test Cases

- Single phase liquid with and without irreversible pressure losses at the area changes
  1. $K_{cont} = 0.0$, $K_{exp} = 0.0$
  2. $K_{cont} = 0.5$, $K_{exp} = 1.0$

- Single phase vapor with and without irreversible pressure losses at the area changes.
  1. $K_{cont} = 0.0$, $K_{exp} = 0.0$
  2. $K_{cont} = 0.5$, $K_{exp} = 1.0$

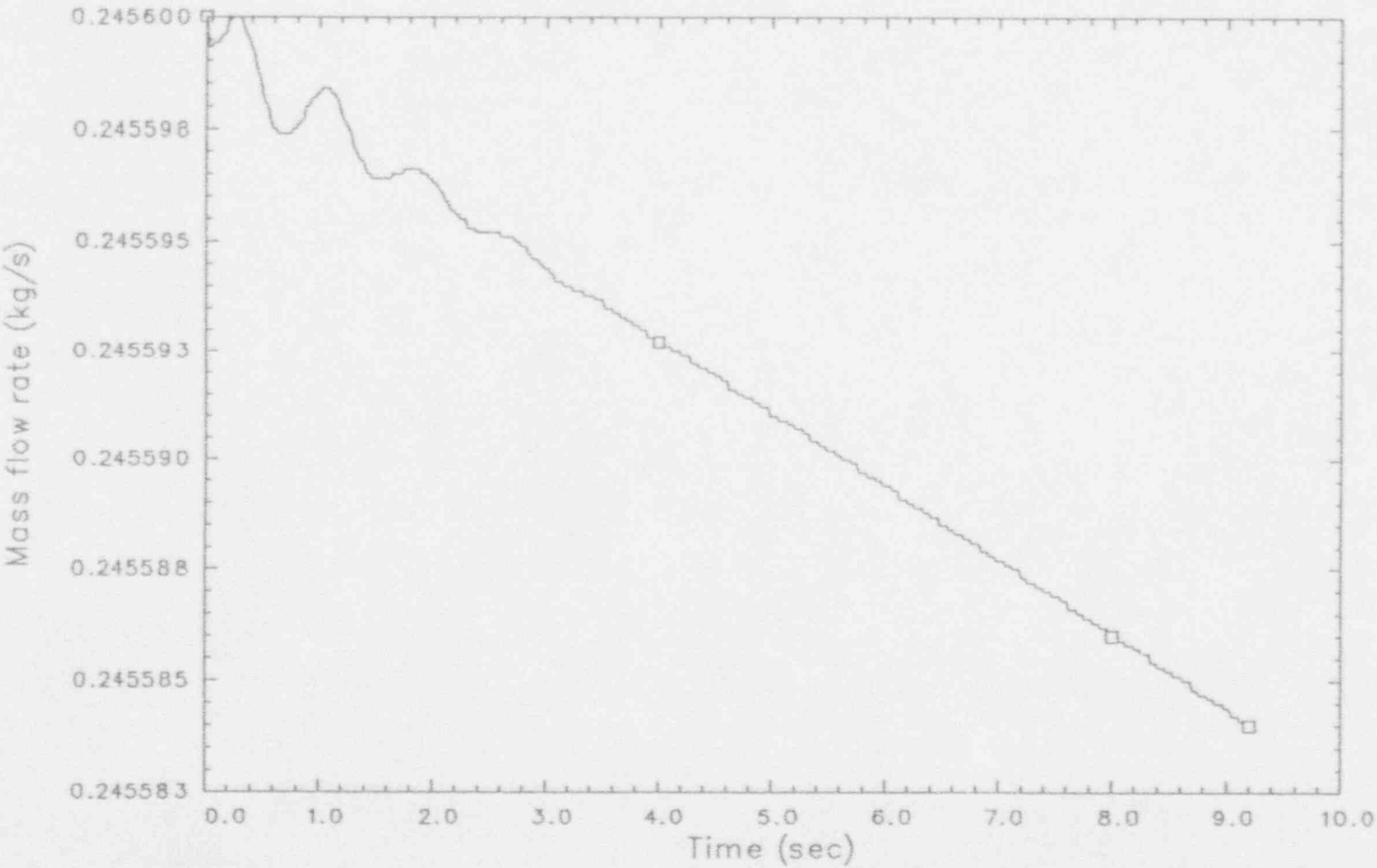
Single Phase Liquid Test Case

Insignificant numerical losses relative to physical losses



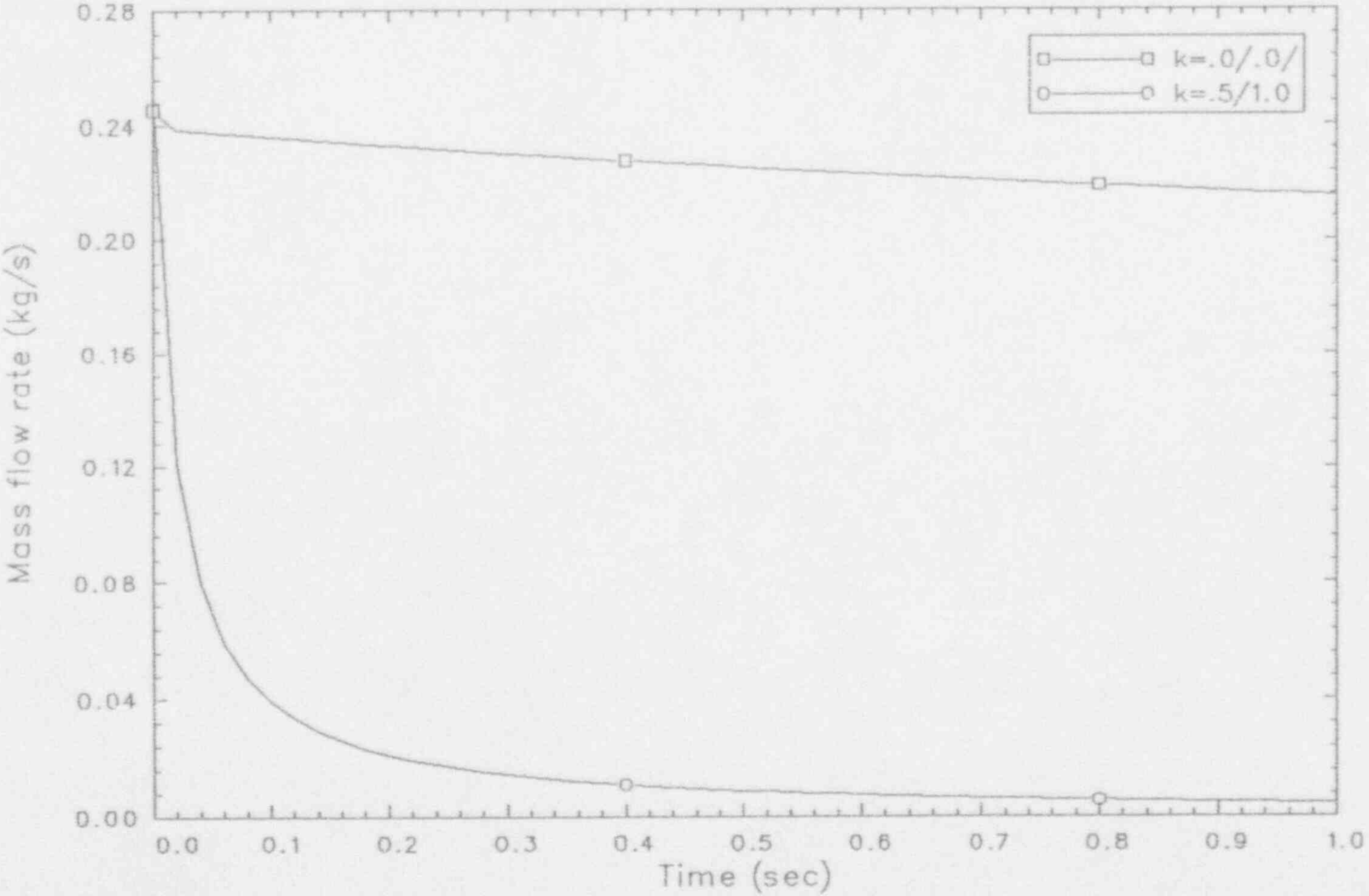
Single Phase Liquid Test Case

Numerical losses in 6th significant place for single phase liquid



Single Phase Vapor Test Case

Small numerical losses for single phase vapor relative to physical losses



Summary of Momentum Flux Test Case Results

- Summary of test results
  - Loop problem
    1. Velocity remains constant to 5 significant figures during 2000 advancements for single phase liquid with no physical losses
    2. Velocity decays rapidly when physical losses introduced for single phase liquid.
    3. Small velocity loss for single phase vapor with no physical losses.
    4. The physical loss through an area change much larger than the numerical loss for single phase vapor

Summary on Momentum Equation Modeling

- Simulation of a variety of simple "thought" problems, separate effects tests and integral tests have not uncovered any observable effects of the "non-conservative" form of the momentum equations

References for natural circulation test cases

Vol III of RELAP5/MOD3 manual reports results of LOFT L3-7 Small Break simulation with natural circulation as well as results of the simulation of the Semiscale Natural Circulation test series

P. R. McHugh and R. D. Hentzen, Natural Circulation Cooling in U.S. Pressurized Water Reactors, NUREG/CR-5769, EGG-2653, Jan 1992 shows results of simulations of BETHSY natural circulation tests as well as the Seimscale NC test series

Idaho National Engineering Laboratory

RELAP5/MOD3 Interfacial Heat and Mass Transfer Modeling

Presented by
Gary W. Johnsen

Advisory Committee on
Reactor Safeguards
Bethesda, Maryland
January 4-5, 1994

Consultants' Comments Addressed

- Modeling of interfacial area is simplistic
- Interfacial heat transfer coefficients are ad-hoc
- Mass transfer "at the wall" confusing

RELAP5 Interfacial Area Modeling uses idealizations that have proven to provide adequate results when tested using relevant experiments

Regime	Interfacial Geometry
Bubbly	Spherical bubbles, size distribution
Slug	Spherical bubbles & Taylor bubbles
Annular mist	Spherical drops & liquid film
Mist	Spherical drops
Stratified	Quiescent surface

Interfacial Heat Transfer Coefficient Modeling

- The combination of possible thermodynamic states and flow regimes requires the specification of 48 phasic heat transfer coefficients (h_{if}, h_{ig})
- For non-metastable thermodynamic states 18 of 24 heat transfer coefficients are from the literature
- Large coefficients are used for the metastable states (i.e., superheated liquid, subcooled vapor) to ensure they are transitory

Note: Volume 4 is in error. $Nu \neq 10^4$ for bubbly SHG. Rather
$h_{ig} = 10^4\ W/m^2K$, which was suggested by Bruker and Sparrow

Summary of Interfacial Areas and Heat Transfer Coefficients

Flow Type	a_{gf}	$h_{if,SCL}$	$h_{if,SHL}$	$h_{ig,SHG}$	$h_{ig,SCG}$
Bubbly	$\frac{3.6\,\alpha_{bub}}{d_b}$	Unal	Lee-Ryley Plesset-Zwick	$10^4\,f(\Delta T_{sg})$	$10^4\,f(\Delta T_{sg})$
Slug:					
Bubbles	$\frac{3.6\,\alpha_{gs}(1-\alpha_{TB})}{d_b}$	Unal	Lee-Ryley Plesset-Zwick	$10^4\,f(\Delta T_{sg})$	$10^4\,f(\Delta T_{sg})$
Taylor bubble	$\frac{4.5}{D}\,\alpha_{TB}(2.0)$	Seider-Tate	$3\text{x}10^6\,f(\Delta T_{sf})$	Lee-Ryley	$10^4\,f(\Delta T_{sg})$
Annular mist:					
Drops	$\frac{3.6\,\alpha_{fd}}{d_d}(1-\alpha_{ff})$	Brown $f(\Delta T_{sf})$	$\frac{k_f}{d_d}f(\Delta T_{sf})$	Lee-Ryley	$10^4\,f(\Delta T_{sg})$
Liquid Film	$\frac{4}{D}(1-\alpha_{ff})^{1/2}(2.5)$	Theofanous	$3\text{x}10^6$	Dittus-Boelter	$10^4\,f(\Delta T_{sg})$
Horizontal stratified	$\frac{4\sin\theta}{\pi d}$	Dittus-Boelter	Dittus-Boelter $f(\Delta T_{sf})$	$10^4\,f(\Delta T_{sg})$ Dittus-Boelter	$10^4\,f(\Delta T_{sg})$
Vertical stratified	$\frac{A_c}{V}$	McAdams	$h_{if,REG}$	McAdams	$h_{ig,REG}$

SCL = subcooled liquid; SHL = superheated liquid; SHG = superheated gas; SCG = subcooled gas;

$f(\Delta T_{sg})$ = function of $\Delta T_{sg} = T^S - T_g$; $f(\Delta T_{sf})$ = function of $\Delta T_{sf} = T^S - T_f$; REG = flow regime when not vertically stratified

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Summary of Interfacial Areas and Heat Transfer Coefficients (cont'd)

Flow Type	a_{gf}	$h_{if,SCL}$	$h_{if,SHL}$	$h_{ig,SHG}$	$h_{ig,SCG}$
Inverted annular:					
Bubbles	$\frac{3.6\,\alpha_{bub}}{d_b}(1-\alpha_g)$	Unal	Lee-Ryley Plesset-Zwick	$10^4 f(\Delta T_{sg})$	$10^4 f(\Delta T_{sg})$
Vapor film	$\frac{4}{D}(1-\alpha_B)^{1/2}(2.5)$	Dittus-Boelter	$3x10^6$	$\frac{k_g}{D} f(\Delta T_{sg})$	$\frac{k_g}{D} f(\Delta T_{sg})$
Inverted slug:					
Drops	$\frac{3.6\,\alpha_{drp}}{d_d}(1-\alpha_B)$	Brown $f(\Delta T_{sf})$	$\frac{k_f}{D} f(\Delta T_{sf})$	Lee-Ryley	Lee-Ryley
Taylor drop	$\frac{4.5}{D}(\alpha_B)(2.5)$	Brown $f(\Delta T_{sf})$	$\frac{k_f}{D} f(\Delta T_{sf})$	$\frac{k_g}{D} f(\Delta T_{sg})$	$\frac{k_g}{D} f(\Delta T_{sg})$
Mist (dispersed)	$\frac{3.6\,\alpha_{drp}}{d_d}$	Brown $f(\Delta T_{sf})$	$\frac{k_f}{D} f(\Delta T_{sf})$	Lee-Ryley $f(\Delta T_{sg})$	$10^4 f(\Delta T_{sg})$

SCL = subcooled liquid; SHL = superheated liquid; SHG = superheated gas; SCG = subcooled gas;
$f(\Delta T_{sg})$ = function of $\Delta T_{sg} = T^S - T_g$; $f(\Delta T_{sf})$ = function of $\Delta T_{sf} = T^S - T_f$; REG = flow regime when not vertically stratified

Experiments Used to Validate RELAP5 Interphase Heat and Mass Transfer Models

Experiment	Model Assessed
Edwards Pipe Blowdown	Vapor generation model in the bulk, H_{if} superheated liquid in bubbly flow
Christensen Subcooled Boiling Test 15	Vapor generation model near the wall, condensation model in the bulk, H_{if} for subcooled liquid in bubbly flow
MIT Pressurizer Test ST4	Wall condensation model near the wall, condensation model in the bulk, H_{if} for subcooled liquid in vertical stratified flow
ORNL THTF Void Profile Test 3.09.10i	Vapor generation model in flow regimes for void fraction 0 $\longrightarrow$ 1

Experiments Used to Validate RELAP5 Interphase Heat and Mass Transfer Models (cont'd)

Experiment	Model Assessed
ORNL Bundle CHF Tests 3.07.9B, 3.07.9N, 3.07.9W	Vapor generation model in flow regimes for void fraction 0 → 1
Swedish Royal Institute of Technology Heated Tube CHF Test 261	Vapor generation model in flow regimes for void fraction 0 → 1
Bennett's Heated Tube CHF Tests 5358, 5294, and 5394	Vapor generation model in flow regimes for void fraction 0 → 1
Bankoff Cocurrent Flow Condensation Tests 253 and 279	Condensation model in the bulk, H_{if} for subcooled liquid in horizontal stratified flow
Aoki Steam-Water Mixing Experiment	Condensation model in the bulk, H_{if} for subcooled liquid in mist (dispersed) flow

Experiments Used to Validate RELAP5 Interphase Heat and Mass Transfer Models (cont'd)

Experiment	Model Assessed
Chen Heated Tube Post CHF Tests 174-93 and 318-84	Mass transfer model at high void fraction
Neptunus Pressurizer Test Y05	Condensation model in the bulk, H_{if} for subcooled liquid in vertical stratified flow
Semiscale Pressurizer Insurge Test	Condensation model in the bulk, H_{if} for subcooled liquid in vertical stratified flow

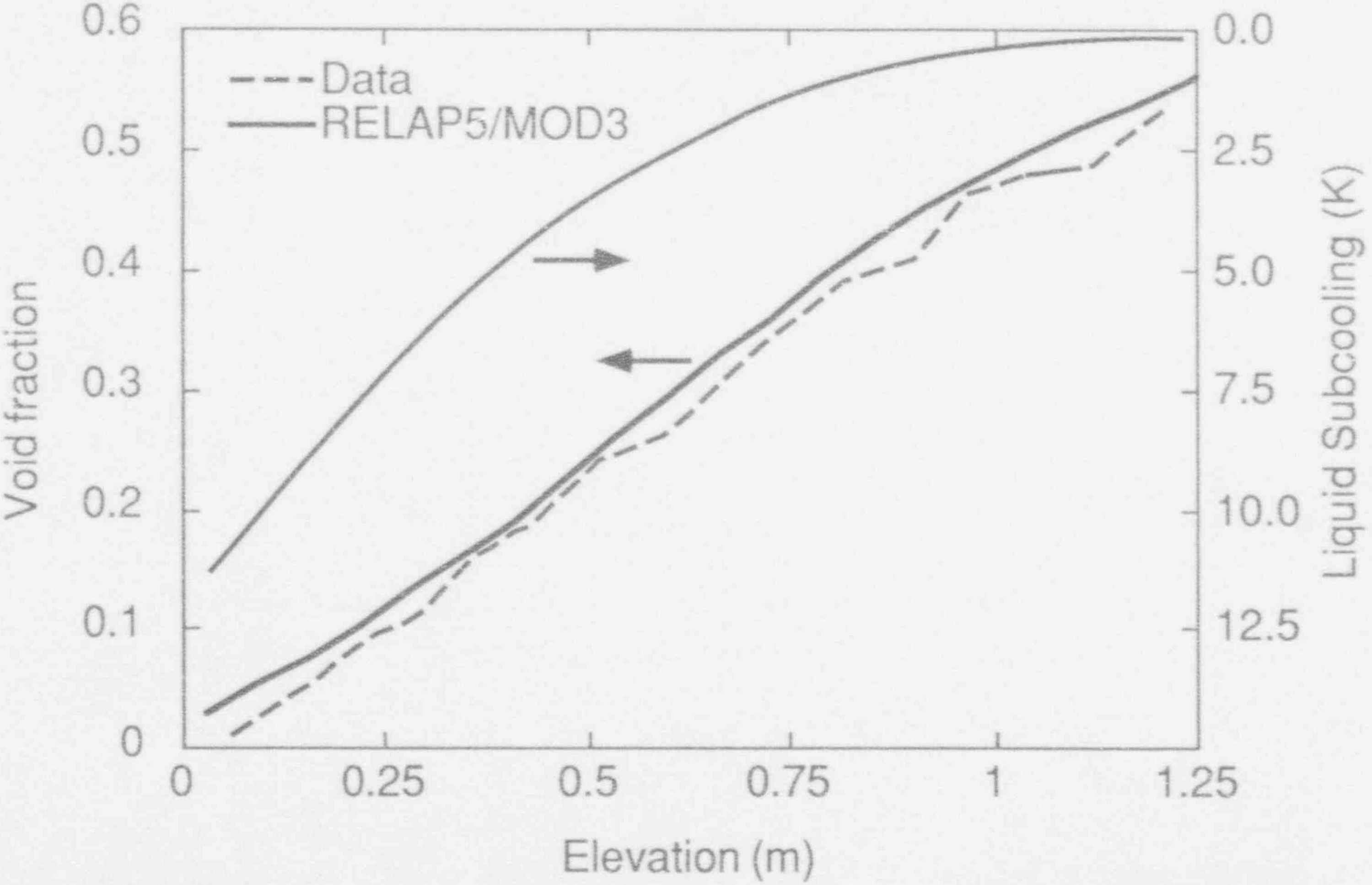
Christensen Subcooled Boiling Experiment 15

Objective: Assess interphase mass transfer and subcooled nucleate boiling model

Description:

- Pressure = 5.512×10^6 Pa
- Power = 70 KW
- Flow Rate = 1.15 m/s
- Inlet subcooling = 12.5 K

Measured and RELAP5/MOD3 – Calculated Axial Void Fraction for the Christensen Subcooled Boiling Test 15



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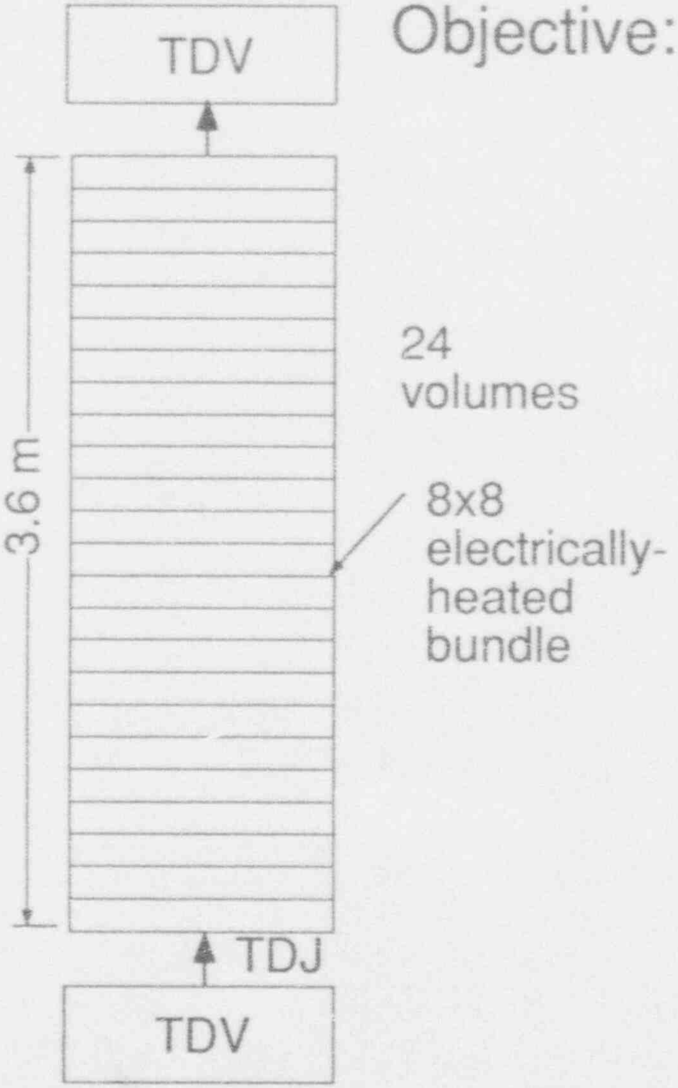
ORNL THTF Void Profile Test



Objective: Subcooled boiling, void generation, void propagation

Test Conditions

- Pressure = 4.5 MPa
- Heat flux = 0.38 MW/m^2
- Mass flux = 29.8 $Kg/s-m^2$
- Inlet subcooling = 57.6 K°

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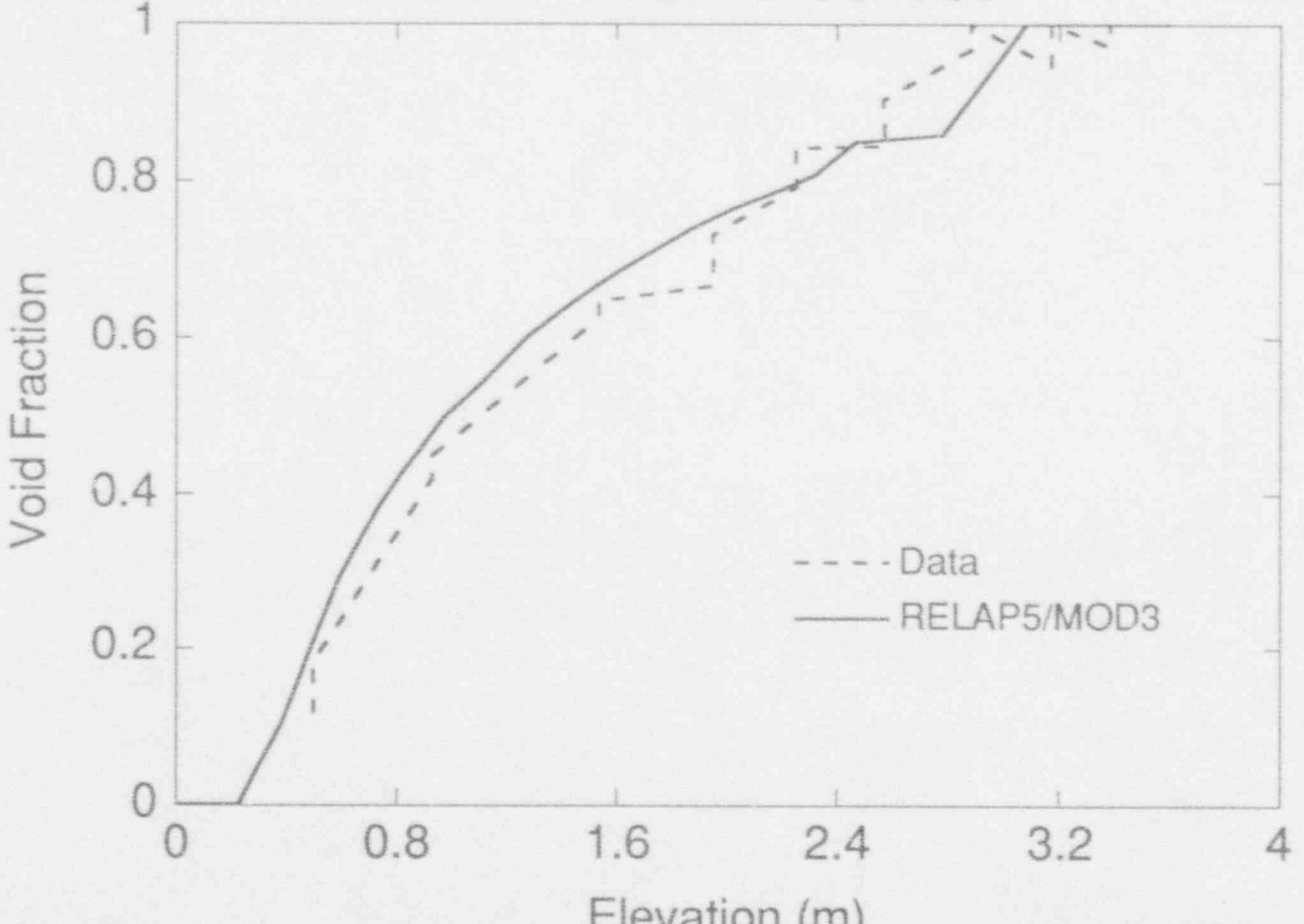
Measured and Calculated Void Fraction
THTF Test 3.09.10i



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Bankoff Cocurrent Steam/Water Mixing Experiment

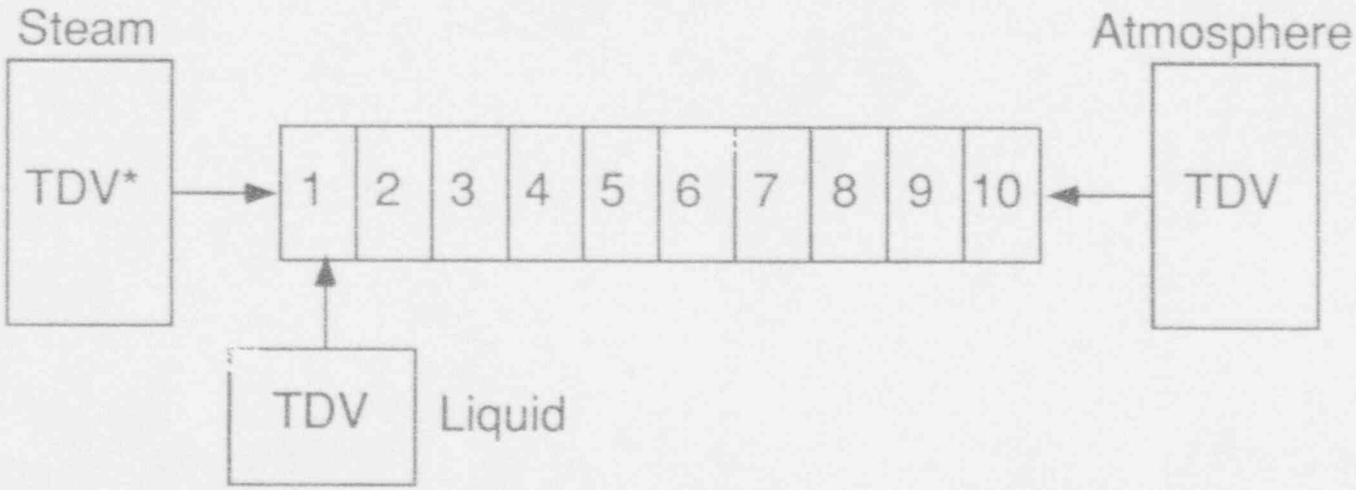
Objective: Assess condensation for stratified flow

Description:

- Atmospheric pressure
- Rectangular horizontal test section (6.4 cm x 30.5 cm x 160 cm)
- Liquid and steam flow at entrance of test section

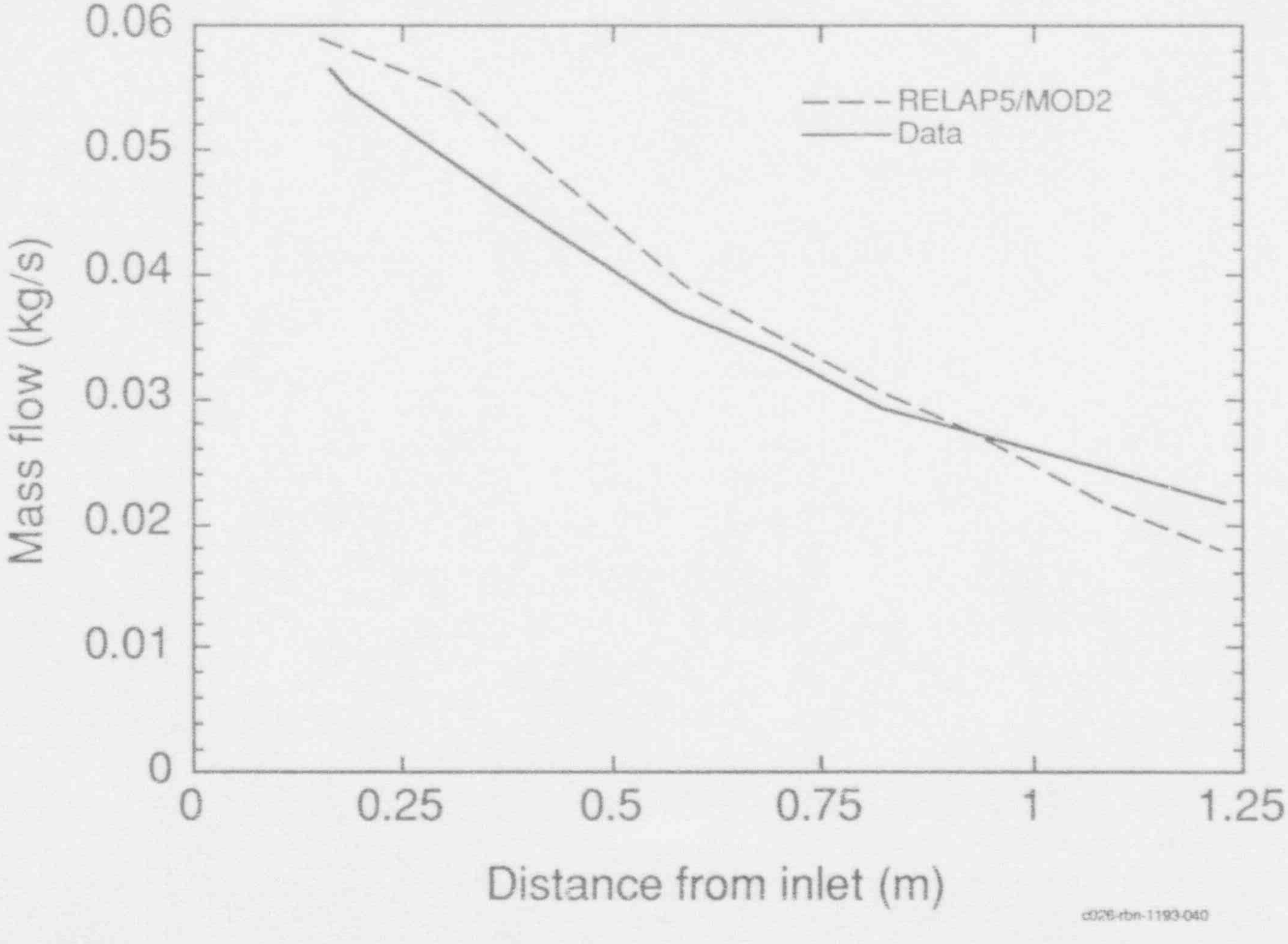
	Run 253	Run 279
Liquid		
Flow (kg/s)	0.657	1.04
Temp (K)	295	298
Steam		
Flow (kg/s)	0.0651	0.160
Temp (K)	411	415

RELAP5/MOD2 Nodalization for Bankoff Experiment

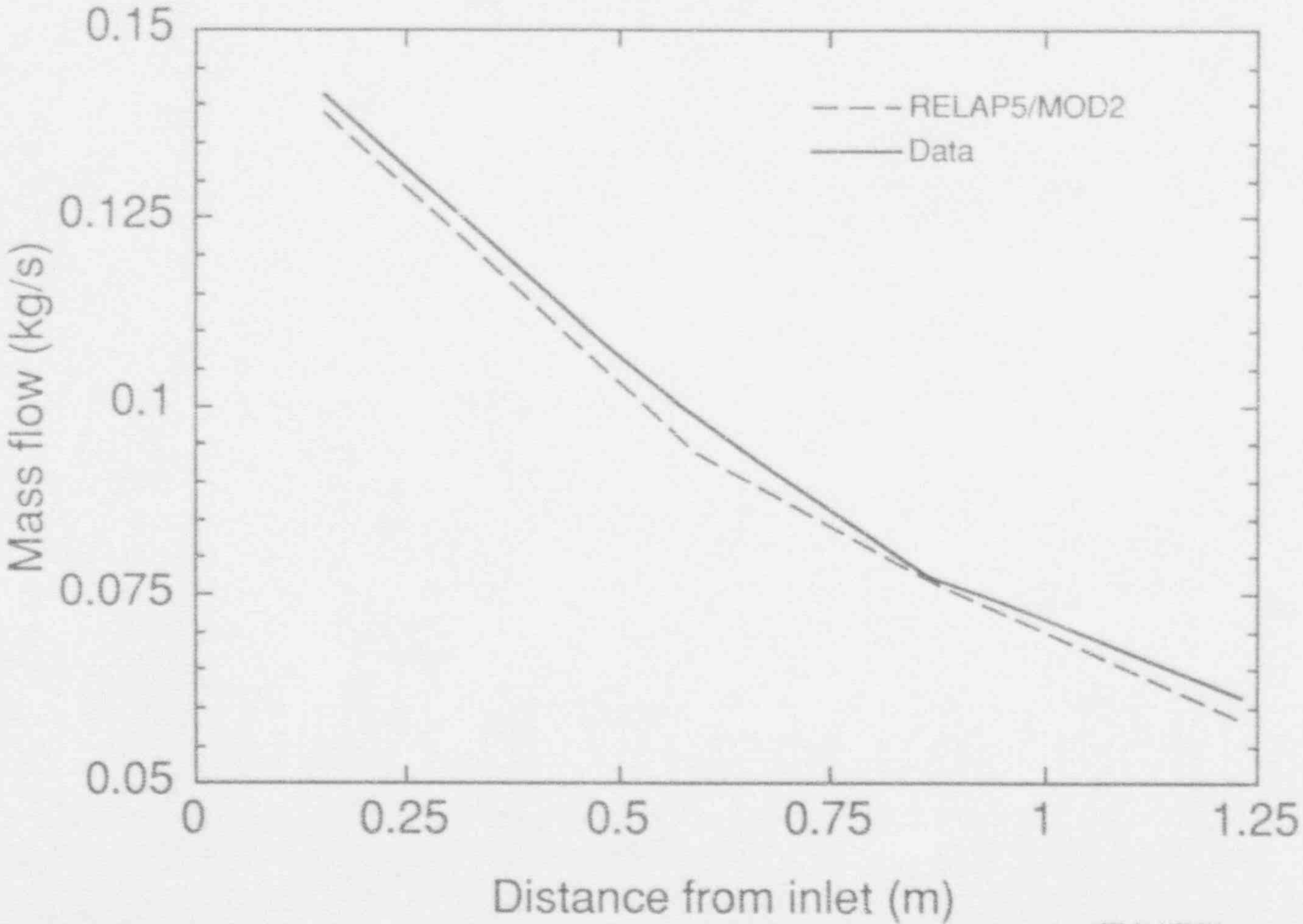


*TDV = Time Dependent Volume

Calculated and Measured Axial Steam Flow Profile, Bankoff Run 253



Calculated and Measured Axial Steam Flow Profile, Bankoff Run 279



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Measured and Predicted Liquid Outlet Temperatures Bankoff Cocurrent Flow Experiments

	Outlet Temperature (K)	
Test Run	Data	RELAP5/MOD2
253	342	337
279	358	357

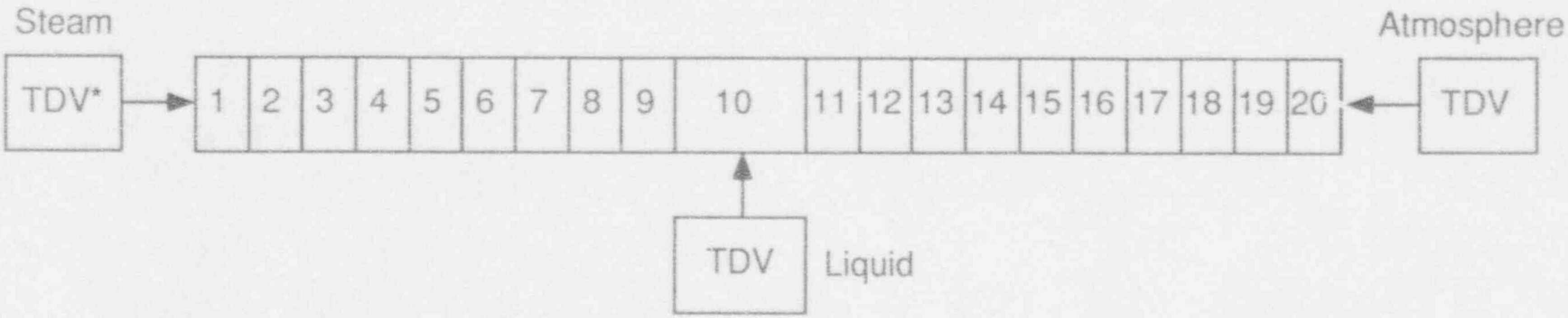
Aoki Steam/Water Mixing Experiment

Objective: Assess condensation model for dispersed flow

Description:

- Atmospheric pressure
- Rectangular horizontal test section (2.86 cm x 60 m)
- Steam flow at entrance to test section (60 kg/s-m^2 at 373 K)
- Liquid flow at center of test section (330 kg/s-m^2 at 293 K)

RELAP5/MOD2 Nodalization for Aoki Experiment


Steam

TDV*

1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20

Atmosphere

TDV

TDV

Liquid

*TDV = Time Dependent Volume

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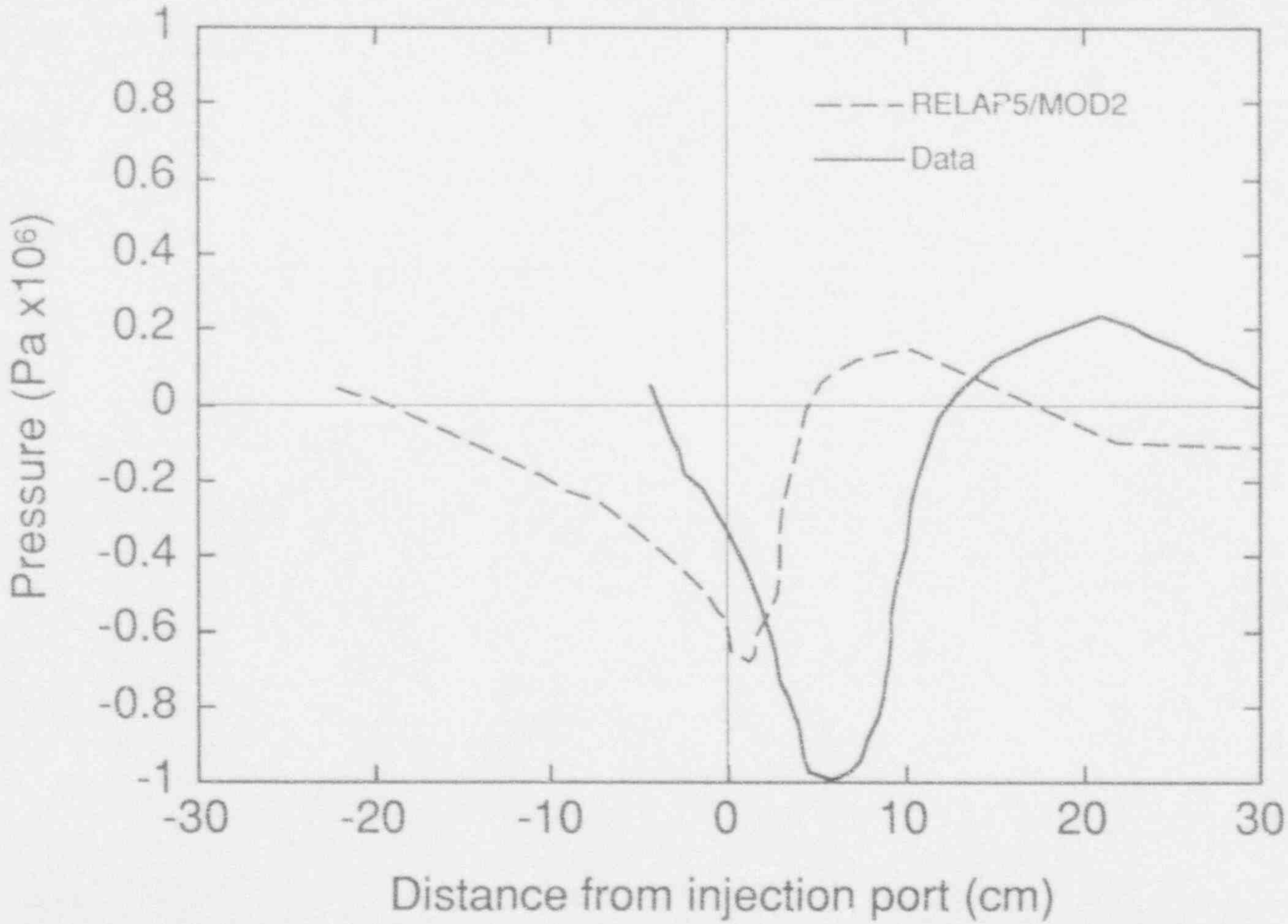
Calculated and Measured Axial Pressure Profiles for Aoki Experiment



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Vapor Generation Model

- The mass transfer model is of the form

$$\Gamma_g = \Gamma_{ig} + \sum_j \Gamma_{wj}$$

- The term Γ_{ig} is the mass transfer at the interface in the bulk (away from the wall) and is given by

$$\Gamma_{ig} = -\frac{H_{if}(T^s - T_f) + H_{ig}(T^s - T_g)}{h_f^* - h_g^*}$$

- where h_f^* and h_g^* are bulk or saturation enthalpies (discussed in Volume 1) and H_{if} and H_{ig} are interfacial heat transfer coefficients (discussed in Volume 4)

Vapor Generation Model (cont'd)

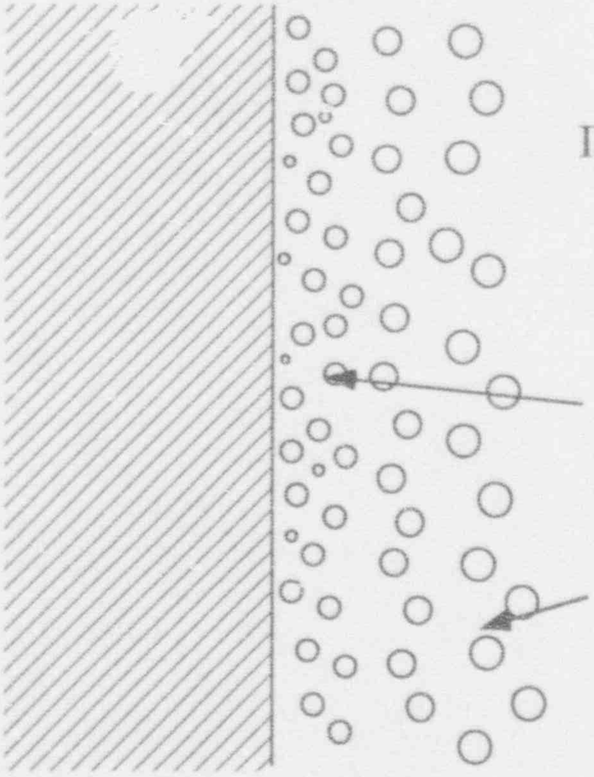
- The term Γ_{wj} is the mass transfer at the liquid-vapor interface near the wall of heat structure j attached to the volume

- Because RELAP5 has just one liquid temperature in a volume and does not calculate thermal gradients in the wall boundary layer, another model must be used for Γ_{wj}

- For subcooled boiling, the bulk liquid can be subcooled while water in the boundary layer is warmer and is flashing to steam, resulting in net vapor generation

Subcooled Boiling Mass Transfer

(Net Vapor Generation)



$\Gamma_g = \Gamma_{ig} + \Gamma_{wj} > 0$

$I_{wj} > 0$
boiling near
the wall

$\Gamma_{ig} < 0$
condensation
in the bulk

Wall j

Vapor Generation Model (cont'd)

- To capture this effect, the mechanistic method proposed by Lahey is used for Γ_{wj}
- These words are currently in Volume 1 and 4 in the constitutive relations section, and similar words will be added to the field equation section when Γ_{wj} is first introduced.

Summary

- RELAP5's constitutive modeling includes idealizations and engineering approximations where further sophistication is either not possible or impractical
- Validation using relevant data is the essential element for acceptability

FILM-SIDE HEAT TRANSFER RESISTANCES

- FALLING FILMS
  - Laminar Smooth
  - Laminar Wavy
  - Turbulent

- SHEARED FILMS: Cocurrent Downflow
  - Laminar Smooth
  - Laminar Wavy
  - Turbulent

- MODEL SUMMARY

- ASSESSMENT PLANS

FALLING FILMS: LAMINAR SMOOTH

- NUSSELT MODEL ASSUMPTIONS:

1) Fully Developed, Steady, Laminar Flow: Flat plate
2) Constant Properties: $\frac{1}{2}\left(T_W + T_{sat}\right)$
3) Subcooling of Liquid Negligible in Energy Balance
4) Inertia Effects Negligible in Momentum Balance
5) Interfacial Shear Stress Negligible
6) Liquid-Vapor Interface is Smooth (No Ripples)
7) Heat Transfer Across Film is by Conduction
8) Wall Temperature is Uniform Axially
9) Saturated Vapor with X = 1 at Inlet
10) Condensation is only Limited by Liquid Resistance

FALLING FILMS: LAMINAR SMOOTH

* NUSSELT MODEL :

- Film Thickness

$$m^* = \left(\frac{3}{4} \cdot \mathrm{Re}_f\right)^{\frac{1}{3}}$$

where

$$m^* = m \cdot \left(\frac{\rho_f \cdot \Delta\rho \cdot g}{\mu_f^2}\right)^{\frac{1}{3}}$$

$$\mathrm{Re}_f = \frac{4\Gamma'}{\mu_f}$$

- Local Nusselt No.

$$Nu^* = \left(\frac{3}{4} \cdot \mathrm{Re}_f\right)^{-\frac{1}{3}} = \frac{1}{m^*}$$

where

$$Nu^* = \frac{h}{k_f} \cdot \left(\frac{\mu_f^2}{\rho_f \cdot \Delta\rho \cdot g}\right)^{\frac{1}{3}}$$

FALLING FILMS: LAMINAR SMOOTH

- NUSSELT MODEL :

  - Average Nusselt No.

$$\left\langle Nu^{*} \right\rangle = \frac{1}{L} \cdot \int_{0}^{L} Nu^{*} \cdot dz$$

$$\left\langle Nu^{*} \right\rangle = \frac{4}{3} \cdot \left(\frac{3}{4} \cdot \mathrm{Re}_{f}(L) \right)^{-\frac{1}{3}}$$

$$\left\langle Nu^{*} \right\rangle = 0.943 \cdot \left\{ \frac{h_{lv} \cdot \rho_{f} \cdot \Delta\rho \cdot g \cdot L^{3}}{k_{f} \cdot \mu_{f} \cdot (T_{i} - T_{w})} \right\}^{\frac{1}{4}}$$

FALLING FILMS: LAMINAR SMOOTH

* NUSSELT TYPE ANALYSIS :

- Condensation Heat Transfer Coefficient:

$$h_{cond} = \frac{k_f}{m}$$

$$q_w = \frac{k_f}{m} \cdot (T_w - T_i)$$

- Bulk Fluid Temperature:
  - Parabolic velocity profile with linear temperature profile yields

$$T_f = \frac{5}{8} \cdot T_i + \frac{3}{8} \cdot T_w$$

- Wall-Fluid Heat Transfer

$$R_{wl} = R_{film} \cdot \left[\frac{T_f - T_w}{T_i - T_w} \right] = \frac{5}{8} \cdot R_{film}$$

$$h_{wl} = \frac{8}{5} \cdot h_{cond} = \frac{8}{5} \cdot \frac{k_f}{m}$$

$$Nu_w = 1.8$$

FALLING FILMS: LAMINAR SMOOTH

* NUSSELT TYPE ANALYSIS :

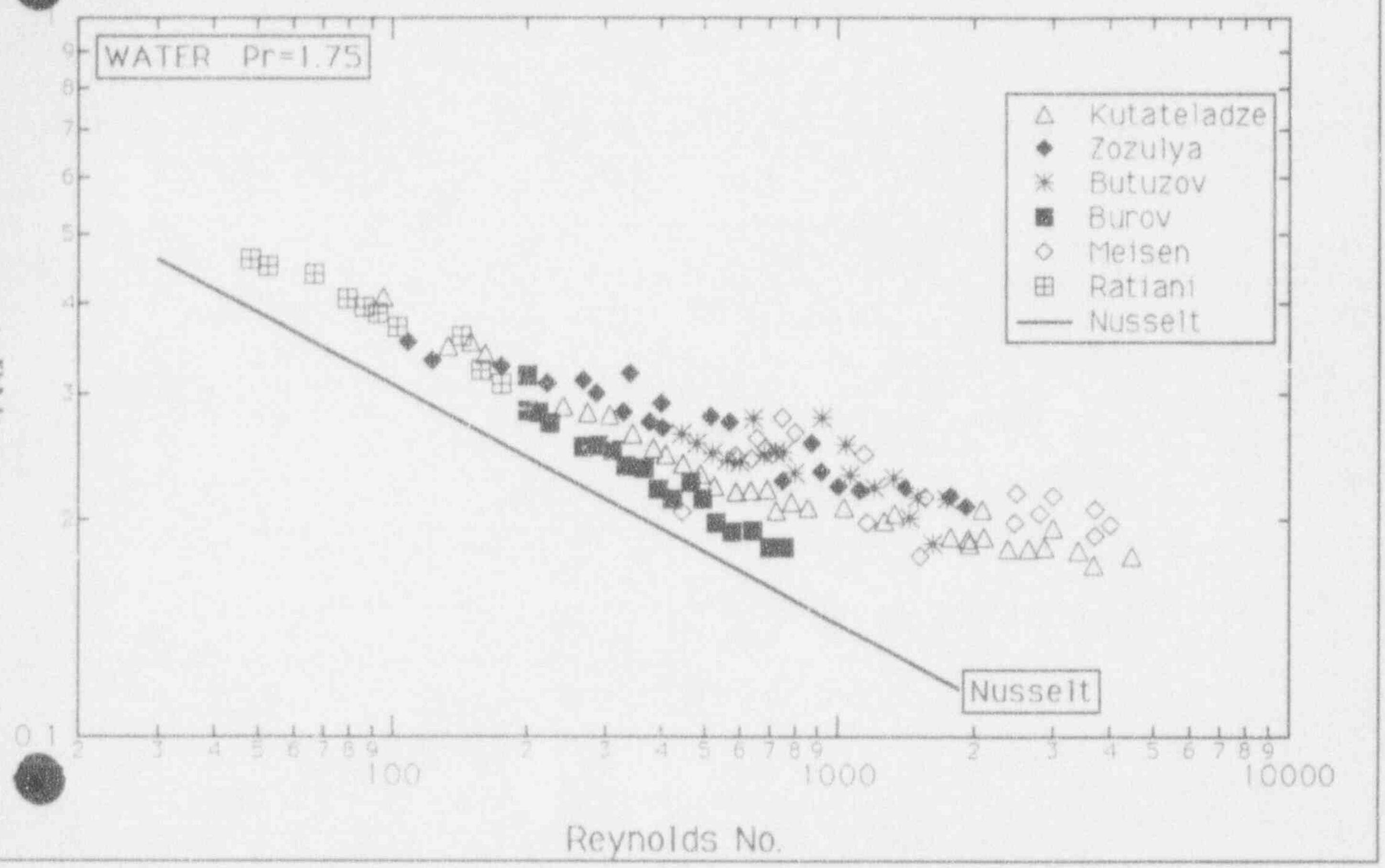
- Interfacial Heat Transfer: Liquid-Interface

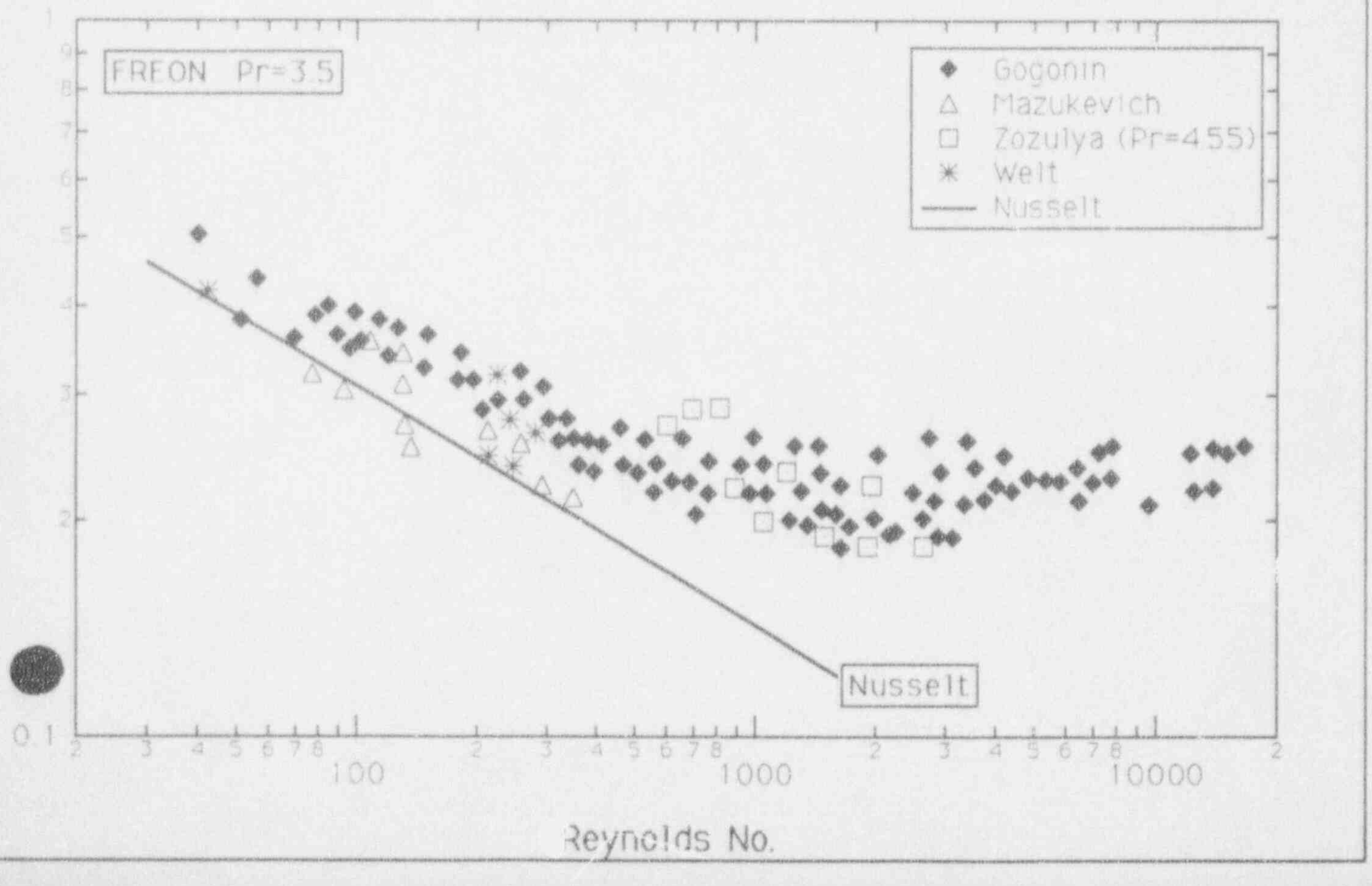
$$R_{li} = \frac{3}{8} \cdot R_{film}$$

$$h_{li} = \frac{8}{3} \cdot h_{cond} = \frac{8}{3} \cdot \frac{k_l}{m}$$

$$Nu_{li} = 2\tfrac{2}{3}$$

FALLING FILM: CONDENSATION



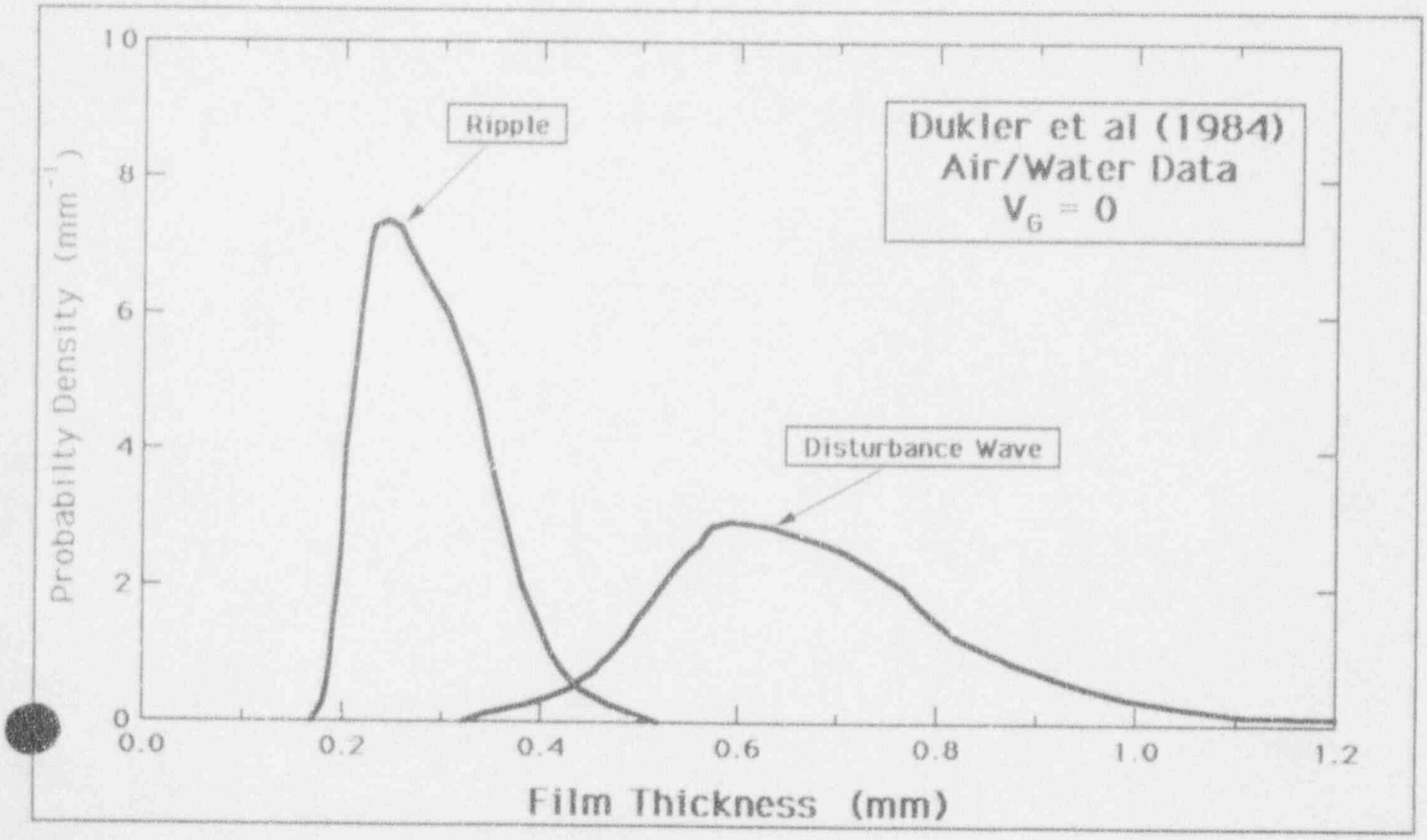


FALLING FILMS: LAMINAR WAVY

- EXPECTED BEHAVIOR
  - RIPPLE WAVES:
    - Reduce Film Thickness
    - Induce Velocities Normal to Interface
    - Recirculation under Wave Crest?
  - DISTURBANCE WAVES:
    - Kinematic: significant fraction of fluid can be transported by waves, thereby reducing substrate.
    - Mixes Film under Wave Crest: a 'developing length' for each wavelength.

WAVE CHARACTERISTICS

	Ripple Wave	Disturbance Wave
Film Height (mm)	0.280	0.722
Avg. Amplitude (mm)	0.054	0.290
1/<m>	3.571	1.385
<1/m>	3.858	1.535
Enhancement	1.08	1.11



FALLING FILM: LAMINAR WAVY

- ENHANCEMENT DUE TO RIPPLING

  - Reduced Film Thickness (Kapitsa, 1948)

$$m^{*} = \left[\frac{2.4}{4}\cdot \mathrm{Re}_{f}\right]^{\frac{1}{3}}$$

$$\frac{m_{Ka}}{m_{Nu}} = 0.9283$$

  - Effect of Averaging Inverse of Thickness

$$\left\langle \frac{1}{m} \right\rangle_{wavy} \approx 1.08 \cdot \frac{1}{\langle m \rangle}$$

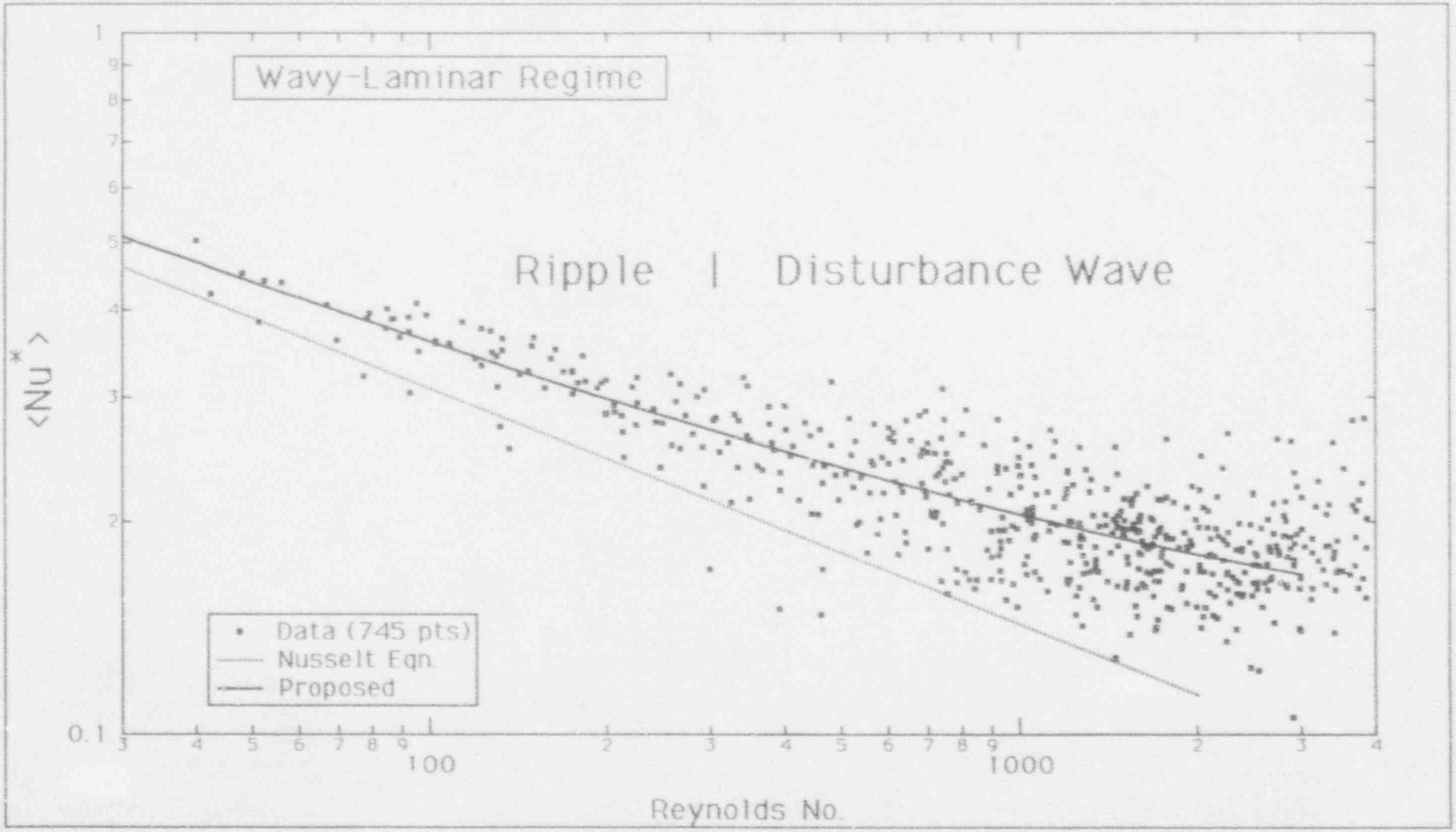
  - Enhancement Relative to Nusselt

$$\left\langle \frac{1}{m} \right\rangle_{wavy} \approx 1.08 \cdot \frac{1}{\langle m \rangle_{Ka}}$$

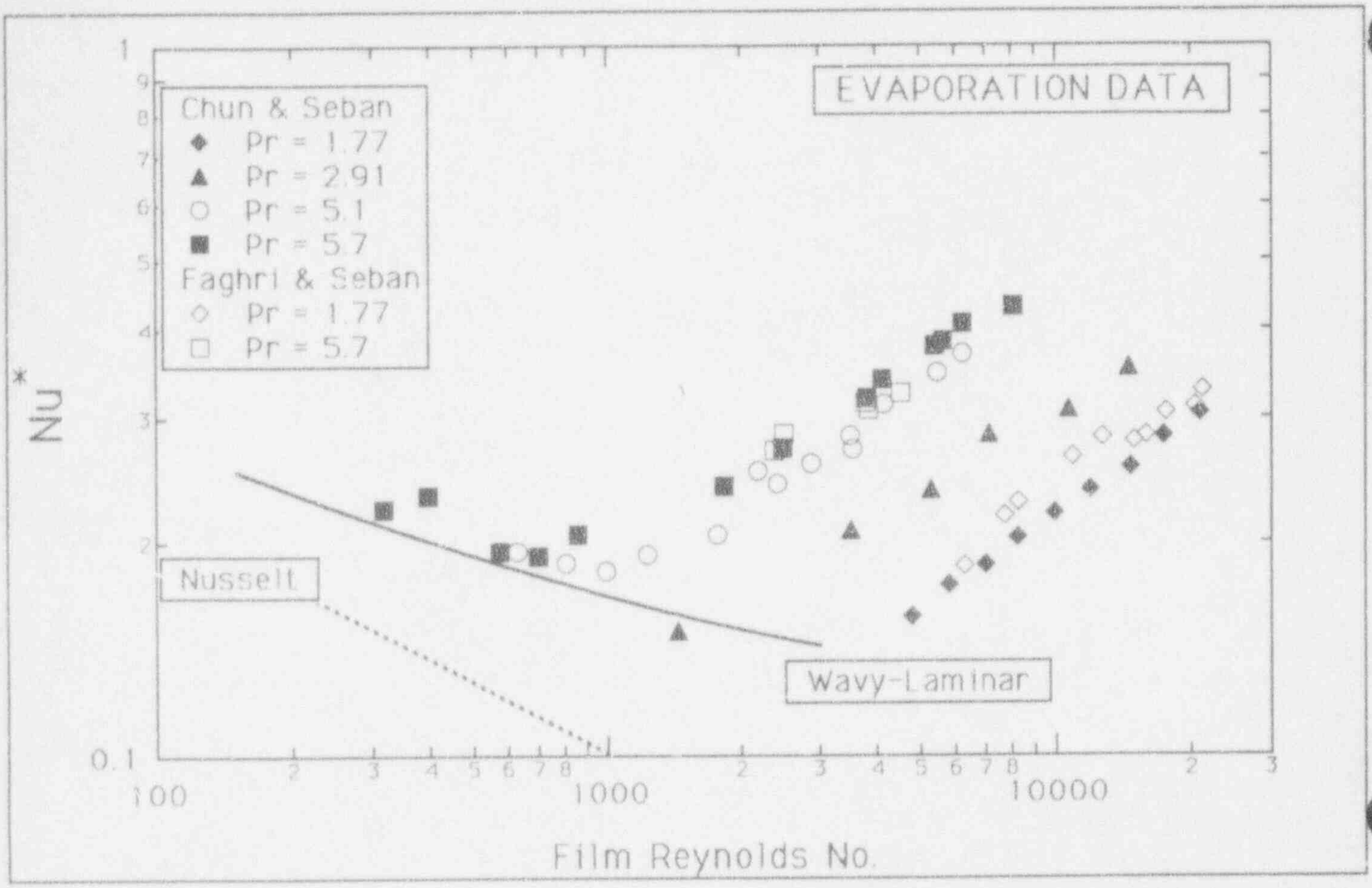
$$\left\langle \frac{1}{m} \right\rangle_{wavy} \approx 1.16 \cdot \frac{1}{\langle m \rangle_{Nu}}$$

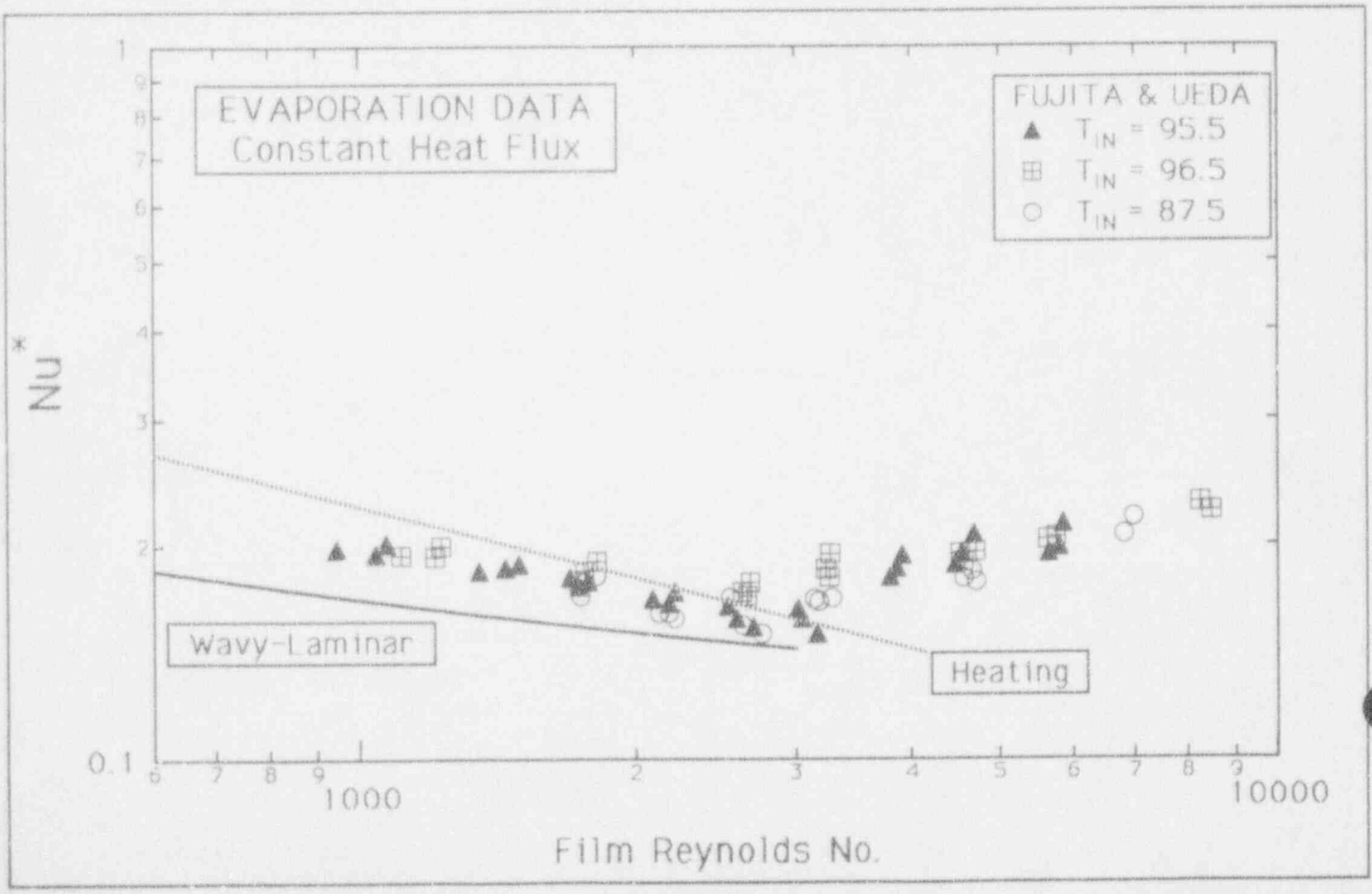
Wavy-Laminar: Proposed Correlation

$$\left\langle Nu^{*}\right\rangle_{wavy} = \left\langle Nu^{*}\right\rangle_{Nu} \cdot \left(1 + 0.0164 \cdot \mathrm{Re}_{f}^{.46}\right)$$

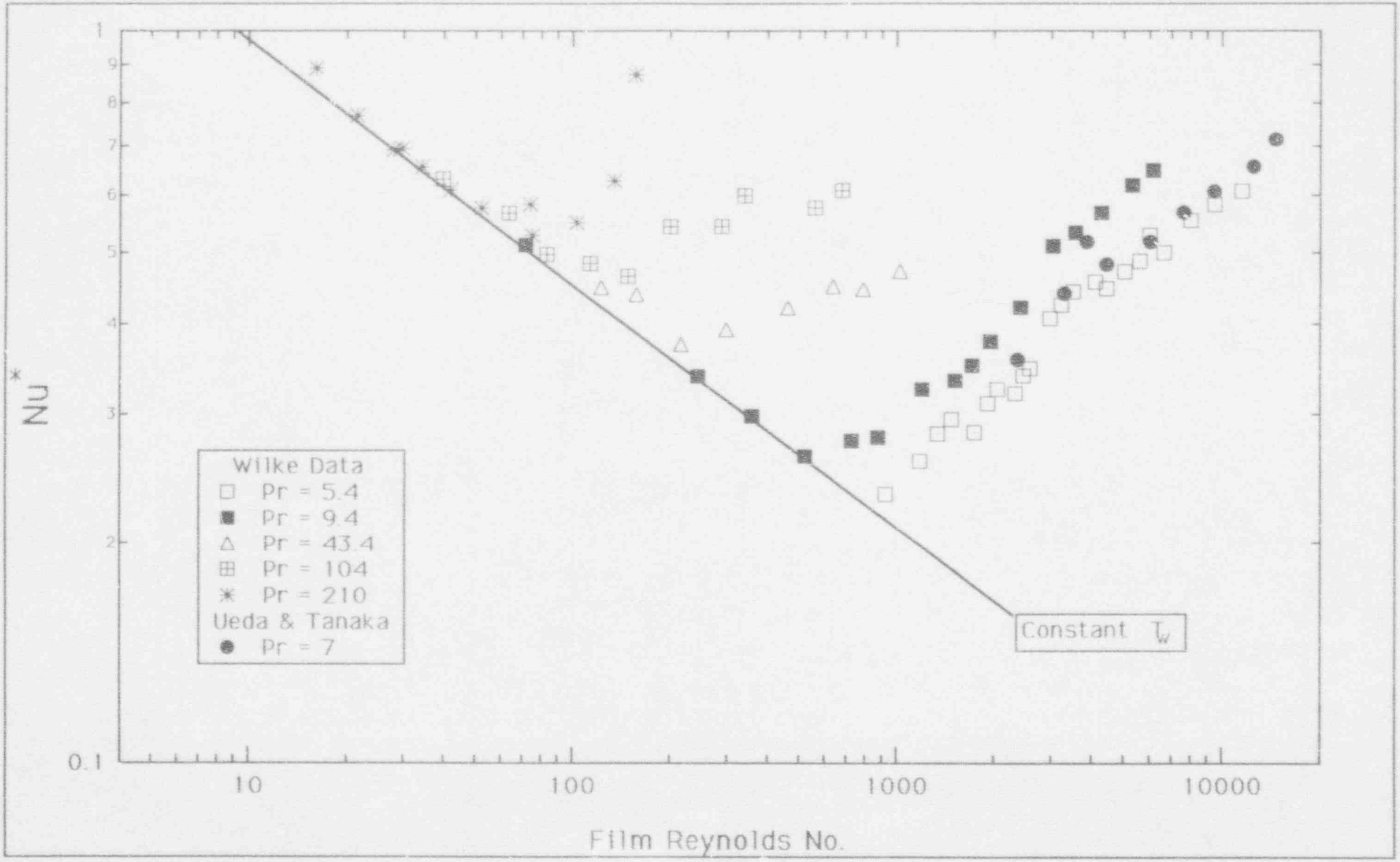


FALLING FILM: EVAPORATION





FALLING FILM: Heating Data



FALLING FILMS: LAMINAR WAVY

- **Condensation Heat Transfer Coefficient**

$$Nu^* = 1.10 \cdot \left(\mathrm{Re}_f\right)^{-\frac{1}{3}} \cdot \left(1 + 0.022 \cdot \mathrm{Re}_f^{46}\right)$$

$$h_{cond} = \frac{k_f}{m} \cdot \left(1 + 0.022 \cdot \mathrm{Re}_f^{46}\right)$$

- **Wall-Fluid Heat Transfer**
  - No effect of waves evident in heating data
  - Use asymptotic Nusselt No. for film heating/cooling with constant wall temperature b.c.

$$Nu = 1.9$$

$$Nu^* = 2.09 \cdot \left(\mathrm{Re}_f\right)^{-\frac{1}{3}}$$

$$h_{wl} = 1.9 \cdot \frac{k_f}{m}$$

FALLING FILMS: LAMINAR WAVY

- Interfacial Heat Transfer: Fluid-Interface

$$R_{li} = \frac{1}{h_{cond}} - \frac{1}{h_{wl}}$$

$$R_{li} = \frac{m}{k_l} \cdot \left(\frac{1}{Enh} - \frac{1}{1.9} \right)$$

$$h_{li} = \frac{k_l}{m} \cdot \left(\frac{1.9 - Enh}{1.9 \cdot Enh} \right)$$

where,

$$Enh = \left(1 + 0.022 \cdot \mathrm{Re}_l^{46} \right)$$

FALLING FILMS: Turbulent

* EXPECTED BEHAVIOR

- Transition expected to begin: $1000 < Re_f < 2000$

- Transition region expected to persist to much higher Re_f than for pipe flow:
  - Film thickness must be greater than that of buffer layers (wall & interface).

- Primary resistance to heat transfer is due to 'laminar' sublayers at wall & interface.

- At high Re_f, dependence on Re_f should tend to 0.8 power.

- Prandtl No. dependence should be between 0.4 (wall cooling) and 1/2 (interface mass transfer).

FALLING FILMS: Turbulent

- CONDENSATION HTC'S:
  - Grigull (1942):

$$\left\langle Nu^{*} \right\rangle = 0.0131 \cdot \mathrm{Re}_{f}^{\frac{1}{3}}$$

  - Colburn (1933):

$$\left\langle Nu^{*} \right\rangle = 0.056 \cdot \mathrm{Re}_{f}^{0.2} \cdot \mathrm{Pr}^{\frac{1}{3}}$$

  - Kirkbride (1933):

$$\left\langle Nu^{*} \right\rangle = 0.0077 \cdot \mathrm{Re}_{f}^{0.4}$$

  - Soliman et al. (1968):

$$Nu^{*} = 0.0132 \cdot \mathrm{Re}_{f}^{292} \cdot \mathrm{Pr}^{65}$$

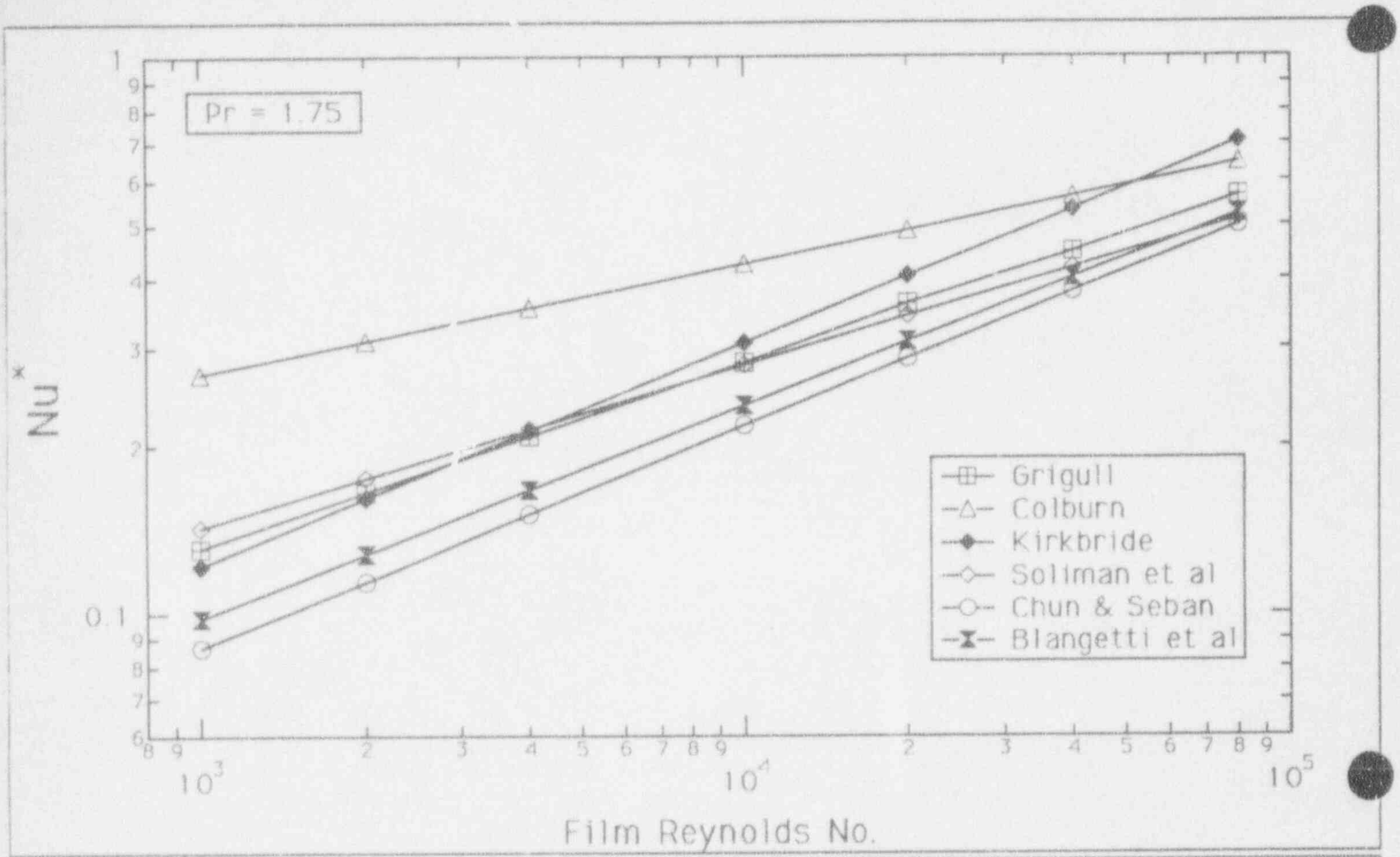
  - Blangetti et al. (1982):

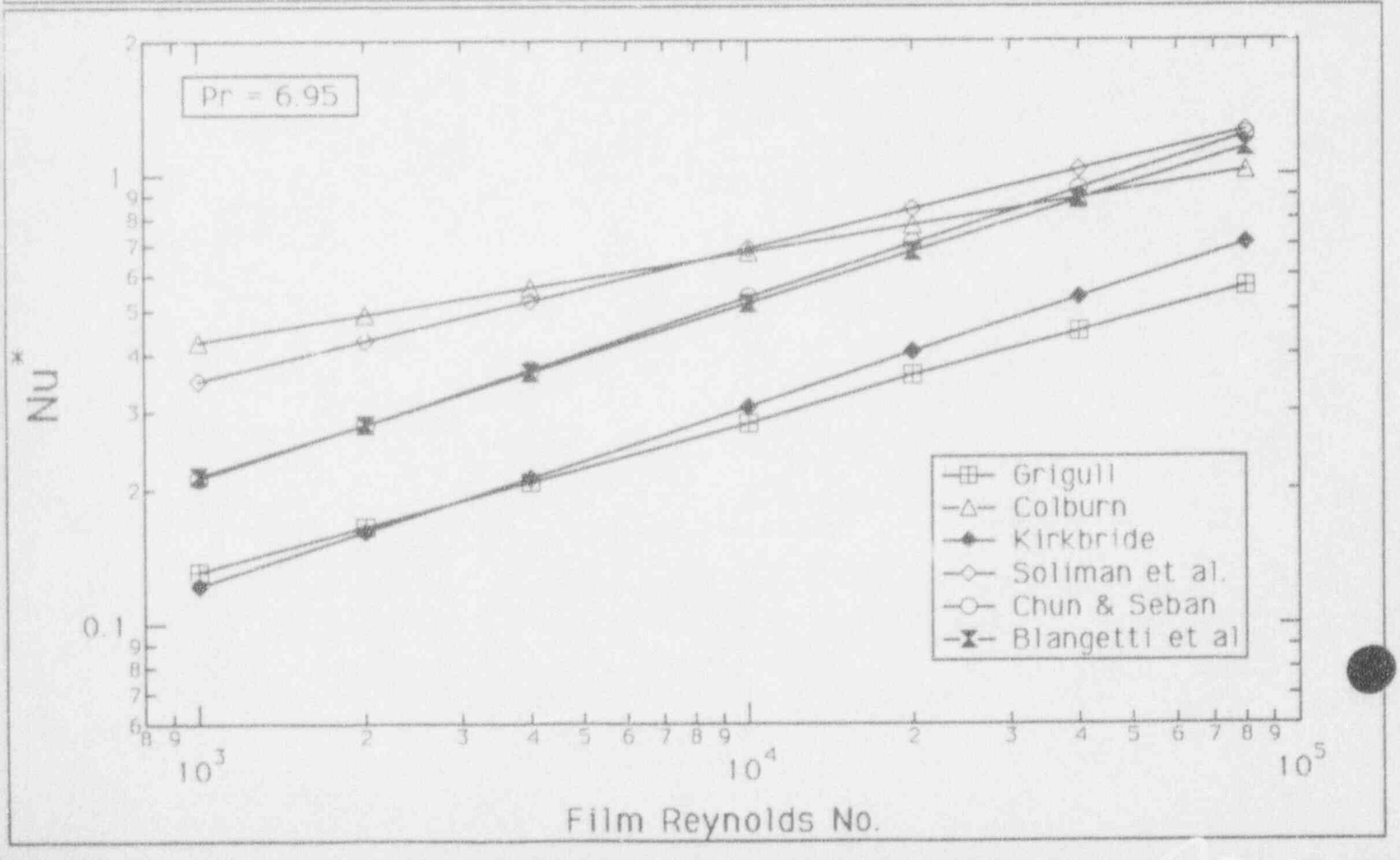
$$Nu^{*} = 0.0051 \cdot \mathrm{Re}_{f}^{382} \cdot \mathrm{Pr}^{5689}$$

- EVAPORATION HTC'S
  - Chun & Seban (1971):

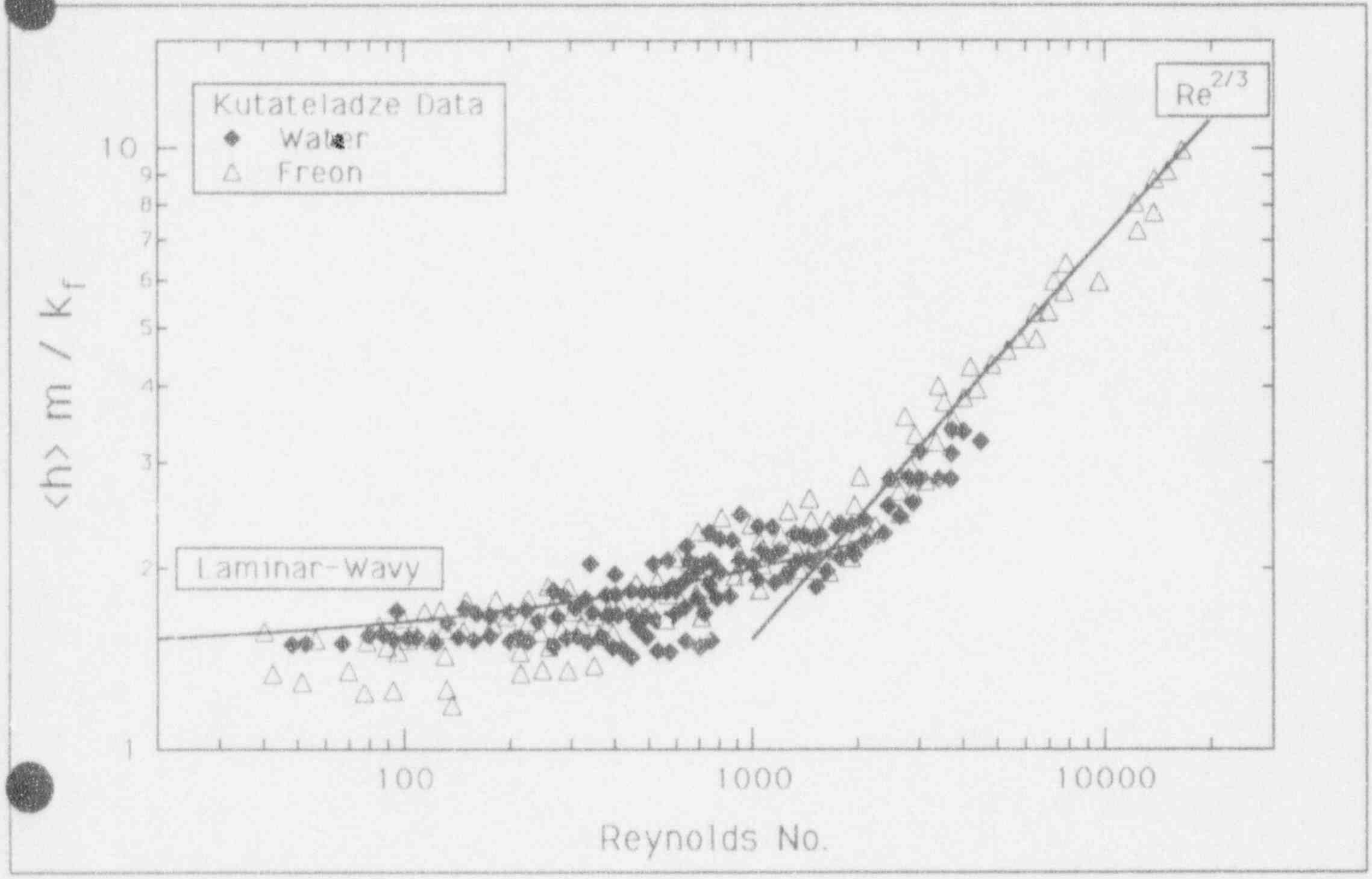
$$Nu^{*} = 0.0038 \cdot \mathrm{Re}_{f}^{0.4} \cdot \mathrm{Pr}^{65}$$

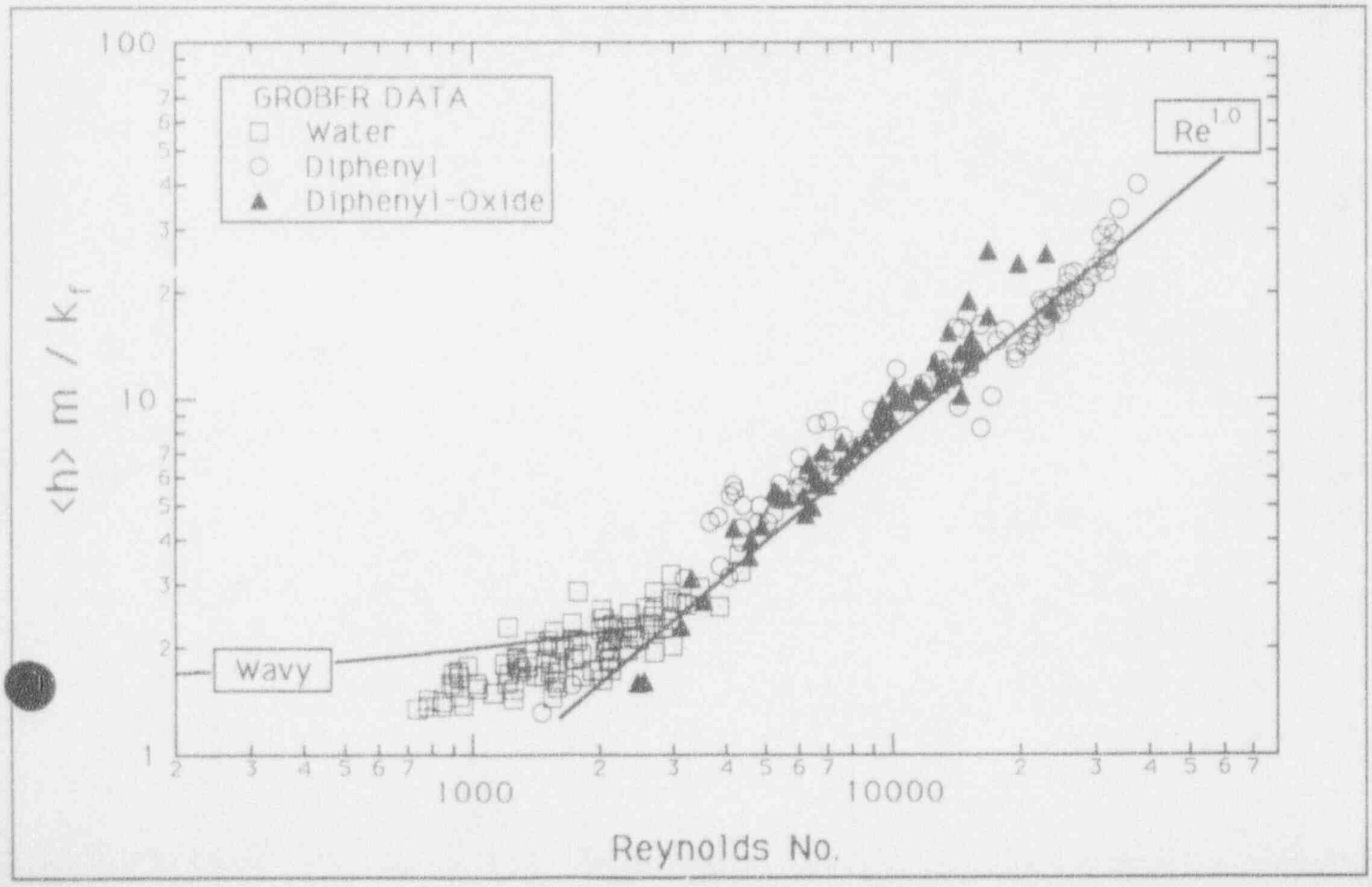
FALLING FILM: Condensation HTC



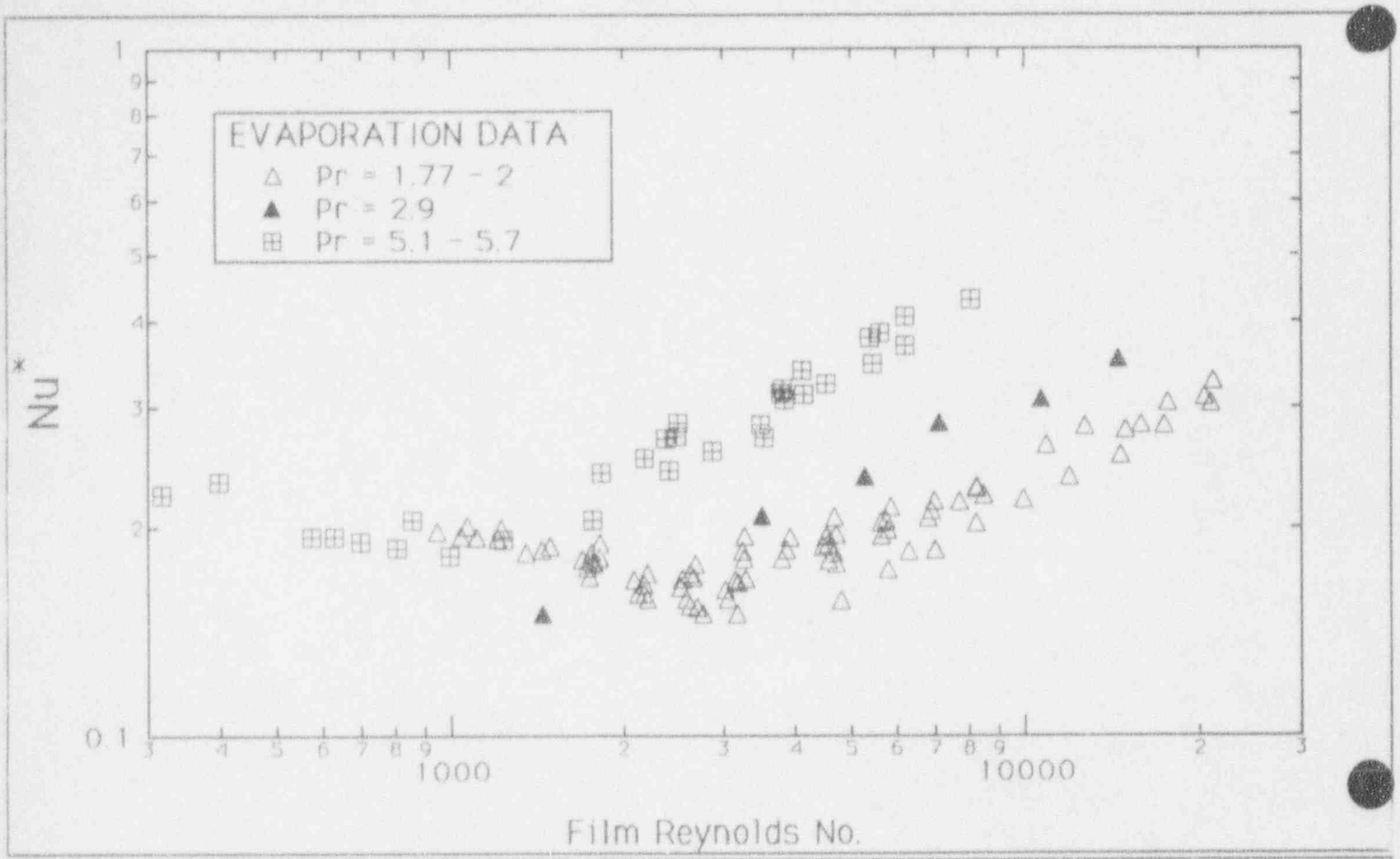


FALLING FILM: CONDENSATION





FALLING FILM: Evaporation Data



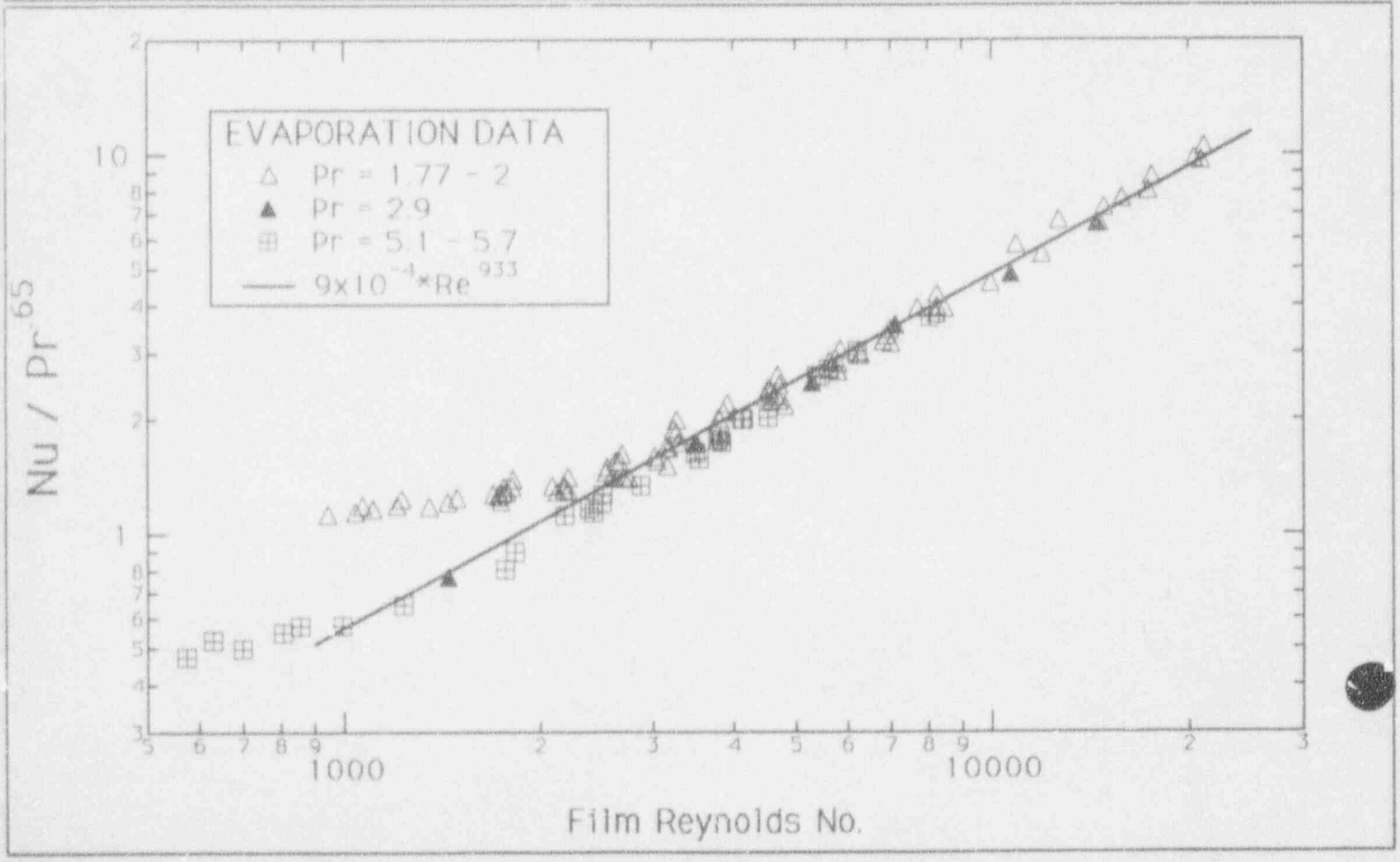


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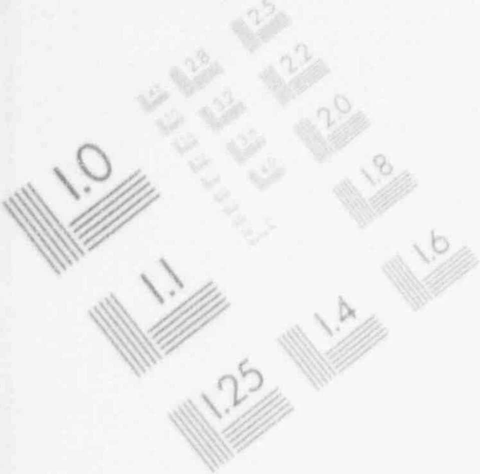
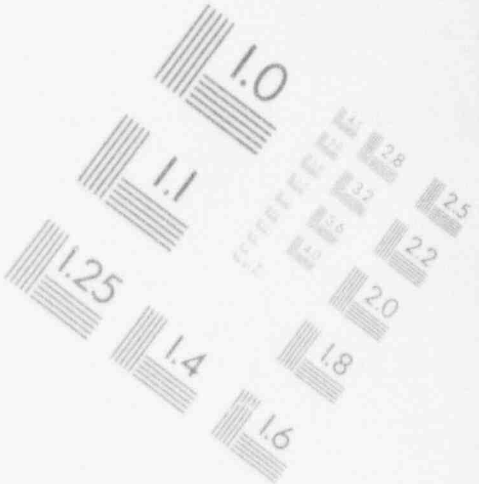
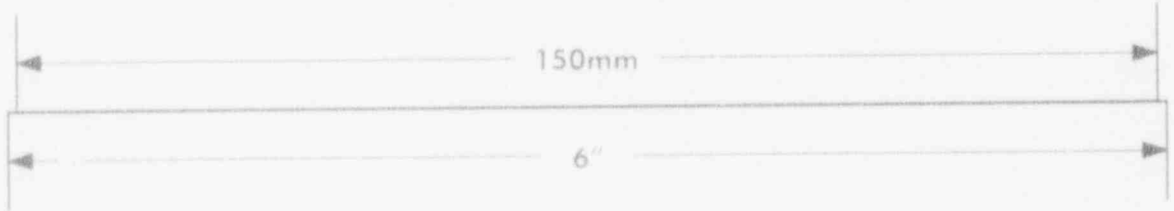
IMAGE EVALUATION
TEST TARGET (MT-3)







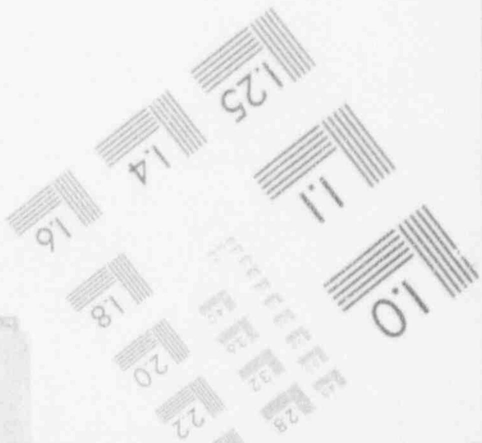


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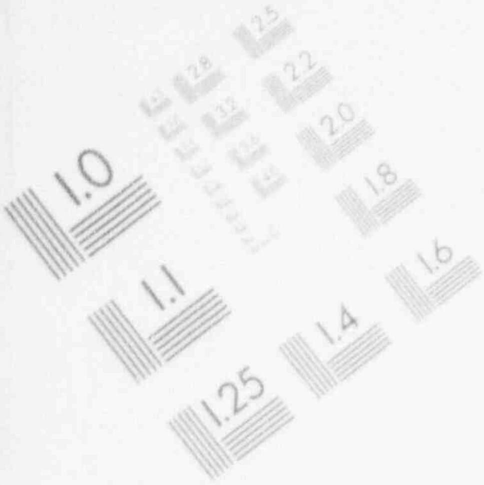
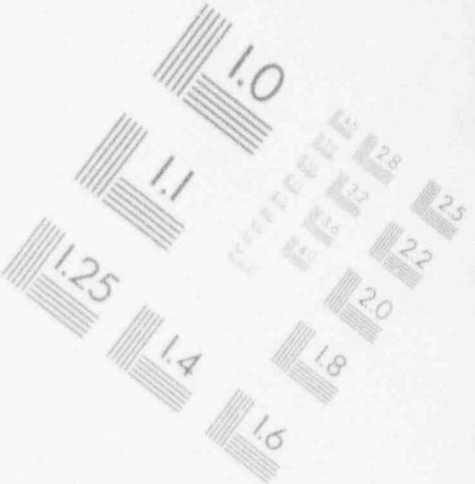
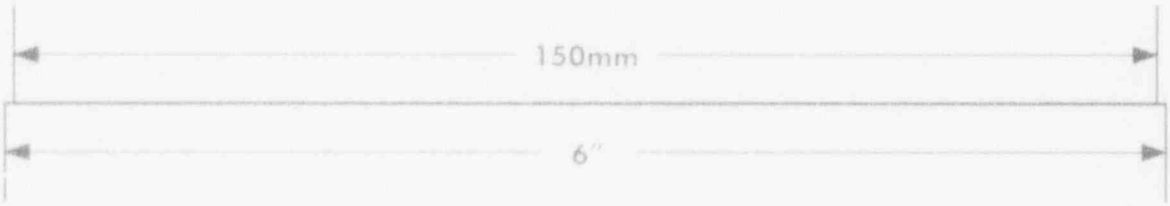
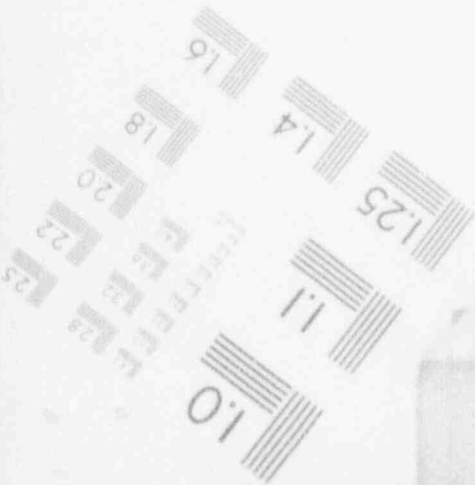
IMAGE EVALUATION
TEST TARGET (MT-3)









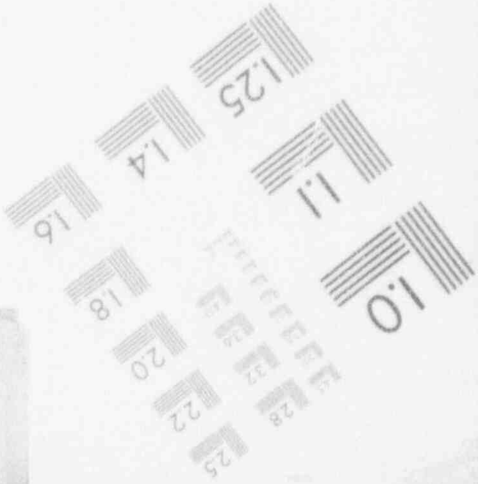


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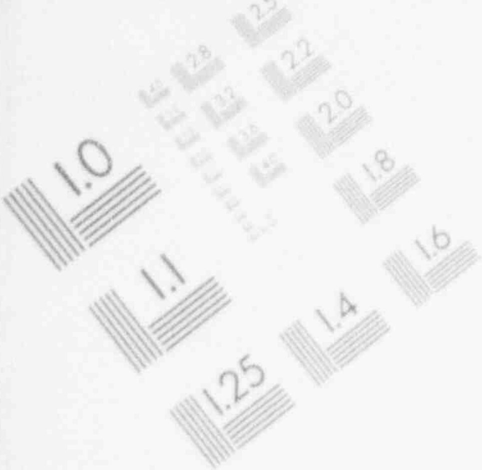
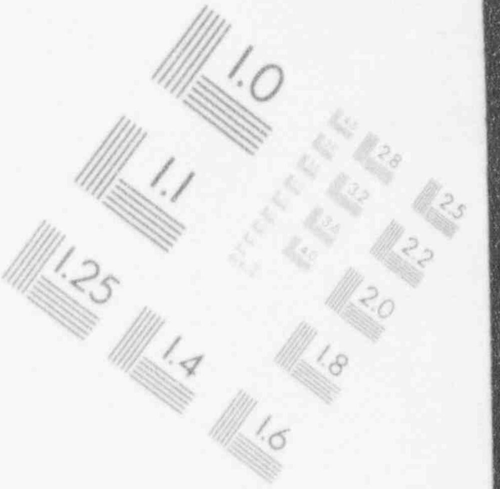
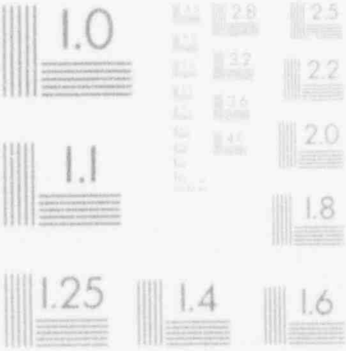
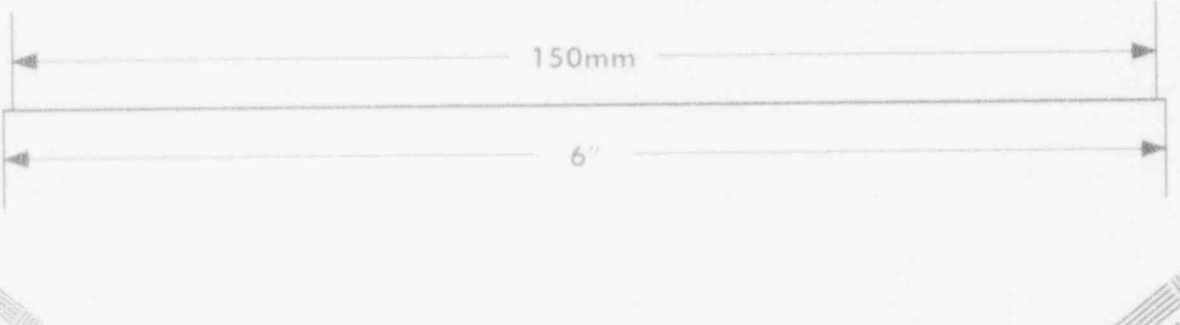
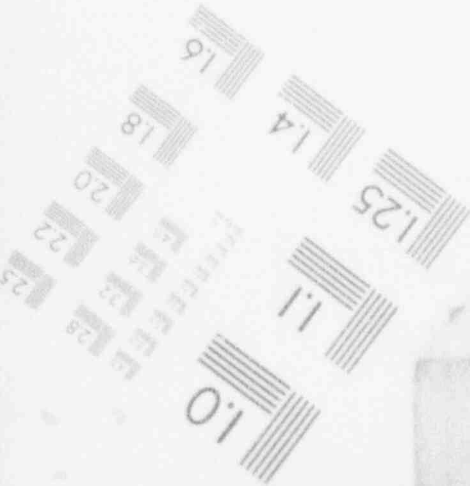
IMAGE EVALUATION
TEST TARGET (MT-3)











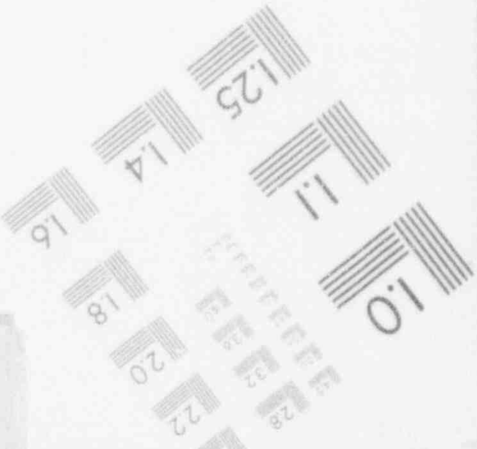


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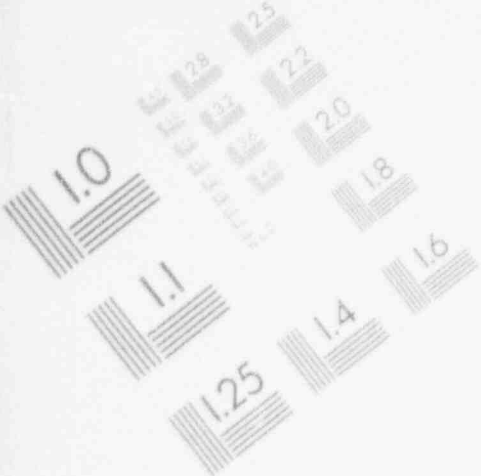
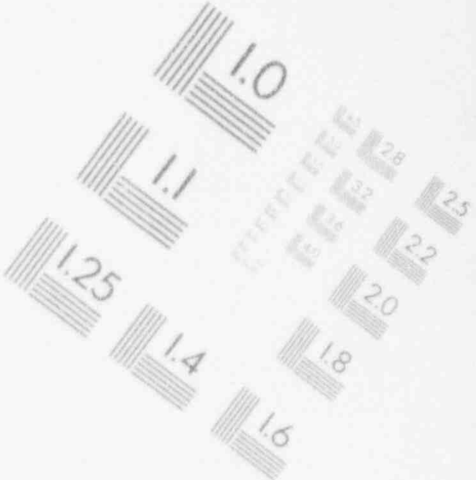
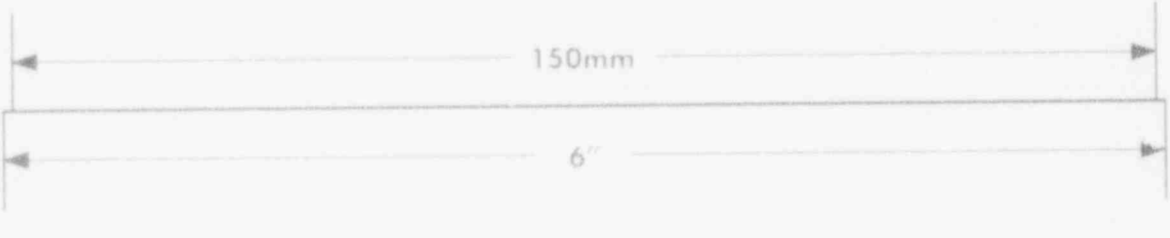
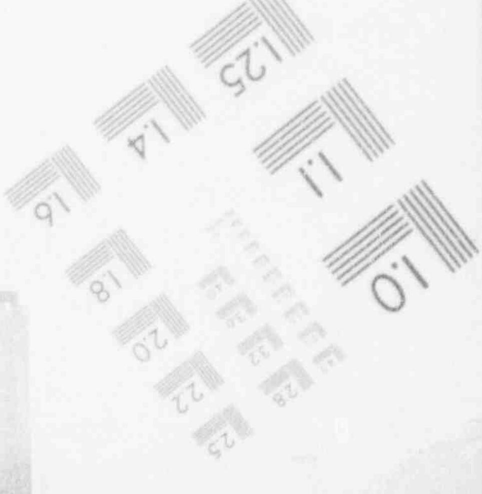
IMAGE EVALUATION TEST TARGET (MT-3)

FALLING FILMS: Turbulent

* HEATING HTC'S:

- **Wilke (1962):**
  - **original form:**

1) $Nu = 1.9$ $\qquad \mathrm{Re}_j \leq 2460 \cdot \mathrm{Pr}^{-6.5}$

2) $Nu = .0292 \cdot \mathrm{Re}_j^{\frac{8}{15}} \cdot \mathrm{Pr}^{.344}$ $\qquad 2460 \cdot \mathrm{Pr}^{-.046} \leq \mathrm{Re}_j \leq 1600$

3) $Nu = 2.12 x 10^{-4} \cdot \mathrm{Re}_j^{1.2} \cdot \mathrm{Pr}^{.344}$ $\qquad 1600 \leq \mathrm{Re}_j \leq 3200$

4) $Nu = 1.81 x 10^{-3} \cdot \mathrm{Re}_j^{\frac{14}{15}} \cdot \mathrm{Pr}^{.344}$ $\qquad 3200 \leq \mathrm{Re}_j \leq 11500$

  - **or**

1) $Nu^{*} = 2.07 \cdot \mathrm{Re}_j^{-\frac{1}{3}}$ $\qquad \mathrm{Re}_j \leq 2460 \cdot \mathrm{Pr}^{-.646}$

2) $Nu^{*} = .0322 \cdot \mathrm{Re}_j^{\frac{1}{5}} \cdot \mathrm{Pr}^{.344}$ $\qquad 2460 \cdot \mathrm{Pr}^{-.646} \leq \mathrm{Re}_j \leq 1600$

3) $Nu^{*} = 1.28 x 10^{-3} \cdot \mathrm{Re}_j^{.634} \cdot \mathrm{Pr}^{.34}$ $\qquad 1600 \leq \mathrm{Re}_j \leq 3200$

4) $Nu^{*} = 1.10 x 10^{-3} \cdot \mathrm{Re}_j^{.367} \cdot \mathrm{Pr}^{.344}$ $\qquad 3200 \leq \mathrm{Re}_j \leq 11500$

FALLING FILMS: Turbulent

* HEATING HTC'S:

- Carey (1985):

$$Nu^{*} = .0259 \cdot \mathrm{Re}_f^{292} \cdot \mathrm{Pr}^{\frac{1}{3}}$$

- Gimbutis (1974):

$$Nu^{*} = \left(.165 \cdot \mathrm{Re}_f^{16} - 0.4\right) \cdot \mathrm{Pr}_f^{34} \cdot \left(\mathrm{Pr}_f / \mathrm{Pr}_w\right)^{\frac{1}{4}}$$

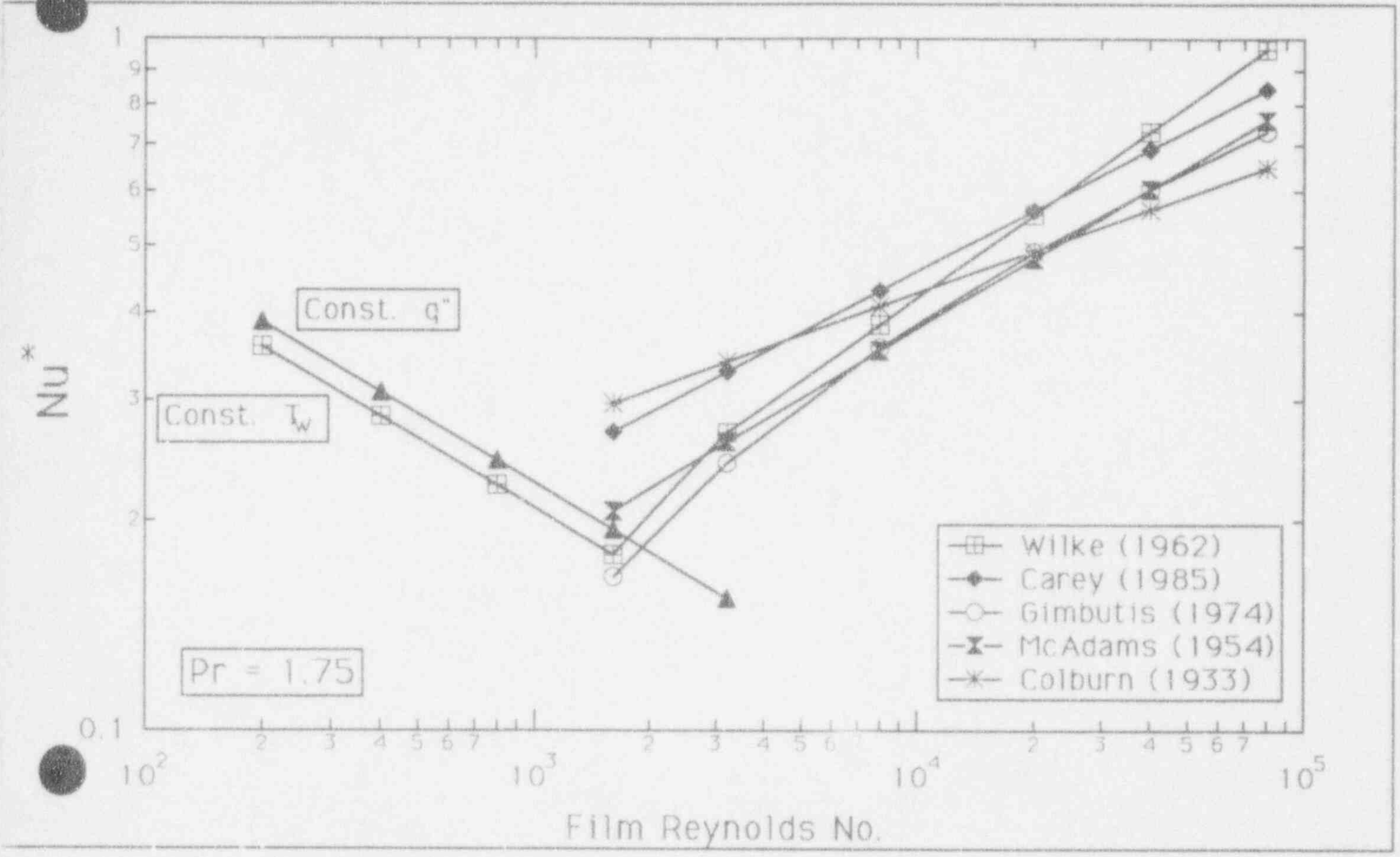
- McAdams (1954):

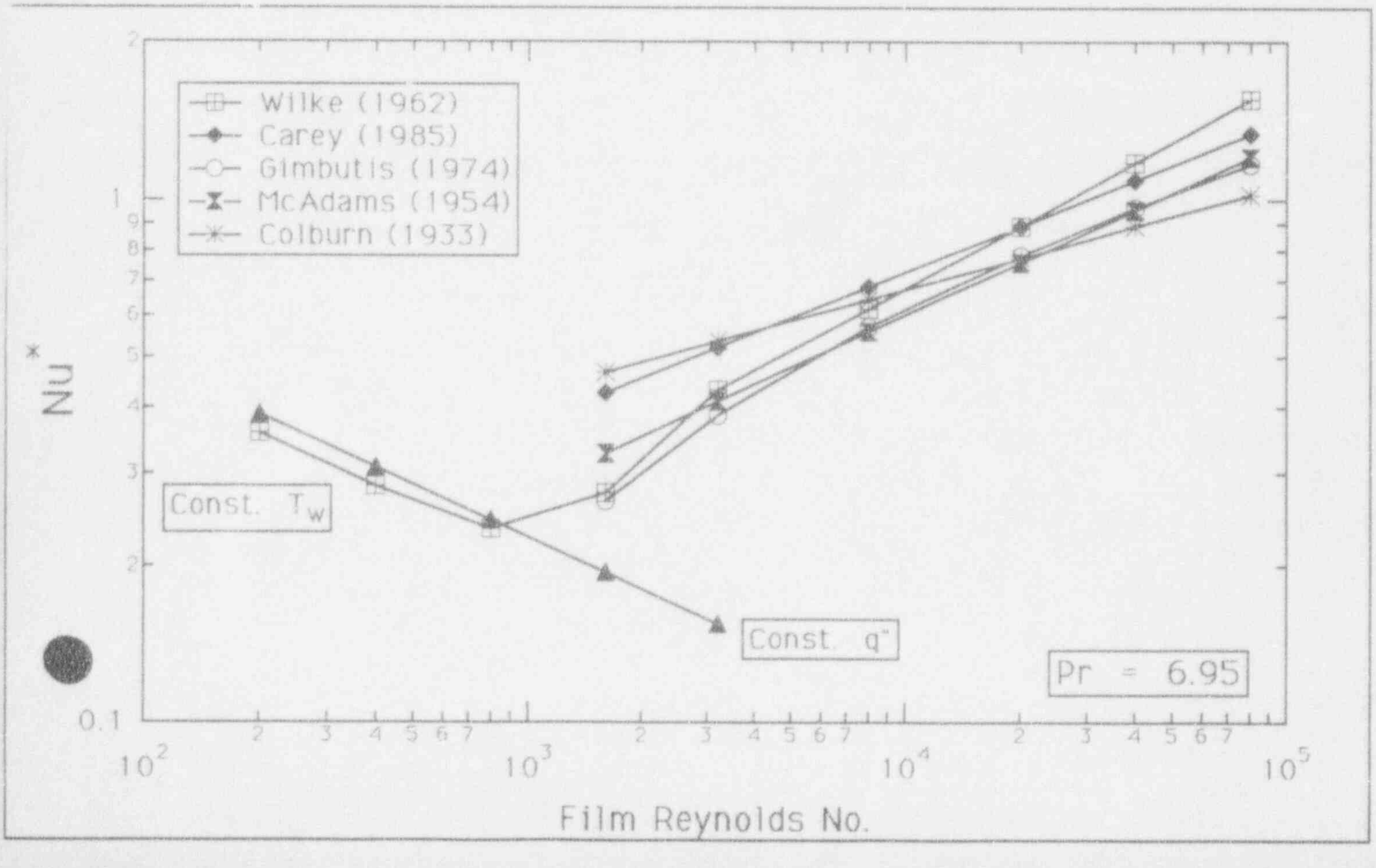
$$Nu^{*} = .01 \cdot \pi^{\frac{1}{3}} \cdot \mathrm{Re}_f^{\frac{1}{3}} \cdot \mathrm{Pr}^{\frac{1}{3}}$$

- Colburn (1933):

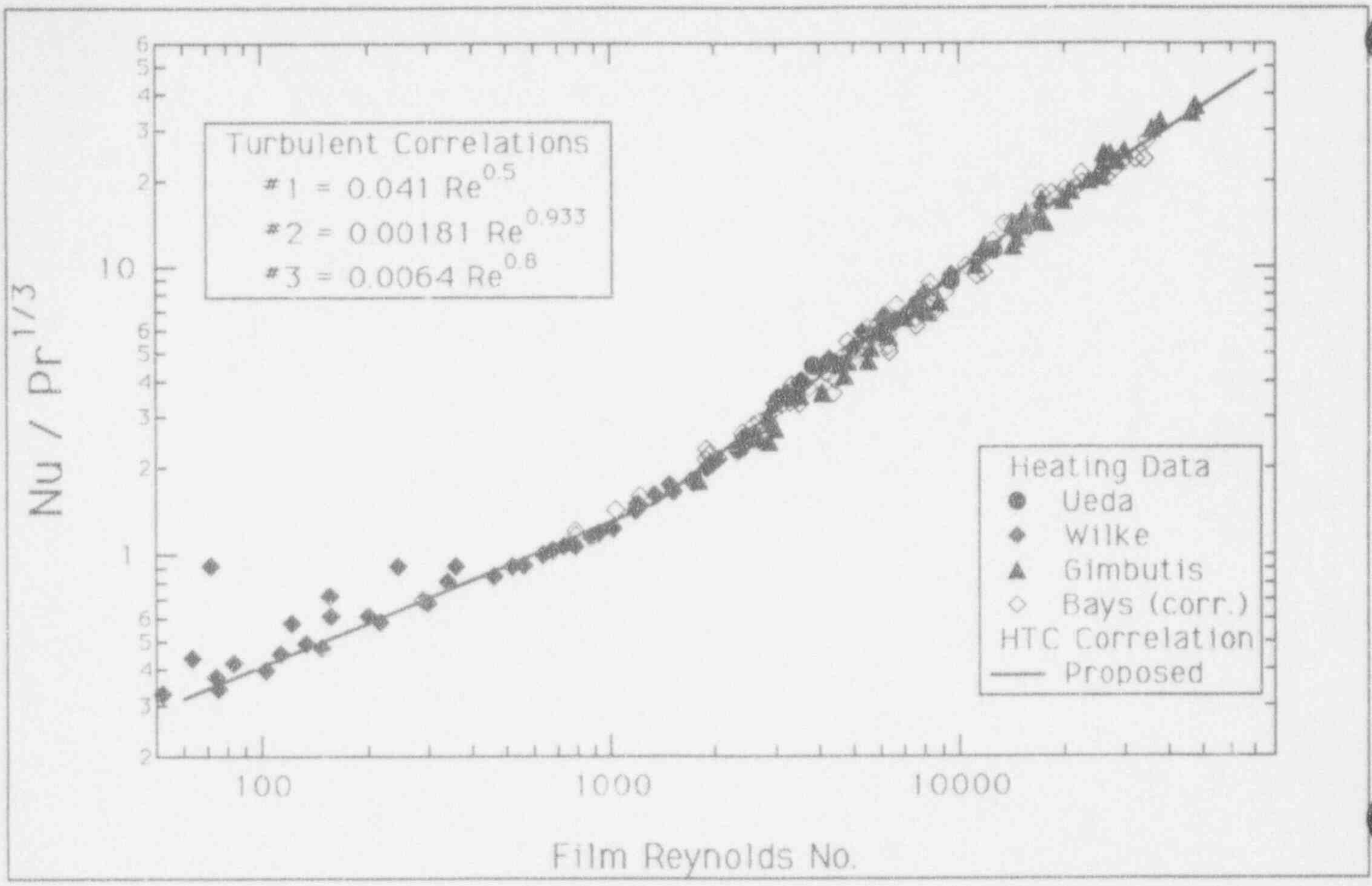
$$Nu^{*} = .056 \cdot \mathrm{Re}_f^{2} \cdot \mathrm{Pr}^{\frac{1}{3}}$$

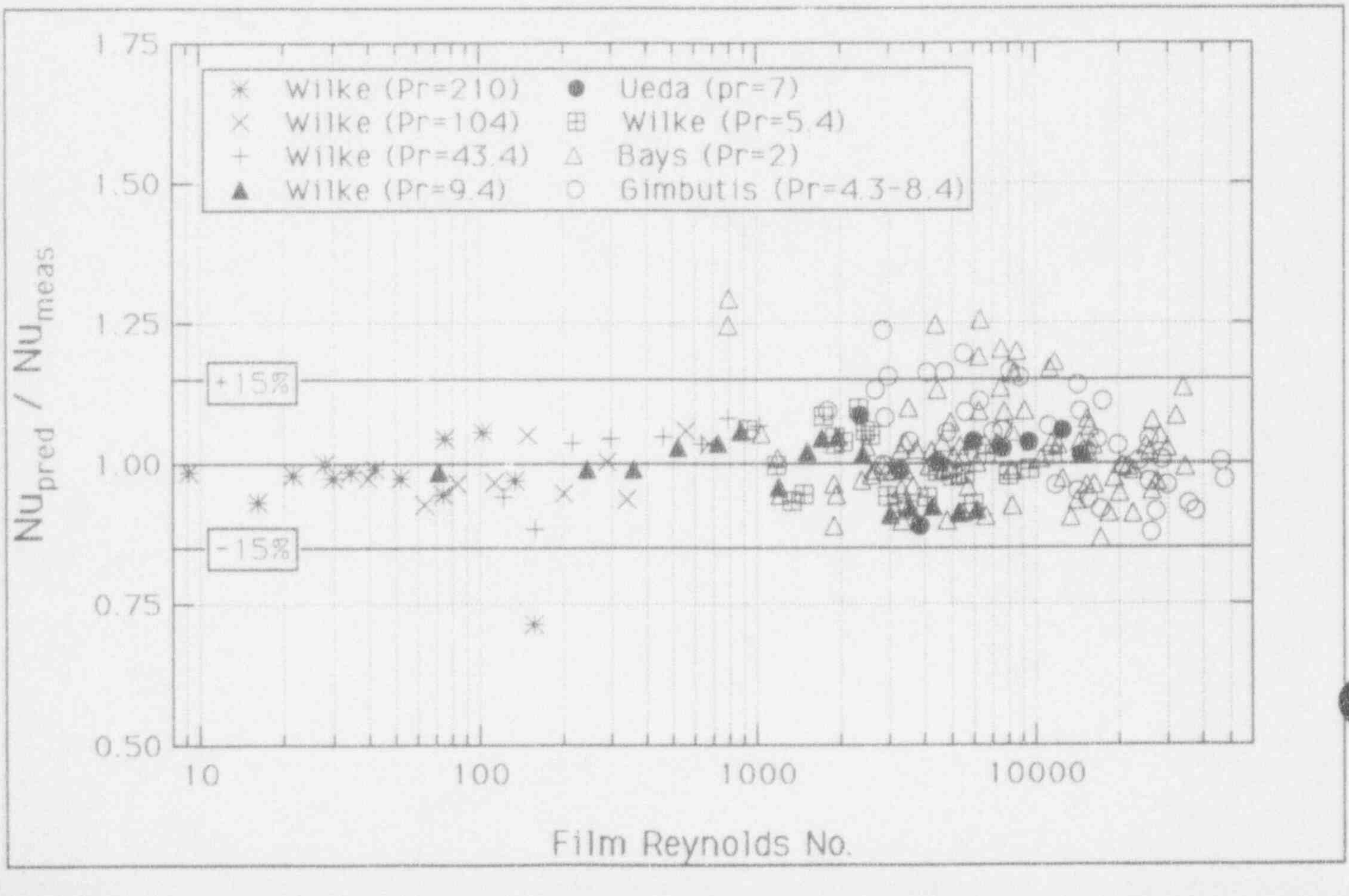
FALLING FILM HEATING CORRELATIONS





FALLING FILM: Heating/Cooling





SHEARED FILMS: Laminar-Smooth

* Dimensionless Film Thickness:

$$m^{+} = 0.707 \cdot \mathrm{Re}_{f}^{\frac{1}{2}}$$

* Thickness vs. Interfacial Friction:

$$m^{*} = \frac{0.707 \cdot \mathrm{Re}_{f}^{\frac{1}{2}}}{\left(\frac{2}{3} \cdot m^{*} + \tau_{i}^{*}\right)^{\frac{1}{2}}}$$

* Nusselt No. Dependence:

$$Nu^{*} = \frac{1}{m^{*}} = \frac{\left(\frac{2}{3} \cdot m^{*} + \tau_{i}^{*}\right)^{\frac{1}{2}}}{0.707 \cdot \mathrm{Re}_{f}^{\frac{1}{2}}}$$

SHEARED FILMS: Laminar-Wavy

- Enhancement due to Interfacial Shear:

$$Enh_i = \left(1 + \frac{3 \cdot \tau_i^*}{2 \cdot m^*}\right)^{\frac{1}{2}}$$

- QUESTION: Does Interfacial Shear affect the enhancement due to waves?

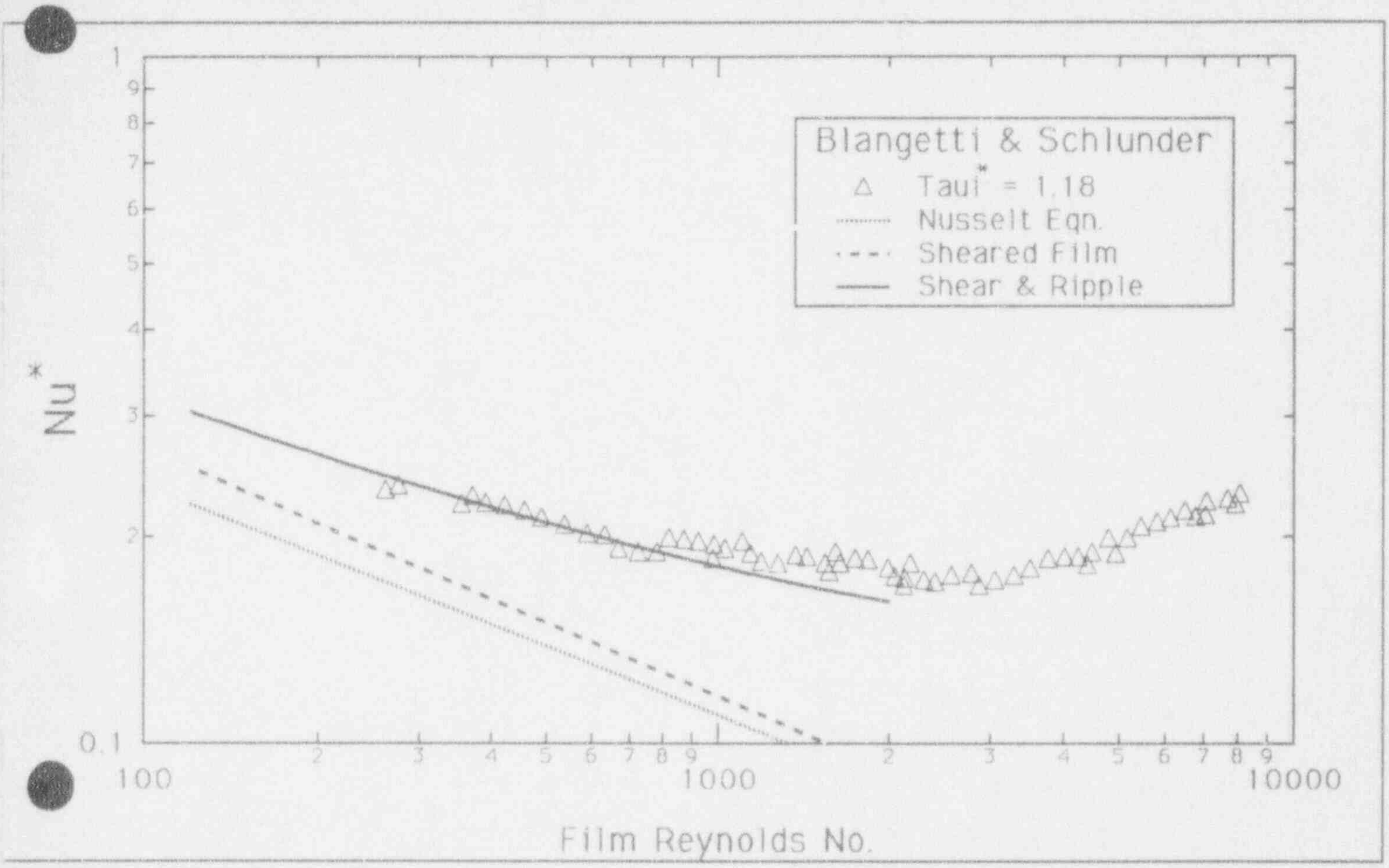
- CONCLUSION: for a laminar film the wave and interfacial effects appear to be additive.

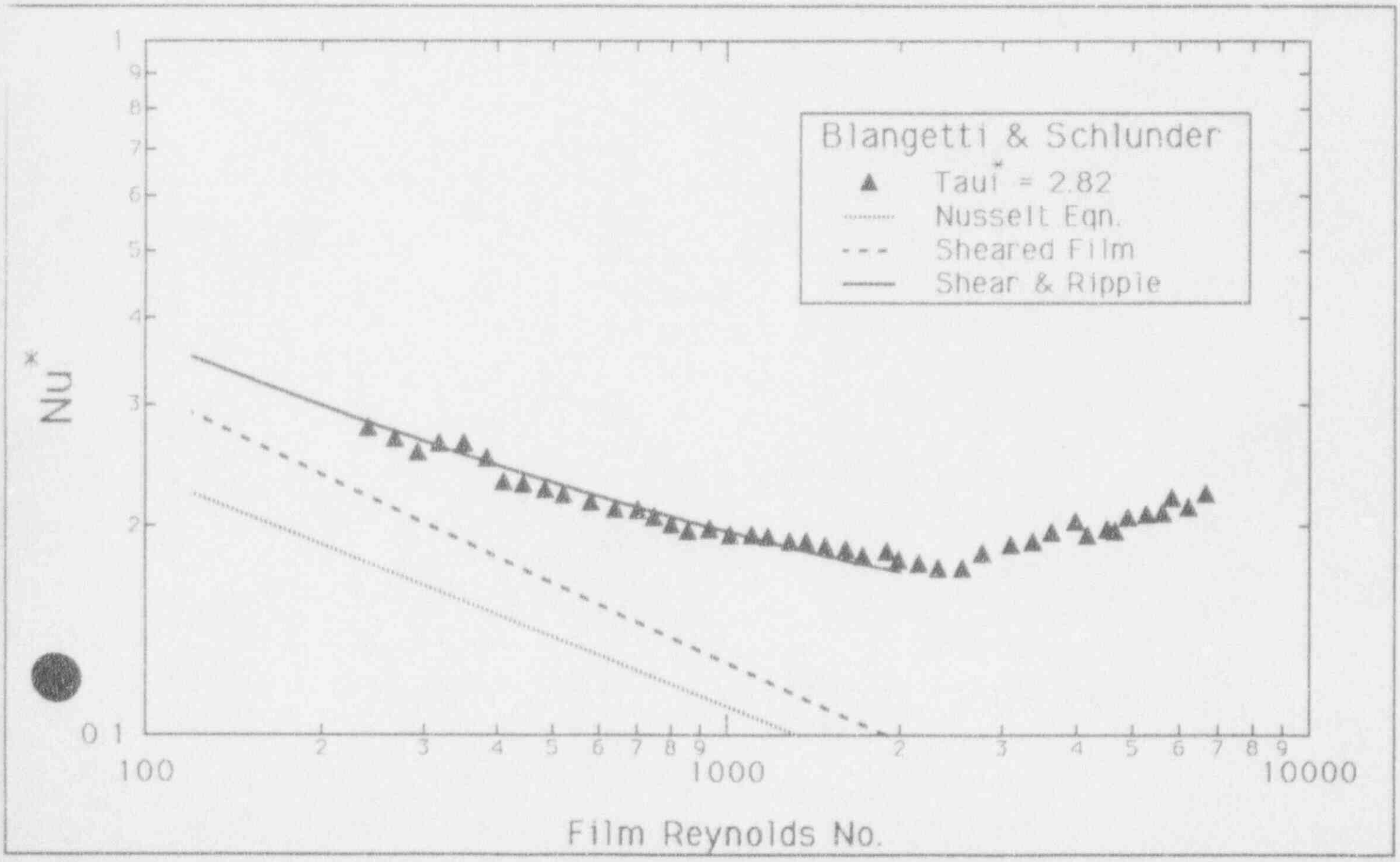
$$Nu^* = Nu_0^* \cdot Enh_{wavy} \cdot Enh_i$$

thus,

$$h_w = \frac{k_f}{m} \cdot Enh_{wavy}$$

SHEARED FILM: Laminar





SHEARED FILMS: Turbulent

- Assume total HT resistance across film is due to 'laminar sublayers' (wall & interface)

$$Nu = \frac{h \cdot m}{k_f} \approx \frac{m}{y_{sl}}$$

where y_{sl} represents combined thickness,

$$y_{sl}^{+} = \text{[illegible]} \cdot \frac{\sqrt{\tau_c / \rho_f}}{\upsilon_f}$$

then,

$$y_{sl} = const \cdot \frac{m}{m^{+}}$$

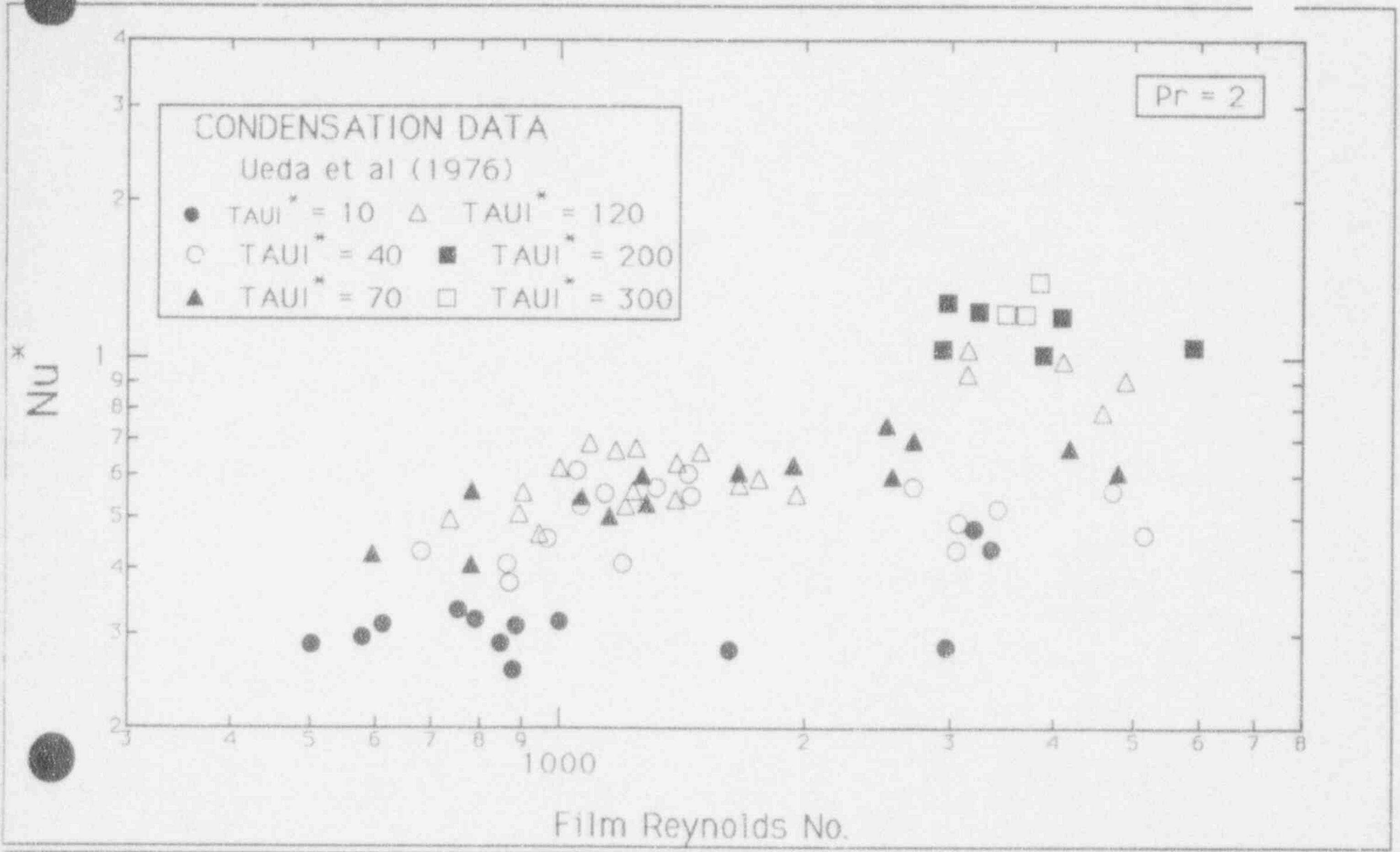
- Nusselt No. Behavior:

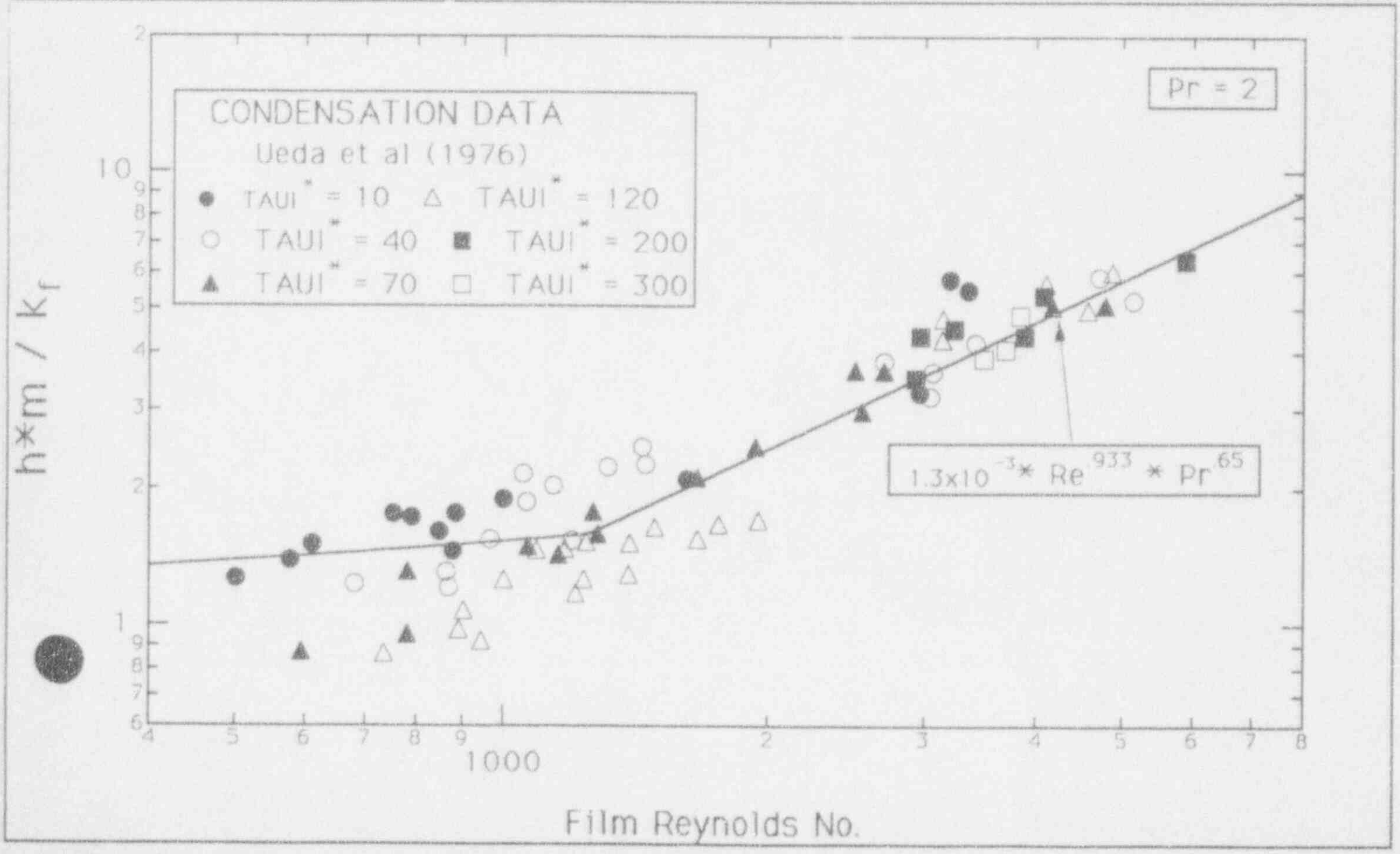
$$Nu = const \cdot m^{+} = const \cdot \mathrm{Re}_f^{85}$$

$$Nu^{*} = const \cdot \frac{m^{+}}{m^{+}} = const \cdot \mathrm{Re}_f^{284}$$

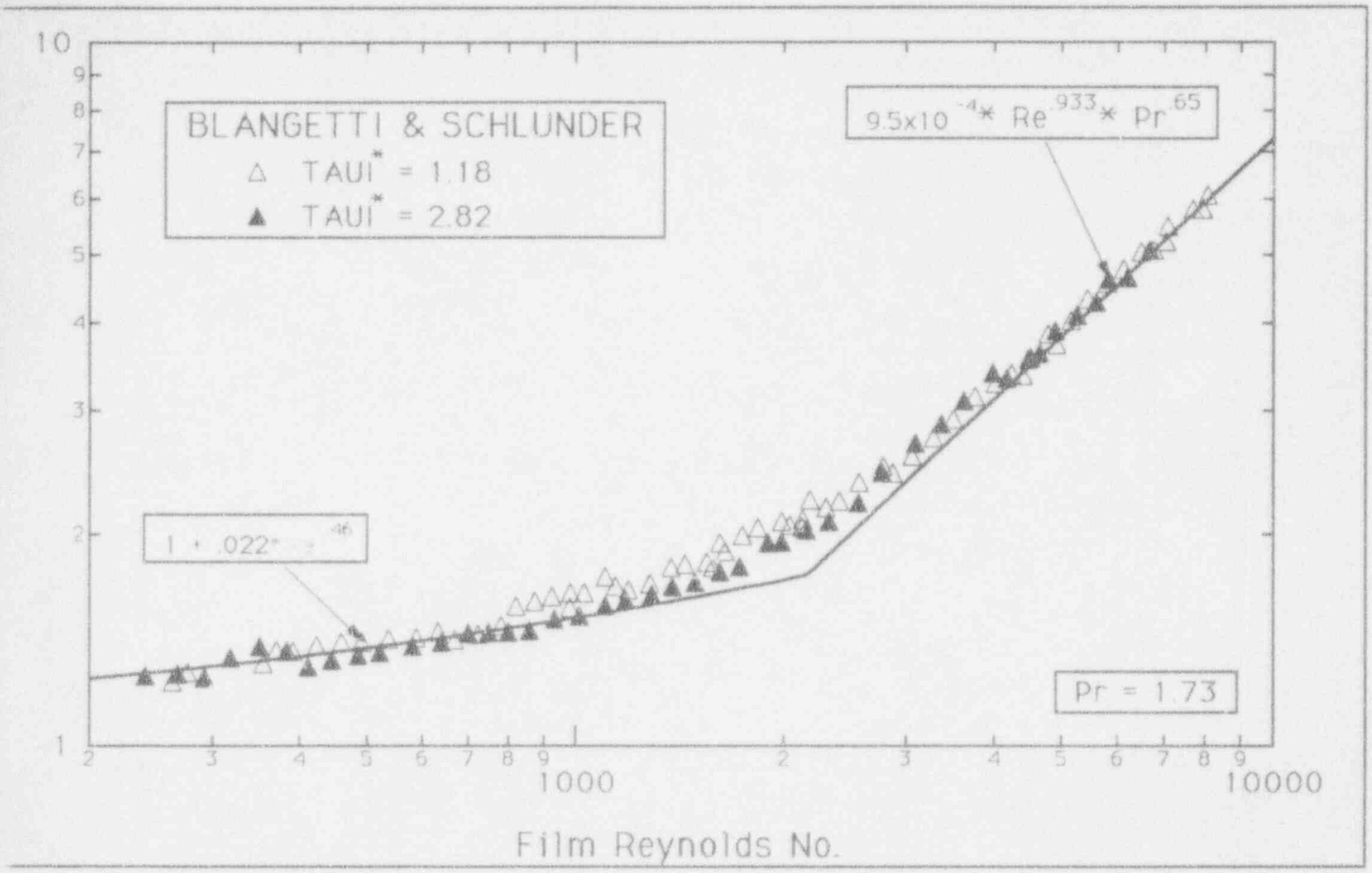
→ same for sheared & falling film.

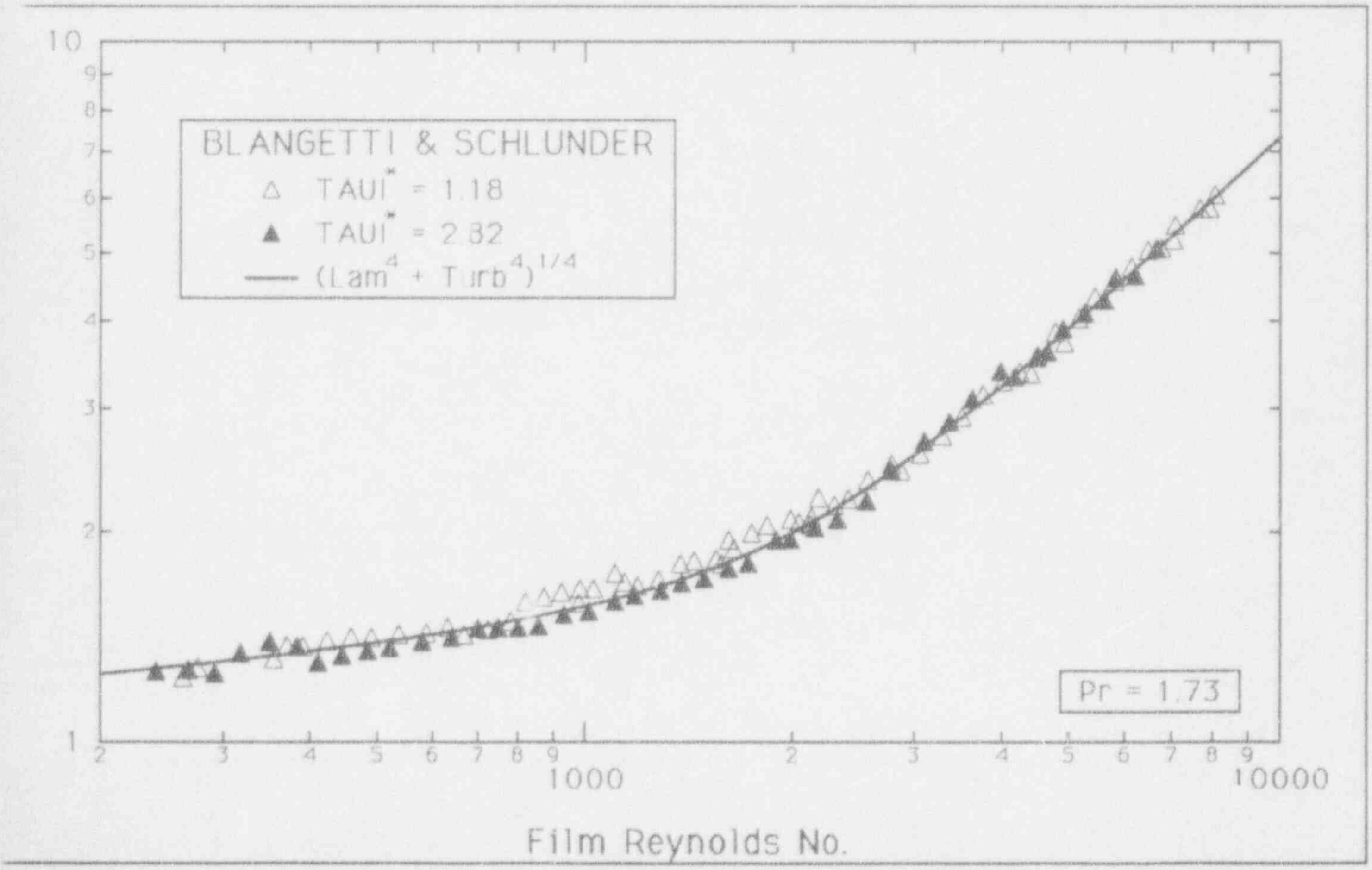
SHEARED FILMS: Condensation Data



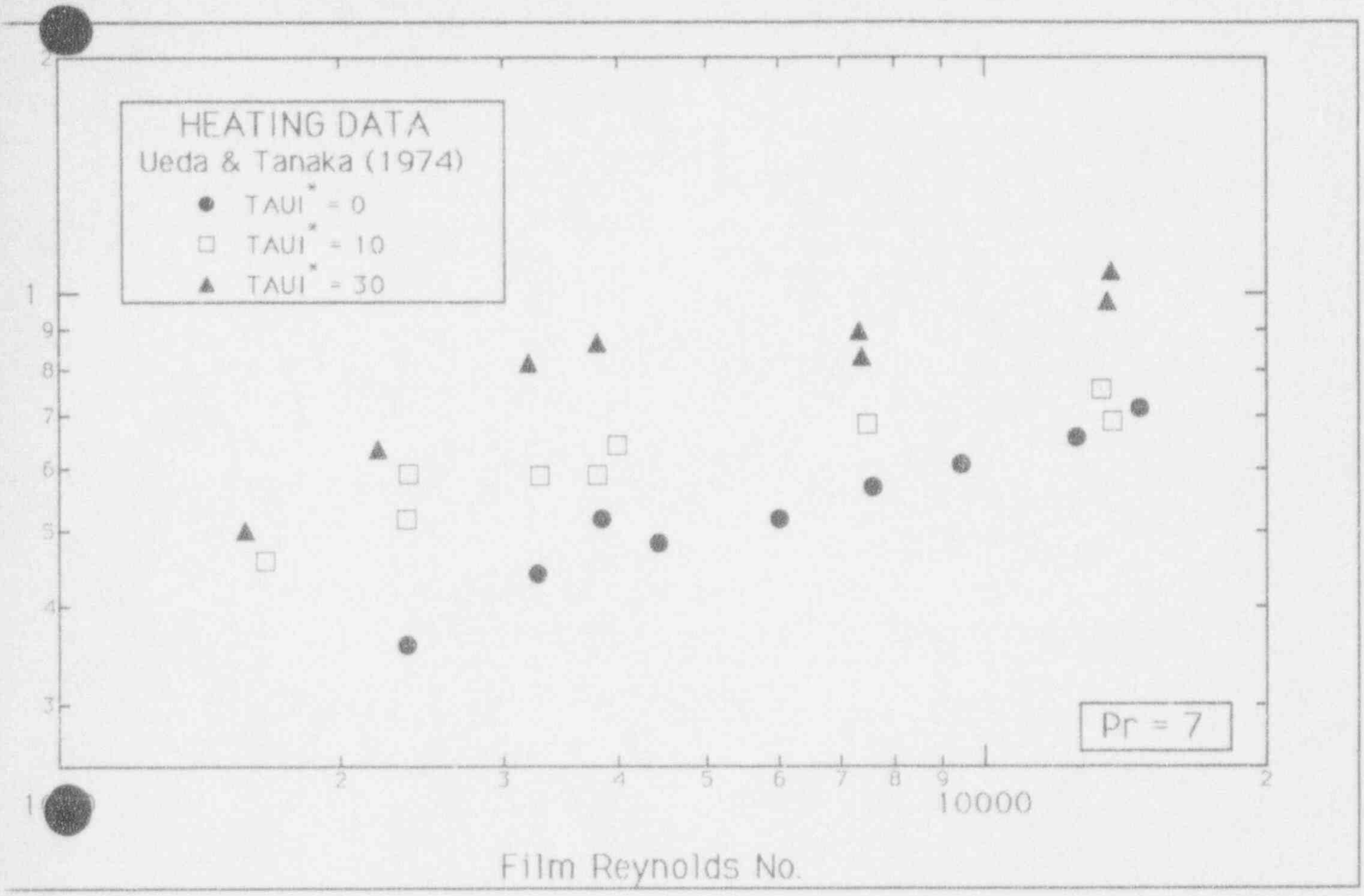


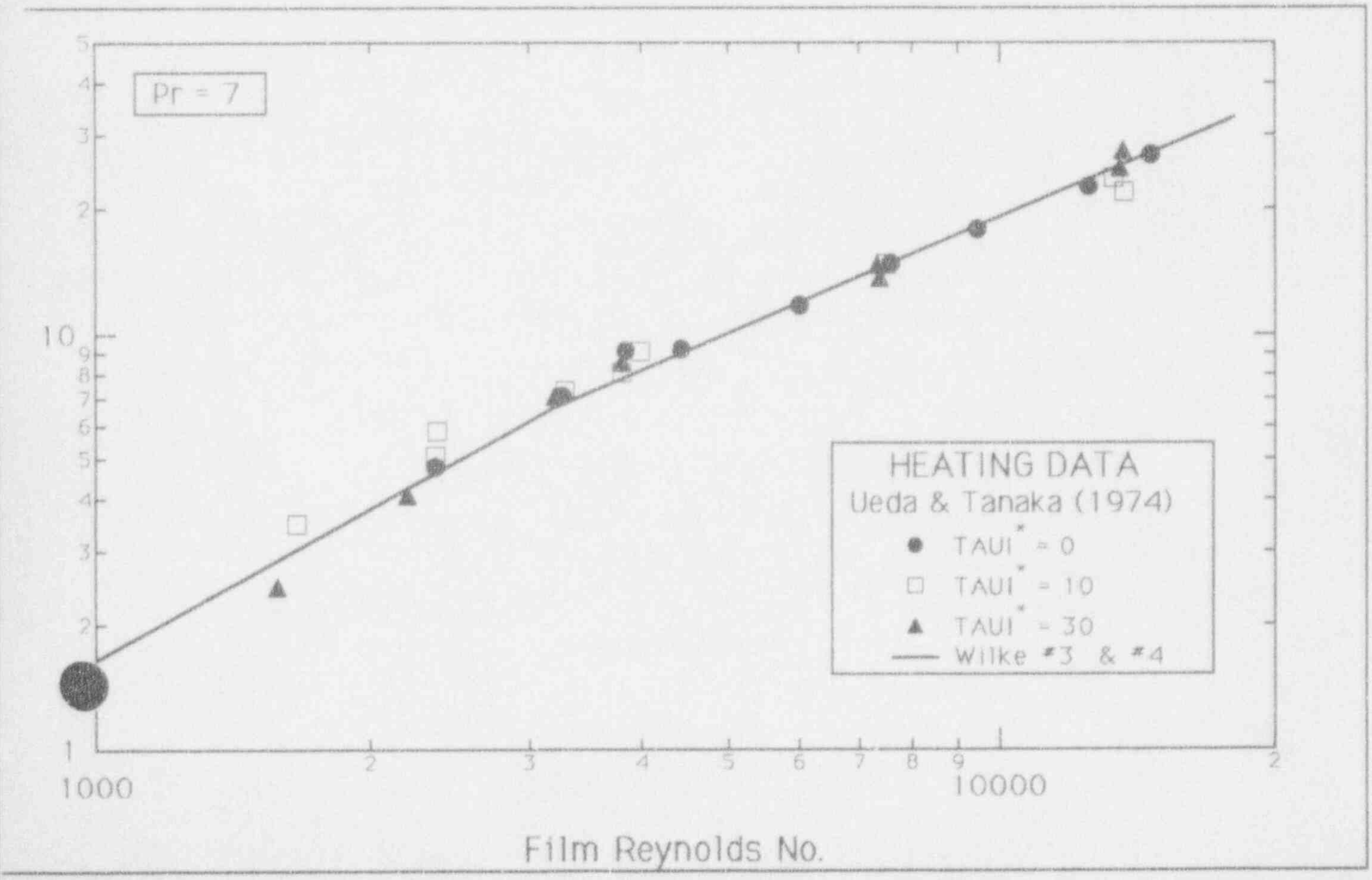
SHEARED FILMS: Condensation Data





SHEARED FILMS: Heating Data





FILM-SIDE HEAT TRANSFER

* SUMMARY

- Laminar Falling Film:
  - Condensation Heat Transfer enhancement by surface waves:

$$Enh = 1 + 0.022 \cdot \mathrm{Re}_j^{.46}$$

  - Wall-Fluid Heat Transfer relatively unaffected by waves:

$$Nu = 1.88$$

- Turbulent Falling Film:
  - Condensation data too scattered to determine Reynolds and Prandtl No. dependence.
  - Evaporation Heat Transfer data fit by:

$$Nu^* = .0054 \cdot \mathrm{Re}_j^{.367} \cdot \mathrm{Pr}_j^{.65}$$

$$Nu = 9x10^{-4} \cdot \mathrm{Re}_j^{.933} \cdot \mathrm{Pr}_j^{.65}$$

FILM-SIDE HEAT TRANSFER

* SUMMARY

- Turbulent Falling Film: (cont.)
  - Heating Data well fit by Wilke:

1) $Nu = 1.9 \qquad \mathrm{Re}_f \le 2460 \cdot \mathrm{Pr}^{-.646}$

2) $Nu = .0292 \cdot \mathrm{Re}_f^{\frac{8}{15}} \cdot \mathrm{Pr}^{.344} \qquad 2460 \cdot \mathrm{Pr}^{-.646} \le \mathrm{Re}_f \le 1600$

3) $Nu = 2.12x10^{-4} \cdot \mathrm{Re}_f^{1.2} \cdot \mathrm{Pr}^{.344} \qquad 1600 \le \mathrm{Re}_f \le 3200$

4) $Nu = 1.81x10^{-3} \cdot \mathrm{Re}_f^{\frac{14}{15}} \cdot \mathrm{Pr}^{.344} \qquad 3200 \le \mathrm{Re}_f \le 11500$

where #2 is used for Pr > 2, and at high Re

$$Nu = .0064 \cdot \mathrm{Re}_f^{8} \cdot \mathrm{Pr}_f^{\frac{1}{3}}$$

FILM-SIDE HEAT TRANSFER

* SUMMARY

- Sheared Films:
  - Important effects of interfacial shear are captured through the film thickness.
  - Falling Film Heat Transfer Correlations are applicable to Sheared Films when the Nusselt No. is based on the film thickness.
  - Thus:

$$Nu = \frac{h \cdot m}{k_f} = \frac{m}{k_f} \cdot fn\left\{\mathrm{Re}_f , \mathrm{Pr}_f\right\}$$

→ same for sheared & falling film.

NONCONDENSABLE GASES

- MODELING APPROACH:

  - Follows Method of Peterson et al (1992) and Kageyama et al (1993).

    - Energy Balance at Interface:

$$q_w'' = q_c'' + q_s''$$

where,

q_w'' = total heat flux to wall

q_c'' = flux of latent heat

q_s'' = flux of sensible heat

and,

$$h_w\left(T_i^s - T_w\right) = h_c\left(T_b^s - T_i^s\right) + h_s\left(T_b - T_i^s\right)$$

where,

T_i^s = sat. at interface vapor pressure

T_b^s = sat. at bulk vapor pressure

h_c = HTC for condensation at interface

h_s = HTC for convection to interface

NONCONDENSABLE GASES

* ASSUMPTIONS:

- VAPOR/GAS MIXTURE: Perfect Gas
    - Mole concentrations expressed as partial pressures

- CLAUSIUS-CLAPEYRON EQN. APPLIES
    - $(P_{vb} - P_{vi}) \rightarrow (T_{vb} - T_{vi})$

- VAPOR/LIQUID SPECIFIC VOLUME APPROXIMATED:
    - $v_{lv} \approx \dfrac{R \cdot T_{ave}}{M_v \cdot P_T \cdot x_{v,ave}}$

- DIFFUSION COEFFICIENT APPROXIMATED:
    - $D \cdot \dfrac{P}{T^2} = D_0 \cdot \dfrac{P_0}{T_0^2}$

NONCONDENSABLE GASES

* MODEL FORMULATION:

- CONDENSATION 'CONDUCTIVITY': assume T_i & x_i
  - $$k_c = \frac{h_{lv}^2}{\Phi \cdot T_{ave}} \cdot \left\{ \frac{M_v^2 \cdot P_0 \cdot D_0}{R^2 \cdot T_0^2} \right\}$$

  where,

  - $$\Phi = \frac{x_{g,ave}}{x_{v,ave}} = \frac{Ln\left[\left(1 - x_{gb}\right) / \left(1 - x_{gi}\right)\right]}{Ln\left[x_{gb} / x_{gi}\right]}$$

- CONDENSATION HTC:
  - $$h_c = \frac{k_c}{d} \cdot Sh$$

- INTERFACE TEMPERATURE:
  - $$T_i^s = \frac{h_w \cdot T_w + h_c \cdot T_b^s + h_s \cdot T_b}{h_w + h_c + h_s}$$

- WALL HEAT FLUX:
  - $$q_w'' = \frac{h_c\left(T_b^s - T_w\right) + h_s\left(T_b - T_w\right)}{1 + \left(h_c + h_s\right)/h_w}$$

NONCONDENSABLE GASES

* ADVANTAGE:

- Iteration Parameter, Φ, is Less Sensitive:
  - better initial guess
  - fewer iterations

* FURTHER ASSUMPTION (?)

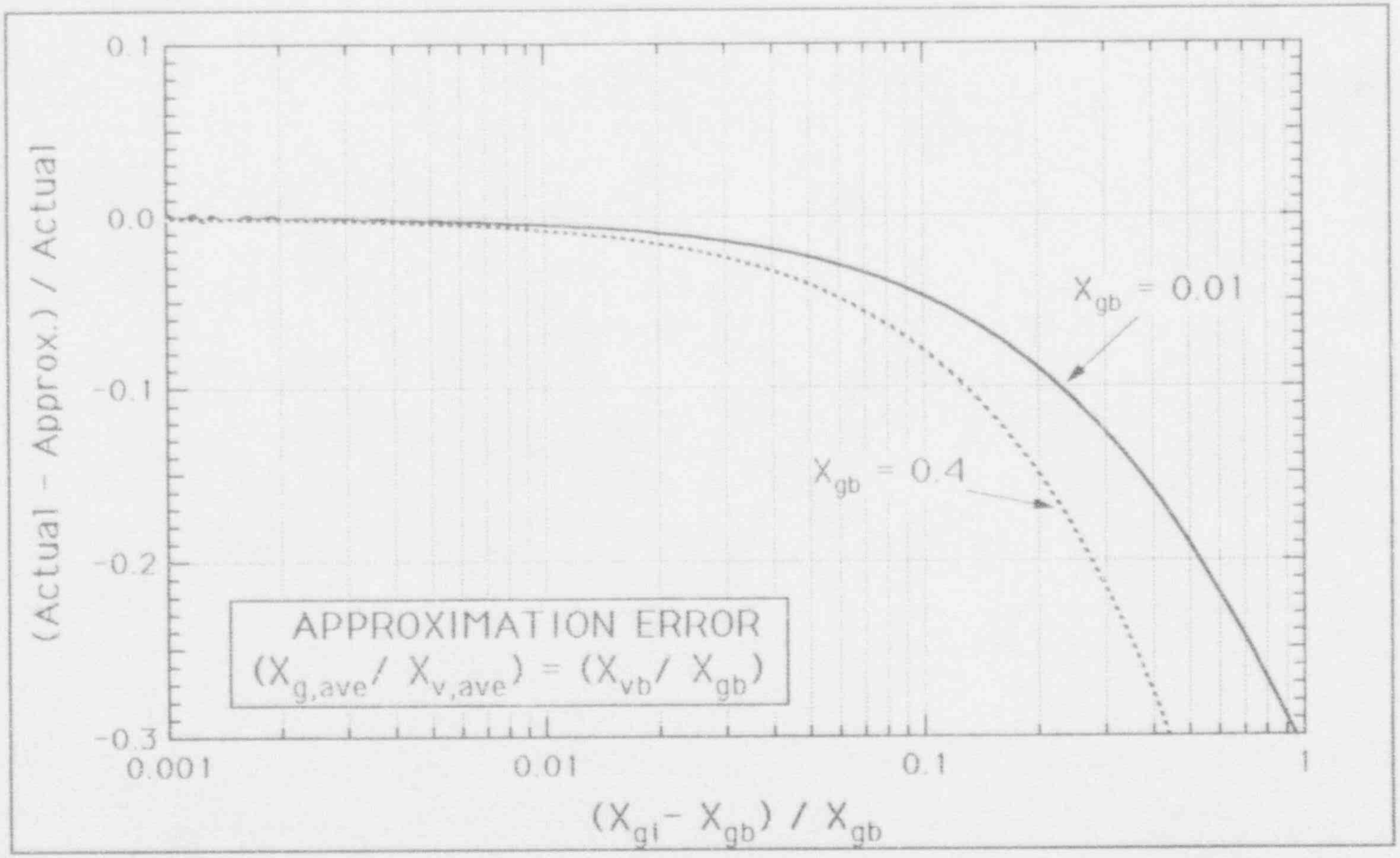
- when NC gas effects are important:
  - x_{gb} is appreciable (i.e. not infinitesimal)
  - Condensation heat flux is not large
  - thus, $\left(x_{gi} - x_{gb}\right)/x_{gb}$ is not large
  - then,

$$\Phi = \frac{x_{g,ave}}{x_{v,ave}} \approx \frac{1 - x_{gb}}{x_{gb}}$$

$\Rightarrow$ No Iteration Required !

NONCONDENSABLE GASES

* FURTHER APPROXIMATION:



NONCONDENSABLE GASES

* CONSTITUTIVE MODELS REQUIRED:

- Convective HTC: h_s
- Ratio of Sherwood No. to Nusselt No.

* Convective HTC: Effects to consider

- Entrance Effects
- Boundary Layer Suction
- Roughness (interfacial waves)
- Mist Formation

NONCONDENSABLE GASES

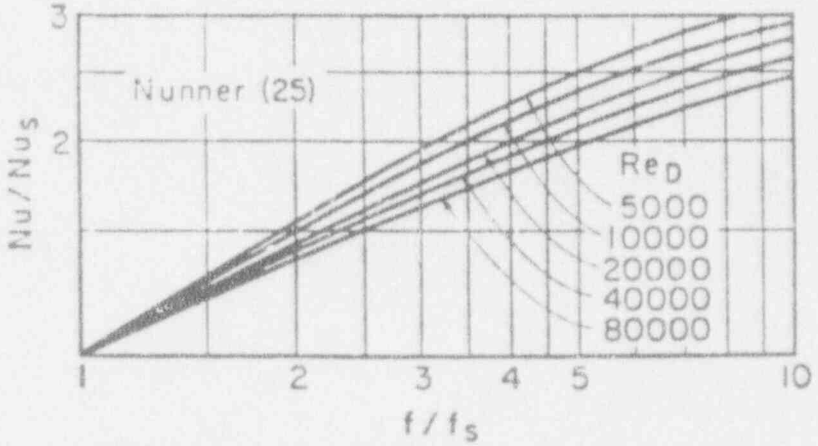
* HEAT TRANSFER ENHANCEMENT:

- ENTRANCE EFFECTS: Possibly important for the PCCS and ICS (L/D = 50).

- BOUNDARY LAYER SUCTION: probably small, neglect initially.

- ROUGHNESS EFFECT: approximate as

  - $$\frac{h_{rough}}{h_{smooth}} = \left(\frac{f_i}{f_s}\right)^{\frac{1}{2}}$$



- MIST FORMATION:
  - Mori & Hijikata (1973): $h_s \approx 2 \cdot h_{conv}$
  - Peterson et al (1992): $h_s \approx 7 \cdot h_{conv}$

NONCONDENSABLE GASES

* HEAT/MASS TRANSFER ANALOGY:

- $\frac{Sh}{Nu} = \left(\frac{Sc}{\Pr}\right)^n$

  - Exponent n ?

    - Dittus-Boelter (cooling): n = 0.4

    - Mass Transfer (gas absorption): n = 0.5

    - Gilliland & Sherwood (1934): n = 0.44

    - Kageyama et al (1993): n = 0.6

ASSESSMENT PLANS

- DEVELOPMENTAL ASSESSMENT
  - Forced Convection
    - Pure Steam:
      - Goodykoontz & Dorsch (4 tests)
      - B & W (2 tests)
      - M.I.T. (8 tests)
    - Noncondensables:
      - M.I.T. (Siddique & Husanein)
- APPLICATIONS ASSESSMENT
  - CMT Tests (steam, steam& NC)
    - Empty (wall condensation)
    - Stable Level (wall & interface)
    - Drain Test
  - U.C.B. Tests (?)
  - PANTHERS (ICS & PCCS)

Idaho
National
Engineering
Laboratory

Level Tracking Model

Presented by
Gary W. Johnsen

Advisory Committee on
Reactor Safeguards
January 4, 1994
Bethesda, MD

Outline

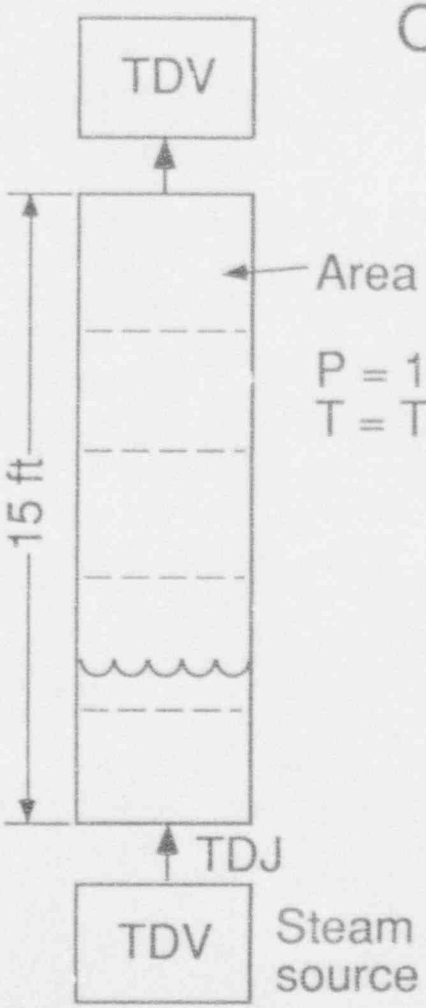
- Level Tracking versus Vertical Stratification Models
- Preliminary Verification Results
- Description of the New Level Tracking Model
- Planned Assessment

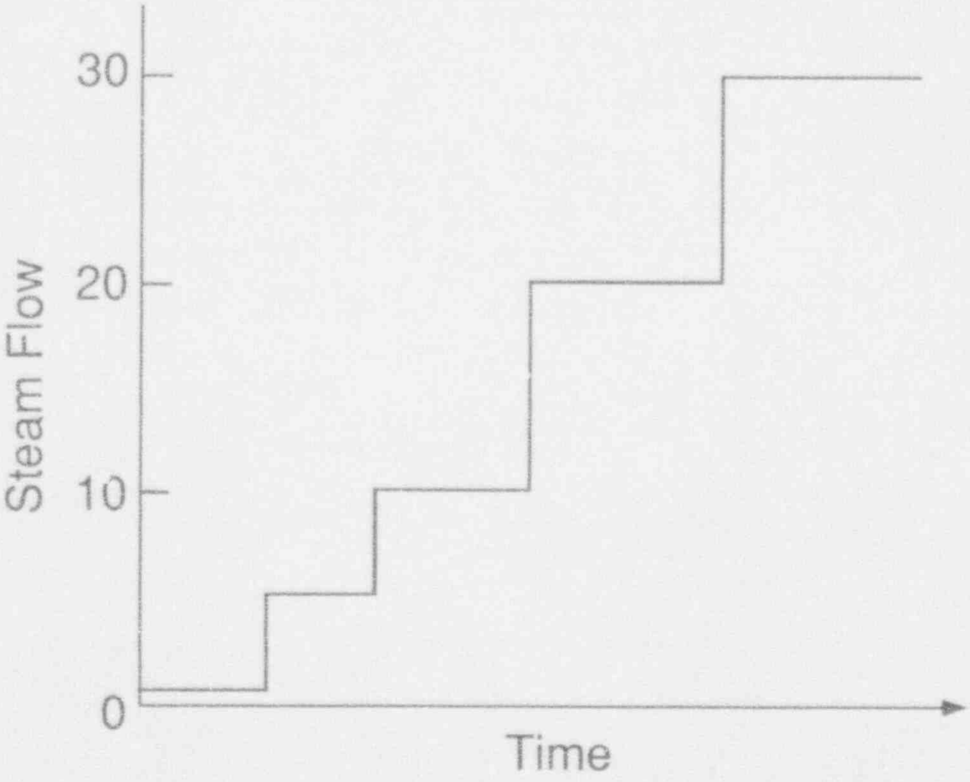
Level Tracking versus Vertical Stratification Models

- The model described in Vol. 4 is a vertical stratification model, not a level tracking model
- The need for a true level tracking model was recognized in the identification of modeling capabilities for the advanced light water reactors, particularly for modeling the CMT
- A new model has been implemented and will be described

Level Tracking Test Problem–Bubbling Steam Through Liquid

Objective: Verify Functionality





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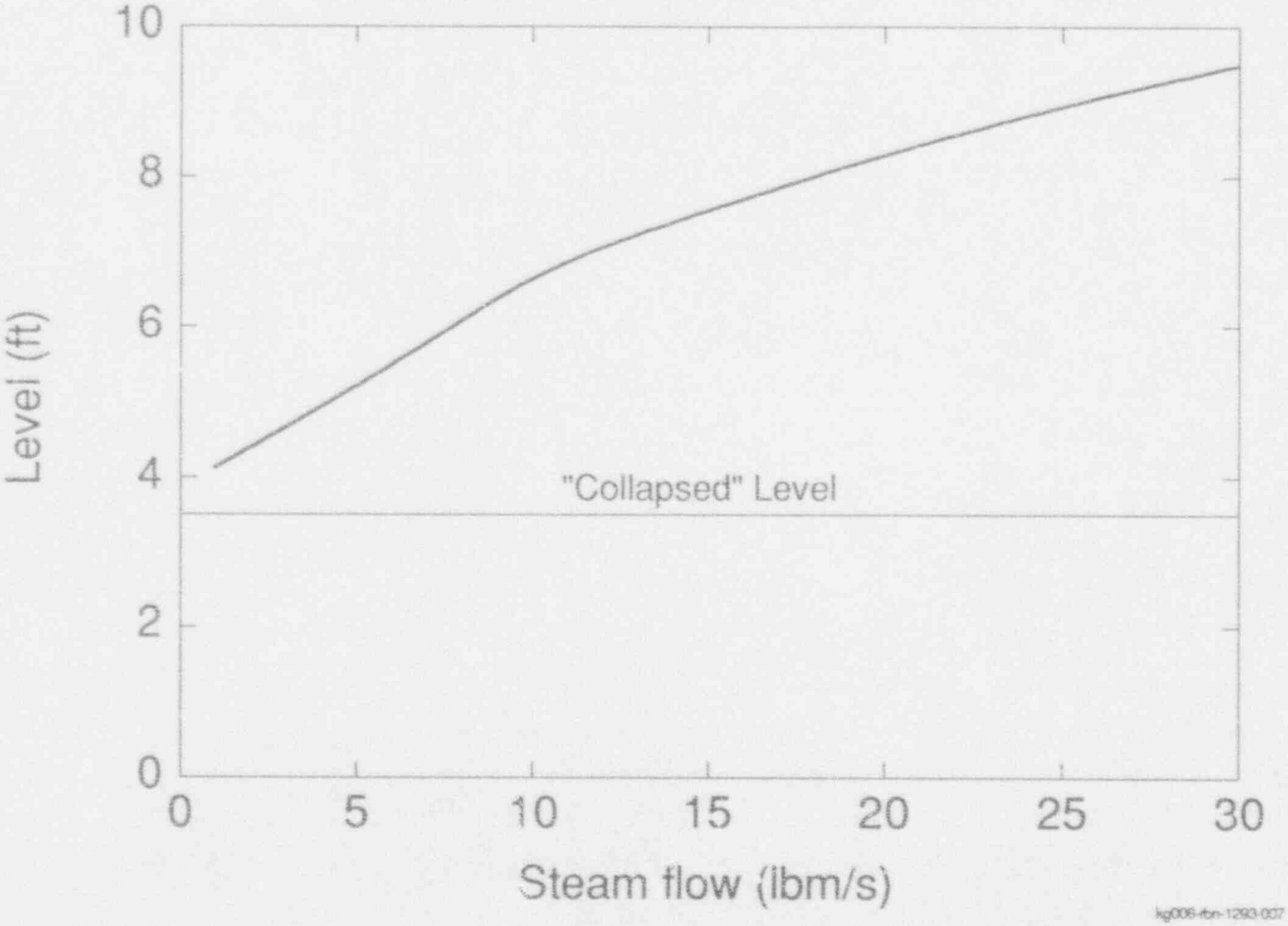
Mixture Level vs. Steam Flow



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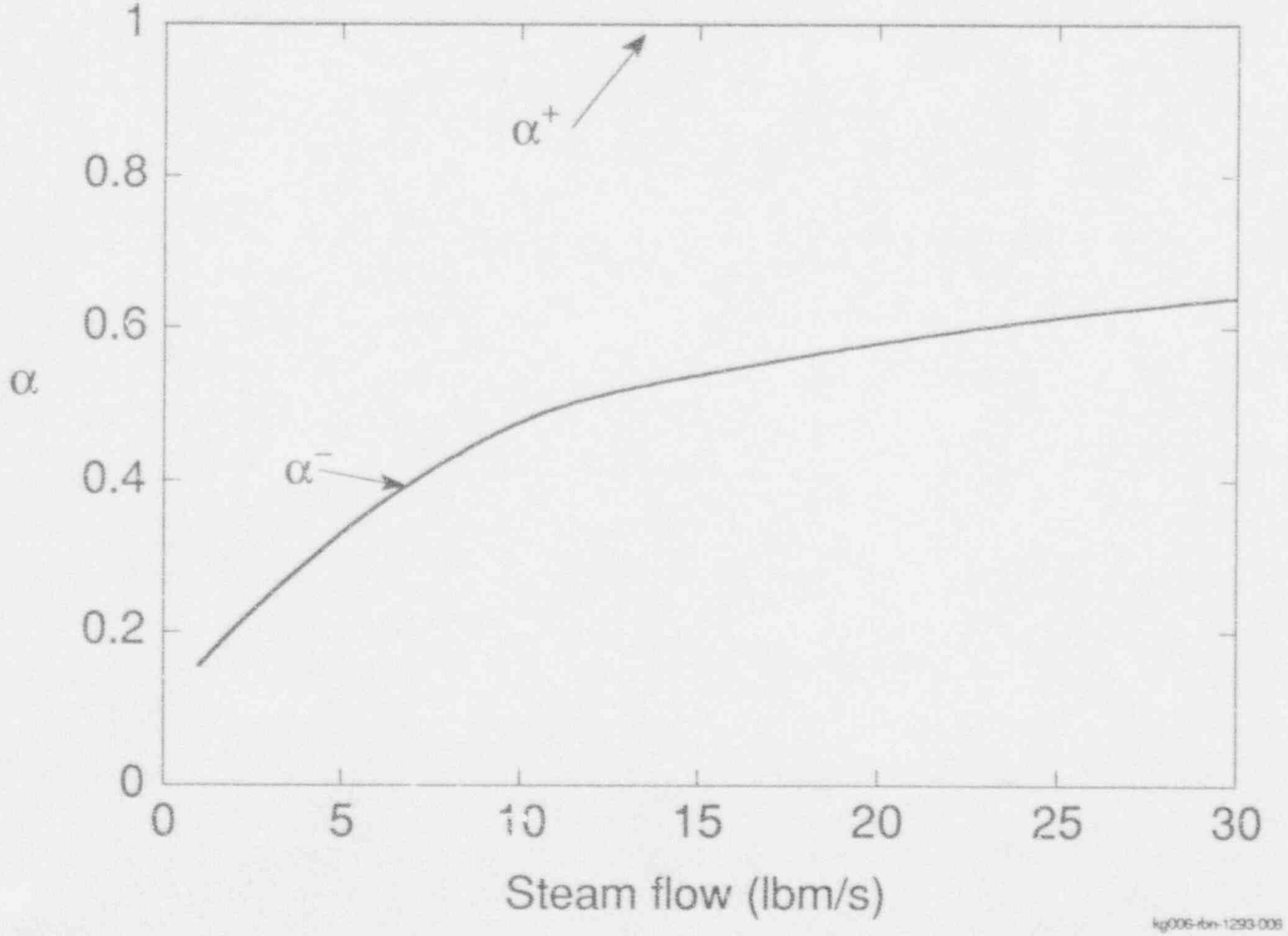
Void Fraction Above and Beneath Level vs. Steam Flow



Level Tracking Model

- Purpose:

  Recognize and account for the effects of a sharp void gradient representing a mixture level in a vertical component

- Principal features:

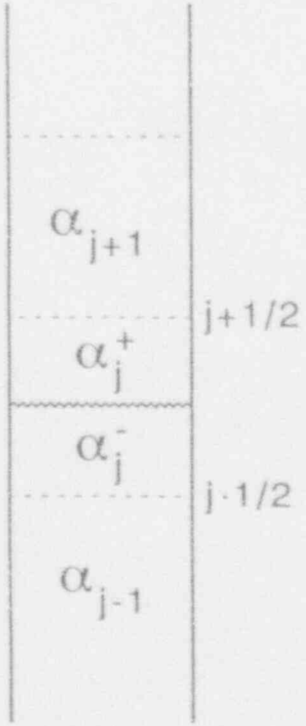
  - Level detection

  - Calculation of mixture level parameters

  - Alteration of convected terms

  - Alteration of wall heat transfer

Mixture Level Detection Logic (Focusing on volume j)



Criteria for a mixture level:

1. No level in j+1, j-1

2. $\alpha_{j+1} - \alpha_j > 0.2$

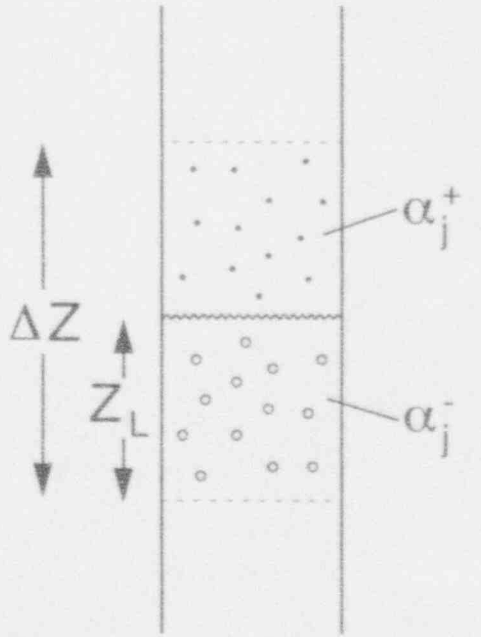
   or

   $\alpha_j - \alpha_{j-1} > 0.2$

3. $\alpha_{j+1} > 0.7$

Calculated Mixture Level Parameters

- Void fractions above and below level
- Location of level within cell
- Velocity of level



Void Fractions Above And Below Level

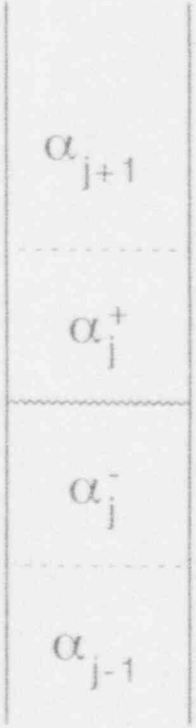
- $\alpha_j^- = \alpha_{j-1}$
- $\alpha_j^+ = \alpha_{j+1}$ (no entrainment case)

$$= 1 - \frac{G_{ent}}{\rho_f V_f} \text{ (entrainment case)*}$$

- Bounding limits

$\alpha_j^- \leq \alpha_j$

$\alpha_j^+ \geq \alpha_j$

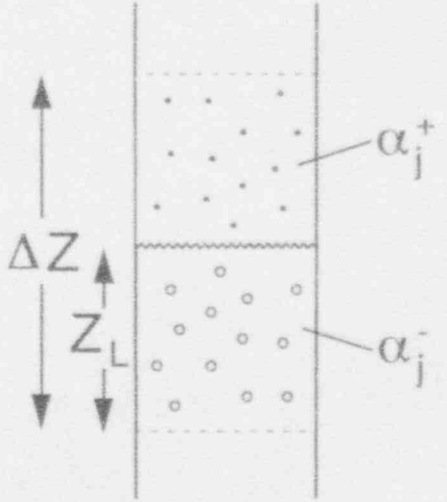


*G_{ent} is taken from A. Rosen, et al, Teploenergetikia, No. 11, p. 59, 1976.

Position and Velocity of Level

- $Z_L = \Delta Z \left(\frac{\alpha_j^+ - \alpha_j}{\alpha_j^+ - \alpha_j^-} \right)$

- $\overset{\circ}{Z}_L = \frac{\Delta Z \frac{d\alpha_j}{dt} - Z_L \frac{d\alpha_j^-}{dt} - (\Delta Z - Z_L) \frac{d\alpha_j^+}{dt}}{\alpha_j^- - \alpha_j^+}$



α_j = avg. void fraction in cell j

Consistency of the Level Model And the Jump Condition

- Position of the mixture level

$$Z_L = \Delta Z \left(\frac{\alpha_j^+ - \alpha_j}{\alpha_j^+ - \alpha_j^-} \right) \tag{1}$$

- Mixture level velocity by differentiation

$$\mathring{Z}_L = \frac{\Delta Z \frac{d\alpha_j}{dt} - Z_L \frac{d\alpha_j^-}{dt} - (\Delta Z - Z_L) \frac{d\alpha_j^+}{dt}}{\alpha_j^- - \alpha_j^+} \tag{2}$$

- Assumption: $\frac{d\alpha_j^-}{dt} = 0, \quad \frac{d\alpha_j^+}{dt} = 0$ (3)

- Simplified form of (2)

$$\mathring{Z}_L = \frac{\Delta Z \frac{d\alpha_j}{dt}}{\alpha_j^- - \alpha_j^+} \tag{4}$$

Consistency of the Level Model And the Jump Condition (cont'd)

- Total Average Volume Void Time Derivative

$$\frac{d\alpha_j}{dt} = \frac{A}{V}\left(j_g^- - j_g^+ \right) \qquad (5)$$

$$V = A\Delta Z \qquad (6)$$

$$\frac{d\alpha_j}{dt} = \frac{1}{\Delta Z}\left(j_g^- - j_g^+ \right) \qquad (7)$$

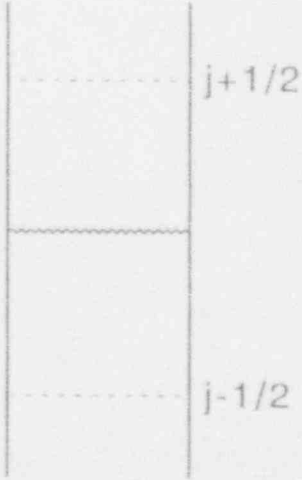
- Substitute (7) into (4)

$$\dot{Z}_L = \frac{j_g^- - j_g^+}{\alpha_j^- - \alpha_j^+}$$

Alteration of Convected Terms

- Without Level Tracking model, RELAP5 convects cell-centered, average volume properties
- Level Tracking model alters the donored void in the conservation equations:

Junction	V_g	α_g	V_f	α_f
j+1/2	> 0	α_j^+	> 0	$1 - \alpha_j^+$
	< 0	α_{j+1}	< 0	$1 - \alpha_{j+1}$
j-1/2	> 0	α_{j-1}	> 0	$1 - \alpha_{j-1}$
	< 0	α_j^-	< 0	$1 - \alpha_j^-$



Alteration of Hydrostatic Head Terms

- Without Level Tracking model, RELAP5 presumes homogeneous flow for computing hydrostatic head term

$$\Delta P_H = \frac{\rho_{j+1}\, g\Delta Z_{j+1} + \rho_j\, g\Delta Z_j}{2}$$

- Level Tracking model produces a more accurate computation using the position of the level

Alteration of Heat Transfer Calculation

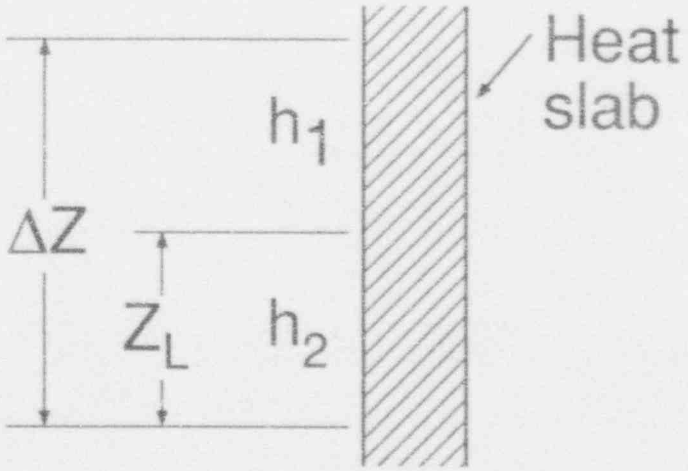
- Without Level Tracking model, wall heat transfer uses volume average properties and parameters

- Level Tracking model "partitions" heat transfer based on position of level

$$q_{WF} = h_2 (1 - \phi) (T_W - T_F)$$

$$q_{WG} = h_1 \phi (T_W - T_G)$$

$$\phi = \frac{\Delta Z - Z_L}{\Delta Z}$$



Planned Assessment

- G.E. Level Swell
- THTF Boiloff

} In-Process

- CMT, ROSA, OSU, SPES Tests

UNITED STATES NUCLEAR REGULATORY COMMISSION

FILM CONDENSATION MODELING

J.M. Kelly

January 4, 1994

FILM CONDENSATION MODEL

- * OBJECTIVE:
  - Develop a modeling approach for film condensation heat transfer within the framework of the two-fluid model.

- * FOCUS:
  - Address concerns of NRC T/H consultants and of the ACRS, specifically
    - Heat Flux Apportioning (Liquid/Vapor)
    - Surface Partitioning (Wet/Dry)
    - Effect of Noncondensables

CONTENTS

* INTRODUCTION
* BACKGROUND: Two-Fluid Model
* MODELING CONCEPT
* FILM THICKNESS
* FILM-SIDE HEAT TRANSFER RESISTANCES
* NONCONDENSABLE GASES
* ASSESSMENT PLAN
* SUMMARY

INTRODUCTION

* WHERE IS FILM CONDENSATION POTENTIALLY IMPORTANT?

- AP600:
  - CMT WALLS: Drain Period
  - PRHR TUBES: ATWS & SBLOCA (1st ADS)
  - SG TUBES: Reflux Cooling (?)
  - HORIZONTAL PIPES (?)
  - CONTAINMENT RESPONSE (CONTAIN)

- SBWR:
  - ICS TUBES:
  - PCCS TUBES:
  - CONTAINMENT DRYWELL (CONTAIN)

INTRODUCTION

What are the Regimes of Interest?

Component	Pressure (bar)	Vapor Re	Film Re	Noncondesable Gas Fraction (%)
CMT	5 - 75	1E+3 - 1E+4	< 5E+4	?
PRHR	70 , 140	< 1E+5	< 3E+4	0
ICS	70 - 80	1E+3 - 1E+4	500 - 1000	0 - 33
PCCS	2 - 5	1E+3 - 1E+4	250 - 1700	0 - 40

NOTE: Values are extremely approximate and intended for illustration only.

BACKGROUND

- * PHYSICAL REPRESENTATION
- * TWO-FLUID REPRESENTATION
- * HEAT FLUX APPORTIONING
- * SURFACE PARTITIONING
- * NONCONDENSABLE GAS EFFECTS

BACKGROUND

* PHYSICAL REPRESENTATION

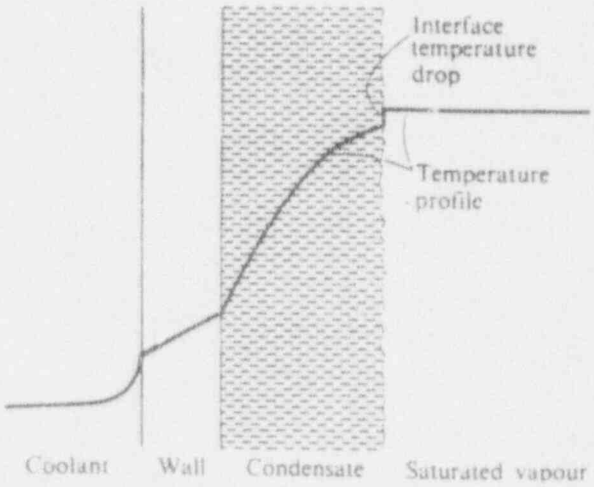


FIG. 18.2. Temperature profile for condensing a pure saturatec vapour.

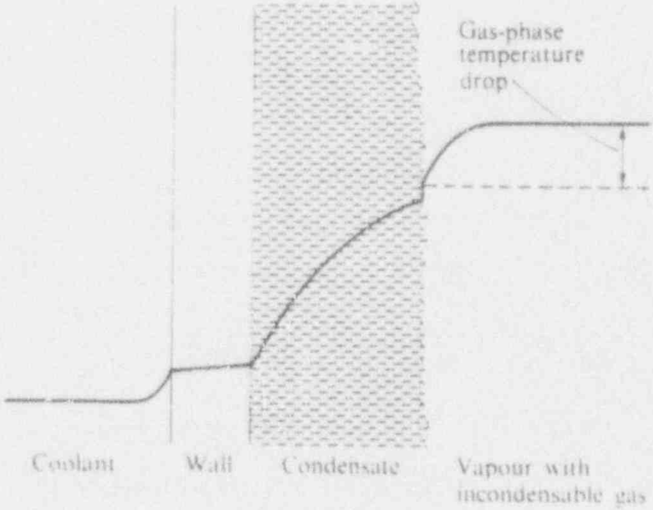


FIG. 18.3. Temperature profile for condensing a vapour which contains incondensable gas.

BACKGROUND

* TWO-FLUID REPRESENTATION

- WALL HEAT TRANSFER RATE:

$$q_w = q_{wl} + q_{wv} + q_{wi} \quad \Rightarrow \quad \text{Wall Conduction}$$

where:

$$q_{wl} = h_{wl} \cdot A_w \cdot \left(T_w - T_l\right) \quad \Rightarrow \quad \text{Liquid Energy Eqn.}$$

$$q_{wv} = h_{wv} \cdot A_w \cdot \left(T_w - T_v\right) \quad \Rightarrow \quad \text{Vapor Energy Eqn.}$$

and,

q_{wi} is the heat transfer to the interface in a near-wall fluid layer where the local fluid temperature is significantly different from the bulk temperature (e.g. subcooled nucleate boiling).

NOTE: Typically, only one of the three wall heat flux terms, q_{wl}, q_{wv}, or q_{wi}, is non-zero.

BACKGROUND

* TWO-FLUID REPRESENTATION

- CONDENSATION RATE:

$$\Gamma = \frac{q_{li} + q_{vi}}{h_{vs} - h_{ls}} + \Gamma_w$$

where:

$$q_{li} = h_{li} \cdot A_i \cdot (T_l - T_i)$$

$$q_{vi} = h_{vi} \cdot A_i \cdot (T_v - T_i)$$

and,

Γ_w is the mass transfer due to q_{wi}

- EXAMPLE FILM CONDENSATION: (Pure Steam)

$$T_i = T_{sat} = T_v$$

$$q_{vi} = q_{wv} = q_{wi} = 0$$

$$q_w = q_{wl} = q_{li}$$

$$\Gamma = \frac{q_w}{(h_{vs} - h_{ls})}$$

BACKGROUND

- HEAT FLUX APPORTIONING
  - RELAP5 MOD 3.0:
    - Wall Heat Transfer Rate:

$$q_w = h_{cond} \cdot A_w \cdot (T_w - T_{sat})$$

$$q_{wl} = q_w$$

$$q_{wv} = q_{wi} = 0$$

    - Condensation Rate:

$$\Gamma = \frac{q_w}{(h_{vs} - h_{ls})} = \frac{h_{li} \cdot A_i \cdot (T_l - T_{sat})}{(h_{vs} - h_{ls})}$$

    - Numerical Problem:
      - use of T_{sat} rather than T_l as the sink temperature decouples wall HT from fluid energy eqn. This results in 'freezing' liquid.

BACKGROUND

* HEAT FLUX APPORTIONING

- RELAP5 MOD 3.1:
  - Wall Heat Transfer Rate:

$$q_w = h_{cond} \cdot A_w \cdot (T_w - T_{sat})$$

but,

$$q_{wl} = h_{cond} \cdot A_w \cdot (T_w - T_l)$$

$$q_{wv} = q_w - q_{wl}$$

  - Condensation Rate:

$$\Gamma = \frac{q_{wl} + q_{wv}}{(h_{vs} - h_{ls})}$$

$$\Gamma = \frac{h_{li} \cdot A_i \cdot (T_l - T_{sat}) + h_{vi} \cdot A_i \cdot (T_v - T_{sat})}{(h_{vs} - h_{ls})}$$

  - Numerical Problem (Mod 3.0) Solved
  - Problem: **Not Physically Based**
    - Vapor subcooling
    - Liquid temperature depends on h_{cond}/h_{li}, and these are not consistent.

BACKGROUND

* SURFACE PARTITIONING

- RELAP5 MOD 3.0:
  - Minimum Film Thickness: Fraction of surface wet
  - Wet Surface: Film condensation
  - Dry Surface: SPV convection

- RELAP5 MOD 3.1.1:
  - No surface partitioning, if ($T_w < T_{sat}$), then condensation heat transfer with ramp to single phase liquid.

BACKGROUND

* NONCONDENSABLE GAS EFFECTS

- RELAP5 MOD 3.0:
    - Degradation factor model used, but of unknown origin and limited assessment.

- RELAP5 MOD 3.1.1:
    - UCB Vierow-Schrock Model:
        - Film-Side: Nusselt with empirical flow factor
        - Gas-Side: empirical degradation factor
    - High Pressure Option:
        - Film-Side: Max[Nusselt,Shah]
        - Gas-Side: Colburn-Hougen type model
            - no sensible heat transfer
            - Gilliland correlation for mass transfer.

FILM CONDENSATION MODEL

- CONCEPT

  - Model Objectives:
    - Condensation Rate
    - Film Subcooling
    - Large Data Base

  - Model Constraints:
    - Physically Based
    - Compatible with Two-Fluid
    - Avoid Numerical Problems

- PROBLEMS

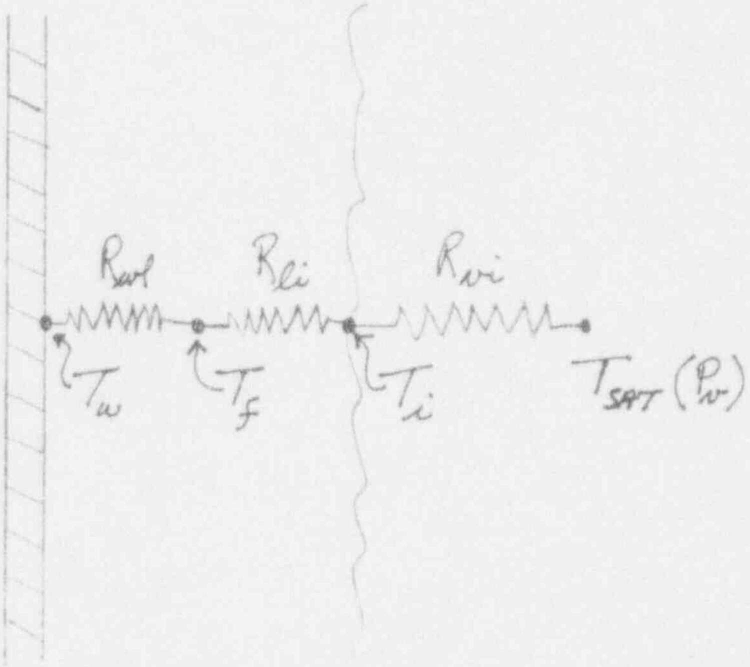
  - Condensation HTC's:
    - use T_{sat} as sink temperature

  - Interfacial HTC's:
    - how to specify h_{li}

  - Noncondensable Gas Effects:
    - better model needed

FILM CONDENSATION MODEL

* CONCEPT



- Use Heat Transfer Resistances in Series:
  - R_{wl}: wall – liquid
  - R_{li}: liquid – interface
  - R_{vi}: vapor – interface

FILM CONDENSATION MODEL

* CONCEPT

- Film-Side Resistance:
  - Condensation HTC:

$$h_{cond} \cdot A_w = \frac{1}{R_{wl} + R_{li}}$$

  - Film Heating/Cooling HTC:

$$h_{wl} = h_{heat}$$

$$R_{wl} = \frac{1}{h_{heat} \cdot A_w}$$

  - Infer Liquid-Interface Resistance:

$$R_{li} = \frac{1}{h_{cond} \cdot A_w} - R_{wl}$$

  - Use Mass Transfer Data as a Check?
    - Data scatters widely .
    - Sc << Pr, penetration depth is much less than thermal boundary layer, different controlling phenomena ?

FILM CONDENSATION MODEL

* CONCEPT

- Vapor-Side Resistance:

  - Pure Steam: **Negligible**

$$Nu_{vi} = 10^4 \cdot F_6 \cdot F_{10}$$

where

F_6 = smoothing between SCV and SHV

F_{10} = smoothing as film disappears

  - Vapor with Noncondensables:
    - use turbulent mass transfer coefficient
    - find interface temperature, T_i

NOTE: From kinetic theory (Butterworth & Hewitt, 1977)

$$h_{vi} \approx 10^7 \quad (W/m^2 \cdot K)$$

for pure water vapor at 1 bar.

FILM THICKNESS

* IMPORTANCE:

- Heat transfer resistances scale with film thickness.

* FALLING FILMS

- Laminar Smooth
- Laminar Wavy
- Turbulent
- Proposed Model

* SHEARED FILMS: Cocurrent Downflow

- Laminar Smooth
- 'Universal' Dimensionless Thickness
- Interfacial Shear Model
- Effect of Entrainment
- Assessment Results

FILM THICKNESS

* FALLING FILMS

- Laminar Smooth: Nusselt

$$m^{*} = \left(\tfrac{3}{4}\cdot \mathrm{Re}_f\right)^{\frac{1}{3}}$$

where

$$m^{*} = m\cdot\left(\frac{\rho_f\cdot\Delta\rho\cdot g}{\mu_f^2}\right)^{\frac{1}{3}}$$

$$\mathrm{Re}_f = \frac{4\Gamma'}{\mu_f}$$

and,

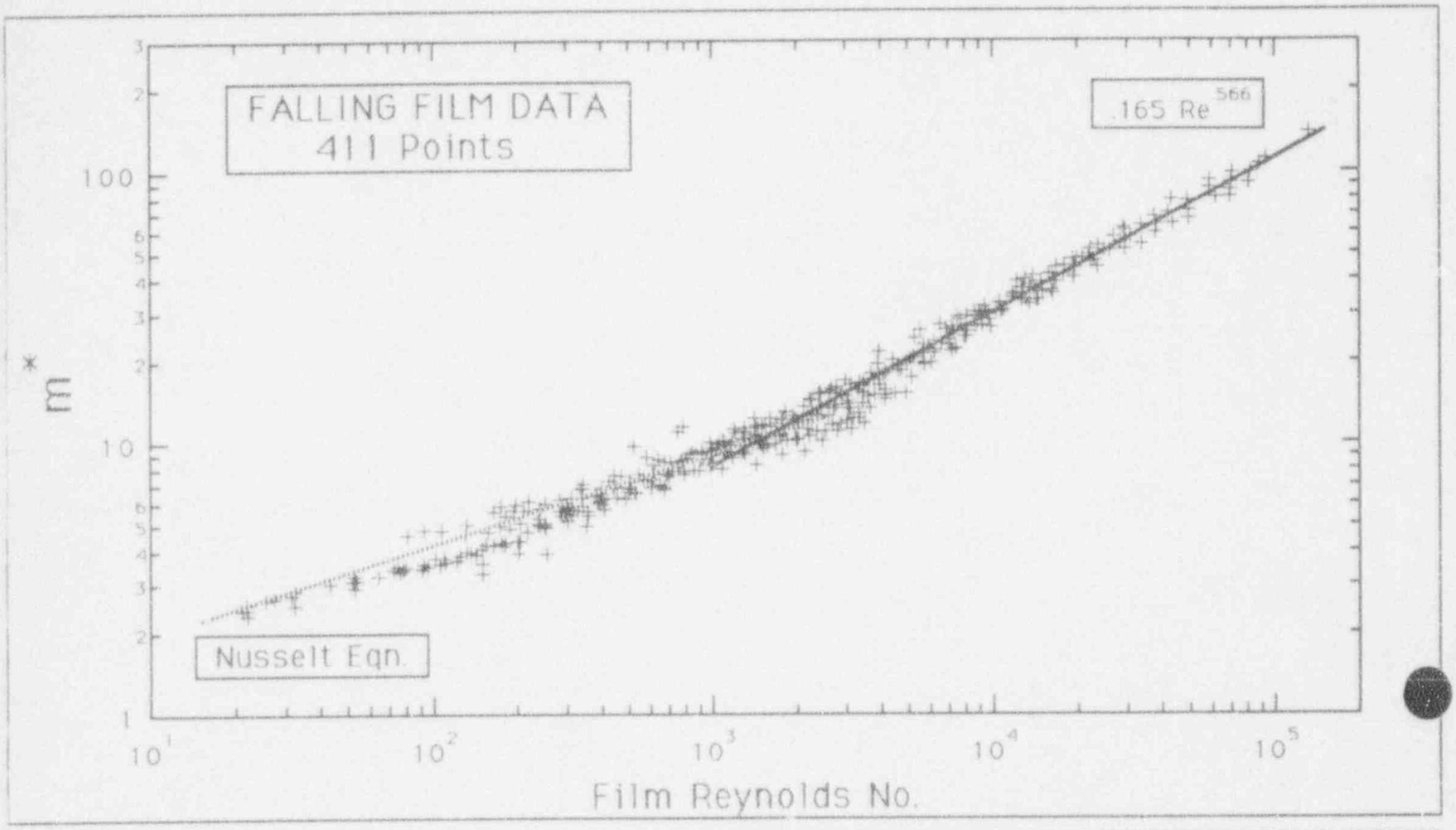
Γ' is the flowrate per unit perimeter

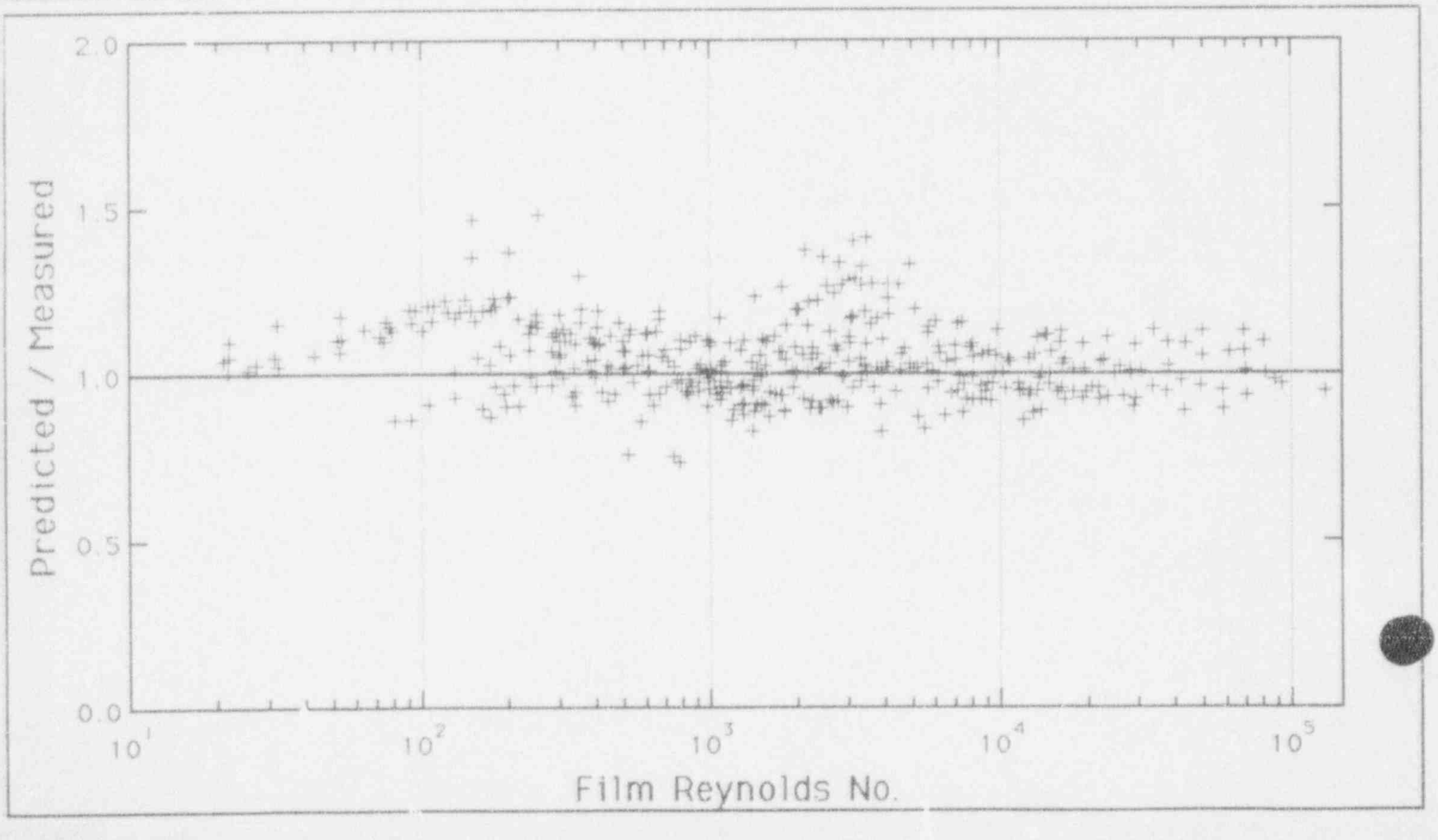
- Laminar Wavy: Kapitsa (1948)

$$m^{*} = \left(\tfrac{2.4}{4}\cdot \mathrm{Re}_f\right)^{\frac{1}{3}}$$

FALLING FILM: THICKNESS

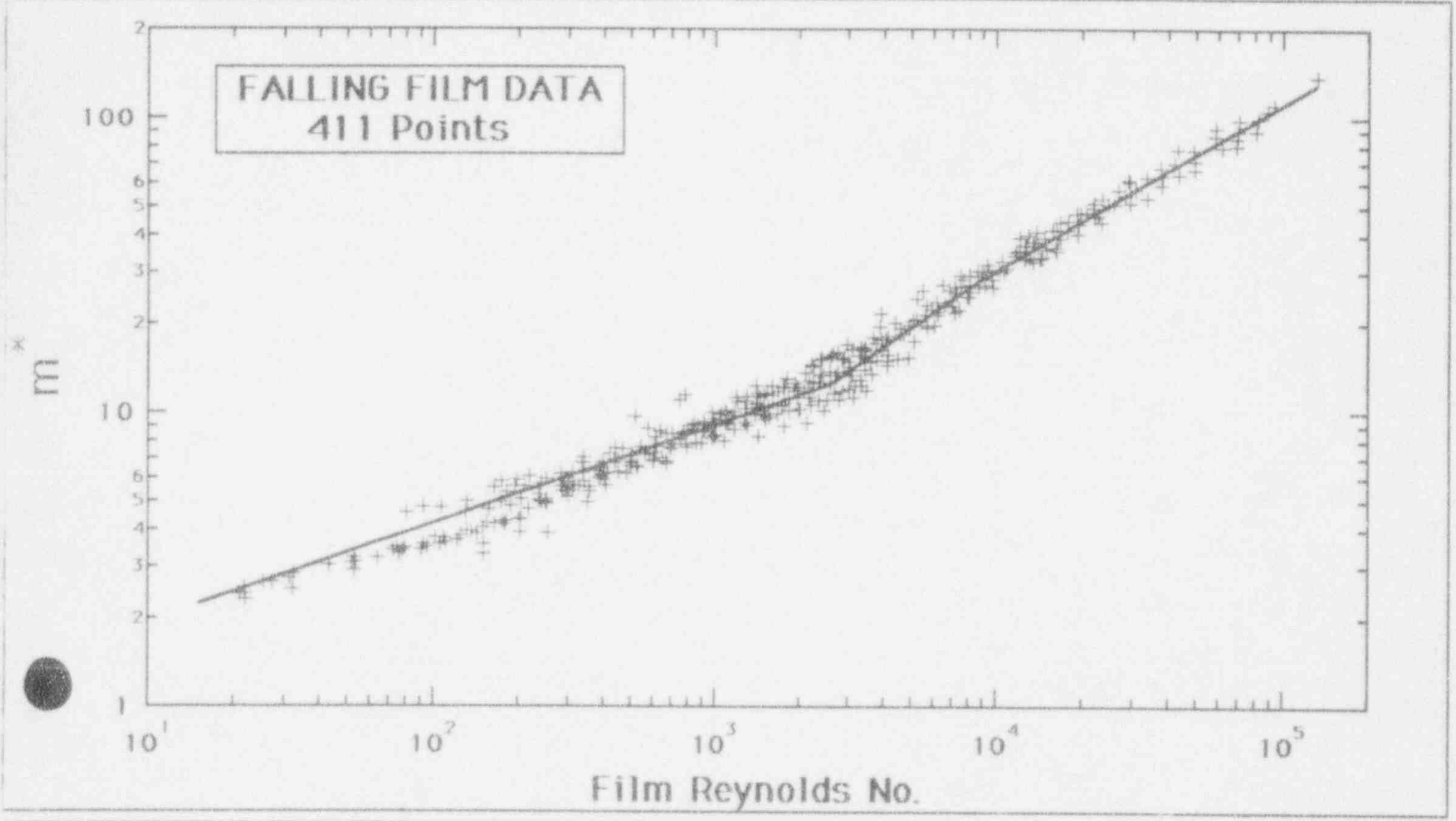
$$m^* = \max[\text{Nusselt}, .165 \cdot Re^{.566}]$$

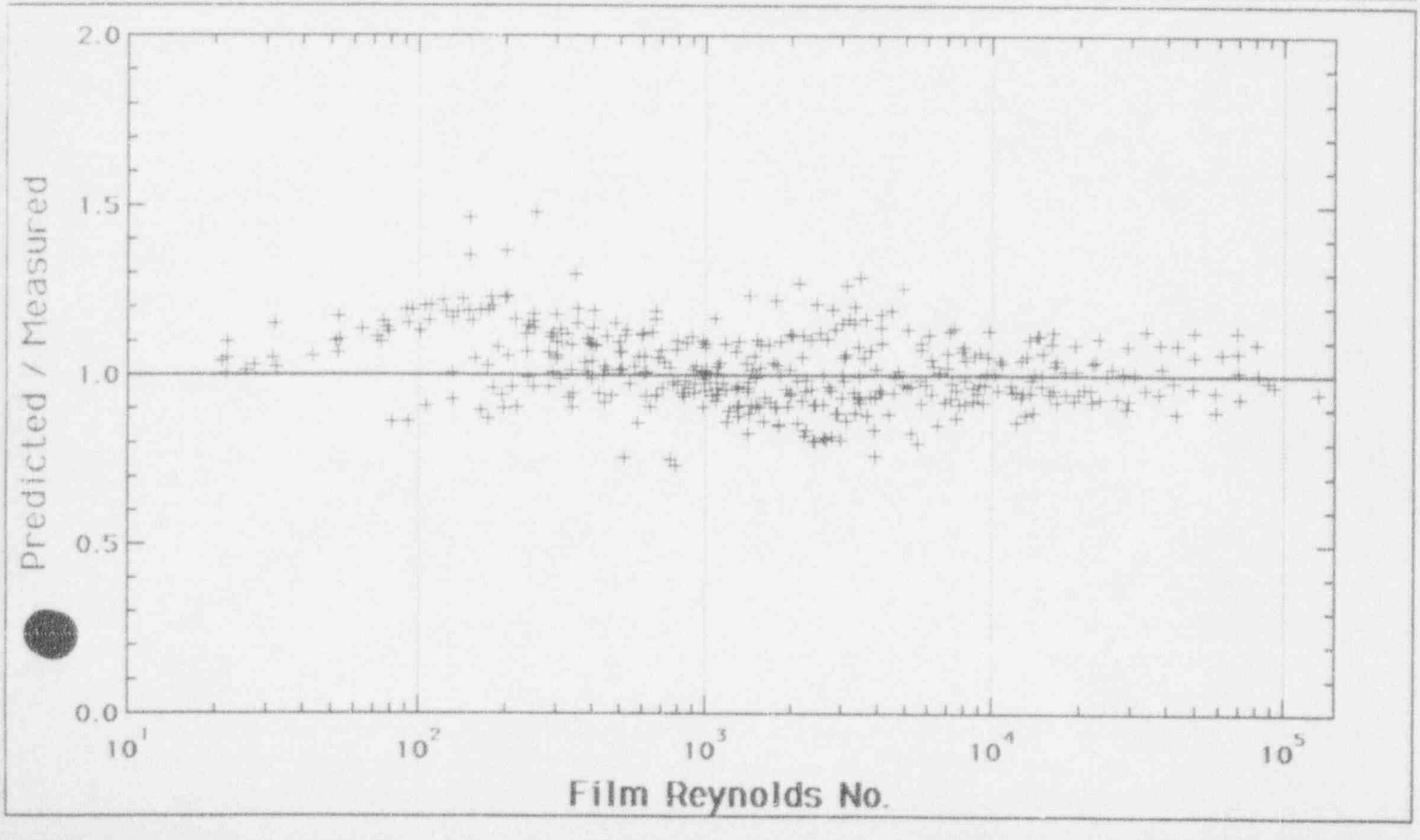




FALLING FILM: THICKNESS

- $$m^* = \max\left\{\textbf{Nusselt}, \min\left[.0663\cdot Re^{\frac{2}{3}}, .165\cdot Re^{.566}\right]\right\}$$





FILM THICKNESS

* FALLING FILMS

- Proposed Model:

$$m_L^* = .9085 \cdot \mathrm{Re}_f^{\frac{1}{3}}$$

$$m_T^* = .165 \cdot \mathrm{Re}_f^{.566}$$

- Wall Friction Factors:

$$f_L = \frac{24}{\mathrm{Re}_f}$$

$$f_T = \frac{.144}{\mathrm{Re}_f^{0.3}}$$

FILM THICKNESS

* SHEARED FILMS

- RELAP5:
  - Void fraction, and hence film thickness, results from solution of field equations.

- CONSTITUTIVE MODELS REQUIRED:
  - Wall friction factor
  - Interfacial friction factor
  - Entrainment fraction
  - Momentum transfer due to phase change

FILM THICKNESS

* SHEARED FILMS – Laminar Smooth

- Shear Stress Distribution:

$$\tau = \tau_i + \Delta\rho \cdot g \cdot (m - y)$$

- Velocity Profile:

$$V_f = \frac{1}{\mu} \cdot \left\{ \tau_i \cdot y + \Delta\rho \cdot g \cdot \left(m \cdot y - \tfrac{1}{2} y^2 \right) \right\}$$

- Average Velocity:

$$\overline{V_f} = \frac{m}{\mu} \cdot \left\{ \tfrac{1}{2} \cdot \tau_i + \tfrac{1}{3} \Delta\rho \cdot g \cdot m \right\}$$

- Film Reynolds No.

$$\mathrm{Re}_f = 2 \cdot \rho_f \cdot \left(\frac{m}{\mu} \right)^2 \cdot \left\{ \tau_i + \tfrac{2}{3} \Delta\rho \cdot g \cdot m \right\}$$

FILM THICKNESS

* SHEARED FILMS - Laminar Smooth

- Dimensionless Film Thickness:

$$m^{+} = 0.7071 \cdot \mathrm{Re}_f^{\frac{1}{2}}$$

where

$$m^{+} = m \cdot \left(\frac{\tau_c}{\rho_f} \right)^{\frac{1}{2}} \cdot \frac{1}{\nu_f}$$

$$\tau_c = \tau_i + \tfrac{2}{3} \cdot \rho_f \cdot g \cdot m$$

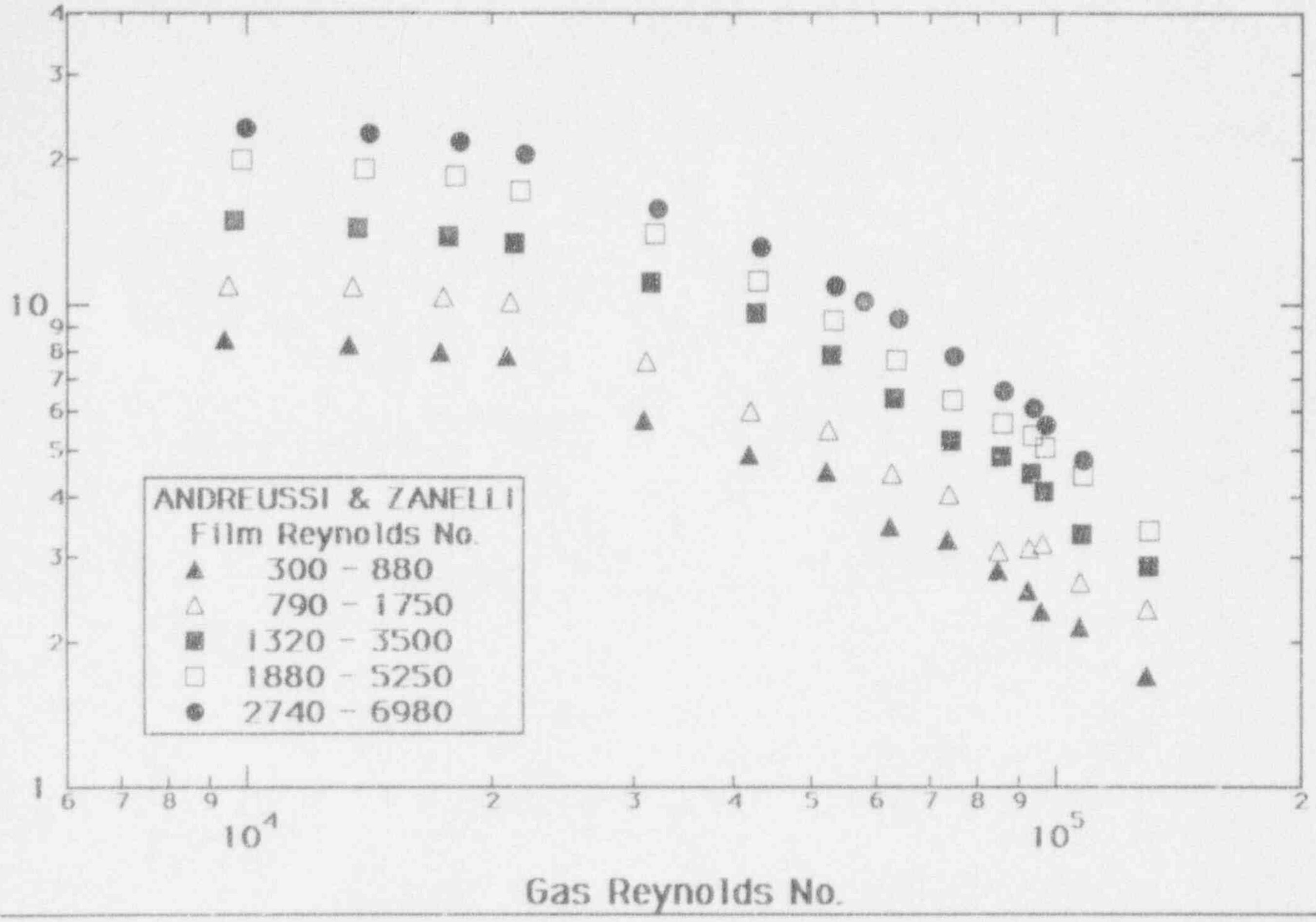
- Note: for a falling film:

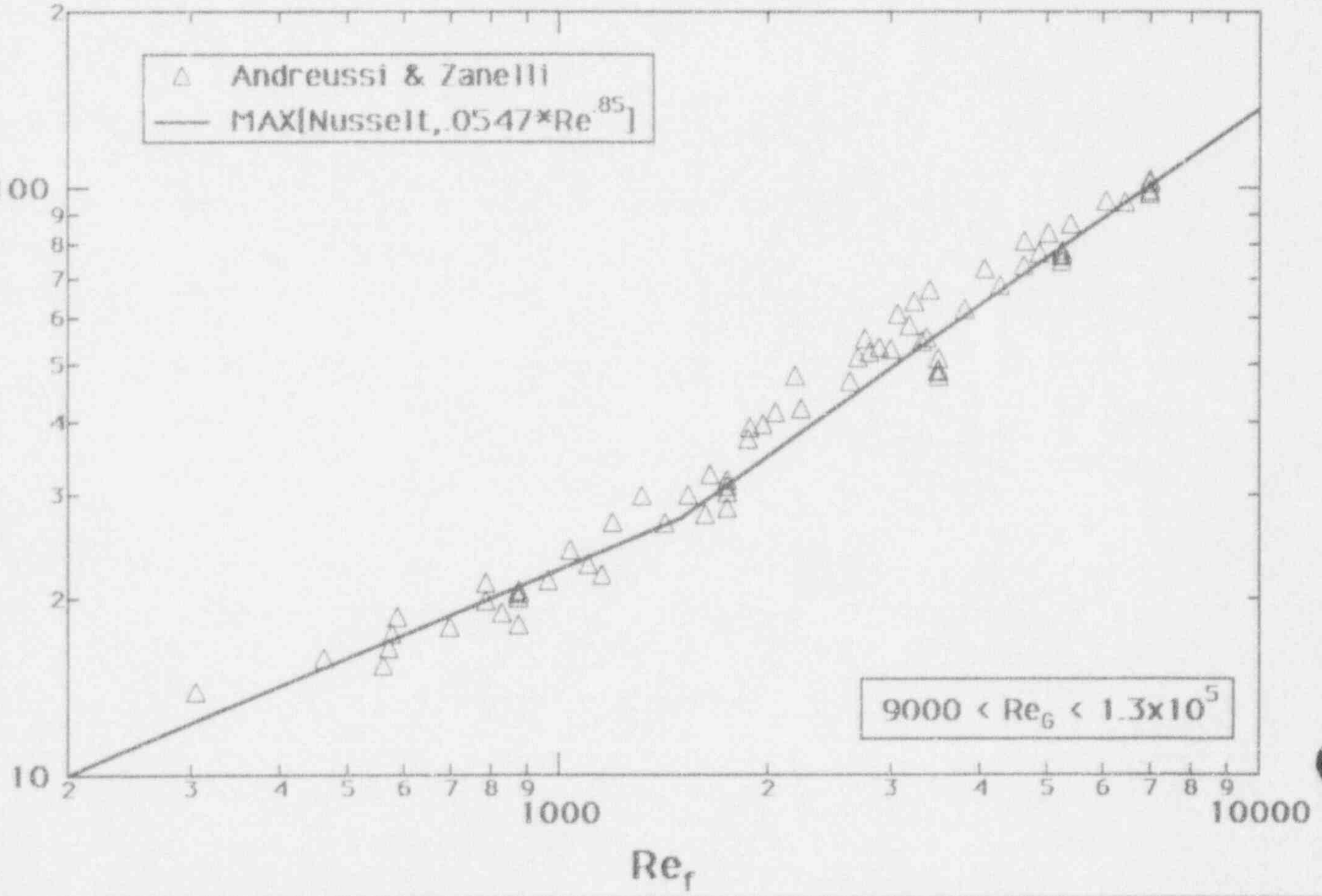
$$m^{+} = \left(\tfrac{2}{3}\right)^{\frac{1}{2}} \cdot \left(m^{*}\right)^{\frac{3}{2}}$$

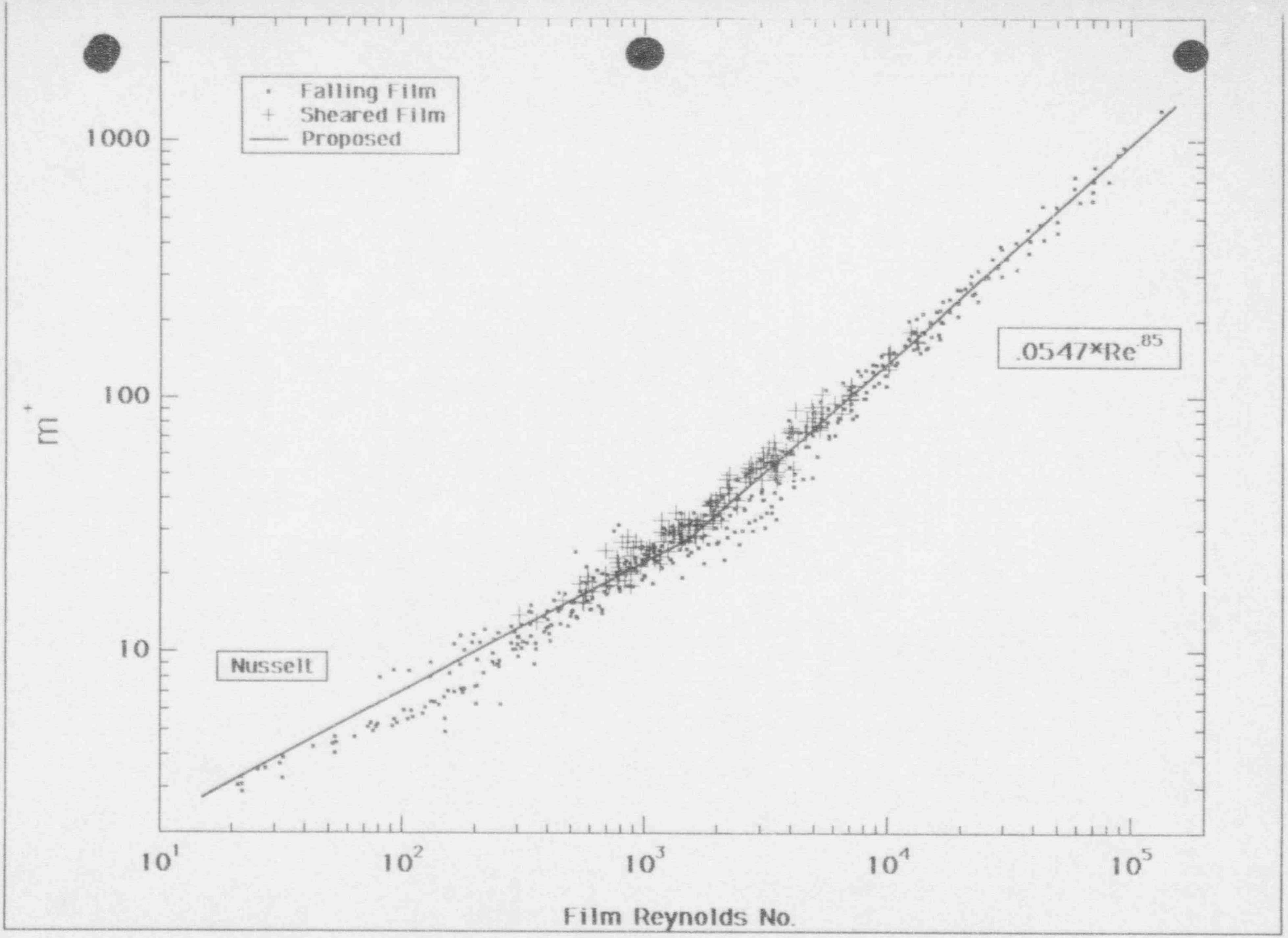
so, for a turbulent falling film:

$$m_f^{+} = .0547 \cdot \mathrm{Re}_f^{85}$$

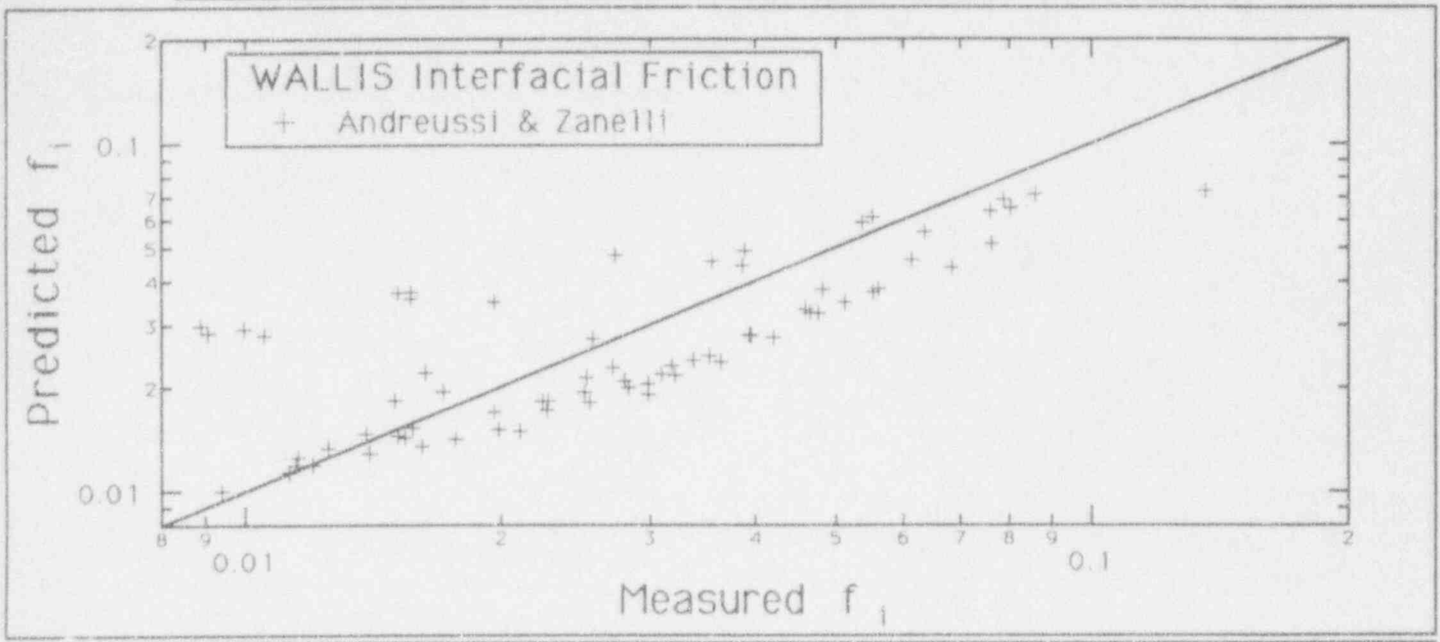
* QUESTION: is m^+ a unique function of Re for turbulent sheared films also ?

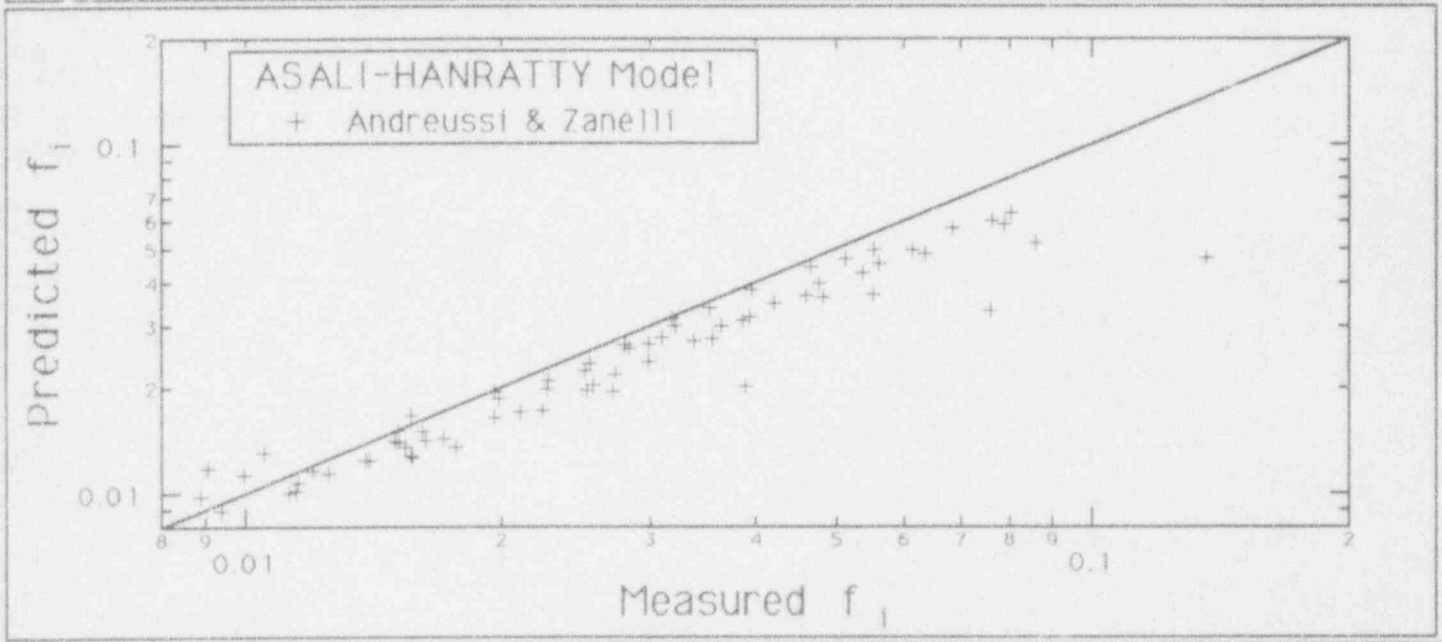

ANDREUSSI & ZANELLI
Film Reynolds No.
▲ 300 - 880
△ 790 - 1750
■ 1320 - 3500
□ 1880 - 5250
● 2740 - 6980
m*
1
2
3
4
5
6
7
8
9
10
2
3
4
6
7
8
9
10^4
2
3
4
5
6
7
8
9
10^5
2
Gas Reynolds No.

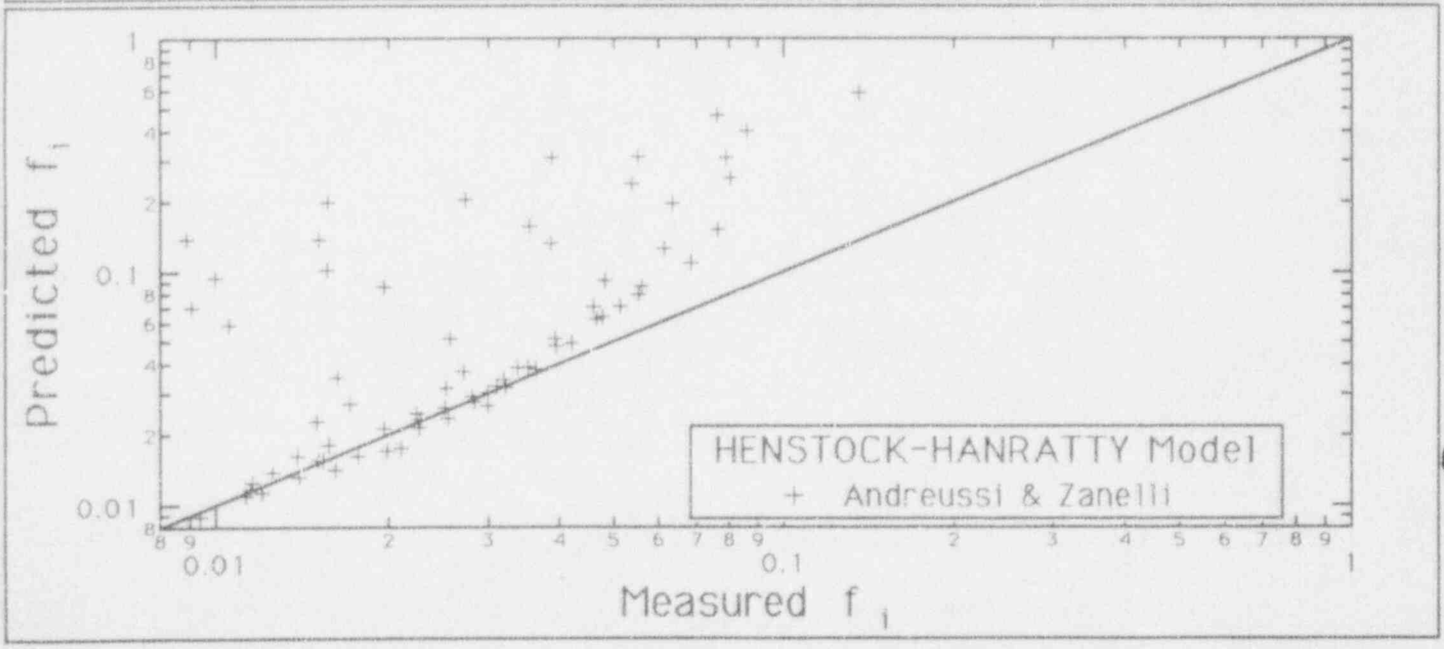

△ Andreussi & Zanelli
— MAX[Nusselt,.0547*Re$^{.85}$]
9000 < Re_G < $1.3x10^5$
m^+
10
2
3
4
5
6
7
8
9
100
2
2
3
4
5
6
7
8
9
1000
2
3
4
5
6
7
8
9
10000
Re_f


Falling Film
Sheared Film
Proposed
1000
100
10
m^+
Nusselt
.0547×Re$^{.85}$
10^1
10^2
10^3
10^4
10^5
Film Reynolds No.

INTERFACIAL FRICTION







FILM THICKNESS

* INTERFACIAL FRICTION MODEL

- Interfacial Shear Stress:

$$\tau_i = \tfrac{1}{2} \cdot f_i \cdot \rho_v \cdot \left(V_v - 1.5 \cdot V_l\right)^2$$

- Interfacial Friction Factor:

$$\frac{1}{\sqrt{f_i}} = -1.8 \cdot \log\left\{ \frac{6.9}{\mathrm{Re}_v} + \left(\frac{\varepsilon/d}{3.7}\right)^{1.11} \right\}$$

- Interfacial Roughness:

$$\frac{\varepsilon}{d} = \mathrm{function}\left(m^+, \mathrm{Re}_v\right)$$

  - to be determined.