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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

IN THE MATTER OF:

SUBCOMMITTEE MEETING

on

EMERGENCY CORE COOLING SYSTEM

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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

SUBCOMMITTEE MEETING

on

EMERGENCY CORE COOLING SYSTEM

- - -

Westbank Motel Coffee Shop,
475 River Parkway
Idaho Falls, Idaho

Tuesday, 28 August 1979

The ACRS Subcommittee on Emergency Core Cooling System met, pursuant to adjournment, at 8:00 a.m., Dr. Milton S. Plesset, chairman of the subcommittee, presiding.

PRESENT:

DR. MILTON S. PLESSET, Chairman of the Subcommittee

MR. WILLIAM M. MATHIS, Member

P R O C E E D I N G S

DR. PLESSET : Let's reconvene. We have one presentation from yesterday that I would like to go to immediately. That is the RELAP 5 simulation of the Marviken test.

Mr. Trapp.

MR. TRAPP: Thank you. You heard most of the results from RELAP yesterday; but there is one test result that we have run. We ran the first Marviken almost a year ago and got results recently. We would like to go over those test results.

Marviken is unique in one sense in that it is a full-scale experiment. The test itself, Test 4 and 24, are blowdowns through a half-meter nozzle. It's a good-sized nozzle. We were mostly interested and learned most from this problem in the area of subcooled choking. We used this problem to help us understand that phenomena, to help us to develop critical flow models for the subcooled flow regions. They are very important in the small-break analysis.

(Slide.)

Basically, Marviken is a vessel with a discharge pipe at the bottom through which the blowdown takes place. There's saturated water at the top of the vessel which turns into, as the vessel key pressurizes, a two-phased mixture, which result in a low quality choking at the ends of these tests. Then there is about 30 degrees subcooling at the bottom of the tank which results in the initial subcooled critical flow criterion in the early parts of the run, in the early parts of the tests.

(Slide.)

I show this slide only to show that the nodalization is uniform throughout the pipe. Basically in meter cells, a meter in length, both in the tank and in the discharge pipe, and in the nozzle itself. The run times for our Marviken runs, the test results are about 50 seconds to the emptying of the vessel. Our computer runs are anywhere from a minute to a minute-and-a-half on the CDC 7600.

I'll just show you some results from Test 4 first. That is a test with a nozzle with an L/D of 23. Test 4 has a larger nozzle. This result shows both the code calculations and then the data on the overlay.

(Slide.)

You can see a little bit of nonequilibrium which the code follows that took place in the experiment. That's really not significant when it comes to the critical flow behavior, but it is something that is modeled in the code calculations.

This slide, the next slide, shows the flow rates as a function of time, first for the calculations. This shows dramatically or at least visually the two regions of operation of the nozzle that are in this test. Initially it's subcooled, critical flow in the first part of the test, where the velocities are very high. As it depressurizes, it goes through some transition region. The latter part of the test is basically 's3

low quality, two-phased flow. Then an overlay of the data on top of the code calculations. The data is actually measured in two different ways, resulting in the two experimental curves.

(Slide.)

I would like to take a moment to show you why we went to a choking criterion instead of using a fine nodalization, and then take a few moments to explain the actual critical flow model we used in this calculation.

This slide shows the sound speed which translate into critical flow speeds for different equation sets. They were done by a characteristic analysis looking at the derivative terms in different equation sets.

The first is a standard two-fluid model. Analysis is taken out of Wallis where he calculates the sound speeds in a two-fluid mixture. The sound speed varies as quality varies or vapor fraction varies between the pure phase gas sound speed and the pure phase liquid sound speed. That's what he calls a stratified sound speed. The point to any in this two-fluid model sound speed is that it's very much higher than what is actually observed in experimental results which is somewhere near the equilibrium results.

That's the result that comes out of all the two-fluid flow model analyses.

The next one down here, I have indicated that by "homogeneous."

cs4

If you want to analyze the sound speed in a system of differential equations, it's basically the derivative terms that enter into the characteristic analysis that determine what the sound speed or what the choke velocity will be. This system of equations has been modified by adding a rate derivative or an inertial drag, a drag between the phases that depend upon the relative acceleration. That changes the time derivatives and space derivatives in the system of differential equations. That enters into the characteristic analysis and changes the sonic speeds.

With that kind of term added, with a fairly large coefficient for that relative inertial drag, the sound speed becomes greatly depressed and comes more in line with the data as far as order of magnitude.

The last system of differential equations that's analyzed here is one in which the mass transfer rate is assumed to be the equilibrium mass transfer rate which again depends upon derivatives. The equilibrium mass transfer depending upon time rate of change of quality or pressure, depending upon how you express it. Those terms result in again another increment in the depression of the sound speed.

It turns out in our equations, the two-fluid model equation, we have all the terms that are involved in this stratified sound speed as basic convective, time derivative terms. But the additional derivative terms that result in a

cs5

lowering of this sound speed which brings it more in line with the data are derivative terms that appear in the interphase drag and derivative terms that appear in the mass transfer.

We felt because of the state of the art that there's not very many people that have tried to find correlations for the derivative terms in the interphase drag. There are not many mass transfer models that depend upon the derivative terms. They are mostly relaxation times.

Those models for interphase drag and mass transfer, because they don't involve derivatives, would give an analysis of a stratified type of sound speed. If our numerical scheme is gridded up to follow those equations, we would also predict such a sound speed.

For that reason, we decided it would be better to go back and look at the choking phenomena separately from our basic equations and then impose a choking criterion that takes into account the mass transfer and the inertial drag just in that choking model.

We added an inertial drag and ran it with an equilibrium and used a small cell size. I think it was $1x10^{-5}$ for the delta X. That did predict a sound speed within about 1 percent of each one of these curves at a particular void fraction. The analysis is thus borne out by the code results.

The next one shows the idealized picture we have. I am going to just go over that this morning.

cs6

(Slide.)

We looked for a start at an idealized flow, a homogeneous equilibrium flow in the subcooled mode to see what was happening to give us guidance for our model. This idealized model is basically what was used to generate those Marviken Test 4 results.

Here we have a nozzle. Assume it has a high upstream pressure in this direction, water flowing through the nozzle. We begin to lower the downstream pressure to see what happens. In this stream, with a high downstream pressure, it flows water through the nozzle. As you begin to lower the downstream pressure, eventually a point is reached where the saturation pressures reach the throat. As we try to lower the downstream pressure further, there will be flashing in this idealized case that will take place at the throat; and further lowering of the downstream pressure essentially fills up the diffuser section.

The picture we have at that time, which is the operation in the subcooled critical flow mode, looks something like this.

(Slide.)

Upstream, there's water that has a high sound speed so it's not choked by a Mach number 1 in the water but is governed by the new type equation, a momentum equation. Saturation pressures reach the throat. The velocity at the throat can be calculated from the upstream pressure and the saturation pressure.

The velocity at the throat is really much higher than the sound speed, the homogeneous equilibrium sound speed that exists just downstream of that throat section. The two-phased mixture has a low sound speed, as seen by the flat part of that flow rate in those tests at the end of the run; whereas, during the subcooled part where the flow is higher, the velocity is actually higher than the two-phased sound speed so that the flow in the downstream part of the nozzle is actually supersonic. It is really, in this idealized case -- there is no place where the Mach numbers equal 1.

This is basically the criterion that we use. We calculated a saturation pressure at the throat, calculated from Bernoulli's equation of velocity. We use that as the critical flow until, as we depressurize, the velocity begins to slow down. Eventually this velocity equals the two-phase sound speed at the throat.

At that time we have a transition. From now on if the pressure in the tank becomes any lower, there will be a two-phazed mixture, not only at the throat but back upstream. We can use our regular two-phased critical flow criterion, which is not quite the homogeneous equilibrium sound speed. We analyzed a set of differential equations which we believe have ccrrect models for mass transfer and inertial drag and analyzed a sound speed from that mixture which we impose in the two-phase region.

Those results, like I said, were run with this idealized nozzle, choking criterion. We found just by an experimental adjustment that we got a little better result with the experiment if we used a 5 percent area reduction, which was done in the case of those slides.

After we had done Test 4, about a year ago, we felt very encouraged. We were quite excited because the results were quite good. We were asked to do a blind calculation on Test 24, I guess about a month ago. When we ran our result, we did the same thing we did on Test 4 as far as the critical flow model and got about 10 or 15 percent error in our mass flow rates which caused us to go back and try to investigate exactly what was going on.

One thing we did right away, we looked at our results and thought, "Let's see if we can make one adjustment." We will make that based on the fact we like the flow rate to become higher.

We thought if there's some nonequilibrium in the throat field, then the throat pressure would be less than the saturation pressure indicated by the nonequilibrium. We made one run with just the throat pressure some percentage of saturation. We used .87 percent of saturation to give us an indication of the nonequilibrium.

We made a second run to see if we were in the ballpark.

These results are from that case.

(Slide.)

This was on Test 24 with a short nozzle. There was the pressure in the tank at the bottom. There was the data. The same kind of phenomena. We were close but not as close as we were on Test 4.

The next slide shows the flow rates for that same Test 24.

(Slide.)

Both the code calculations showing the subcooled region and the two-phased region, then the data for that problem.

(Slide.)

Because we saw a difference in the modeling of these two tests, one of them using essentially equilibrium and the other to get reasonable results, we used a nonequilibrium throat pressure that was 7/8 of the saturation pressure.

MR. THEOFANOUS: Excuse me. Did you do that all through the calculation?

MR. TRAPP: Only in the subcooled part. The things I am trying to emphasize relate to our subcooled model right now.

This caused us to go back and say, "Let's look at the data and see if we can figure out a little more of what is happening."

cs10

We took a preliminary look at some of the data. This is what we came up with, again looking at the subcooled region, basically at 10 seconds. We picked 10 seconds to make our first look at the data for Test 4 and Test 24. We also did it for Test 22, which is on the chart, and did it for four other tests. We did it for all the nozzles that have a half-meter diameter and a nozzle with a .3 diameter.

We noticed this. All the results on the slide are just the data. Saturation pressure -- look at Test 4, in particular. Saturation pressure at the nozzle; the throat pressure measured in the experiment. For Test 4 the ratio of the saturation pressure to the throat pressure. There is about .96 parts of equilibrium. The throat pressure was 4 percentage points down from saturation.

Then we could take the throat pressure in the experiment and the pressure at the bottom of the tank and from Bernoulli's equation calculate the velocity that would exist at the throat.

Taking that velocity and the actual measured flow rate, we can figure out how much of the pipe must have been flowing full and get an area reduction. For Test 4 it came out to .84, a 2-D effect for air conduction.

We did the same for a number of tests. In all the tests, we basically noticed, in the subcooled region, about a 15 percent area reduction.

The worst case was Test 24.

There appears to be about a 15 percent vena contracta effect even in the Marviken nozzle.

It turns out the equilibrium is about .9 in every test except this Test 24, which has a very short nozzle. In fact, .96 was the highest one out of the six tests we ran the data through for.

We feel we may have a little better understanding that there is some nonequilibrium going on in the Marviken tests, especially in the short nozzle that was chopping off. Apparently there is a fairly consistent vena contracta effect in all the tests. We intend to go back and run some of these tests over, using these parameters in our critical flow model, to see if we have a correct understanding of what was going on in those tests.

All the tests show something, but Test 4 gives us another problem that comes up in all of these critical flow or experimental results. We ran this Test 4 with -- we ran it with PSAT. We began to ask how did we get such good results when it doesn't compare with the data?

It turns out this nonequilibrium effect and this area effect are in opposite directions. If you take this idealized PSAT flowing area as a reference case, when the throat pressure is less than the saturation pressure at the throat, that causes the velocity to be faster at the throat;

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right?

So that would be more mass flow rate than the idealized case. Because there is a vena contracta effect, that sees less area. It goes back in the direction of the idealized case.

We said, "Let's assume we had a PSAT here in the data and calculate what the data's area reduction would be." It turned out to be .94. We ran with a .95 on the first test.

It shows we can get reasonable results. We have to be very careful. We might be not modeling, what we think we are modeling. We might be mixing up effects.

(Slide.)

One of the conclusions I have from our analysis is that for experimental results it would be helpful to have extremely smooth nozzles to eliminate geometric effects. That way we could have a better handle on what is really going on in the actual critical flow process. It would give us better control over the flow rate of the depressurization rate, which is the thing that is being controlled in those experiments.

We also saw, in just a summary, that a critical flow model must be used in these codes unless one is willing to develop the correlations, the time-dependent -- not time-depemdent -- rate-dependent correlations for interphase and mass transfer.

Two of the effects do complicate the pictures. Sometimes they ard hard to sort out. If we hadn't had pressure measurements in the throat of the nozzle, we could not have sorted out what was due to the nonequilibrium and what was due to the two-dimensional effects. The Marviken tests do show some nonequilibrium, as indicated by those numbers in that data analysis.

Thank you.

DR. PLESSET : Thank you, Mr. Trapp.

Any comments or questions?

MR. THEOFANOUS: What is the critical pressure ratio in these tests?

MR. TRAPP: Vic, do you remember that?

MR. RANSOM: What do you mean?

MR. TRAPP: You mean in the entrance to the throat?

MR. THEOFANOUS: Yes.

MR. TRAPP: I think this chopped-off one was something like .6.

MR. THEOFANOUS: .6?

MR. TRAPP: Wasn't it?

MR. RANSOM: I guess I am confused.

MR. THEOFANOUS: Critical pressure ratio.

MR. RANSOM: The actual measured value was about .6, yes.

MR. THEOFANOUS: In the subcooled region.

cs?

MR. RANSOM: That's relative to the saturation pressure.

MR. THEOFANOUS: No. Critical pressure ratio just compared to the stagnation pressure.

MR. TRAPP: Back in the tank?

MR. THEOFANOUS: Yes, back in the tank.

MR. TRAPP: I think that is reasonable, .6. Some of them went down to -- I think the chopped-off nozzle went down to .4. I am sure it did. I can remember the graph now. The minimum is around .4.

DR. FABIC: The pressure taps in the exit, they are not that close together, to each other?

MR. TRAPP: No. Farther down --

DR. FABIC: But you see what you call the throat pressure is right at the end of that. In fact, only the latest nozzles have an abrupt ending at the end. Others had steps. It complicates the picture much, much more. I don't want to go into that one.

The later nozzles have an abrupt ending. The pressure traps near the end are not that close together. Yet, we see that there is a very steep pressure flow line near the end. I don't think we have knowledge of the throat pressure. You can't claim much about the mechanistic models based on that knowledge.

MR. THEOFANOUS: How about your calculated pressure

cs

ratio.

MR. TRAPP: Can I say something? The longer nozzles, like in the Test 4 with an L/D of 3, there were measurements at the throat. The measurements Stan was talking about are three measurements downstream.

Even there, to get the throat pressures, the taps are not that close and we just took the minimum one. They do show --

MR. THEOFANOUS: What would you be calculating ideally based on your model?

MR. TRAPP: In Test 4, we calculated about the same as the data in our idealized case. There were only like nine --

MR. THEOFANOUS: I mean if you took the pressure that your model calculates right at the throat?

DR. FABIC: I am confused as to what he calls the throat.

MR. TRAPP: We took the minimum -- they have some pressure taps all the way along the discharge pipe with a larger number right at the exit; but up near the entrance to the nozzle, there's three or four taps. We took the first one, which really turns out to be the minimum one. If you plot a pressure line down the pipe, taking all the taps, the ends, the ones near the throat, the minimum always appears at the first pressure tap. We took that one.

DR. FABIC: The minimum is right at the end of the pipe?

MR. TRAPP: Okay. There is a minimum and it goes up and rapidly falls.

DR. FABIC: That is just the vena contracta effect?

MR. TRAPP: That's what I was calling the throat.

MR. RANSOM: The minimum pressure point -- John, maybe you can clarify that --

(Simultaneous discussion.)

MR. TRAPP: There are questions whether it's choking there or at the end. It has to do -- that's a complicated picture, like Stan says. If you assume there was choking at the end, where it was flowing full, then you'd have different parameters. These parameters --

MR. THEOFANOUS: With this understanding, can I ask you again to please tell me in very few words what is the prescription that you used in your critical flow calculation?

MR. TRAPP: We assume at the throat section, which is the section which we had as the first -- I can show you what we call the throat section on the slide in the numerical model.

If we look at our nozzle, we really had the first section -- one at the very exit and one in the middle of the discharge nozzle. At this section here, our choking criterion, we applied the minimum section where the nozzle began. At that point, we imposed in the idealized case for Test 4 -- saturation pressure existed at that throat, and that gave us

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essentially momentum equation.

MR. THEOFANOUS: You just used only --

MR. TRAPP: Or in the later runs, we had some ratio of the saturation pressure. That was the value used to get a flow, a critical flow, in the subcooled regions.

MR. THEOFANOUS: I see.

MR. TRAPP: When that value -- when the two-phased criterion -- you notice, if there was some quality at that point, when that two-phased sound speed was larger than that, we transitioned to a two-phase critical flow model which had a different criterion.

MR. ZALOUDEK: What did this discharge pipe discharge into? Was it a free discharge?

MR. TRAPP: Essentially free. As I remember, there was a deflecting plate.

MR. ZALOUDEK: Then do you feel that the flow was attached to the walls or detached from the walls at the vena contracta?

MR. TRAPP: In this region here or out here at the exit?

MR. ZALOUDEK: In the pipe. In the vena contracta within the pipe?

MR. TRAPP: We felt from the data it indicated that some kinds of separation, maybe some cavitation at the throat, as it flashed. It wasn't flowing full at the throat.

MR. ZALOUDEK: Since it wasn't flowing full, the critical flow situation should have been at the vena contracta or the last place where it attached to the walls, because if it's not attached to the walls, of course, it can't choke.

MR. RANSOM: John, could I make a comment on that? This is something I feel very strongly about. I have been urging people to run these critical flow tests with idealized nozzles at least for a reference case so we could separate some of these geometric effects from separation and/or cavitation which may be occurring at the point of vena contracta. We can put together a picture; even if it's separated, it could still be choked in that the vapor flow in the separated region or cavitated region could be supersonic.

Actually, from the data, looking at tests like this, you cannot separate out which of these kinds of phenomena are occurring. I think John will agree with me from these looks that he's taken of a number of tests, an idealized nozzle, an ASE bell-mouth nozzle with a smooth entrance, 10 degree entrance or so, would eliminate at least one of these variables which would allow the critical flow model to infer more about what is happening in terms of nonequilibrium and the choking phenomena, or choking criterion.

I don't see why they don't use this kind of a nozzle, especially when the only objective of the test is to set a flow rate for a given depressurization rate.

I understand that Marviken wanted to model an actual nozzle inlet like you might have on a reactor vessel. That's a reasonable objective. But they should have included at least one ideal nozzle to eliminate and separate the geometric effects.

DR. PLESSET : I am going to let Stan have the last word. We have to move on.

DR. FABIC: These large tests that we get were not meant to be model development tests.

Why use those to develop models? Why should they check these models for the effect of scale using test data from Marviken? I think it is wrong to start developing basic models based on tests like this. I think that's wrong.

That's just -- what you call it -- trying to fit the model to agree with the data.

DR. PLESSET : All right.

MR. THEOFANOUS: Could I ask you on the basis of what you learned from that, what do you recommend to use for a critical flow for a reactor calculation?

MR. TRAPP: We feel good about using this model, this idealized case, as a reference case. We would like to be able to quantify it a little better. Some departures from nonequilibrium at the throat so we have the capability of putting in some throat pressure that is different from saturation.

MR. THEOFANOUS: So at this point you don't have anything to recommend specifically?

MR. TRAPP: Other than that idealized model with some paramaters to take into account the nonequilibrium.

Thank you.

DR. PLESSET : Thank you.

Our next presentation is on the BEACON code.

MR. BROADUS: Good morning. I am Chuck Broadus. I am responsible for the BEACON code development program here at INEL. This morning, in discussing the BEACON program, I am going to be covering the purpose and philosophy behind BEACON, as well as some of the methods used in the BEACON model development. I will present then what our current released version of the code consists of, that is, BEACON Mod 2, and some of the results we obtained by running this version of the code.

Finally, I will wind up with a summary of the current work that is ongoing in the BEACON code.

(Slide.)

DR. PLESSET : Any condensation to save time would be appreciated.

MR. BROADUS: Okay. The purpose of the BEACON code is to provide a best-estimate containment analysis capability in order to evaluate the actual transient or phenomena associated with a reactor containment.

To discuss the reasons this is important, I want to cover what the BEACON philosophy is. To put it into perspective, I would first like to cover what the current licensing philosophy is for reactor containments in the United States.

(Slide.)

The current licensing philosophy in the United States consists of using conservative codes with conservative input to come up with a conservative analysis.

What this does, this produces a "conservative result," which is more than adequate for the licensing of nuclear power plants. However, it does have some drawbacks.

First of all, we do not know the degree of conservatism involved in the analysis. We know that it is conservative, but we don't know the safety factor involved.

Second, the use of conservative or homogeneous-type codes may cover up important problems or phenomena.

Finally, for some analyses, high pressures and temperatures are not conservative. For some analyses you want low pressures and temperatures; and in this case, if you have a code that by its nature calculates conservatively high pressures, all you can do is try to go for conservatively low input data. Then, again, you don't know where you stand as far as the margins go.

(Slide.)

Comparing this to the best estimate or BEACON philosophy, we use a best-estimate model in order to predict the actual transient phenomena. That is, we are trying to include in the best-estimate code the actual physics of the problem or at least as much of the physics of the problem as it's possible to do.

What this buys us is that we will have a benchmark or a basis for comparison with the licensing codes so that we can determine what our safety factor is.

Secondly, if we have done our homework and have included enough of the physics of the problem, it should show up all of the problems -- potential problems -- and phenomena that would be associated with a given analysis.

Also, the best-estimate or BEACON type of code may be used for European licensing type of analyses where you calculate a best-estimate analysis, and then either add or subtract a safety factor, depending on if you need a high pressure to be conservative or a low pressure to be conservative.

Finally, we feel that BEACON is a general enough tool as far as the geometry, the inclusion of the physics of the problem, that it can be used to handle further problems that may crop up in the containment analysis area. There have been a number of problems that have occurred in the past where BEACON would be a good general tool that could maybe not handle the problem completely in an ideal way but could at least get a handle on the order of magnitude of the problem.

(Slide.)

I will now move on to the methods used in the development of BEACON. In general, we tried to use suitable existing models where this has been possible. These models include at the heart of the BEACON program the LASL KFIX numerical scheme, which is a two-dimensional, two-phased, non-equilibrium type of a calculation.

We added to that the INEL heat conduction subcode to account for our wall and structure heat transfer.

We have two water properties or equation of state packages, one which is a quick-running ideal-properties package based on a Los Alamos package; and the other is a slower-running ideal-properties package based on the Brookhaven National Lab.

We also included the INEL dynamic storage routines in order to conserve the core usage for a given problem and the INEL intraprocessing and plot packages.

(Slide.)

However, in cases where suitable models were not available, we have developed our own. These included the use of the addition of an air pump on an end to the KFIX numerical scheme. This is necessary in a containment code.

We added a mass momentum and energy source model into BEACON. This is primarily to model break locations, either directed or nondirected. However, the same source model can be used tomodel such things as containment sprays, fans, and

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coolers.

We added a wall film model to track and calculate the mass of film that becomes de-entrained from the bulk fluid flow onto the walls or may condense on the walls.

We have developed a set of heat transfer correlations which are based on the physics of the problem and utilize the unique information which BEACON provides. This includes localized velocities, the localized thermodynamic properties, as well as the film thickness which would tend to modify the heat transfer coefficient.

We also added a number of options which allow us to model a general geometric -- properties that are found in most nuclear power plants. These include a generalized mesh coupling scheme whereby we can connect a number of meshes in a very generalized way. We have a variable mesh spacing option where we can mesh very finely in a certain area, where we are interested in some phenomena that we are expecting there; as we move away from that area, increase the cell dimension.

We have added a partial flow blockage capability whereby we can partially block off a cell and add a loss coefficient to account for a small piece of equipment or pipe running through that cell.

We added a lump parameter region model in order to model large rooms we are not particularly interested in the variation of thermodynamic properties; and finally, we added a restart capability so that expensive codes can be run a little bit at a time, or expensive problems can be run a little bit at a time so that you don't waste a lot of money on a problem that's going to bomb.

Finally, all of these items were included in modular fashion so as future models come along, better models, it will be very easy to remove the current model from the BEACON code and introduce the improved model.

All of these models have been incorporated into BEACON and are included in the currently released version, BEACON MOD 2-A. BEACON MOD 2-A is a two-dimensional, non-equilibrium model capable of handling two components, air and water; and two phases, the air vapor phase, as well as the liquid phase.

It can handle complex modeling or complex geometries. It is good for the short to the intermediate term; and it has heat transfer capabilities from structures to individual cells or group of cells; and it can also track a film that may be covering those heat structures and modifying the heat transfer coefficient.

(Slide.)

Now I would like to present some of the results that we have obtained using the released version of BEACON MOD 2-A. Specifically, I would like to cover the Battelle-Frankfurt D-15 test which was the basis of a CASP or containment analysis standard problem which we participated in in Frankfurt, Germany.

This consisted of six rooms. Rooms 6 and 8 were connected by a duct which passed through Room 4. The other rooms were all connected by way of orifices.

The break location was up in Room 6 in the corner; and it consisted of steam. This was originally intended to be a blind problem. We did participate in that. The results I am going to show you today are not the blind problem results that we turned in.

At the time that we ran, we had to turn in the blind problem results, we had just integrated the heat transfer model and the wall film model into the BEACON code. As a consequence, we still had a number of coding problems which caused an overprediction of the temperatures in the compartments. This primarily had to do with the interaction between the wall film and the cell. We also had oscillations which tend to obscure the results. The oscillations, it turned out, were caused by Modeling Rooms 7, 4, 5, and 9, two rooms away from the break as lumped parameter regions connected by lossless orifices.

At this time in the MOD 2-A version of the code we do not have the capability of modeling irreversible losses. This set up oscillations between the lumped parameter regions which carried back into Rooms 6 and 8 which were modeled as two-dimensional mesh regions.

The results I will show you today use the exact same data that we used for the initial submittal. However, the rooms away from the break were also modeled as two-dimensional regions. Those are the only changes in the data.

(Slide.)

This is a comparison of the BEACON calculations for the pressure in Room 6 shown as the red dashed line with the measured data in that same room.

In Room 6 we overpredicted the pressure transient; and in Room 8, in this room we underpredicted the pressure transient. The reason for this is that, as I mentioned before, in this version of the code, we do not have the capability of modeling loss coefficients or some kind of an irreversible loss. We attempted to account for this loss or this effect by reducing the area.

This worked well for all the orifices in the problem; but evidently, for that duct between Room 6 and 8, we overdid it, causing the overshoot of pressure in Room 6 and the underprediction in Room 8.

In all of the other rooms away from the break, we had an excellent agreement with the data.

(Slide.)

This is a comparison of the temperature data in Room 6 for three different thermocouple locations. The first two lines represent the thermocouple location closest to the break; the next two, a little farther away; and finally, the last two curves represent thermocouple location located near the duct entrance into Room 8.

A number of things can be seen from this graph. First of all, just the fact that BEACON is calculating three different thermocouple locations points out its ability to calculate a nonhomogeneous two-phase type of a problem. We were the only one that participated in the CASP problems either of the Americans or Germans which were able to show up this capability.

The next thing were oscillations in the temperature curves for BEACON. This is due directly to using too large of a transfer for the heat calculations. If we reduce the time step to where it's more on the order of the fluid calculations time step, then those oscillations disappear.

Next, I want you to notice on the slide the delay that occurs right here, or the overprediction of BEACON as compared to the data during the early transient. I will cover that later on a different slide.

The last thing here is the underprediction or the apparent underprediction of the data of the thermocouple away from the break.

This is due to the fact that BEACON cannot calculate a steam front passing through the room. You can see the effects of the steam front as it hits the thermocouple at this point where we get a rapid rise in the thermocouple temperature.

BEACON, because of its nature -- it's a finite difference code -- tends to diffuse the front over a number of cells. The air fraction, which has a significant effect on that thermocouple, or on the temperature calculated by BEACON, is not purged out of that cell as rapidly as it is in the regular problem.

If you look at a thermocouple in these rooms as a heat sink, a small but finite heat sink that is initially cold, the first steam that is going to come through there will treat the thermocouple as any other heat sink. It will condense out on the thermocouple.

You expect during the early transient for the thermocouples to be measuring more of the saturation temperature than the actual temperature in the room.

(Slide.)

This is the same data as in the previous slide. However, what is plotted, superimposed in red, are the thermocouple location saturation temperatures as calculated by BEACON. This produces much better results, and we tend to converge directly onto the measurements of the thermocouples. So from this, we concluded that at least during the early time period after the steam has passed the thermocouple, that what the thermocouple is probably measuring is the saturation temperature.

(Slide.)

I will move on to the subject of the delay time or the overprediction you saw in the first slide. This shows the temperature transient in Room 7. This is a room away from the break. That's the temperature -- the data is the blue line. the BEACON-calculated gas temperature is represented by the gold line on this graph.

As we participated in the CASP workshop in Germany, we noted that most of the other codes that participated had the same trend, that is, to overpredict the data during the early transient. Further, it was noted that this transient could just about be backed out or predicted based on the pressure transient and an adiabatic pressure-type calculation.

When we modeled in BEACON, very crudely, a thermocouple -- crudely in that we used the proper dimensions, but we had to use some average properties from what we knew of the thermocouple. We put it in Room 7 and made a run. The results you see are represented by the red curve. The red curve does match the data much better. This is the center-line temperature of the thermocouple.

We submitted these results to Germany; and they have done some testing on their thermocouples. They were claiming thermocouple delay times, response times, only on the order of milliseconds; but they had calibrated them based on dunking them in hot water or putting them in a steam jet. This is quite a bit different from a thermocouple sitting in a relatively still-air atmosphere.

They are now saying they don't really know what the thermocouple response is in this early time period, but it looks to be more like on the order of 1, 2, or 3 seconds.

The other thing to note here is the way BEACON underpredicts the temperature transient toward the end or after 1-1/2 seconds. This again is when the steam front comes through the room and comes in contact with the thermocouple. There again BEACON has diffused the steam front and it's not going to pick that up immediately. We would expect, if we ran the problem out further, for the BEACON-calculated temperature to eventually move up to the thermocouple temperature.

It is not plotted on here; however, the saturation curve for BEACON for this particular cell tends to follow the lower curve out to about .4 seconds; and then it makes kind of a parabolic increase up to matching this data out at about this point. Again that would tend to point out that what the thermocouple is measuring during that part of the transient is probably more saturation temperature.

I will now move on to the current work that is going on in the BEACON program.

(Slide.)

We are currently involved in developing BEACON MOD 3. This involves the addition of three major models. The first model -- and by far the most involved model -- is the best-estimate correlations model to govern the interphasic heat, mass, and momentum transfer. Dr. Sahota will discuss that in a few minutes with you.

The second model we added is the form and friction loss model, to allow us to handle wall friction as well as entrance and exit losses.

The third model is illustrated by the figure -- what we call an out-of-plane coupling model. This increases our geometric modeling ability. We connect an interior cell of one mesh to an interior cell of another mesh by way of a one-dimensional region.

(Slide.)

As the BEACON MOD 3 program is wound up and issued to the National Energy Software Center, we will be moving, as Dr. North pointed out yesterday, into a period of developmental assessment where we are going to be evaluating the program. The purpose of this developmental assessment portion of the program is to define the range of conditions and problems to which BEACON is applicable. We are going to do this primarily in four steps -- four steps for each particular problem, I should say.

The first step will be the problem setup and documentation. That is, we are going to examine the geometry, the boundary conditions, and then set up the problem to the best of our ability and document why we modeled the given problem in a certain way.

The second step will be a blind run. "Blind" here should be in quotes, because we will have access to the data. However, we are going to attempt to not look at the data until we do get a satisfactory first run, just based on what we have done with the input.

The third step, then, would be the data comparison where we will compare the results of the blind run with the data.

The fourth step will be our learning phase where, provided the results of Step 3 are not satisfactory, we will modify the data within our best-estimate limitations, that is, such things as nodalization, in order to determine where we went wrong and to learn how to better model future problems.

The types of problems that we are going to be investigating fall into two different categories. We have the separate-effect-type problems which are intended to look at a certain capability or phenomena that BEACON calculates. These include such problems as the entrainment/de-entrainment experiments which are being performed at Drexel, as well as the Lahey two-dimensional flow work being done at Rensselaer.

The other category of problems are the integrated containment problems. For these we are relying heavily on the Battelle-Frankfurt C&D series tests, because we have good access to the data, and the type of data that they have taken is particularly suited to the BEACON developmental assessment.

The other test we will be looking at -- other type of tests is like the CVTA -- Carolinas-Virginia Tube Reactor test -- to evaluate stratification-type capabilities.

This concludes my presentation. Do you have any questions?

DR. PLESSET : Let me ask you one. No, I have two questions. You can say yes or no.

Do you expect to get the capability to analyze an ice condenser containment?

MR. BROADUS: This would be included in future work. As I mentioned, we are moving on to a period of developmental assessment which should last about a year. After that, it will be assessed whether we should continue working on it or not. Then we would consider a problem such as that.

DR. PLESSET : Would you be able at some time to evaluate the effect of a hydrogen burn on the containment?

MR. BROADUS: We have not --

DR. PLESSET : In particular, an ice condenser plant.

MR. BROADUS: These are, again, future models. We do not have a hydrogen tracking model in there now. It has been considered for future work.

DR. PLESSET : Thank you. I think we will go on to the next part of the presentation. Again I plea for brevity.

MR. SAHOTA: Thank you. I would like to mention, even though we have the capability of including an inert gas -- I think in the type of problem, we really need an equation. It may not be easy.

DR. PLESSET : I didn't say it would be easy. I thought it might be important.

MR. SAHOTA: It might be important. We have talked about including mass diffusion.

(Slide.)

In the best estimate, we have tried to maintain reality, sometimes at the expense of -- the idea has been to develop equations which apply specifically to the containment applications. However, the coordinations should be general enough to cover our situation. Consequently, the equations are correlations, but we feel they should be reasonably activated not to invalidate the core if run under the extreme situations.

(Slide.)

Mr. Broadus mentioned BEACON is a two-dimensional transient, two-component, two-phase nonequilibrium code. The unequal velocities between the two phases lead to interphasic drag.

The second thing is the thermodynamic nonequilibrium give rise to heat and mass transfer in the presence of inert gas. Since that is present, the problem is further complicated.

(Slide.)

For the analysis, we considered a very simple flow regime approach. The first one is the dispersed droplet-type flow.

The second is bubbly flow. Then the flows with void fraction close to .5, and then we have flows with other void fractions. Actually, in your slides, this is incorrect. It should read 0.5 less than 1, and 0 less than 0.5.

We have given rigorous treatment to the dispersed droplet-type flow, feeling this is the most frequently encountered type of situation in containment application.

However, we also have tried to handle bubbly flows in a similar manner, and less emphasis has been placed on the other two types of flows.

Basically, the approach is for dispersed droplet and bubbly flows. We try to get the interphasic exchanges for a single droplet and then consider the distribution of the droplets or the bubbles. We interpret those rates over various sizes.

For void fractions close to .5, since there is some doubt whether bubbles in liquid or droplets in gas, we assume a varied type of interphase and used some steady-state correlations to predict the rates.

The other flow situations are simply hindered by the combination of the above results.

Let's assume a droplet has dispersed in an air vapor mixture. One can calculate the interphasic rates of heat, mass, and momentum transfer from some standard correlations.

(Slide.)

This is the well-known equation for drag. Once the drag equation is unknown -- which is basically this data -- in our standard textbooks, the development for the drag equation yields an expression like this.

All I can say is this is only dependent upon Leonard's number. Once that is known, which is known from the BEACON calculations, we can calculate the drug coefficient.

For the mass transfer in the presence --

MR. THEOFANOUS: This is for solid spheres?

DR. PLESSET : Yes.

MR. THEOFANOUS: Do you expect any difference for the particles?

DR. PLESSET : Or even for the detection of the possibility of flow?

MR. SAHOTA: Yes. As I said, we have tried to keep a very simple approach. However, Washington State has done some type of work. He has had reasonable success assuming droplets as spherical, solid spheres.

However, I would like to mention that his work has been primarily in the nozzles where the droplets are really small droplets. I will mention about the momentum exchange between the droplets and the gas phase. That is automatically included in the BEACON calculations. Like if a droplet evaporates and there is a momentum exchange between the droplet, subphase and the gas phase, which could affect these results. Those are included.

MR. THEOFANOUS: We are talking about the drag coefficient here, not the solid effect. There's quite a bit of information that indicates the drag coefficient can be quite a bit higher.

MR. SAHOTA: Right.

MR. THEOFANOUS: When the drops are deformable. I think the study you mention -- it is probably true. In the nozzle, the particles are so small there is no --

MR. SAHOTA: Either the particles are so small, or --

MR. THEOFANOUS: That wouldn't be the case in your case. The particles wouldn't be that small.

MR. SAHOTA: What I was really going to say is we have some other large uncertainties which completely supersede this unknown effect, like droplet-like distribution. We have assumed a droplet-like distribution. We have established a maximum droplet radius based on numbers which was sort of a mechanical trade-off.

DR. PLESSET : Maybe you should take a global view.

MR. SAHOTA: Some people have done it. We take the droplet approach; you take something and it applies very well under certain conditions. It does not hold that well under other conditions. So what you end up doing is putting in 10 or

15 equations to cover different types of situations. That's what we tried to avoid. I don't claim they would be accurate; but at the same time they should give us answers within an order of magnitude and should really not vary.

Anyway, the mass balance in the presence of inert gas gives you an expression like this, where W is the mass fraction of the volume at the interface, and infinity is the ambient extreme.

GM is the mass transfer coefficient. I would discuss the calculation of GM, which is the mass transfer coefficient in a moment. However, I would like to add that in the case of the inert gas not being present, we use the expression given by Shadl.

MR. THEOFANOUS: That means you consider the important step to be the condensation step from the gas phase to the liquid. Wouldn't it be important that the heat has to be conducted away from the interface? Don't you think that would be the limiting step, not the transition from the gas to the liquid?

MR. SAHOTA: I don't understand.

MR. THEOFANOUS: It would simply mean under your conditions the kinetic limitations would be unimportant and the actual consideration would be lying in the conduction away from the droplet interphase into the bottom of the drop.

MR. SAHOTA: That is what it is. We are taking the

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production into droplets in the gas phase.

MR. THEOFANOUS: I realize that. I am saying that doesn't agree with it. It would seem under your conditions kinetic limitations would be unimportant. That's just the opposite.

MR. SAHOTA: You are saying the conduction in the droplet would be important?

MR. THEOFANOUS: More than kinetic.

MR. SAHOTA: Liquid is not --

MR. THEOFANOUS: We are not -- you say when you talk of gas, you use the science equation. That tells me, then, in that case you are using kinetic limitation. I am saying kinetic limitation shouldn't be important and conduction should be important.

MR. SAHOTA: I take it back. I was thinking about this inert gas phase. You are right. Again, how many applications do we encounter in the containment where there's no inert gas? What I pointed out is we are trying to maintain complexity. The point is you might end up in a flow situation where there is no air.

I just mention that we are adding that expression. I don't believe that is accurate, but that is just to force the computer calculation.

MR. THEOFANOUS: Fine. I don't want to belabor the point. It seems to me, however, you want to avoid glaring inconsistencies or glaring mistakes. I think it's an error to put something like that in the calculation. It could be that during an actual calculation a subcompartment is getting some of the air, and what is left is not too much air. Maybe somebody then that applies the correlation is not being so careful and is using the wrong calculations in the place where the droplets are growing, condensing, without the presence of air.

MR. SAHOTA: Yes.

MR. THEOFANOUS: That's enough.

MR. SAHOTA: I get your point. But I missed the point; I thought you were talking about the presence of inert gas. That is where our main effort has been present, in inert gas and dispersed droplet type of flow.

(Slide.)

Similarly, the energy balance at the interphase gives rise to expressions like these.

This is really valid for number one. You get more expressions.

Also, we assumed it to take advantage of the analogue in the species and energy equations.

GH is equal to GM, which is the mass transfer coefficient. These two can be calculated in terms of heat or mass tranfer coefficients in the absence of an inert gas. The distribution of the species equation, if it's given in years, in an expression like this, so that if one knows the heat or mass transfer coefficients in the absence of inert gas, one can calculate the other quantities in the presence of inert gas.

This GM and GH could be calculated from well-known equations.

Once one knows the mass effect on the droplet, all one has to do is use droplets. To do that we considered distribution on the droplet size. The maximum droplet radius -- R is the dimension of the droplet. The maximum droplet radius is calculated based on mechanical calculations.

Once the droplet sizes are known, we just took those analytic expressions and interpreted it over all droplet sizes. The total drag is given by this expression. The B/R is given by this expression. We were able to give analytical numbers by interpreting these.

DR. CATTON: Did you get the same kind of analytical results?

MR. SAHOTA: That is what that is.

DR. CATTON: To one-half?

MR. SAHOTA: I might have an error there, but I think this equation is correct.

DR. CATTON: I thought it was for flow.

MR. SAHOTA: That is for black leg.

DR. WU: Low and intermediate.

MR. SAHOTA: That's correct. If you really take a maximum droplet size and calculate -- look at the velocity between the two. You get reasonably small numbers.

(Slide.)

Allow me to talk briefly about the bubbly type of flow. For the drag coefficient, again I am going to have some objections here. What we did was considered a single bubble and used the same approach as for droplets to calculate the drag on a single bubble.

An energy balance on the bubble yields expressions like these where GH is the heat transfer coefficient in either case. Ideally, one should calculate this liquid heat transfer coefficient from the distribution of transient conduction equations, or, in other words, using it, and one should be able to calculate the heat transfer coefficient that way.

However, to do that, one needs to have the age of the bubble and the history, the coefficient history in the liquid case which requires a tremendous amount of computer storage.

So again we said, okay, we would use this equation, consider it quasi-steady, and calculate this heat transfer in an approved manner just because of the computer storage problem.

Once the energy is known, again we interpreted those overall bubble sizes to calculate the macroscopic exchanges.

(Slide.)

The third type of regime is void fraction which is close to .5. In the absence of any other information we assumed a wavy liquid air-vapor interphase. One could calculate

cs

the stress at the interphase if that were known. One could also calculate a standard number based on well-known solutions, based on macroscopic exchange rates.

Those exchanges can be calculated by multiplying by the area. That is based on the existence of droplets.

(Slide.)

The other flow regimes are simply the extensions of the previous flows, flow regimes. That is a void fraction of .5 and 1.0. We assumed the velocity and temperature -- we ignored the velocity and temperature gradients. The phenomena is basically in the gas phase. Therefore, one could simply calculate the drag heat and mass coefficient.

However, Between void fractions of 0 and .5, the conduction is in the liquid phase.

Similar types of approaches have been used for the heat and mass transfer coefficients.

Just to reiterate, all I really want to reemphasize is that our main emphasis has been this droplet flow in a containment in the presence of inert gas. However, sometimes we do run into situations where sometimes, because of numerical errors, a particle or cell would give you some mass fraction. We need to know the flow domains. Therefore, we have added sort of generalized best-estimate interphasic correlations.

However, there are other types of correlations existing in the BEACON calculations which are meant for special

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emphasis. If one wants, you could use those.

That concludes my presentation.

DR. PLESSET : Thank you.

You have a very ambitious program. It is commendable. If you are successful, it would have applications in other areas aside from the containment flow problems. I wish you luck.

MR. SAHOTA: Thank you. I think we will need that. I was surprised to find that it was really based on some -- like -- bubble formations during that depressurization. We have been able to successfully use that correlation for containment applications. We said if we could use that, which was really based on bubble formation and bubble growth, assuming constant equilibrium during the life of the bubble -- if we could use that for droplet type of flows, we certainly stand a better chance of using some approach like that.

DR. PLESSET : Thank you.

MR. SAHOTA: Thank you.

DR. PLESSET : I think we will have to have a change of gears which is required for the next subject. We are going to have a presentation by Dr. Rosztoczy on the NRC Office of Nuclear Reactor Regulation needs.

I am sure their needs are very modest.

(Laughter.)

This shouldn't take long.

DR. ROSZTOCZY: Always.

(Laughter.)

DR. ROSZTOCZY: Mr. Chairman, as you indicated, my responsibility here is to give a brief summary of some of the most important licensing data needs that we have. I would like to specify that I am going to talk about only those data needs which relate to Semiscale or LOFT. Also, I am going to talk about short-term data needs, basically programs that we expect can be completed within the next year or two.

With those two qualifications, let me go to the slides.

(Slide.)

The first area where we have some data needs is the upper head injection system. As you might recall, we have completed our review of the upper head injection system back in 1977. We presented the results of those reviews to the committee at that time.

One of the main conclusions of the review was that the available calculational techniques had very large uncertainties associated with them. These were sufficiently large to kind of mask the possible benefits or drawbacks of the UHI system. Therefore, we couldn't really decide how much of a benefit those UHI systems represented.

In order to be able to have a better understanding of other systems, like UHI and possibly other systems, ECC systems, c-47

there is a definite need for more experimental information in this area.

This experimental information should have the understanding of the UHI system performance, and it should help us to develop and verify calculational methods which can then be used to analyze these systems.

We have requested this type of work back at that time. The program has been developed for this purpose. The program was ready to start approximately a year ago last summer. In the first test -- there were three tests -- we ran into some difficulties. Some of these are known as the S-07-6 problem. We will come back to that later.

We fully agree with the people involved in running this program that some of these problems have to be resolved before we can go and complete the program.

We are waiting for the resolution of those, but we would like to emphasize that this is an outstanding program and the completion of this is important and urgent to us.

I would like to point out that the last word on the slide should be "practicable." We would like to see this program completed as soon as it is practicable.

DR. PLESSET: What is the Westinghouse view regarding the validity of the proposed tests? I am sure there has been discussions with them about that. In advance of the test, what do they say?

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DR. ROSZTOCZY: We have had numerous discussions with Westinghouse on this subject. Westinghouse people in general are not completely pleased with this program. We have asked them repeatedly if they can propose or if they are planning to run on their own any programs which could do a better job than this. Throughout the past three years, this discussion has been going on. They could not come up with anything better than this program.

We all realize that a program, short of being a full-scale program, will not provide all the information that one would like to have.

The information presently available is very sketchy and very limited. This program would put us one step, one big step further ahead. It would not resolve everything completely. We think it will be very valuable.

Should something develop in the program that indicates that certain variations on this or some changes would be helpful, they will be factored into the program.

Final resolution, I believe, was that Westinghouse is not doing pretests for the program. We will run the first test series. We are going to look at the data and see if we are getting meaningful data out of the test program. Provided the data is meaningful, Westinghouse will do all the necessary calculations, blind calculations, that are necessary.

DR. PLESSET : What is your feeling about the calculations, I think being made with COBRA? We have seen some results. What are you feelings about that?

DR. FABIC: I would like to update the information you have.

DR. PLESSET : Shall we do that, Zoltan?

DR. ROSZTOCZY: Please, go ahead.

DR. FABIC: The information you saw in Los Angeles showed the results of the COBRA calculations for the vessel only, where the boundary conditions were given by SATAN. We agree this is important to the contrasting requirements, conflicting requirements. We have since then been able to merge COBRA with TRAK so that now COBRA calculates the complete system. They are redoing these UHI Westinghouse plain calculations for the complete system, not just the vessel. So it is a separate system calculation.

DR. PLESSET : When do you think those would be available?

DR. FABIC: I was on vacation for a month. When I come back, I will know the answer to that.

DR. PLESSET : It's not too far in the future?

DR. FABIC: They already ran the calculations for the system. They are now running the Westinghouse plant on installments. It's a lengthy calculation. Their access computer is very limited. They use Brookhaven. It has very limited access.

DR. PLESSET : What is your attitude toward the usefulness of these calculations?

DR. ROSZTOCZY: I would be interested to see them. I have not seen them yet. Once I have an opportunity to see them, examine them, I will be very happy to comment on them.

DR. PLESSET : Okay. That sounds like a lawyer.

(Laughter.)

DR. ROSZTOCZY: We all learn to talk that way.

(Laughter.)

(Slide.)

DR. ROSZTOCZY: Another area where we do need experimental data is the small-break loss-of-coolant accident. We have had one small-break Semiscale test run back a number of years ago. This was S-02-6. This was the only small-break test. It was selected as a standard problem, as part of the standard problem. Most of the vendors have been doing pretest predictions for this test.

The report predicting the calculations was published back in the spring of 1978, somewhat more than a year ago. The conclusion from the comparisons was that there are comparatively large uncertainties in the calculations. The two main uncertainties that we have observed were -- one of them is in the depressurization rate. Some of the calculations were far off on that.

The other large uncertainty that we observed was in the behavior of the calculations at the time when cold ECCS water is being introduced into the system.

The data shows some change in the depressurization, acceleration in the depressurizations, but just a relatively small amount at that time. Some of the calculations just depressurized very fast. Because of these reasons we asked for -- there were also some problems with the data. I am not sure from the details, but I believe maybe the discharge flow was measured. There were problems with the data.

As a result of this, we have asked for additional small-break tests.

We have also observed from the licensing order we have received last summer -- related to some changes in the variation model of one of the vendors -- B&W -- in the review of that, we have noticed that the calculations are rather sensitive to changes, the small-break calculations. There are relatively minor changes, and they can produce 500 or 600 temperature variations in the calculations.

At the same time, the tests required by Appendix C were applied for large breaks. When applied to small breaks, they make a relatively small difference.

Putting all of this together led us to the request for additional tests and additional small-break analysis. The purpose of this would be to evaluate the uncertainties of the small-break calculation and to validate the calculational

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methods.

We have also requested -- this is not a standard problem, but there is a definite request from the NRC that each of the PWR vendors should predict this test.

The actual test was completed in December of 1978. The calculations from the vendors are not in yet. There have been other complications while the LOFT test program was going on. We have been asking for pretest predictions.

There was a change of priorities. The LOFT test was done first. The small-break test has been locked in.

We have sent a letter to each of the vendors urging them to provide these calculations as soon as possible. We asked for it by September. We are in discussions with them on the actual date when the calculations will come in.

(Slide.)

More recently, due to the TMI events, there has been an increased emphasis on small breaks. This is a continuation of my small-break slide.

TMI-II brought attention to various facts in connection with small breaks. One is that the very small LOCAs are occurring with a higher frequency than what we previously considered. In the B&W case, there have been four events within 30 reactor years of operation, which is a relatively high frequency.

The other thing we learned is that the plant responses to very small breaks differ from the plant responses to the more common small break that has been looked at and analyzed in the safety evaluation models. Very small breaks are not large enough to remove the decay heat from the core. The discharge cannot remove all of this heat. Because of this, it is very important to have other modes of heat removal.

Natural circulation to the steam generators, therefore, plays a very important role in this very small break. Because the heat is not removed, overpressurization is possible. If the break is at certain locations, like in the pressurizer, at the pressurizer level, it will not empty. There are a few other complications depending upon where the reactor coolant comes from.

In order to be able to handle these very small breaks, the calculational techniques have to be carefully removed for this purpose, keeping in mind these serious phenomena to see if they can be handled.

Up to now, our observation is working with the various vendors. Each of them need some changes in their variation models in order to account for all of this.

We also found that there is not sufficient experimental information available. We are at the point now when we think the codes are predicting the basic phenomena. Where they are, we do not know yet. We do need experimental evidence.

We, therefore, request tests -- the word is used c-54

here, a "demonstration" test. A demonstration means that during the past three months in our work with the various vendors, they have predicted through the analytical techniques of how the plants could respond to the small breaks. They predicted for some of these breaks that there will be depressurization when the various ICCS systems come into play.

For some small breaks, there will be a hang-up of pressure close to the secondary site pressure. There will be no further depressurization for the next three-year period of time. For other breaks, there will be pressurization up to high pressures. Other means are needed to reduce the pressure.

We are asking for tests to demonstrate these three basic behaviors of very small breaks, the depressurization, the pressure hang-up, and repressurization.

The test program is being planned to augment each of these. They will provide at least one basic test result for each of the cases.

We have also predicted -- we have requested reactor suppliers provide pretest prediction for selected tests. The tests we are asking them to provide have not been selected. It appears now that it will be one or two tests which will be run later this year, or late this year, or early next year.

This has been spelled out in the latest record, new Reg 0578.

The small-break tests have a very high priority.

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We would expect to see a program ongoing on this in the very near future.

(Slide.)

The third document I would like to discuss is Semiscale Test S-07-6. This was run as one of the first tests in the new Semiscale MOD 3 system. This test was run when voiding on the downcomer occurred during the test, which kind of caught our attention.

As a result of this multiple voiding, it was rather pronounced and rather long-delayed in the quenching of the core.

The peak clad temperatures have not changed significantly; so the calculated peak clad temperature and the actual pressured peak clad temperature weren't too far apart. I don't have a slide with me that shows the temperature, but the difference there is not much.

However, there is this large difference in the quench time. So the oxidation, the zirc-water reaction, because of the time period, is quite different than what the code calculations would show.

In our licensing work during the past few years, since 1974, since the ECCS criteria was published, shows that in some cases the calculated zirc-water reaction is very close to the limit specified in the acceptance criterion. There have been a few cases in the past when the plant was limited by the zirc-water reaction. They ran into the zirc-water reaction

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before and into the peak clad temperature.

The extended time period here could result in relatively large uncertainties in the prediction of the zirc-water reaction, like something by a factor of 2 or maybe more.

So, therefore, we paid quite a bit of attention to this test.

The first question was: Can the codes predict and can they calculate this test? We have tried the RELAP 4 code. We have tried the TRAK code in its 1-D version.

The conclusion from both of those tries was that the codes were unable to correctly predict the experiment.

Yesterday you saw some slides which were done with the RELAP 5 code for this very same test. They have shown some promising effect for the early portion of the transient.

In order to put it into proper perspective, those calculations were only for the blowdown portion of this transient, because the RELAP 5 code is not ready yet to be used for the other version.

We cannot run the complete version with RELAP 5 at this time. Therefore, they do not know whether RELAP 5 would be predicting correctly or approximately correctly the delay in the quenching of the core. Some of the code modifications which were outlined yesterday will provide this capability. My own estimation is that RELAP 5 would have this capability next year sometime, but not earlier.

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There was also a divergence observed in the temperatures, the calculated temperatures relative to the measured temperatures of the first turnaround in the temperature that is not fully understood yet, what caused it. I think more work is needed before some of this is ready.

The conclusion is at the present time we do not have a satisfactory production of these test results by any of the computer codes available to us. Right now we cannot calculate this step.

We have requested both experimental and analytical studies to decide whether this phenomena is typical for PWRs and whether it is significant for the licensing type of actions, licensing type of decisions.

We do not have in all the results of the various things that are ongoing to try to resolve this problem. We don't have a final answer yet. However, up to now, we have learned a number of things. Those are listed under the present status.

One of them that we have learned, and we are convinced of, that this phenomena is a real phenomena and it can occur in large systems just as well as in small systems.

DR. PLESSET : How do you base that can occur for large systems? Why do you feel that?

DR. ROSZTOCZY: That it can occur in large systems? The phenomena is basically a phenomena we all know from our

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high school physics; and from there on it's sometimes called chi-square.

If you get into a condition that the water is saturated in the downcomer everywhere, taking into account that the pressures vary in the downcomer, is saturated everywhere, then if you produce a void somewhere so you have just a little more heat, then you have an unstable situation.

In the production of the void, it reduces the heat, reduces the pressure, produces more boiling, more flashing, which then pushes up more water, and you throw the water off on the system.

Hence, it is not limited in size. Those of you who might have a little more time can go to Yellowstone and see the action at Old Faithful.

DR. PLESSET : It should occur in LOFT, in other words?

DR. ROSZTOCZY: Since it came out in Semiscale in a very pronounced manner, we have gone back and asked the question: Is this unique to Semiscale, or has this not been observed in the other test apparatus where tests have been conducted?

The answer is it has been observed in every single apparatus on which tests have been conducted. It has occurred in the German test, in the Japanese, in LOFT, in the special test done on large scale, actual scale.

DR. PLESSET : No doubt it occurs, Zoltan. Maybe it has trivial effects. If it's trivial, you don't make a big thing of it.

DR. ROSZTOCZY: Yes.

DR. PLESSET : Would you take me further on this nontriviality?

DR. ROSZTOCZY: Yes. Let me just continue along the lines of the slide. I think it will bring this up.

DR. PLESSET : I think Dr. Tong wants to make a remark.

DR. TONG: May I caution you that S-07-6 test was caused by a poor insulation in the downcomer. We reported that, and we are modifying it. The Semiscale-downcomer interrelation is going to be repaired and repressed.

DR. PLESSET : Yes, I know

DR. TONG: The consequence you got from S-07-6 may be contributive to the poor insulation. It may be atypical. There should be a caution on that interpretation from that, that it may be misleading.

DR. PLESSET : I think that's a very good point, Dr. Tong. I remember when the subcommittee heard about the S-07-6. There were strong reactions to the lack of insulation or effective insulation.

DR. TONG: Yes.

DR. PLESSET : I don't call it a downcomer; I call it a hot pipe, which is what it is, connected to another hot pipe.

That is Semiscale in this context.

I think we should be a little less -- I don't know what -- I have been careful with my literal-minded friend about the significance of this for other prototypical or large BWR systems.

Now, Zoltan is being very serious about it. I think it's an interesting thing, but its pertinence may be a little bit open. It could be a difference of opinion. I see Zoltan doesn't look happy.

DR. ROSZTOCZY: No. I think I agree with most of those remarks. I will come back to those at the end.

One thing -- in our present status we are saying the phenomena is real. Whether it would occur on a PWR, we don't know it yet; but we don't think it's something limited to small sizes.

DR. PLESSET : One-dimensional configurations?

DR. ROSZTOCZY: That's correct.

The second thing that we know is that looking at the licensing code in RELAP 4, and the codes used by the vendors, it becomes obvious that some of the basic physics needed to predict or handle this phenomena is simply not in the codes. So we know that the codes which are today being used for licensing just don't have the capability to handle this phenomena.

Therefore, going back to the available licensing

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calculations cannot answer the question. We need some other means to answer the question of whether it is important for PWRs.

So we did the best that we could. We went to our most elaborate and most difficult code, which is TRAK. We have done calculations with TRAK in 3-D vessels for an actual PWR. This is being done at LASL by the TRAK people. We had a presentation from them. We know the results they are getting.

The evaluation of this is that they do see this phenomena actually occurring in their PWR calculations.

The fourth observation here is kind of a side observation, that using the RELAP 4 code, it did not predict this behavior. There was a problem with the code, and in order to resolve this, they have repeated the calculations, putting less ECCS water into the system than actually was done in the past. The purpose was to avoid the stacking up of water in some of the boilers.

When they did this, then they got results which are much closer to the test result. This is a possible indication that maybe in the test some subcooled water is being by-passed that doesn't enter the downcomer and doesn't enter into the process the same way.

Summing all of this up, our present point is that, yes, we are well aware of the fact that the heat flux in Semiscale is larger than expected heat transfer in a PWR.

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Nevertheless, the phenomena being a real phenomena, we would like to know under what condition could this happen; and then we would want to compare that to the predicted conditions in a PWR and see if there is any overlapping.

And we know from calculations that if you change some of the other parameters -- not the heat flux but some of the other parameters -- you can step up the same phenomena. Basically if you get saturation in the downcomer, you get the phenomena.

For example, with a much lower heat flux than what was in Semiscale, like taking the heat flux of the PWR, but decreasing the subcooling of the ECCS water, the same phenomena are seen.

We also learned that the important test factors are that cold water enters in the actual systems in the hot legs. There are relatively small steam flows in the hot legs. In the cold leg, when you enter the ECCS water, there are relatively large steam flows.

This preheated water -- the water going into the downcomer -- is not on the temperature of the ECCS water but is already radiated. The heat flux is very important.

It's very important how much steam is generated in the reactor core. If in the reactor core the flooding stops momentarily because there is voiding in the downcomer and there is a possible flow reversal, then the steam generation is going to change in the core unless you properly account for the steam generation. You will get the correct pressure. If you don't have the correct pressure, you don't have the correct temperature. Some of these are put in the calculations by a simple assumption. You have to put all the basic physics in there and then see what happens. Once we have a tool in our hand, then we can take into account the low influx of the PWR; we can take into account the proper temperature, the proper core behavior, the proper core lengths. The core length seems to make a significant difference here. That may be why it was not as pronounced in the earlier tests.

When you take all of this into account, you will know under what condition it exists in PWRs, whether it's real in terms of whether you expect to see it in an actual loss-of-coolant accident. We think it's very important to resolve this as soon as possible.

One other item that comes up here is that we have provided a presentation last September to each of the licensing boards who are faced with PWRs.

We have updated this information that I just presented to you. We have updated this. That was in May 1979. There have been no licensing actions since May 30 because of other complications, mainly Three Mile Island.

As we go back to our normal licensing work, this will be introduced. I think the urgency is on to try to resolve it

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as soon as possible.

Yes, it is possible that the outcome will be that it is not important for PWRs. It is also possible that the outcome will be that under certain circumstances it is important for PWRs.

DR. PLESSET : Dr. Tong mentioned a very, very important point, the so-called downcomer, the fact it wasn't insulated. It's also one-dimensional downcomer; right?

DR. ROSZTOCZY: Yes.

DR. PLESSET : I know you are very much interested in the multidimensional behavior. I know what your attitude was about the German upper-plenum test facility. You were very strong in having a 360 degree rather than using a plane of symmetry with 180 degrees. You are quite sensitive to multidimensionality of a downcomer?

DR. ROSZTOCZY: Certainly.

DR. PLESSET : Good. On that basis, I would say there is another reason to be suspicious of the significance of this Semiscale on 7-6 because of the -- well, it wasn't insulated. Maybe that will take care of it; but even beyond that, the one-dimensional nature of that injection system, what do you think of that?

DR. ROSZTOCZY: That's exactly the reason why we went to the three-dimensional calculations and went to the actual size and actual geometry. We don't have the means or

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capability to check out the large three-dimensional test.

Up to now, those three-dimensional calculations show that this actually occurs; but again, please keep in mind, because it is in a large system, because it is in three dimensions, because the heat fluxes are different, what we see there is not as pronounced and not as strong as it was in the test.

Now, the great value and the reason why we think S-07-6 is a great test is because it's somewhat simpler than an actual three-dimensional case and provides a good basis to check the codes, whether some of the important physics is present in the code and works correctly.

Once we check those out, then we can go and do the necessary calculations either to show that we don't have to worry about this problem or to show under what conditions do we have to be careful?

DR. PLESSET : You discount the measurements and calculations for LOFT?

DR. ROSZTOCZY: No. Nowhere is there any discounting.

DR. PLESSET : Because the effects aren't very serious there as I guess from what we have heard.

I also gather you discount the calculations we heard yesterday about Zion. They are rather detailed calculations.

Yes, Stan?

DR. FABIC: S-07-6 provides an interesting test data, but I am very dubious that it is a good test to verify the codes primarily because the heat flux from the walls of the downcomer deployed in that pipe downcomer is really not known at all. They measure temperature history, but that is not in flux. You can specify anything you like and get the results. That doesn't change the code.

DR. PLESSET : I think that's a third point, really. I mentioned LOFT; I mentioned the Zion calculations. I think this is a very difficult thing for a code which otherwise might be quite useful. I think I agree with you on that point. But this is a hard calculation to try to carry through with any kind of confidence. If I understand you correctly, you agree with it?

DR. FABIC: The boundary conditions are not known.

DR. PLESSET : That makes it very difficult.

We will come back to you, Zoltan, so you have the last word.

DR. CATTON: I would like to add a comment on the codes. If it's an instability mechanism, there are two solutions. In the written code, you get one of them, not the other. I don't know how you are going to carry out checking out the codes for this particular problem.

DR. PLESSET : I think Theo has a comment.

MR. THEOFANOUS: Along the lines that Milton and Stan were talking about, also I am puzzled, because I see your Items 3 and 4. I would think that one-dimensional codes would have this effect more pronounced than two-dimensional codes or multidimensional codes. Yet, I think I got it from what you presented that the TRAK 1, 1-D, was not able to predict the phenomena. RELAP 4 was not able to predict that. You emphasized that strongly.

Yet, you are saying you somehow were able to fix the infinity track and are able to predict some resemblance at least to the phenomena. Do you have any comments about that?

DR. PLESSET : Yes, maybe if you calculate long enough --

MR. THEOFANOUS: If you try hard enough to do something, you get some resemblance.

MR. MATHIS: You get the answer you want.

(Laughter.)

DR. ROSZTOCZY: Okay. Let me start with Stan. I didn't mean to imply that the only thing we need is the S-07-6 data as it stands and then you can qualify the codes against that. No. Obviously, we need additional tests. Already some of them have been done which have spoken to the downcomer. New insulation is put into the downcomer to reduce the heat flux to a low level so the influence of the heat flux will be smaller than at the present time, and also you would get much better cut-off.

But the fact that the system is one dimensional doesn't mean that we cannot test the code against the system -- all the codes for licensing. They cannot predict with a one-dimensional system. Then something is wrong with the code.

DR. PLESSET : That was Stan's point.

DR. ROSZTOCZY: It's a necessary but not sufficient test.

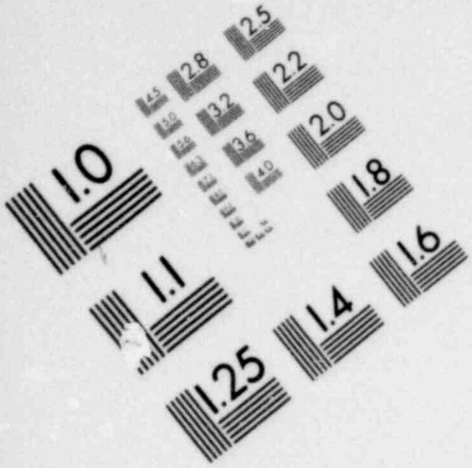
As soon as the data has been corrected and new test results are available which don't have to be answered in terms of the insulation, then you can check the codes against them.

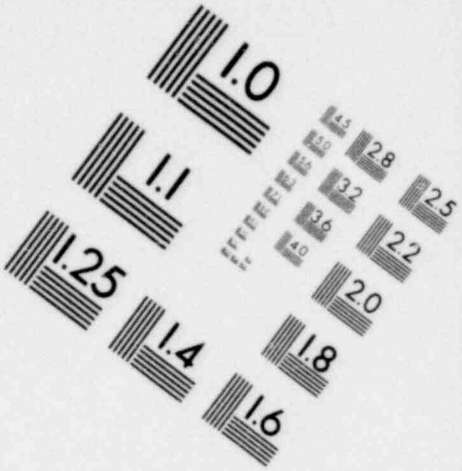
Let me go to Dr. Catton's comment as to why the codes can't do it. There is a simple reason why they can't. Nobody kept this in mind when they developed the code. We have spent approximately or somewhat over $100 million, we and the industry, on code development for loss-of-coolant accidents. Among the code developers, nobody had this in mind until we saw the S-07-6 results. In each of the licensing codes, there are basic assumptions which completely make it impossible for the codes to predict this phenomena.

For example, when the steam generation, the steam flow out on the top of the core is arbitrarily assumed to be a fraction of the inflow into the core, then that code cannot handle flow reversal, which brings saturated water into the downcomer.

MR. THEOFANOUS: You are going farther. Why do you limit your remarks to licensing boards?

DR. ROSZTOCZY: I am going to address all of them.





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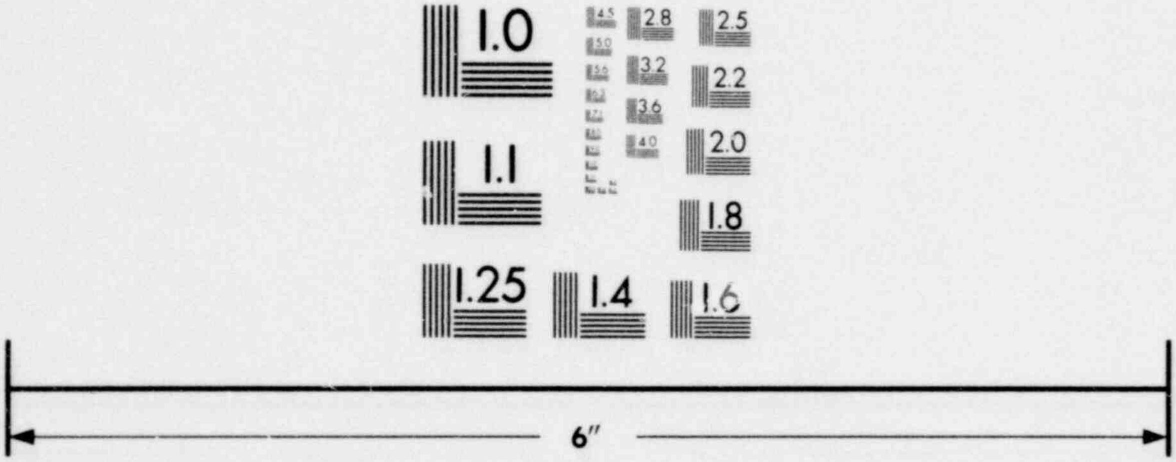

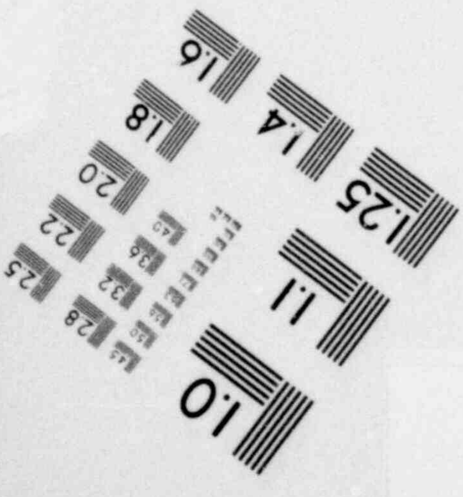

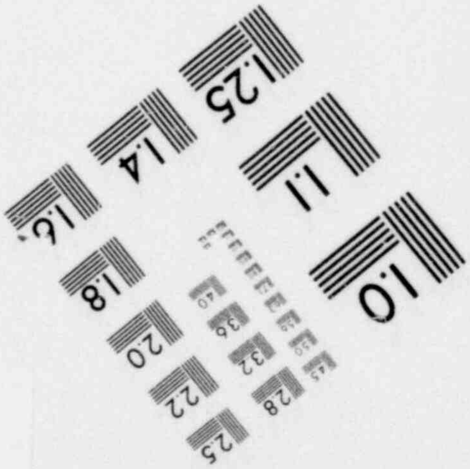

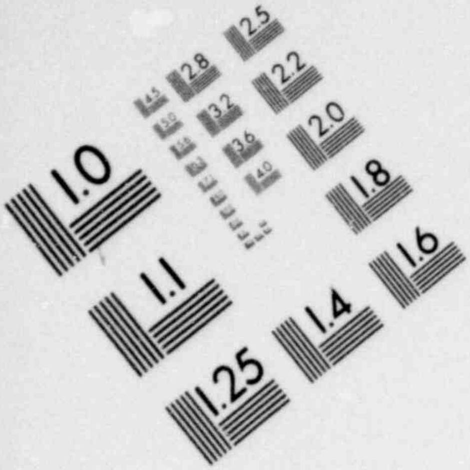

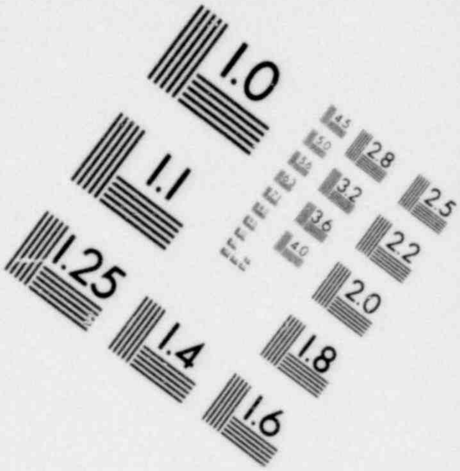

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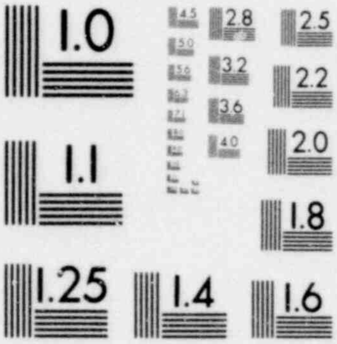

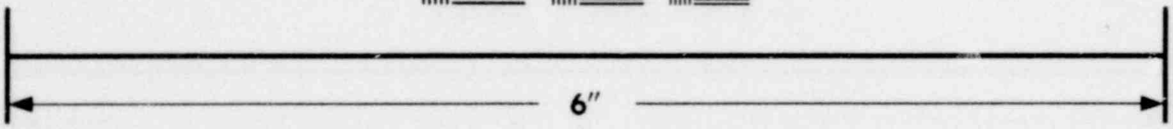

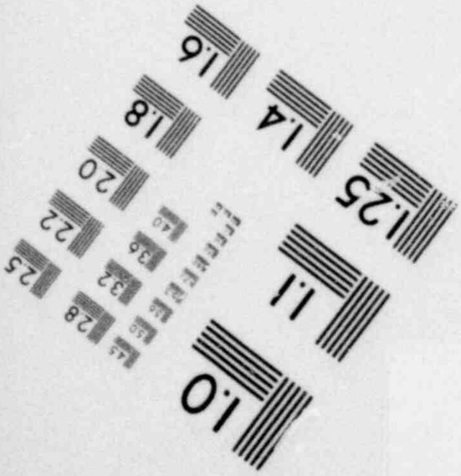

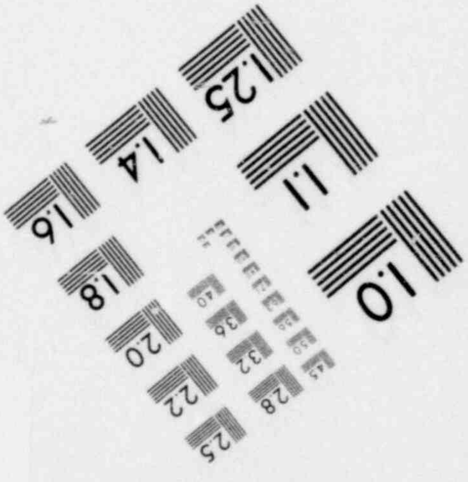

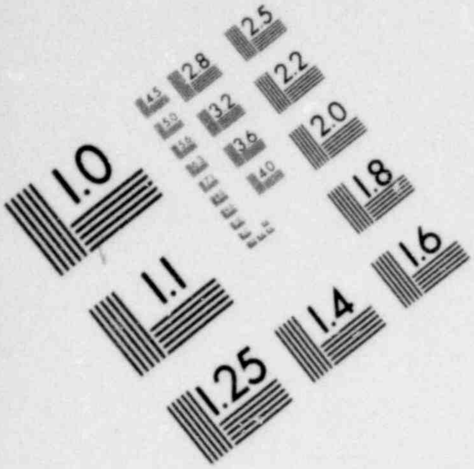

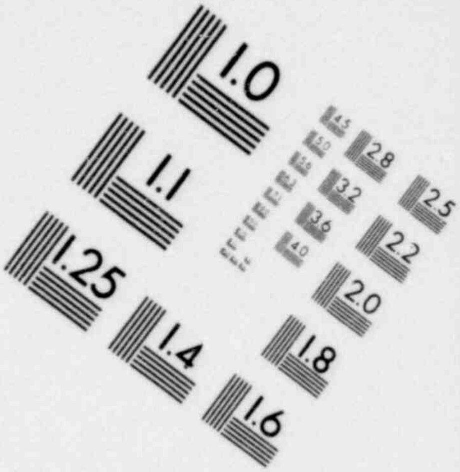

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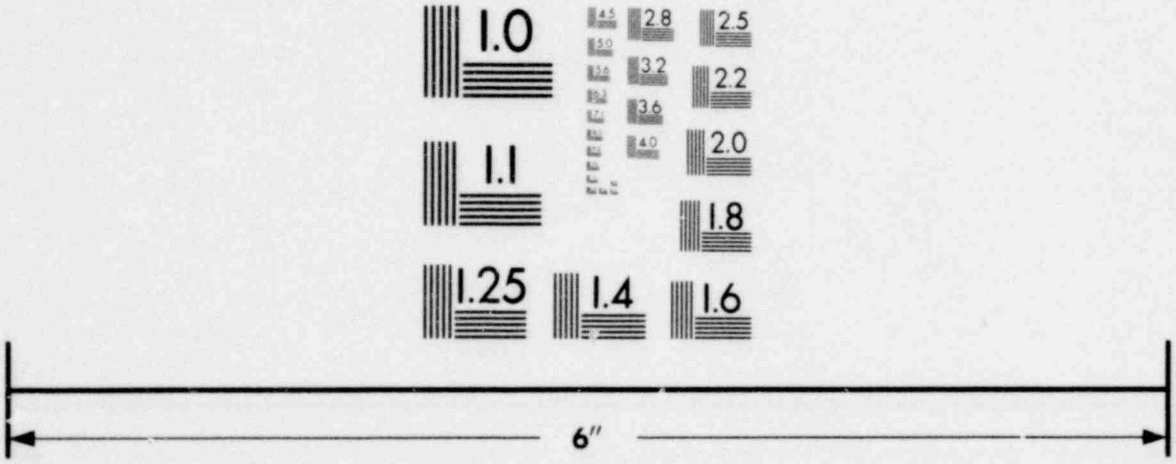

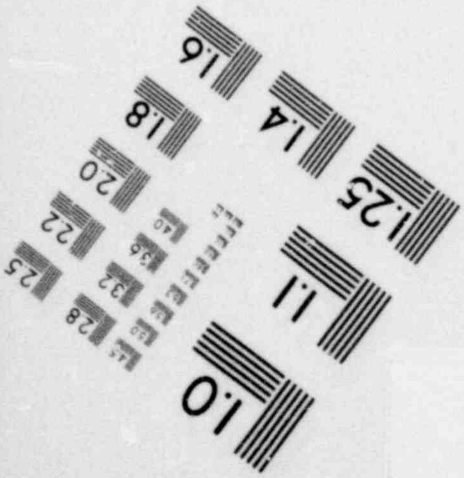

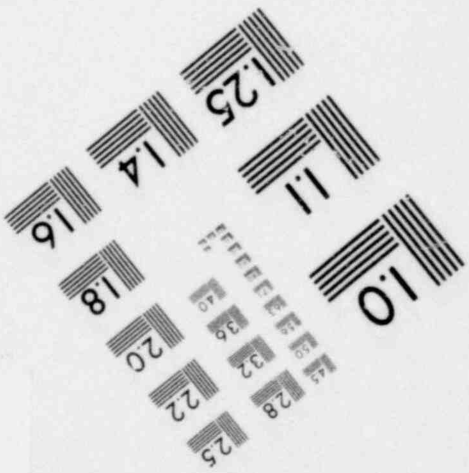

MR. THEOFANOUS: You still --

DR. ROSZTOCZY: That's the rather common assumptions. Some of the licensing codes use that. They don't introduce the cold water into the coolant. Therefore, they don't mix it with the hot steam that's going around in the core. Instead, theyput the cold water into the lower plenum.

DR. PLESSET : It's a good place to have it.

DR. ROSZTOCZY: Part of the phenomena -- it would be a good place in the real world if it would go there. It doesn't go there, however, in the calculation that is being put in.

There are some of these assumptions going into the calculations which made the calculation smoother. They could progress maybe on a shorter calculational time; but those phenomena -- when we go to the more elaborate codes -- and I think that was Dr. Theofanous' question -- the TRAK 1-D is entirely different than the TRAK 3-D. The difference is not just going from one dimension to three dimensions. TRAK 1 was using a reflux model while TRAK 3-D handles the problem entirely different.

The conclusion I got from the Los Alamos people working with TRAK 1-D is that they are unhappy with the reflux model in TRAK 1-D. They gave up on the prediction of this problem until TRAK 1-D will be converted into a two-fluid mode next year. Then they will come back and calculate it.

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The basic problem is the two-phased flow model in that version of the code. The two-phased flow model in TRAK 3-D is entirely different. They have more faith in that than they have in the TRAK 1-D.

Right now they believe that the calculation that they are doing for a PWR with the 3-D version and the code two-phased model is a more appropriate model.

DR. PLESSET : Stan?

DR. FABIC: Just a point of clarification about nomenclature.

As you are aware, the two-fluid model was used in TRAK for anything inside the vessel. The reflux 1-D was for all the loops outside the vessel.

They decided that when they looked at this problem, to look at that pipe, which is called the downcomer, it was not part of the vessel, as part of the loop. So they used the 1-D flux model to describe the phenomena in the so-called downcomer.

We have known that our reflux models, physics in the reflux were not very good.

That's the reason why they were not happy with the result of the calculation. Now they are trying to do it all again.

That's the nomenclature.

DR. ROSZTOCZY: Yes.

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DR. FABIC: There is no such thing as a TRAK 1-D.

DR. ROSZTOCZY: The main difference, as far as this program is concerned, is that this is handled differently in the calculation.

DR. PLESSET : I think Stan made a good point.

DR. ROSZTOCZY: The other concern we have is that when the reactor designers designed the ECCS system, they did not have this phenomena -- they didn't think of this phenomena. It wasn't in their design calculations. Should this arise in some of the loss-of-coolant accident, it could set some requirement for the ECCS system. It's possible that those requirements can be very easily met. The subcooling is a very important parameter. It is important that you not put in water than a certain temperature. I am certain you can cut it out from the PWRs. If you know the subcooling is not hot enough, it will not happen in a PWR. This is not a design requirement. There is no such limitation on those plants, depending on where the plant is located or what time of the year they are at. There is different temperature water available for ECC.

It is possible that what is available is sufficiently low.

DR. PLESSET : I hope you don't make this requirement yet to control the water temperature.

DR. ROSZTOCZY: We made one requirement. That one is

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spelled out here. We have required the vendors to provide an evaluation where they can show, (a) that the calculation is proper even if it doesn't follow the actual physics, but that that calculation is still appropriately conservative. Or if they cannot show that, modify the calculations to account for this phenomena should this phenomena come into play after some break size or some calculation, that the calculation would hold. That's the only requirement.

DR. PLESSET : Maybe we should go on.

DR. ROSZTOCZY: So. The only other point is that we do need this analytical tool. We are encouraging everything in that direction to see what can be done with that.

(Slide.)

The last group of data needs that I am going to talk about relates to transients and accidents.

Transients -- and I mean here transients like loss-of-feedwater transient, excess load type of transient, rod withdrawal, turbine trip, station blackout, and so on, typical transients recognized. These transients are a lot more mild and the consequences are a lot more favorable than the consequences of what we call accidents.

Because of this, we have been doing significantly less in terms of core development, in terms of actual calculations for loss-of-coolant accidents.

One recent test program performed on the Peach Bottom

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reactor -- this was in the spring of 1977 -- showed that the calculational techniques for these transients, the error can be a significant error.

Boiling water reactors, there is void in the core due to normal operation. Once you go through on a transient, then this void might collapse or might increase in value which affects the reactivity of the core.

The problem found in the Peach Bottom test was that the void collapsed; it wasn't correctly treated in the code. Because of this, the code results gave us quite different consequences than was measured.

At that time we issued a Board notification concern on this, and this problem is still being resolved. General Electric has developed a new computer code that accounts for the phenomena, and they have shown that that code can predict the test results obtained in Peach Bottom. They are going to do new licensing calculations for those transients which have void collapses.

Challenging test results for PWRs are not available. As part of the start-up of the plants, there are usually some transient tests. Those tests are oriented strictly to activity. They demonstrate through those tests that various equiment are working. They don't have the instrumentation for the severity in the tests which provide challenging tests for computer codes. So we do not have any real good data on

PWRs. Because BWRs don't have a void in the core during normal operation, we think they are probably less sensitive and we think the accuracy of those calculations is significantly better. Nevertheless, we would like to see some confirmation of this.

We are seriously considering requiring some selective tests on selective plants, both pressurized water and boiling water reactors, to provide a data base for the verification of the dimension of these codes.

Once we go to the accidents, we are reviewing the steamline break and feedline break analysis methods. In this review, we are again finding kind of a shortage of available experimental data. There is some separate-effect tests available to justify certain problems in the codes. There is no good test to test the code in any sense.

Analysis methods for steamline tube rupture have not been submitted and have not yet been reviewed in detail due to some of the failures in operating plants. We are paying more attention to this and are requiring the vendors to submit calculational methods in the future. We are going to look at those carefully.

Experimental verification will be needed. Our feeling is that the best way to attack this is to have some data on experiments to be done on an operating plant, and to also provide some additional data on test equipment like LOFT and the combination of the two should be sufficient to provide that.

For the accidents, the steam generator dynamics have been carried out. They are important.

Codes have been developed for that and should be really representative.

These are basically the test programs that we are looking for.

In closing, I would like to make just a few remarks. What we have observed during the past couple of years is that most of the information relative to plant safety evaluation is coming from the experimental programs. Quenching of the core is a good example of that; the voiding of the Semiscale downcomer, the observed high steam generator in the EPRI test, and the void collapse as it was observed in the Peach Bottom test.

Unfortunately, the calculational techniques, including our sophisticated calculational techniques, at the present time are not good enough to predict this phenomena. So any phenomena we are running into we might not have accounted for is simply coming from the experimental.

At this stage of development we find that -- it is our opinion that it is very important to keep on doing some of this testing. It is very important to have test facilities available should the need arise for some urgent testing. So

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if some problem comes up somewhere, because we cannot test the calculations to the DBS, it is important for us to have some test facility available to check this out. We are hoping for the continuation of Semiscale testing in the future.

I would like t- point out one weakness that we have observed during the test itself in the past few years. This is the analytical support that has been provided for the test programs. We fear that the analytical support hasn't been as strong as it should be.

For example, the tests that are being done at GE and San Jose where EG&G has the responsibility to perform that analysis. For example, the computer code used for the analysis of the test doesn't qualify to do some of the work that the test was set out to do. We need a better code for some of the tests.

The Peach Bottom tests, which were very significant tests for boiling water reactor transients, there were no pretest predictions done by any means available to NRC. The only predictions were done by GE. NRC as a whole did not make an effort to do the pretest predictions. These tests are rather expensive. We will see only a few tests of this sort. I think it is important that we try to get them.

So I would like to urge both the contractors like EG&G as well as the NRC to pay increased attention to the analytical support work of this test, the data, the information that comes out that can be interpreted faster and more accurately into the licensing procedure.

As you know, we have instructions from the Commissioners to report to the licensing board every bit of new information that we run into, even if we haven't had a chance to resolve that, whether it is important or might not be important for licensing process. If we operate in this way, I think it's rather important to follow up on the experimental points as to whether we know if they are important or not important. That way we can keep the licensing process going in an efficient manner.

That completes my presentation.

DR. PLESSET : Thank you, Zoltan.

With regard to reporting to the licensing boards, do you report to the licensing board new data on heat transfer? Mr. Nelson showed us this picture, the data bank. There are some new things. Do you report those?

DR. ROSZTOCZY: We report to the licensing boards on every information that could significantly -- that has a potential to affect the licensing of the particular plant.

DR. PLESSET : If we had another bit of information on heat transfer like -- well, this Iloege was mentioned yesterday. That could be very significant.

DR. ROSZTOCZY: Yes.

DR. PLESSET : Do you report that?

DR. ROSZTOCZY: Whenever we run into something that has a significant effect, we do. What was observed there is that there was more heat transfer than possible in the calculation. That has been reported both in terms of the increased steam generation and in terms of the possible improved heat transfer. That has been reported in that sense to the licensing boards, yes.

Now, if we run into a case which shows us that a given licensing type of calculation is most probably better off because of some new item that we found, but we have already found the plant fully acceptable as it was, then the fact that it's even better off than how we found it is not that important in the sense, because we are 100 percent recommending to the licensing board that the plant stand fully behind it There is less emphasis on reporting those things which make an already acceptable case even better.

There is a lot more emphasis to reporting those items which could possibly lead to other requirements beyond those presently in the technical classification of the plant.

DR. PLESSET : So you don't put particular emphasis in a report to, for instance, the LOFT results?

DR. ROSZTOCZY: The LOFT results are showing quenching of the core earlier than it was expected.

DR. PLESSET : They also showed very low peak clad temperatures altogether.

DR. ROSZTOCZY: No. I am afraid we are not arriving at that conclusion. As a matter of fact, we are looking at now in some detail -- as you know, we have done a so-called licensing calculation for LOFT tests; so did Exxon Nuclear Corporation. When we take up from that calculation some of the items which were introduced and it's specific to LOFT, but it didn't exist -- namely, that the LOFT fuel is a low-density fuel that goes through on much more fuel densification -- when we take some of those atypicalities off, then the remaining margin between our licensing process and the actual temperature measured in LOFT is significantly lower than the margin introduced into the ECCS hearing back in 1972 during the hearing.

We do have a concern that possibly the margin might be lower than we all thought it throughout the years.

DR. PLESSET : Some LOFT might not be licensable?

DR. ROSZTOCZY: No. That is not so. We have learned a lot; a lot of tests have been run. Lots of calculations have been run since 1972. Seven years have passed. During the seven years -- these were very intensive years in terms of loss-of-coolant accidents. A lot has been learned. Maybe this is an appropriate time now to pull all this information together, sit back, think about it a little bit, see what have we learned, and see if our licensing approach that was passed on back in '73 needs any update.

DR. PLESSET : Well, I must admit to being a little taken aback by what you are saying and the kinds of things that should be of concern for the licensing boards by way of helping them, which is what one is supposed to do; right?

DR. ROSZTOCZY: We have two responsibilities. One of them is to help them in any area wherever they ask for our help. The second one is simply to inform them. It is our responsibility to bring to their attention various things that are going on in various results, calculational results, that we have obtained which might have an effect on the licensing that they are faced with. It is our responsibility to provide this for them.

We also provide it with an appropriate explanation and an appropriate -- presenting the problem in an appropriate manner.

DR. PLESSET : One thing I am trying to maybe get at is that I think Semiscale certainly is a useful and significant test facility; but I would regard it more in the class of a separate-effects facility rather than as really giving information with not a lot of qualification and reservation in its applicability to a PWR. This is what I was trying to get at. I don't know if anybody else agrees with me or not. If so, maybe they can -- or disagrees. Please feel free.

Does anybody else want to comment on this last point?

MR. ALLEMAN: I don't quite understand how what you said, Zoltan -- although the predictions for LOFT look to be conservative, you don't think they would be conservative for licensed reactors?

DR. ROSZTOCZY: Let me say maybe a few more words about that so we understand it better.

Back in 1972 and '73, when the hearings were going on, and there was various information in the hearings, calculations were introduced into the hearing by most of the parties where they showed what kind of peak clad temperature they calculated as a so-called model. They showed what kind of peak clad temperature they would expect to see in the real world.

There have been some calculations of this sort. I believe Dr. Zyrmak has done some. There was information available. When we went to the LOFT test, one way one can get more information of this sort is to do a calculation with the licensing codes for LOFT and compare the results of that calculation to the test. It has been done. It was done by running the test. The calculation was performed.

What has been done post-test and is still going on is to take this calculation and then take off some of the assumptions one by one to try to find out how much is accounted for by the calculation.

Two licensing calculations were done by LOFT, one by Exxon, one by the NRC. The Exxon calculation -- if you want, I can pull out the slide.

DR. PLESSET : That's all right.

DR. ROSZTOCZY: The accident calculation was higher than the actual measured temperature; but the difference between the two was not as large as the difference shown in the hearing calculations or in the Zyrmak study that was done.

The difference between the two calculations was large, was as large or larger than what has been shown in 1972 and what has been shown in Dr. Zyrmak's work; but there were a few things in the calculation which are very unique to LOFT.

One of the problems is that there is a large penalty on fuel densification. Because the LOFT fuel has been manufactured a long time ago, it's a low density fuel. If somebody would introduce that type of fuel into a reactor, we would - still require the same penalty on fuel densification. So they put into the LOFT calculation this fuel densification penalty.

If we remove that and replace it as much as we can with something which would be more appropriate, our calculation comes very close to the actual calculation.

So the extra margin we have there is due to -- I think there was one other factor. It appears there is no major disagreement with the Exxon calculation now; but the margin that you see there is not as large as we have seen before.

Again let me emphasize what was emphasized earlier: One has to be careful. One can't pick up this number and ultimately carry it to a PWR. You have to check and look at

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the fact that there are other things which might provide a margin on the PWR but does not show up on this test.

After you compare all of those, then you can say my evaluation today is that all these assumptions provide margins.

MR. ALLEMAN: You say the margin is statutory?

DR. ROSZTOCZY: The basic principle which has been stated by the Commissioners in the comments that they made at the time when they issued the ECCS criteria was that there should be sufficient margin to cover the uncertainties of the calculations, plus a safety margin beyond that. It has never been specified in terms of how big the extra margin should be. Instead, a judgment was made that the requirements specified in Appendix C should be put into the calculation, all of them provide sufficient margin to meet these principles.

What I am suggesting is that maybe are getting a lot more information available today. Maybe we are getting to a phase where one can specify this a little bit more accurately. Maybe one can get a handle on what is the uncertainty of our codes.

Once you can quantify both of these, you can compare one against the other and see what comes out.

My personal judgment would be that nothing would come out of it.

Then we would change our way and how to handle this.

For example, one might find that we have enough

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margin in the large-break calculation but not in the small-break calculation. Maybe you change the way you handle the small break.

MR. ALLEMAN: Our uncertainty of the code is less now than it was?

DR. ROSZTOCZY: No. Unfortunately, I cannot say that. Again based upon the limited knowledge available, people tried to formulate some idea of the uncertainties. I don't think those are accurate. There are possibly some statements about that in the hearing notes.

This margin that was shown, as I recollected, was on the order of 500, 600 different margins. So that was supposed to cover the code uncertainties.

Now, if we go back and turn to the people working with the code and ask how good these codes are, then I think they are good to something like plus or minus 500 degrees.

That's not very good as a result at this time.

We learned a lot about the codes, but the overall picture of the codes is not that up to date.

Maybe it is because of our ignorance.

DR. PLESSET : You indicate a lot of reservations, which are commendable, regarding the optimism with which we should apply LOFT data to a PWR. I wish you had the same amount of negative reservations regarding the applicability of Semiscale to a large-scale PWR.

(Laughter.)

DR. ROSZTOCZY: Dr. Plesset , I do; I do. You expressed most of those before I got to them. Our vision of Semiscale is no one should take the 400 seconds delay. That would be complicating the problem. The only thing that is meaningful is to have an understanding of the processes that the heat plays, then feed those into our extrapolation technique which we have as computer codes and then see with these extrapolation techniques what is the prediction for a PWR. We have stated that very carefully in both the Board notification notice, which was issued last year in September, and also in the update issued on May 30 of this year.

We are skeptical. We do not know if this phenomena has a significant effect for PWRs. We would like to know it and know it as soon as possible. For that reason, we find it's a very urgent problem. We hope to resolve it fast. If we can resolve it in two months, put it behind us, we are better off.

DR. PLESSET : I think we can go down the line.

Ivan?

DR. CATTON: I have a couple of comments. First, I would like to take an opportunity to emphasize what I feel is a need for more balance between experimental analysis. I think you were bringing this out earlier, Zoltan. I have the same sort of reaction.

Also, Semiscale is a highly 1-D system. It serves, as far as I can tell, as an excellent research tool; but with what I feel is in the past a lack of this kind of balance, many of the scaling questions are not answered. I just don't see how tests like the S-07-6, without knowing how to scale from one system to another, other inadequacies, can be viewed without a great deal of qualification.

However, I do, I think, side with you on your view of the 400 degrees. I think it's a serious question and probably ought to be looked at, but not without stating all of the qualifications.

DR. ROSZTOCZY: If you would develop a model from basic physics, just to start physically with that, and you compare it, you put it into one dimensional code, and compare it against the Semiscale result, and get back the test result, it would give you some comfort for using that code.

DR. CATTON: I feel somewhat unsettled that we have a multimillion dollar code program, a 1-D facility. In a sense, I am not surprised. It is indeed an instability. I wouldn't expect the code to predict it. I think you have to look for instabilities to predict.

DR. PLESSET : Theo?

MR. THEOFANOUS: In the interests of time, I don't want to ask Zoltan a question, because he might take a very long time to reply.

DR. PLESSET : So you are making a statement?

MR. THEOFANOUS: I want to make a comment.

One, I agree with you, Milton, on your view of Semiscale; I think it is a useful facility. It is the kind of thing that can trigger our imagination to see certain things and think about the problem. However, as we learned many times in the past, many times we have to go back and explain things away that are not relevant to PWRs instead of the other way around.

So every time you look at Semiscale results, I think you should take it with a grain of salt.

Secondly, we should try to explain them. I think that we should be very careful before we try directly to apply them to PWRs.

As far as Zoltan, I want to disagree with you, Zoltan, with your rather grim view of how much we learned with the new codes and with the new analytical efforts that we have been over for the past three years. I myself found them very useful and relatively large contributions to the understandings of loss-of-coolant accidents.

DR. PLESSET : Thank you; particularly the last comment which I echo which should relieve the pressure building up in Stan. He didn't even say anything.

(Laughter.)

DR. PLESSET : Thank you, Zoltan. You certainly stimulated us. That's good.

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I think we have to look at the program a little bit, and Mr. Mathis, the other ACRS member, has been a big help to me.

What he is suggesting is perhaps we have [illegible] minutes for program accomplishments, with emphasis on TMI work. As he pointed out, we are pretty much up to date on these things. I wondered if we might not omit that, perhaps get another update at another meeting, if that will not be too painful.

We do want to hear the discussion of the scaling. We are all very concerned with that. Perhaps while you mull that over and accept that negative offer, we might take a break at this time for 10 minutes.

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(Recess.)

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DR. PLESSET: We will go to a presentation on Semiscale scaling that Mr. Larson will present.

MR. LARSON: Gentlemen, my name is Larson. I am going to have the honor today to discuss with you the controversial topic of scaling.

(Laughter.)

DR. PLESSET: I am glad to hear that opinion.

MR. LARSON: One thing I would like to point out before I start is that yesterday's comments necessitated the addition of a few slides to this presentation. I think you should have a supplemental copy.

(Slide.)

It is obvious that as long as I have worked for Semiscale, there has never been a comlete presentation on the scaling of the system. any of the systems, the philosophy, the approaches, the different criteria, and the comparables. I think the interest that is obviously apparent here dictates that I go through some of those types of discussions.

It's also clear there is a considerable amount of interest in scaling from a standpoint of extrapolation of data from Semiscale and/or LOFT to the larger-scale facility. I would like to make it clear at this point that in the past we have not advocated extrapolation of data from the facility. We don't necessarily intend to in the future. We realize by virtue of the scaling — and I think there was a comment that Dr. Plessett made yesterday — by virtue of scaling, the system is and has problems of one nature or the other. We have to consider those things.

We also need to look at scaling from the standpoint of how good is the data from a code standpoint, usability of the codes, for development and assessment and verification, whatever you want to call it.

To address some of these concerns, I would like this morning to go over three areas:

Number one, the approach taken in Semiscale as far as scaling is concerned. I would like to discuss three different techniques that could have potentially been used and then discuss the advantages, the disadvantages of those techniques.

I'd also like to discuss the criteria that we have established for scaling of the MOD 3 system and then discuss a few of the compromises, resolutions to those compromises, that we think we have some kind of a handle on at the present time.

As far as the discussion of compromises, I will discuss very briefly some of the large-break scaling. I will call them distortions for lack of a better word.

I will also discuss some of the things we are currently looking at for small breaks.

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It is not my intention here to answer all of the scaling problems and questions. I am not at this time prepared to do that. What I intend to do is give you an idea of the kinds of analyses that we are doing and the direction that we are taking at the present time.

(Slide.)

First of all, I think it is probably advantageous to start with a definition of scaling under which we labor. That is perhaps simplistically stated as the technique whereby an important phenomena in some reference system are simulated in a smaller scale, smaller geometry system. Scaling is necessary for obvious reasons of cost, et cetera.

I think, as I mentioned earlier, Dr. Plesset made a comment yesterday that was appropriate, in that when scaling there are certain things that you have to keep in mind.

Number one, in general I don't think it is possible to maintain geometric, dynamic similarity in a scaled experiment for all kinds of concern. It is simply not possible. An example of this is the difference between blowdown and reflood. For blowdown, surface area-volume ratios may not be as important as they are during reflood when steam generation rates are extremely important.

Another concern or something that must be kept in in mind is that distortions will exist in the system where multidimensional effects are present. That is a result simply of the fact that some of the scaling considerations that we work with dictate the need to maintain elevation and perhaps also volume.

That introduces L/Ds in the system that are large, hence causing the system to be largely one-dimensional.

(Slide.)

There are at least three possible scaling techniques, probably more. I would like to discuss some of those at the present time and then indicate why we picked the particular technique that we did.

The first technique is linear scaling, whereby L/Ds from the referenced system are maintained in the scaled system. This technique has its advantages and disadvantages, like the other three I will discuss, some of which are in linear scaling. Acoustic times can be maintained, given some time-scaling factor. The technique has the advantage that it's been used in small-scale, steady-state systems somewhat successfully.

It also has disadvantages from an integral transient standpoint in that time scales are shifted so that rate processes change, such as volume production, flashing; extremely small linear dimensions result, also area and volume. They change by a scale factor squared with the

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other. We also have the problem that, as an example, in the linear scaled system of 2 megawatts, such as Semiscale, you have heater rods that are extremely small, the size of a piano wire. It's utterly ridiculous to scale from that standpoint. You can't get 2 megawatts into a bundle that is 40,000 piano wires high.

Dimensionless numbers are a technique. This has been used in mechanics for many years. It has a sound basis from the pi-type theory or equations in motion. In a transient facility, however, there are many numbers to consider. The controlling numbers may not be the same throughout the whole transient. I think, as an example, Barnett tried to scale — I think successfully — CHF with dimensionless numbers. He wound up with 14 dimensionless numbers.

In a system such as Semiscale, there are compromises, and it is not possible to maintain dimensionless numbers such as Reynolds numbers for all periods of time. Reynolds numbers are powerless for that matter.

Volume scaling is also a viable technique and the one in which we have used — it is also used to scale the LOFT system. It has the advantage that the time scale relative to the referenced plant is preserved. That's important from rate process control, production quality, maintaining transitions from subcooled to saturated flow, for example. That occurs at the right points in time.

We also have some considerations in that for loss-of-coolant type transients. We need to have the proper resistance distribution in the system so we get [illegible] splits where we think they will occur in the larger chain.

Volume scaling allows you to do that by maintaining volumes in the system and thereby producing areas in piping, say, that are too fat, such that you can work with them to provide the right resistance distribution.

Volume scaling is not without its problems, of course, just like all the other techniques. You can't simultaneously maintain elevations, volumes, length-area relationships, and resistance at the same time. So by nature of the technique, there have to be compromises.

Another perhaps disadvantage of volume scaling is acoustic transit times are not maintained simply because the links are short relative to the reference point. That can have an effect when subcooled decompression loads are concerned.

(Slide.)

I would like now to discuss the MOD 3 scaling. As I said, we picked the volume scaling approach. The referenced plant, the design of the Loops, was the

Westinghouse Trojan plant. The design for the vessel was basically taken in a scaled sense from a Westinghouse UHI design. That was originally the purpose of the MOD 3 system, to look at UHI considerations.

I would like to point out at this time that the initial scaling of the facility was done by EG&G personnel. We had a lot of help from consultants, reviews with the vendor, and also NRC personnel. The final design was reviewed both by NRC and also our review group. So it's been a joint effort. There have been a lot of compromises. We are all aware of that fact.

The next slide simply presented the results of volume scaling facility. I don't intend to belabor this slide any.

(Slide.)

The important columns, I think, are the two listed percentage totals. It is simply a comparison of the relative distribution of volume in the system, in the referenced plant — plants, I should say — and the MOD 3 system. It indicates essentially that we were quite successful with the exception of the guide support tubes of providing the right distribution of mass and volume assistance.

As I stated before, we considered that important because the distribution of mass at a particular temperature indeed determined the flashing characteristics and indeed determines the flashing characteristics and decompression characteristics of the system.

An obvious question that comes up is what kinds of implications does volume scaling have? We, I guess I should say, labored under what we called modified volume scaling. That is, we did endeavor, as the previous slides showed, to maintain volume distribution in the system. We also had the additional requirement that we wanted to maintain elevations of the system where possible.

(Slide.)

The intact loop, as I will discuss later, we had a Type I steam generator which is short. Elevations relative to the referenced plant are not maintained there. In any event, the attempt was made to maintain geometric and dynamic similarity in the primary heat transfer regions of the system, number one, the core, and in the steam generators.

The compromises that that, by virtue, produces are that the piping line are not maintained; the piping area, hence diameter, is not maintained. As I said earlier, with the use of orifices, one can provide the right resistance distribution in the system, and we considered that more important in terms of providing the right flow splits, mass flow distribution, than maintaining acoustic transit times.

Another particular distortion that volume scaling introduces is that piping surface area is not maintained. I think we have all seen the results from S-07-6, the distortion in the surface area to the volume, which I will briefly toucvh on a bit later, what that does to the characteristics in the downcomer in terms of ECC penetration.

Another thing I mentioned earlier is the concept of the L/Ds being large, hence causing the system to be primarily one-dimensional. I say "primarily because we have run some tests where even in our small bundle, which is about 6 inches in diameter or less, we have seen some multidimensional cooling where we injected steam into the upper plenum.

The question that you may ask right now is, "Well, how well does volume scaling really work?"

(Slide.)

This slide shows a comparison of two counterpart tests, one LOFT L2-2, which you saw the results from yesterday, and results from Semiscale MOD 1 experiment. This is not a MOD 2 experiment. S-07-2 experiments were designed to be counterpart tests, to actually look at the question of how well was the scaling accomplished.

I think this slide indicates that we indeed do get similar break flow characteristics in the two facilities. Now, the Semiscale flow rate here -- the flows have been put on a common basis by dividing one by the volume ratio of the two. You can see that there is some difference here during the subcooled blowdown. One must keep in mind the uncertainties on both the Semiscale and the LOFT data. I think a good estimate of that is plus or minus 20 percent. So that the two do indeed overlay here.

The end result of that in terms of system depressurization characteristics is shown on this slide. Indeed, we do get depressurization characteristics as a function of time that are similar in the two systems. We have some confidence that we are getting the right rate processes, energy generation rates, quality production, at least for blowdown in the facility, so that there is some basis for believing that volume scaling is a viable technique.

There are slight differences here. There were some very slight differences in initial conditions primarily in hot-leg temperature between the LOFT and Semiscale experiments. Our experiement we started with a 580 K hot-leg temperature; in LOFT, the temperature was about 500 — excuse me, in our experiment we started with 587; LOFT was 580. The saturation pressure of the system was a bit different.

I would like now to briefly discuss a couple of the MOD 3 components.

that. The spacing in the core is a little bit off.

We do have structural surface area that is larger than that desired from a scaling standpoint. As I said, we did try to mitigate those consequences through the use of insulation which we discovered now does not work to our satisfaction. We are building new insulators to hopefully combat that problem.

The diferences in the rod properties are handled through two techniques. The electrical power control is controlled in a rather sophisticated fashion to hopefully match the heat flux that we expect from a nuclear rod during a transient where the nuclear rod is seeing the same boundary conditions. There are two techniques for doing that.

We are currently experimenting with an on-line computerized power control which will determine the power as the test is being conducted There is another technique whereby we iterate using results from one test, taking heat transfer coefficients, and then off-line determining what the power profile should be and using that in this test.

There are analytical considerations here in that conduction calculations were done to help identify the need for increased insulation and to mitigate the structural surface area problem.

(Slide.)

The intact and broken loops, as I mentioned before, have the scaling criteria of hydraulic resistance, volume distribution, and also pump suction leg depths. We consider the pump suction leg depth important from a structural standpoint.

I think you heard in the LOFT presentation yesterday that that may not be quite as important as we thought in the past. The only small-break experiment we have ever done, S-02-6, indeed showed that that was a consideration and did indeed determine some of the core recovery characteristics; although in that particular experiment core recovery was not anything substantial.

We did, as was mentioned earlier this morning by Zoltan, run an additional small break. However, it is a standard problem. We have not analyzed that data and established the influence of the pump suction leg.

There are compromises in the loops in terms of, as I said, lengths, flow areas, and again the surface area. We also had instrumentation that influences such things, perhaps, as the flow regimes and resistance. These treatments have been both experimental and analytical, as you will see in a moment.

We do hydraulic resistance tests, single-phase on the system, to determine that the resistance is what we desire. Conduction analysis has been done to look at the influence of the surface area relationship.

(Slide.)

One of the big concerns perhaps in a small break, especially, is the steam generator characteristics. As I think you are probably all aware, in the present MOD 3 system, we have two different type generators, one called the Type 1, basically scaled for LOFT; and a Type 2, basically scaled for a PWR. It contains full elevations and pressure drops.

That in itself constitutes two compromises.: One, length effects for the intact loop; number two, the broken loop has an oversized secondary volume. That is a consequence of two design considerations: One, the fact that we design the generator with the thought in mind we would like to use it in the intact loop as well as the broken loop; number two, it is extremely difficult without the use of filler pieces in that generator, just by virtue of the small number of tubes and the height requirements to get the secondary volume correct.

We are currently looking at the fixes for that. I have some results that I will present later, I think, that show we may be able in the near term, anyway, to drain the generatore carefully as a function of time, to provide the right surface heat transfer area as a function of level. The long-term fix for that is, of course, to attempt to put fillers in the secondary to maintain the right volume.

(Slide.)

I would like now to discuss briefly — and I don't intend to dwell on this, simply because it's not a topic of primary concern — the scaling considerations for large breaks. I think there are necessarily positive points by virtue of our scaling criteria. Some of these are listed. I mentioned probably all of them earlier in the presentation.

We do have the right resistance distribution, which is important in terms of flow splits, particularly in the core, whereas the stagnation point — this question came up several times yestereay.

Our energy and mass distribution is correct, which provides for the right rate processes, the right amount of relative energy exchange.

Our core geometry in terms of pitch, rod diameter, and length is correct. By virtue of control of break area to volume, we can get the right time scale; and we have attempted, where we can, to maintain elevations, relative elevations.

Our vessel is full length, as is our downcomer, which should provide proper elevations for such things as reflood head and the like.

There are particular concerns. That includes

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metal-stored energy and surface area to fluid volume ratios. The loops are fat, so the velocities are not correct. There is a concern about pump characteristics and, of course, steam generator characteristics.

I would like to address a couple of these things now with some of the data we gathered from tests in the past.

(Slide.)

I think most of you have probably seen this famous slide before. It is a comparison of the heat flux or estimation of heat flux from Test S-07-6, a conduction-limited calculation for a PWR downcomer that was assumed to see the same fluid temperature condition as was experienced during this experiment, and was conduction-limited.

Also, a calculation with our new insulator design, the honeycomb, which, by the way, has a conductivity, an overall conductivity of about .09. Just as a point of interest, the honeycomb conductivity is on an order of 2. It is not a very effective insulator. As a matter of fact, it is a reasonable conductor.

I think the results from this calculation are encouraging in that it shows what was about a factor of three or more higher in heat flux in that experiment relative to that estimated for the PWR. We are now down in the ballpark where the heat flux will be quite a lot closer, at least the calculations so indicate.

I would like to point out at this point that there are some scalings that have gone on to make these comparable.

We still have a a surface area to volume area show that is about a factor of 8 in the downcomer. What has been done is that the Semiscale values of heat flux have been multiplied by 8 to put them on a basis comparable with the PWR.

The effects of pumps in a large-break blowdown, I think, were discussed in some detail yesterday by John Linebarger.

(Slide.)

We in the past have found similar things. This slide shows a comparison of the downcomer flow. This is from a RELAP calculation, in the case where the pump speed was left on, and also in the case where the pump was tripped. We have also done two experiments during the course of testing in Semiscale Test Series 6 where we experimentally investigated this and found a similar phenomena.

The product of this was a change in the DNB characteristics in the core, so that in the case where the pump was tripped, you have a delayed DNB, by virtue of more flow into the core or out of the core.

Where the pump was left on, it appears that the intact loop hot leg was provided enough flow to satisfy the breaks so that we got a low enough flow in the core to provide an early CHF.

That is the effect of pump trip versus no pump trip.

There is also, I think, an analysis done in terms of small pumps versus large pumps, Semiscale versus LOFT, and what the effect of the degradation in the two pumps is. I think for a large break in general, both pumps degrade significantly by between 5 and 7 seconds; so those kinds of considerations are not important in a large break. They are, however, for a small break, as we will see in a moment.

(Slide.)

We have also looked at the influence of steam generator heat transfer during the course of MOD 1 testing. This slide shows three results, one from a RELAP calculation, two from experiments that were conducted back in the isothermal test services. Test S-01-5 was run with nitrogen volume maintained on the secondary side of our steam generator, essentially making the steam generator adiabatic. The othe was run with the steam generator in a hot standby condition.

What we are seeing here is virtually the same

What we are seeing here is virtually the same cold leg mass flow rate. So this difference, at least for the large break over this particular time period, did not influence the blowdown behavior.

For reflood, we can imagine there would be a difference simply because of steam binding and whatnot. As you will see a little bit later in the presentation, however, your intact loop generator does have characteristics in terms of secondary surface area-volume ratios that are approximately correct so that we think we are probably reasonablly well-scaled in that respect for reflood concerns.

I would like to leave the large-break domain and discuss in the remainder of the presentation similar points for small-break scaling. .

(Slide.)

On the slide you are about to see, I have not listed all the positive points I listed for the large breaks. I would like to make it clear that naturally we consider that those positive points that we have in the system for large break are also positive points for the small break. That includes such things as energy and mass distribution, resistance distribution, relative elevations, and the ability to do time scale properly.

There are some positive points, I think, that need to be brought out for the small-break considerations. That is that we do have the Loop seal elevations proper. While it is not completely clear yet how important that is, we do feel that it is necessary to have the loop seal elevations correct so that if it is a concern, and can be shown to be my analysis which is shown in some of the data, it will be a nonconcern.

We do have active broken-loop components which for a small transient, such as a small break, would indeed be important.

We do have the proper core length and geometry which, as Dr. Tong alluded to yesterday, is important from a core uncovery and boil-off standpoint. I listed one thing here that is not necessarily a positive point, I guess, but it is a plus for the system. Should we find that due to the small sizes of orifices that we have to use to scale break flow, should there be problems encountered there in terms of boundary layer effects or whatnot, we can easily change our break area volume and get the critical flow rate we want, say, in terms of LOFT counterpart testing.

I will address that more in a moment.

Some of the concerns, naturally, for a small break are the same that you have in a large break. That includes heat transfer, perhaps in a little different light for a small break, in that losses are important, extremely important. Such things as piping thermal time constants are also important.

For a number of years, Westinghouse people have brought up the consideration of flow regimes. Your flow regimes are perhaps different because of your fat pipes or your pipes that are of improper length. We have looked in this area. The pump behavior is a concern, particularly from the degradation standpoint.

Critical flow is a concern for the reasons I just mentioned. I will talk about what we plan to do and are doing at the present time to look at that.

The steam generators, of course, are a concern, especially from slow transients, natural convection, boiler mode environments.

Dimensionality, which has come up several times, is obviously important from the standpoint of perhaps natural convection, an uncovered part of the core, or perhaps penetration.

DR. PLESSET: There are some absolute numbers -- For instance, if you are concerned with pressurized behavior.

MR. LARSON: I intend to discuss that.

DR. PLESSET: Oh, okay.

(Slide.)

MR. LARSON: The first thing I would like to touch

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on is concerned with external heat losses. The system is insulated with insulation on the external parts of the pipes, the vessel. We have not been terribly concerned in the past about heat losses because of large breaks; the test is over in a rather short period of time. Losses were not thought to be in the area of consideration. When we started to do TMI-type simulations, it became quite apparent that losses were indeed extremely important and, in fact, were of a large percentage of our decay heat.

This slide simply shows the comparison both in terms of percent of total power and also absolute values for LOFT and PWR and Semiscale. For PWR values, it was given to me by a man that works for Combustion Engineering. It was taken from a System 80 plant during the start-up phase.

I can't at this point in time say that this would be the same value of heat loss for a Westinghouse or B&W plant. I expect it would be in the same neighborhood.

The value for LOFT was measured during the L3-0 experiment, I think. Our value was measured during some of our TMI simulations.

I think you can see the progress here. As the facility size decreases, the heat loss in terms of percent of core power increases by roughly layers of magnitude.

The question is what are we doing to try to rectify the heat loss problem? There are a number of determine where the relative losses are. I don't think it's worthwhile to spend much time on this slide except to indicate that our vessel in the downcomer constitutes a similar part of the loss. Some of the fixes for that are to insulate with greater care the outside of the facility, even if you find that you are still going to have significant heat losses. Some ideas such as using external heat tape have come up. We are currently evaluating that in terms of what it will do to our instrumentation in terms of magnetic field and whatnot. We don't think that will be a problem. We are hopeful we can get this loss down to zero, with the use of heat tape.

(Slide.)

There is another concern that was brought up. That is piping and time constants. What happens in a length of pipe when the fluid termperature, say, at the inlet changes by a step change; say, for example, what happens during a loss of feedwater; when the fluid temperature at your inlet could change from 540 to 600 degrees?

We used a rather simple model conduction calculation and entered the balance on a given volume of fluid which can be representative of any kind of pipe you want, whether it be reactor pipe or Semiscale pipe. It accounted for the proper lengths. Essentially we did the balance to see what the effect of the piping metal mass was on the fluid temperature transient.

The results are shown in the next slide.

(Slide.)

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They indicate that that appears at this point in time to be a non problem. Take a look at the time scale here. That's over 40 seconds. It shows that both for the Westinghouse, which is the curve labeled 1, and the B and W type, labeled 2, the outlet of this length of pipe sees the step changing temperature approximately with the transit time which for a 20-foot length of cold leg pipe is something on the order of 3/10ths of a second.

DR. CATTON: What is the limiting factor?

MR. LARSON: The metal mass of the pipe. In Semiscale, we have metal masses that are large. What happens is that it takes a certain amount of time to warm the initial thickness of the pipe up. It turns out to be a non problem here because the differences you see for any significant length of time are less than one degree.

I should point out that this was the case for a pipe that's essentially perfectly insulated on the outside, which we do not have at this time. So these results could be a little bit misleading for the system as it presently is; but with the use of improved insulation and heat tape on the outside, it would appear this is a non problem.

The next slide simply shows a blow-up of that same slide. I think it is just included to show the relative magnitude.

(Slide.)

As I said earlier, flow regimes have been a concern and have been for many years in terms of scaling. Some of the influences that flow regimes can have are that it affects the wetted area which will in turn affect heat transfer. They obviously affect pressure drops. The can affect critical flow depending upon where, for example, the break occurs on the pipe.

Some of the treatments that we have been looking at lately are flow regime calculations using battle type techniques. Eventually when data becomes available from LOFT type small breaks, we intend to experimentally investigate the differences of flow regimes since both the LOFT facility and the Semiscale facility have instrumentation in terms of multi beam densitometers to allow some sort of interpretstion as to what the flow regime was.

The next slide shows the results of application of the Duckler-Taitel method to a reactor pipe which in this case was a cold leg. It's a 27-1/2 inch pipe. And a Semiscale pipe.

The technique used here was to take a mass flux from a PWR calculation and scale that mass flux to that in Semiscale, much the same as the slide I showed of the LOFT versus the Semiscale broken loop.

What essentially was done was to take the mass flux, compute from that a mass flow rate for the PWR, divide that

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by the power ratio which is a scaling factor to produce the flow rate in the Semiscale. That flow rate in turn was used with the Duckler-Taitel method in calculating what the regime was as a function of time.

I think you can see once the system went two phased here, that for a short period of time, both pipes were calculated to be an intermittent which is essentially a sluggish flood flow. There is a slight difference in the flow regimes for the two pipes, from 300 to 400 time frame.

A close examination of what is happening here indicates that for the Semiscale pipe, if you translate the Duckler-Taitel method into JG versus JF, you find you are right on the boundary.

For the reamainder of the periods of time, the velocities for this particular break size were so low that both systems were calculated to be in a stratified type flow regime.

Now, I think this is important in that it does show that our pipes are such that we will get separated flow during a slow transient. It is not clear at this point in time that the fact that we do get separated flow in both systems will rpovide the same response. That requires, I think, some more analysis and is one of the things that we intend to look at in the future. I say that in terms of wetability, what the relative water height is in the two

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different pipes, how much of the pipe is covered. That in turn will affect heat transfer rates and whatnot.

I would also like to point out that similar calculations have been done for other points in the system. This, by the way, was the broken loop pump size break. We -- the comparisons actually look a little but better in the other parts of the system, in other words, the intact lopp hot and cold legs and the broken look hot and cold leg (Slide) pumps are indeed a concern primarily from two standpoints, one, degradation, and two, leakage. By virtue of the small size of our pumps, our leakage rates and also by virtue of the number of stringent transients we put them through tend to have seal leakage rates that are quite high. Generally for large breaks we do not make up that leakage. It should pose no problem for small breaks, because through ingenious techniques we can trap, weigh, and replenish that seal leakage. It's not easy but we can do it.

The primary concern is two phase degradation, small pumps versus large pumps. I think it's a well-accepted fact that a small pump will degrade faster than a large pump.

We do not have some treatment for that in that we are buying a new pump for our intact loop that should be an improved design. It will not, however, replicate the PWR pump. It can't. If you use specific speed as a scaling criteria, the new pumps will have a specific speed of on the

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order of 1500. A PWR pump, on the other hand, is 5200. There will be differences.

We do, however, have plans for a pump testing program. We will quantify the pump performance in all four quandrants, both single-phased and two-phased operations.

The next slide shows, I think, the obvious fact that out pumps do tend to degrade somewhat earlier than a large pump. This is a comparison of one of our TMI experiments with the data from TMI in terms of a normalized loop flow.

The point at which the Semiscale pumps start to degrade is at about a void fraction of 20 percent. We have done some calculations assuming void distributions in the TMI tests and concluded that the big pump will pump relatively well until the void fraction is about 40 percent.

I should, however, point out that the flow rate from the TMI plant comes from what they call a Gentilly tube. I am not all that confident that it's an accurate measurement for two-phase flow. It may be and is something to keep in mind here.

DR. PLESSET: When was that data obtained, this TMI data?

MR. LARSON: This data came from what is called the Reactimeter which I am not sure if you are familiar with the nomenclature. It's a small computer that during normal

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operation was operating on line and recorded something like 25 channels of data, including pump flow, second area pressures, things like that.

DR. PLESSET: So we had that available?

MR. LARSON: Oh, yes. I think this also was available on strip charts.

DR. PLESSET: I didn't know they had that available.

MR. LARSON: Yes. There's 25 channels of data that were recorded at like three-second intervals throughout the whole transient.

DR. PLESSET: Do you know if they ever made any request for this data from the Reactimeter sheets?

MR. LARSON: They?

DR. PLESSET: The operators.

MR. LARSON: I don't know. I doubt it.

MR. ALLEMAN: It was in the room below the main control room.

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DR. PLESSET: So they would have had it?

MR. LARSON: I think there was to be an engineering unit.

DR. PLESSET: Thanks.

MR. LARSON: Critical flow is another topic of concern from the scaling standpoint. As I alluded to earlier, there are two areas; number one, the small size of our orifices. The data shows there may be a change in the critical mass flow rate as diameter increases. We also are getting down in the regime where the diameter of the orifice is such that pound layer effects are going to be important. There are basically two treatments of that. One, we have test data, particularly from Three Mile Island and experiments we have done; we also have some data from S-02-6, although it's got large error bands on it.

We also intend, as an item for the meeting in Washington, D.C., in late July, to do what are essentially calibration tests before we run these experiments on the size of the orifices that we plan to use and also that LOFT plans to use.

These will be done in the LOFT test support facility. What is hoped here is that we can actually get plots of critical flow rate with stagnation and pressure.

The data we got from our Three Mile Island simulations, however, is encouraging, I think. This slide shows the comparison of the flow rate we got through our POV valve simulation, I should say, compared to what we calculated with the HEM model using the familiar correction factor of .48.

The .48 is not an arbitrary factor. It's a factor we found necessary to use way back in the days of blow down testing in the Semiscale MOD I facility where the .48 was found necessary to account for what was thought at that time to be vena contracta effects.

The .48 was also verified by some work at Los Alamos where two dimensional formulations of our break nozzle were compared with one dimensional formulations. The ratio of those two was found to be about .85.

There is an additional bit of evidence for the applicability of that correction factor or contraction coefficient, whatever you choose to call it, in that if a mass balance was done on the system during this particular TMI transient using delta pi's and whatnot, and that was compared to what we calculated with the HEM model, since -- through the whole transient the flow was saturated, either liquid or vapor, that you get a .86.

Now, I do wish to point out here that this was calculated using either saturated liquid density or saturated vapor density. That may be a consideration when we go to two-phase flows in extremely small orifices. There may be effects not shown here. That is indeed the reason we tend to test these orifices in the LOFT test support facility.

The break size for this particular experiment was, by the way, about .3 percent. In other words, .007 squared.

(Slide.)

Steam generators, as I mentioned, have two potential functions, one being an oversized second area, the second being elevation primarily in the intact loop for a small break. That is important in terms of natural convection rates. It also affects the operation of the steam generator and the reflux boiler mode.

We have two treatments. The near term as I mentioned earlier is perhaps -- one viable thing is to drain the steam generator. We are also attempting to quantify the chances of elevation primarily through the use of RELAP.

We are also looking at the operation of the intact loop generator in the reflux boiler mode, since the tubes in that generator are smaller than they are in a PWR. They are .408 inches relative to .775.

The calculations we have done to date using a Wallis type formulation indicate that when the system is operating in the reflux boiler mode, that should not be a problem. The velocities are low enough to allow the liquid to fall back into the loop so there will be a counter current flow situation there in the reflux part of the mode.

The long-term fix is to provide a new steam generator for the intact loop. We are also looking at the design of that carefully so that we can hopefully get a second area volume that is appropriate; and we are also looking at reducing the second area volume in the main generator, if we can, through the use of filler pieces.

I think it is important at this point in time just to indicate on this slide what the relative second area side heat transfer areas are in our facility.

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(Slide.)

That is relative to that in a PWR. Granted, as you will see on the next slide, our second area volume in the type 2 generator is large; but if there is not a heat capacity effect, what we see here is that this is a function of normalized level. We do have a scaled heat transfer surface area. In fact, the intact loop generator is not that far off. I should point out that this type 1 generator actually simulates three PWR generators, so that the surface area was divided by three to make this comparable.

DR. CATTON: What about the flow rate on the primary side?

MR. LARSON: In the horizontal loop or in the tube?

DR. CATTON: Vertical; in the steam generator.

MR. LARSON: As I just said, in terms of the reflux boiler mode operation, we will get counter current flow based on some assumptions in terms of what the steam generation is in the core.

The velocities in general are lower than in the PWR.

DR. CATTON: Your heat transfer coefficients on the primary side might be different.

MR. LARSON: That is possible.

DR. CATTON: So the surface area doesn't have quite as much meaning?

MR. LARSON: That's true.

DR. CATTON: You need to look at the whole problem, not just pick one side.

MR. LARSON: You need to have the right velocities. That's correct.

(Slide.)

This slide simply gives a comparison of some normalized values and some ratios that are important in terms of scaling water volume and whatnot. It shows quite obviously the oversized second area -- or the type 2 generator. It does indicate that the type 1 generator, from a heat capacity standpoint, is quite close. In other words, it's the right amount of volume -- or at least close to it -- to simulate 3PWR generators.

In terms of surface area over primary volume, we are quite close. It also shows the relative elevations of the type 2 and type 1, the type 1 being short.

Dr. Plesset mentioned pressurizer surge line a moment ago.

(Slide.)

Particularly in the TMI transients we have done, there has been a lot of concern on both what the size of the surge line was and also the hydraulic resistance. The simple treatment for that was when we did the TMI simulations, we had two requirements: One was to incorporate the dog leg in relative elevations; two, we wanted to get the right scaled resistance which I think we accomplished.

(Slide.)

This slide shows some pertinent facts regarding the surge line relative to the B and W designed surge line. Our surge line is about .37 inches in diameter, or was. The PWR, on the other hand, is about 8-1/2 inches. As far as the resistance, we calculated the resistance for the PWR surge line and scaled it up by the volume we show, squared, and got a value of about 6800 in English units.

We did the same thing for our surge line to determine what sizes we needed, and we got about 6200. This value was measured and found to be somewhat larger. We were closer to this. I think the value is about 6800 or so.

Much the same as we test our loop components before we operate the facility, we endeavored to get as close to the scaled resistance value as we could on this particular simulation.

In terms of looking at the size of the surge line and in terms of surfcae tension effects and what that does in terms of drain rates, we have looked at a dimensionless diameter concept in the flooding theory.

That, for the PWR, was about 133. For our surge line, it was about 5.8. As near as we could find in the lierature, we accepted a critical value of something like

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.814 for those sizes where surface tension effects and bridging are important enough to allow a line not to drain.

So at least based on the available theory, it would appear that that line size is not sufficiently small to cause those kinds of problems. We have also looked at -- from a Wallis correlation standpoint -- what the critical gas velocities and what the real superficial velocities in the surge lines were for a given state in the TMI transient.

What we did was look at the case where the plant was operating at roughly 1000 psi. We said, okay, the HEM model is a reasonable technique by which to calculate the flow rate; translated that, assuming that there was a continuity -- continuity of mass; what superficial gas velocity was required in the surge line to create cold current flow.

I think as you will see, the critical gas velocities in the PWR was about 13 and about 2 in our surge line. However, the real velocity in the PWR would be about 42, and ours would be about 20. It would appear that in both cases, we have cold current flow. As I said, this is based on a Wallis correlation. 8-1/2 inches may be a diameter large enough where you should use the Kutelatze criteria.

DR. PLESSET: What situation are you going to apply this to? In the hot leg, say you have steam, and you have a filled pressurizer or steam is going into the pressurizer?

MR. LARSON: Exactly. That was the assumption.

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DR. PLESSET: Yes.

MR. LARSON: Those were the ground rules.

DR. PLESSET: I doubt if these criteria are adequate or appropriate. The absolute length of the line will make a difference in the rates. Am I right in that?

These criteria are not appropriate ones. So I would recommend that you look at that again.

MR. LARSON: We have had ideas of looking at this experimentally.

DR. PLESSET: Oh, that's all right. Do that.

MR. LARSON: We haven't proposed this yet, but I think it would be a reasonable thing to do.

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DR. CATTON: You don't have that in Semiscale?

DR. PLESSET: You could do that on a pipe somewhere.

Also, I think that since gravity enters, the absolute heights are important; don't you think so?

DR. WU: For some nondimensional number.

DR. PLESSET: Maybe some nondimensional number.

MR. LARSON: We did maintain the proper heights in terms of the dog leg.

DR. PLESSET: That part. I think the whole geometry of the whole line, its length, the height of the pressurizer relative to the hot leg. So it is not only the dog leg that's important.

DR. ROSZTOCZY: Dr. Plesset, this has been looked at recently in terms of the various PWR vendors. They have been looking at the problem.

As I understand it -- and I think it is the same as has been done here -- the approach is that should you have, number one, the break in the pressurizer, an opening of the valve on a pressurizer, then the flow in the surge line, the steam flow in the surge line, going into the pressurizer -- the question is under what circumstances would this steam flow prevent water from leading back into the system.

DR. PLESSET: Sure.

DR. ROSZTOCZY: The cross-section of the piping is usually uniform, so there is no difference there. The steam velocities are very high. We know the flooding occurred in the end of the pipe. If there is any water, it is pushed into the pressurizer. There will be no drainage back.

What is being done here is a simple flooding check, let's say, at the end of the pipe or at the smallest cross-section, wherever it exists. Usually there is a cone at the entrance which has holes in it. It has a smaller cross-section than the pipe itself.

You do a simple flooding check. You are looking at the gas velocities in that cross-section and see if, with those gas velocities, is it possible to penetrate any water against it.

The answer is what is shown there. The vendors are coming up with the same answer. The gas velocities are significantly higher. How much would be needed to hold up the water; therefore, no water penetration is critical. So for those breaks, independent of the U-2, most of the plants do not have a U-2 type of surge line. They have a straight surge line. Even with the surge line, they do not predict that.

DR. PLESSET: Well, I would remark — you talk about a straight pipe. I am told that there's a horizontal run of 30, 40, 50, 60 feet in some of these installations.

Just a couple of days ago, a man was very proud when he told me this horizontal rod was 60 feet. He seemed pleased with that. I think this is a very important aspect of this gas liquid flow, the fact that they are enormously long. I think if we don't really look at that, we may be neglecting something quite important.

DR. ROSZTOCZY: Then the calculations are done to predict the flow, the flow that you are -- have been going to use to check on, then those calculations are done with the proper flow resistance of the system, so if there is six foot of pipe, the calculations -- the original calculations are done taking full account of the 60-foot length of the pipe, appropriate flow resistance corresponding to this. Obviously if those are something different, the pipe would be shorter.

Once you have that flow, then you can take the calculated flow which accounted for this and compare it against the flooding condition you see, the penetration, and arrive at the appropriate conclusion.

DR. PLESSET: Are these conclusions meaningful?

DR. ROSZTOCZY: Yes.

DR. PLESSET: Are you sure?

DR. ROSZTOCZY: Yes.

MR. ALLEMAN: If it floods for a short length, it would flood for the long one.

DR. PLESSET: I think rates are effective.

DR. WU: The rates may be affected by change of the flow configuration such as there is a stratified concurrent flow. When you increase the pipe length because of the increased friction, it might generate the flood flow. When that occurs, the breathing in the upper layer of this counter-current, might be peaked.

Also the strain above the outlet of the vertical tube inside of the pressurizer also is scale dependent. We have tested some of the spring size. It can be very different. The law of basic physics dictates on a change of the major flow configuration and type of flow.

Those are some of the things you have to consider. It might be a beneficial thing to consider.

DR. ROSZTOCZY: The picture I am getting from this type of calculation is that we are not at the flow velocity at all of the things you are describing could happen. We have an order of magnitude higher in the velocities. There is no chance for any of it to come back.

So if you take the picture and increase the gas velocities by a factor of 10, then it will be somewhat different.

What we were getting here — I think it shows 42 against 11. So a factor of 4. Calculations that we get from the vendors were showing also factors of two, three, four times higher than that.

So I think maybe a factor of 10, but a factor of two, three higher velocities where the tranmission would take place.

If the velocities get smaller, if those two numbers would be comparable numbers, it is rather complex. It is very difficult to predict exactly what will happen.

DR. CATTON: At the outset, the gas velocity is zero. Then there is a transition to the point where this 42 feet per second. I think one of the questions is how long does it take to get there? If that's a short time for this flow to establish, then I think you are right.

DR. PLESSET: It depends on the rate of temperature buildup, the stream.

DR. WU: That's right.

DR. CATTON: If it takes a long time to establish the flow, then there is a consideration.

DR. ROSZTOCZY: Yes. Obviously there is a transition period that you go through when you get into the high velocity zone. If you wait long enough, there will be a transition again and again.

DR. CATTON: The length of time it takes for the transition to occur, and what kind of things are happening during that period, and is it important --

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DR. PLESSET: I think we pointed it out. I think we are at the point where we need further examination.

DR. CATTON: RELAP won't do it.

DR. ROSZTOCZY: These are all calculations done on the side.

DR. PLESSET: Well, go on.

MR. LARSON: Okay. We are almost through.

The last topic I would like to briefly address is that of dimensionality. As I said earlier, our system is largely 1D by virtue of the large L/Ds. We have seem multidimensional phenomena in our core. In the case where we injected it into our upper plenum, there was a significant amount of dimensionality in terms of the core cooling.

However, I think we will largely treat this analytically. It is a difficult phenomena just to do hand calculations with regard to, I think. It is something that, from a scaling standpoint, we will have to evaluate in the future.

(Slide.)

In summary --

DR. CATTON: I didn't quite understand that. Would you put that slide back?

Would you repeat what you just said?

MR. LARSON: In what regard? In terms of the analysis?

DR. CATTON: The 1D versus 2D.

MR. LARSON: What I said was we ran an experiment in the MOD I facility which also was construed to be largely one dimensional by virtue of the large L/Ds. We have run an experiment where we have injected ECC water into the upper plenum. We have noticed two-dimensional cooling characteristics, even though the core is left out. So there are two-dimensional effects in the system.

Whether or not they are typical of a PWR — they are probably not, but there is a potential for two-dimensional effects.

I think in the gross semse, the effects of dimensionality will have to be evaluated via a multidimensional code.

DR. CATTON: If the interest is in a phonemona that may be multidimensional, why do you run a test?

MR. LARSON: Are you referring to UHI?

DR. CATTON: Just anything in particular.

MR. LARSON: Well, let's take UHI as an example. When the scaling for UHI was agreed upon, it was not, in my estimate, the multidimensionality of the effect that was thought to be the important thing. What people were interested in was the regimes of the UHI and, in essence, the gravity dominated situation such as draining that was of interest.

So from that standpoint, it was of interest to have proper core limit.

DR. CATTON: Draining of the upper head?

MR. LARSON: Yes.

DR. CATTON: Isn't that also a multidimensional problem?

MR. LARSON: It could be, but it is a gravity dominated situation.

DR. CATTON: It seems to me if you have multidimensionality, the lateral stands are also important. If you restrict --

MR. LARSON: Full scale.

DR. CATTON: I don't think so.

DR. PLESSET: Dr. Tong?

DR. TONG: I would like to make a comment. In the small break, the flow rates are usually show. I see a three-dimensional effect being much less than in a large break. However, the UHI is an exception. I wish that small tests not talk about UHI for the time being, because most of the plants we are talking about have UHI.

Getting to UHI, we have to systematically study how this one-dimensional -- three-dimensional effect including UHI.

UHI simplified -- it is a different question comparing it with ordinary plant small break studues.

MR. LARSON: I would agree with that.

DR. PLESSET: Thank you.

MR. LARSON: In summary, I think at least the data we have gotten to date leads us to believe that volume scaling is appropriate. We knew that distortions were present in all systems that were scaled. Ours is not different.

Most of the things we have looked at to date are not items that are, from a code standpoint, unmanageable. They are accounted for, so from that standpoint, the data should be useful from code benchmarking standpoints. We also feel the data will be useful for phomena identification. We also need the data to help quantify a log of distortions we discussed.

In general, our small break distortions aren't fully quantified. What I endeavor to do is give you an idea of some of the things we are presently looking at, hopefully to impress upon you that we are working on what we consider to be an extremely important consideration.

(Slide.)

In the small break area, we have a joint effort established with the LOFT program and also we are getting help from the Australian Government.

DR. PLESSET: What are they doing?

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MR. LARSON: They have some free manpower. It is not exactly clear what they are going to do yet, but they have the ability to run codes, engineering analysis.

DR. PLESSET: They also have a reactor that's not being used. Maybe we could use that.

(Laughter.)

VOICE: We have no reactors, but we have the manpower.

DR. PLESSET: I thought you had one that was essentially complete. Is that not correct?

VOICE: We have one completed, but not yet taken into service.

DR. PLESSET: So it is available?

VOICE: It is available.

(Laughter.)

DR. CATTON: Available for what?

MR. LARSON: Some additional scaling work is undoubtedly required for loss of feed water type tests and anticipated transients. We intend to document this work much the say we documented the three scaling efforts we did for the large break testing.

I think there needs to be an assessment of the effects of dimensionality.

That concludes my presentation. If you have any questions, I think all of my scaling friends are here, and I would like to direct the questions to them.

(Laughter.)

DR. PLESSET: I think we expressed most of our concerns and interests. We appreciate your presentation.

We have two more presentations to do. I guaranteed we will finish in time so that people can make that flight that they need. One presentation is by Mr. Johnsen on some TMI work on Semiscale. The last will be on a test plan and upgrade to perform transient tests in Semiscale.

I propose we take out lunch break now and return as close to 1:00 o'clock as we possibly can.

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(Whereupon, at 12:15 p.m., the meeting was recessed, to reconvene at 1:00 p.m. this same day.)

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AFTERNOON SESSION

(1:05 p.m.)

DR. PLESSET: I will call the meeting back into session.

We made a slight shift in the order. The first presentation will be on plans for FY '80—'81 in Semiscale by Mr. Olson.

MR. OLSON: Your handouts have got a more extensive presentation which kind of takes you through the point where we were last year to today. In the interests of brevity, I will just concentrate on our current scope as we see it for FY '80—'81. (Slide.)

DR. PLESSET: I apologize on your name. The agenda had it spelled wrong.

MR. OLSON: I noted that. It was pretty close.

Basically we have discussed in the last couple of days the emphasis on small break testing. That emphasis carries through into Semiscale. We are planning a small break testing effort covering a period from the end of this month into March of 1980.

In Semiscale a small scale testing effort is basically a two-stage process. The first stage has been closely coordinated with LOFT in terms of both the supportive and comparitive role. Tarrell Samuels presented the plan for LOFT. Mr. Harvego will give the Semiscale plans.

Basically we have taken the same concept of a certain number of tests and scaled those tests or designed those tests using the same criteria.

In this manner, we are basically performing tests in which we will be able to directly relate Semiscale and LOFT data and again as we did in the large break situation, give some initial indication on the effects of physical scale between the facilities in the small break phenomena area.

The second stage of small break testing is not yet completely defined. But it will address issues that have been raised yesterday by Dr. Tong relative to some separate effects type investigations and will address some of the outstanding issues that Zoltan talked about this morning.

In designing these tests, it is our intent to specifically address scaling issues in each of those tests and hardware requirements in each of those tests prior to the time that we do conduct those tests so we can assure that they are completely meaningful tests in Semiscale.

In between those two stages of small break testing we will be repeating S-0-76 with the new downcomer insulator installed. That has been designed, fabricated. It has been a state of the art effort in the design and fabrication activitites.

That hardware is not complete, however. We will be ready to perform these tests as soon as we go through the first stage of small break testing.

We are also, as a result of Three Mile Island emphasis, planning a scoping loss of feed water test in Semiscale. Mr. Harvego will discuss the test rationale, objectives, and expected results — basically the philosophy with which we are approaching these tests.

These tests will be performed in a period from April to October basic conforming to the general guidelines trying to obtain more general results by September of 1980. We are at a minimum in conducting these tests intending to include those hardware modifications which will eliminate some of the nontypicality that was discussed this morning, specifically the external insulation problem, the internal insulation, both in the downcomer and in the vessel itself, as well as installation of the type-two steam generator, the PWR steam generator, to eliminate those nontypicalities that we are aware of.

Past that point then, we have scheduled the UHI sensitivity studies which have been delayed significantly because of the lack of capability for the UHI calculation. Zoltan talked about that this morning.

To this point, our schedule is basically that which was presented in December to the NRC with modification because of the emphasis of the small breaks and feed water testing. We are dealing with a projected scope which involves installing — designing and installing new hardware systems in Semiscale which you have been presented information on in past meetings relative to supplemental budget required for Semiscale.

These are the closed-loop secondary system and the two-by four loop simulation.

What I have on this schedule is a minimum — or a schedule based on time phases of testing. This schedule can be accelerated or modified depending upon the priorities. This is our current plan.

We are currently involved in the preliminary central design of both the closed-loop secondary systems and the two-by-four loop simulation. Concurrently, I would like to assure you that while we are doing those preliminary designs, which is necessary in terms of designing standpoint, which is a fairly long-term activity, we are addressing scaling issues in terms of how well we can scale closed-loop secondary -- what is actually needed in the closed-loop secondary in terms of performing meaningful tests for Semiscale.

Those analyses and evaluation we intend to complete before we get to the point where we actually have the hardware. What we are intending to do is go through the cycle where we actually address the specific scaling issues, address the specific tests that can be meaningfully performed in Semiscale and factor that into the design before we go off

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into the hardware.

Basically that is a condensation of what I was going to present. I think Ed will be presenting now the details of the planning for the small break and feed water tests in Semiscale.

DR. PLESSET: Thank you. I see no questions. Thank you very much. Harvego.

MR. HARVEGO: Good afternoon. I am Ed Harvego. As Mr. Olson said I will be talking about the future Semiscale test and schedules.

Specifically, I will be talking about our plans for the upcoming small break test series in the final stages of development. I think it will become apparent as I get into my discussion that they are very closely tied to the LOFT small break tests which were discussed yesterday.

I also will be discussing the rationale for future loss of feed water tests and the plans for implementing these tests; and finally, I will be talking about the methodology which we have developed for defining as yet undefined operational transients which could be run in Semiscale.

(Slide.)

This is the schedule for the next couple of years. I think I will just briefly touch on this as Danny has already mentioned it. The objective or our intention in this schedule is to provide LOFT with immediate information in

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approximately two to three months on the tests that they are going to be running and then to provide additional information on small break tests within a period of about six months.

You will notice on this schedule that we have allowed additional time for some optional tests which I will be talking about in -- a little bit later.

The other point is that if we stay with this schedule, we will be beginning our loss of feed water tests in April and continuing those until the fourth quarter of 1980. The operational transients test series would then begin in the second quarter of 1981. (Slide.)

To begin the discussion of the small break tests, I would first of all like to point out some of the reasons why we feel thses are important tests to be running in Semiscale. First of all, recent events -- specifically TMI -- have pointed out the importance of probabilities and the need to understand the causes and consequences of small break accidents.

Dr. Rosztoczy mentioned that in his talk also. We feel there's limited data available on small breaks. Prior to TMI, Semiscale ran two small break tests. One was in the MOD-1 system; the other was in the MOD-3 system.

During and subsequent to TMI we did a total of eight tests in support of the Three Mile Island incident, and we feel that those test provided important insight into a lot of phenomena that actually occurred in the PWR.

We feel that this type of information, or this type of testing, is important to address a wide range of concerns relating to small breaks. We also feel there is a necessity for experimental data for the codes and benchmarks. I am specifically talking about experimental data on the potentials for core uncovery, the influences of core uncovery on the thermal hydraulics and temperature response of PWR cores. (Slide.)

Therefore, the objectives that we have identified for the small break tests are first of all, to identify the important thermohydraulic behavior associated with a small break accident and the potential effects on the outcome of that transient.

I have also mentioned we are closely tied to the LOFT experimental program; we are also closely tied to the PWR audit calculations.

Therefore in these tests, we expect to compare results from Semiscale and LOFT and hopefully identify some of the scaling influences on the important thermohydraulic behavior we observed there.

Yes, Dr. Catton?

DR. CATTON: I believe the B&W people have indicated that they felt in many respects that the small break was a quasistatic process, basically a series of steady-states.

Also, the Michaelson analysis was performed as if he was looking at a series of steady states. It is very interesting if one of the things that would come out of what you are doing is either to prove or disprove that. Maybe we don't need a new code.

I see you have code evaluation; but are you going to look into that -- that aspect of it?

MR. HARVEGO: What we have in mind is we have -- the PWR audit calculations, we will have pretest predictions for Semiscale. We will have prestest prections for the LOFT experiments. What we hope to do is tie these together, identify the important semihydraulic behavior these codes are predicting for each of these -- for the LOFT and Semiscale experiments, and for the PWR; and hopefully use that information along with the experimental data that we obtain to address some of the question about -- in terms of how well can the codes predict certain phenomena.

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DR. CATTON: That wasn't the question I asked, but I understand the answer.

MR. HARVEGO: Did I answer the question then?

DR. CATTON: Well, you did. You essentially answered it no.

MR. HARVEGO: I think what I am saying is that if in fact we find out that it is a steady-state problem, and you don't need big codes, hopefully then we will recognize that in our experiments.

DR. CATTON: Well, see, I would think that maybe you would do some thinking and calculations sort of back of the envelope beforehand.

MR. HARVEGO: I think we have. I will get into that as I continue along.

(Slide.)

We have five small break tests presently planned for the small break series. The first one is a two and a half percent break corresponding to a four-inch break in a PWR. For this test, we are going to be using the initial conditions from the PWR audit calculations which were performed by our code assessments group. We will be configured in our normal MOD 3 configuration.

The things we are going to be looking at in this particular experiment are such things as the break-flow characteristics; Tom Larson already addressed the scaling concerns in his presentation regarding breakflow. We are also going to be looking at system depressurization characteristics; and finally, the potential for this system depressurization and how — what potential this system depressurization characteristic would have on the potential for core uncovery in both the PWR audit calculations and in calculation that we have been performing in trying to address Dr. Catton's question right now.

We are looking — we saw — our calculations indicate that we will get core uncovery in the semiscale facility. We also saw those in the PWR audit calculations; so that's the type of thing that we are going to be trying to address in this first test.

The second test we have defined is intended to identify differences in initial conditions and LOFT geometry, differences between the LOFT configuration and the PWR audit calculations.

Therefore, this test would again be a two and a half percent break test. The initial conditions would be that from LOFT test L3-1 which was talked about yesterday. We would be -- have our loops configured to represent as closely as possible the LOFT loop configurations; specifically, we are modifying the pump section, the intact pump loop section leg to simulate the elevation effects between the cold leg center line and the cold section invert sbnDD

so that it would be representative of the LOFT configuration.

We are also planning on developing out the broken loop hot leg to give us a break configuration more representative of the non-communicative nature of the LOFT configuration.

The third test we have planned is an optional test in that if we don't find significant difference between the test designed to simulate the audit calculations and the second test which was designed to simulate the LOFT configuration and initial conditions, then we probably wouldn't run this test. However, if we do see significant differences, then we want to try and identify whether those differences are caused by the initial conditions, differences in initial conditions between LOFT and semiscale, or LOFT and PWR, whether those are due to differences in the LOFT configuration.

Therefore, this third test would be a two and a half percent break; but in this case, the initial conditions would be those from the PWR audit calculations, but we would maintain the semiscale system in the modified LOFT configuration.

The fourth test we have planned is the smallest break that we would be running. This is a .16 percent break, corresponding to a one-inch break in a PWR. For this test, the initial condition will be from the PWR audit calculations; and we will again reconfigure it in a MOD 3 configuration.

Again in this test, we will be looking at such things as the breakflow characteristics, scaling effects on breakflow characteristics, system depressurization, and also we will be looking at natural circulation effects. Both our initial calculations and the PWR audit calculations indicate that we will go into a natural circulation mode. So we will be looking at the effects of natural circulation, or the phenomena affecting natural circulation and will also be looking at the potential for recovery from the natural circulation mode once it is stabilized in that condition.

The fifth test is again another optional test. If we did run this, this would be a 10 percent break. The initial conditions would be for an S-07-10 which was the small break test run in a MOD 3 configuration. We would be configuring the MOD 3 — we would have the MOD 3 configuration with upper head injection.

If we ran this test, we would be looking at the potential effects of ECC injection into the upper head on breakflow characteristics and overall small break phenomena.

MR. MATHIS: Pardon me just a minute. Zoltan, you mentioned this morning you had serious questions about the high priorities given to this program. Is this consistent with your desires, needs?

MR. ROSZTOCZY: Yes. The program basically has been developed by EG&G and RES. It has been presented to us back a few weeks ago. We reviewed this in some detail. We had a few comments on it. In general, we found that the program that we developed was very responsive to the request that we gave them.

Some of the details, in the individual tests, we had some comments. I can't recall all the details. Some of them indicated that there might be some tests that might not be needed. One additional one might be needed here or there. I believe those comments have already been incorporated into the program that is being presented here; is that correct?

MR. HARVEGO: Yes.

MR. MATHIS: Fine. Thank you.

MR. HARVEGO: I mentioned that we had the option for running additional tests.

(Slide.)

I added this slide. I don't believe this is in your handouts, but I did want to address this. We are considering running additional tests primarily because of comments from licensing and NRC research.

Specific areas we think we might want to look at on the phenomena are, for exameple, we were thinking about running a duplication of one of the small break tests in which we left the pumps running to investigate the phenomena of performing in effect a separate effects test to investigate effects of pumping -- or the operation on small break phenomena.

We are also looking at the potential of running a number of natural circulation tests, specifically natural circulation tests involving non-condensible gasses in the primary system, reflux natural circulation as a fallback.

Not listed on here, but I think also under consideration, is running a hot leg small break test.

(Slide.)

The results we expect to get from these tests are first of all, we expect to see core uncovery in the larger break tests. As I mentioned, our calculations, the NRR calculations. the PWR audit calculations indicate we will get core uncovery. We will also be looking at natural circulation phenomena.

Again calculations have indicated that the system will be pressurized in the smallest break to something around the second area site pressure and then go to a natural circulation mode.

We will be looking at second area side influences particularly on the natural circulation mode. We will be looking at such things as elevation effects, steam generator elevation effects, and the potential effect of second area side heat transfer on the natural circulation phenomena.

Finally, we will be looking very hard at scaling effects, particularly scaling effects between LOFT and semiscale since we are very closely tied — the two programs are very closely tied together on this series. We expect to get a lot of information on scaling effects in both systems on the overall thermohydraulic response.

(Slide.)

I would now like to turn to our plans for the future loss of feed water test series. Again, to begin with, I would like to give you some indication of why we feel the loss of feed water test series should be the first operational transience test series run in semiscale. The first is relative probabilities.

At the time WASH-1400 was written, operational — or operating data indicated that we could expect an interruption in normal feed water approximately three times per reactor year of operation. I have also got information that was given to me by our reliability group which indicates that the reliability of power-operated relief valves, that class of valve, is something like — the reliability is something on the order of eight failures per one thousand actuations.

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If you assume every interruption in feedwater resulted in an increase in the pressure and the actuation of a relief valve that would indicate that the probability of a small break being initiated by a loss of feed water incident or a transient is something on the order of .018 or one in every 50 reactor years of operation.

This, to me, is a pretty significant consideration. The second thing is that because the probabilities are relatively high for this type of transient, there has been quite a bit of work in the area of failure and event tree analysis. The event tree is fairly well-defined. I will get into a minute the fact that we feel this event tree analysis provides us with a good method of defining future tests in semiscale.

Finally, as with all operational transients, including loss of feed water, there is a significant lack of experimental data in this area to help us in verifying the engineering analysis and in identifying potential safety-related phenomena that the codes might not be able to predict.

(Slide.)

So the objectives for the loss of feed water test would be first of all to investigate the important thermohydraulic events associated with the single initiating event, assuming that all other systems function as designed.

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The second thing — this is where we feel that Semiscale can make a major contribution — is the investigation of potential common mode — or multiple common mode failures. I will discuss that more a little bit later. As I mentioned, we would also be utilizing this experimental data in our code development activities.

(Slide.)

The plan that we propose is that for the loss of feed water test, every test in that series would be initiated with a loss of feed water event. We would then use an event tree approach in that we would begin looking at multiple failures, common mode failures, and increase the severity of the transient until we reached a transient which we call a limiting case, where there is a potential for core damage in a nuclear plant.

(Slide.)

To be more specific, I will show you what we have in mind. I have extracted this event tree for a loss of feed water accident from WASH 1400. Basically what it shows is that the event is initiated with a loss of feed water. Each of these branches defines either the success, cooperation of a particular system, or failure of a particular system to function. You sequentially progress through the various system operations which have to occur.

At the end of each of these paths, we have identified whether or not that particular path would lead to core damage.

As an example, just to clarify this, in this case, this top branch represents the successful operation or a reactor scram. The bottom branch represents the failure of the reactor protection system to scram.

Therefore, this entire category, or this entire sector of that event tree, defines a general category of transients, defined as anticipated transients without scram.

What we have in mind for our tests is to begin with a baseline test which would be the initiation of the event, but the successful operation of each of the reactor systems.

This is the same type of test that could be performed in LOFT.

We hope from this baseline test to utilize the information obtained from our results and compare those with similar tests that would be performed in LOFT.

Where we expect to make a significant contribution to this general category of transients is that in — since we are not a nuclear facility, we are not limited by the safety requirements of a nuclear test factility.

Therefore, we feel like that we can look at more severe — we are more flexible in that we can look at more severe transients. So taking this information, after we compared it with results from WALP, we look at multiple failure modes. We can look at things such as the loss of off-site power leading to the interpretative reactor pumps.

We can look at delayed auxilliary feed water flow. We continue to increase the severity of these transients until we reach a limiting case such as the one shown here where we had a loss of normal feed water -- loss of auxilliary feed water in spite of the other systems functioning as designed. The path still led to core damage.

It's interesting to note that in our Three Mile Island testing -- which I think Greg Johnsen will have a few minutes to talk to you about -- we actually followed one of the limiting paths in this event tree.

In Three Mile Island, the event was initiated by a loss of feed water; there was a successful scram in the core. However, the auxilliary feed water flow was at least delayed sufficiently to place us on this path.

The power-operated relief valve opened. However, it failed to reclose. Although there was ECC injection, the event tree indicates that that particular event led to core damage.

So the results that we expect to get from our loss of feed water test series are first of all, we are going to try to identify the important thermalhydraulic behavior associated with the loss of feed water transient, including the possibility for multiple common mode failures.

We will be using -- secondly -- the Semiscale instrumentation, the extensive Semiscale instrumentation to help us identify the important thermalhydraulic phenomena that are going on and hopefully be able to relate these in some way to a phenomena occurring in a PWR; and in that way, identify the capabilities and limitations of the PWR instrumentation in terms of defining particular condition or particular condition or stte of the reactor at various parts in the transient.

Finally, we will also be looking at our codes in terms of their ability to predict the outcome of these various experimental paths that we would be investigating.

(Slide.)

I would like to now turn to our proposed test planning for future operational tests. I think I will try to shorten this up a little bit, since basically the methodology that we are proposing is very similar to what we have just gone through in our loss of feed water transients.

Each experimental or each transient test series would be defined by a single initiating event. We would use the event tree approach, in that the testing would proceed from a relative -- a baseline test where we looked at a single initiating event, with all systems functioning as designed. We would increase the severity of each of the tests by looking at multiple failure modes until we eventually reached a limiting case which would indicate that we would have — that path would lead to core damage in reactor.

Finally, the last point I would like to make is that from these tests, we had hoped to define additional testing that should be done in terms of looking at such things as the potential for system recovery once the reactor has stabilized in an off-normal condition, or possibly the potential operator interaction, intervention which might tend to mitigate the consequences once you were on a limited case transient.

(Slide.)

The objectives then that we see for these tests would be to look at the single initiating event, identify the different paths that the reactor — or in this case, Semiscale could take as a result of multiple failure modes, common mode failures.

We again would be looking at the capabilities and limitations of the large reactor instrumentation in terms of defining the particular state or condition of the reactor at any point in the transient; and finally, we would be using this information as a data base to evaluate codes in terms of their capabilities to follow the transient and predict the outcome of the particular experiment that we were looking at.

(Slide.)

There are a number of — a wide variety actually of operational transients which you could define to look at; but generally, we feel you can categorize them into these seven general gategories. I won't dwell on these. I think you can look at them for yourself.

I would like to point out that we have identified in parentheses after each one of these transients, short-term, medium- to long-term, and long-term. Specifically what this is indicating is the time frame in which — within which we feel we could begin looking at this partiuclar category of transient.

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The limitations are primarily on hardware. Tests that we could run in the short term are those involving minor system modifications. The medium- to long-term transients would be those which involve some system modifications such as the inclusion of a point-kinetics model in our power control to simulate reactive feedback. These modifications are of a less extensive nature and therefore these — those types of tests could be run on the medium- to long-term.

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The only one at we have identified as a long-term item, the anticipate transient without scram. These particular transients for us to look at would require inclusion of a reactive feedback model into our power control area. We would have to consider secondary site feedback effects. This would require a closed loop secondary; and because of te relatively severe transients, temperature transients that we would see, we would also have to be looking at such things as the capabilities of our rods to follow those transients and in fact survive through those transients. It may require modifications in our heat rod designs.

(Slide.)

In conclusion, the things we expect to get from those operational transients are, first of all, we expect to identify important thermal hydraulic behavior over a wide range of transients, specifically covering those seven generated categories that I presented on the previous slide.

We also would be looking at potential -- the potential for operator interactions or operator actions which might tend to mitigate the severity of the limiting case transients. We would be looking at the ability of the codes to predict these limited case transients and also to define the operating margins that might be associated with them.

Finally, because we consider these tests to be a scoping or paramethods nature, we would use these to help us define future tests or future separate effects testing that could be performed in Semiscale as well as by other individuals or facilities.

I think that concludes my presentation. If there are any comments --

DR. PLESSET: Could we see the event tree slide for a moment?

MR. HARVEGO: Yes.

(Slide.)

DR. PLESSET: I want to look at the first three.

MR. HARVEGO: These three?

DR. PLESSET: Not the first one, the second one. The TU one. TL you have a question. TU you have a no. Is that the way you want it?

MR. HARVEGO: I looked at that. That's the way it is defined in WASH 1400.

DR. PLESSET: What do you think?

MR. HARVEGO: This, to me, is saying that if we have an auxiliary -- if we lose our normal feed water flow, that we have auxiliary feed water; that you don't have to open the pressurizer relief valve; that may be true for this particular plant. I don't know whether it is true for all plants. As a matter of fact, I doubt that it is true for all plants.

DR. PLESSET: I am not questioning the top one.

MR. HARVEGO: Okay.

DR. PLESSET: The next one.

MR. HARVEGO: This is saying that — I think this depends on the break size. You are not going to overpressurize the system. The auxiliary feed water flow may or may not keep the system — allow the system to depressurize.

This, to me, is saying that if you don't -- if you do allow the system to depressurize, the HPIS will come on and you get your core cooling.

DR. PLESSET: You are now assuming that it doesn't come on?

MR. HARVEGO: If it doesn't come on -- and I am not assuming this. This is what WASH 1400 says.

DR. PLESSET: You say there is no core damage?

MR. HARVEGO: There is no core damage.

DR. PLESSET: It is better if it does come on -- what are you saying?

MR. HARVEGO: What I am saying is this says it will not. I tmeans whether you depressurize or not.

MR. MATHIS: You don't need it. That's what it says.

MR. HARVEGO: It says you don't need it. You could -- if it operates, fine; if it doesn't, it makes no difference.

I think there are a lot of questions to these.

DR. PLESSET: Now, on the next one, let's look at that. You get core damage you say?

MR. HARVEGO: Right.

DR. PLESSET: How do you explain that?

MR. HARVEGO: All I can say is that it would appear that there isn't sufficient cooling with the HPIS for that particular break size.

DR. PLESSET: It might or might not be the case?

MR. HARVEGO: It might or might not. This is indicating that it is likely there would be core damage. The extent, I don't know.

I think there is a lot of room for questioning these event trees. That's one of the reasons why we have taken this approach, to not only be looking at the potential for variations in these events. These events assume that either the system does or does not function as designed.

We would be looking at degrees. I think this particular event tree was presented in WASH 1400. Whether it is valid — it is not valid for every plant. Whether it is valid — is truly valid for the particular plant that we are using, I think that's up for questioning.

DR. PLESSET: Well, it juse seems a little gross, that's all, to me.

MR. HARVEGO: What seems gross?

DR. PLESSET: The conclusion you draw from this particular event tree.

MR. HARVEGO: I am not drawing anything. That is what is in WASH 1400.

DR. PLESSET: From that. That WASH 1400 is gross.

MR. HARVEGO: I think it is.

DR. PLESSET: Okay. Any questions of Mr. Harvego?

(No response.)

DR. PLESSET: I guess we are now ready to go to Mr. Johnsen. He has been very patient.

MR. JOHNSEN: I am Gary Johnsen. I am with the Semiscale program. Originally I intended to give you gentlemen an overview of the results and significance of results in the Semiscale program over the last year.

In the interests of time, however, I have a abbreviated talk to focus on the Three Mile Island simulations that were performed in Semiscale. Therefore, there will not be a one-to-one correspondence between my slides and what you are holding right now. In fact, it may be very, very difficult to follow my presentation.

DR. PLESSET: We have trouble anyway.

MR. JOHNSEN: I just handed you a supplementary list of slides, some of which will be in this, the rest of which will be in the original presentation handout.

In any event, as Dr. Wilkins pointed out yesterday, and as has been mentioned previously, the Semiscale program was fairly heavily involved with Three Mile Island starting with the accident itself, during the early crisis hours of the accident and for many weeks following in terms of trying to gain more understanding of what actually occurred.

(Slide.)

During the first few days of the accident, the Semiscale program was asked to investigate particular ideas with respect to recovering the system on the assumption -- under the educated assumption that a large noncondensable gas bubble existed in the upper plenum, upper head region of the TMI Plant.

We conducted two tests which we refer to as the noncondensable bubble tests and the particular idea here was to begin with the Semiscale system, having a condensable gas bubble in the upper head region, scaled to what was then believed to be the size of the bubble at TMI and then to attempt to recover the system, simulate a recovery, that is, in Semiscale by depressurizing the system to the point where the residual heat removal systems could be made operational.

Of course, the pressing question was could that be

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done without uncovering the core by virtue of movement of the expansion of the gas bubble down into the core region.

DR. PLESSET: Did anyone here ever attempt to check the bubble calculations, the size of the bubble?

MR. JOHNSEN: To my knowledge, we were given only the results of those calculations by those who were on the scene. However, I'm not privy to all of the bits and pieces that filtered down to EG&G from that site, as far as what went into the determining what the size of the bubble was.

Perhaps someone else in the audience could elaborate on that.

MR. KAUFMAN: We ran the first test about four hours after we were initially contacted. We began to modify Semiscale to set up to run the tests. In that four hours, we did not check the volume.

DR. PLESSET: I was just curious. I am just trying to learn something.

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MR. KAUFMAN: We did look at the SAR to conclude that it was approximately the right upper volume; and it was not off an order of magnitude, for example.

DR. PLESSET: I think the calculations are suspect, but I don't know for sure. I just thought you might have happened to look at it.

DR. ROSZTOCZY: As you know, they have been checked — there have been some check calculations done in Bethesda that Sunday night. There were some calculations which were later corrected. In the meantime, the bubble disappeared.

DR. PLESSET: That's right. That's interesting. I have been curious about it, have my own theory which I won't expose now. (Laughter.)

DR. ROSZTOCNY: Maybe at some other place, some other time?

DR. PLESSET: That's right.

Sorry to interrupt.

MR. JOHNSEN: That's all right.

In any event, those initial tests which I will discuss very briefly were followed by eight tests which were an attempt to, as faithfully as possible, simulate the actual battery condition events, if you will, that occurred in Three Mile Island as we understand them at that time on the Semiscale system and to measure the response of the Semiscale system; and by that process, to infer what actually occurred. (Slide.)

I am getting ahead of myself to some extent.

The objectives of those two different tests are indicated here. Again the purpose of the noncondensible gas bubble test was to plan, if we could — to find out if recovery by depressurization was a viable technique and the purpose of the objective of the two-hour simulations, if you will, starting from loss of feed water to core uncovery, was to look at -- initially, any way — to look at pressurizer response in the hopes of ascertaining whether or not the Three Mile Island pressurizer response was in fact a valid indication of system liquid inventory as determined by looking at the Semiscale results and also, of course, to investigate what sort of problems we would have with Semiscale in terms of conducting that sort of a simulation.

These objectives -- these latter objectives expanded out as the tests evolved to look at more detailed behavior vis-a-vis the initial pressure increase in the system when the heat injected into the steam generators was lost — whether or not the code safety valves actuated. (Slide.)

Despite the fact that we were in somewhat of a crisis environment early on, we nevertheless recognized that we had in Semiscale a representation of a four-loop flint, not a two-by-four flint. Consequently, we would have to look at making changes in the system as we best could in the short time available to better simulate the Three Mile Island B&W plant.

We made a number of changes. I don't want to dwell too long on some of these. You can see that the first thing we did was to take and remove the surge line that we had in the plant, in the Semiscale system, replace it by one which would be more representative of the B&W configuration in terms of the dog leg which Tom Larson mentioned earlier, in terms of the resistance, and also in terms of the resistance of that line, as well as the elevation characteristics.

We also added in two separate lines exiting from the pressurizer and terminating in orifices to simulate both the code safety relief valves in the Three Mile Island plant as well as the pile on the operated relief valve.

These -- especially the pile on the operated relief valve orifices simulator was sized on the basis of the rated flow capacity as stated in the SAR. Initially that told how to design that orifice.

We also connected a line between the inlet annulus in Semiscale and the upper plenum region to simulate the vent valves in the B&W configuration. We found it necessary to reduce the overall loop resistance so we could increase the flow rate in the system on a scaled basis more closely simulating that of Three Mile Island.

Also we added a orifice in the hot leg nozzle of the intact loop to provide a more symetric response in terms of the draining characteristics of Semiscale. As you can picture in your mind, Semiscale has the two separate loops. Because the intact loop represents three loops, it has a larger diameter of pipe than the broken loop does.

However, the center lines are lined up. That places the bottom of the intact loop cold leg pipe physically lower than the respective location of the broken loop, so the draining would be asymmetric otherwise.

This was a step that was particularly taken in anticipation of the bubble tests rather than the overall simulations that were later conducted. Despite these changes to the system we realized we were stuck with some basic limitations.

Tom Larson already discussed the subject of external heat loss which in our system is disproportionately large. In fact, in terms of our two-hour simulations of the Three Mile Island accident, we utilized our heat loss, if you will, as the primary heat rejection mechanism as opposed to simulating the steam generator heat rejection in Three Mile Island.

The steam generator design was also much different. We have U-2 steam generators versus the ones through in Three Mile Island. This was mitigated by the fact we were not relying on them as a heat rejection mechanism after the initial completion of secondary liquid mass.

Also mentioned earlier by Tom was the primary coolant pump degradation. We feel fairly confident in knowing or recognizing the fact that our pumps tend to degrade more rapidly as a function of voiding in the system than would the larger pumps in the B&W system.

Those were the primary limitations that we were forced to just live with during these simulations.

I don't want to dwell very long on the results of the bubble tests except to make two points. (Slide.)

The first is that in both of the tests we conducted in terms of depressurizing the system with the gas bubble in the upper plenum region, we found that in both cases, the system could be depressurized in Semiscale again without uncovering the core to the point where the residual heat removal system could be activated.

Obviously this piece of information was not acted upon in terms of recovering Three Mile Island, but still provided useful information in the analysis which attended recovery concepts.

Secondly, it is kind of interesting to note that in the context of runnning those bubble expansion simulations, if you will, that we learned that we could not operate both Semiscale pumps simultaneously with the presence of that noncondensible gas in the system. What we found was that neither pump could generate a net positive suction head with that degree of noncondensible gas in the system .

However, if we operated only one pump alone, it

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would force the gas into the other loop and then could — the pump that was operating then could maintain coolability of the core.

So that's an interesting result in terms of an assessment of what sort of recovery techniques might be suggested for the system; a PWR for example, had a large amount of voiding in it, whether it was noncondensible gas or steam.

I would now like to move on to some direct comparisons. (Slide.)

I think this is probably the most interesting aspect of these results. We have here a direct comparison between the small Semiscale system and a big plant, between Semiscale and Three Mile Island, in terms of some basic parameters that were measured in both systems for the — what I call the two-hour simulations.

Recognize that when these simulations were conducted there were several unknowns and still are some unknowns in terms of what was the — what were the primary boundary conditions on the Three Mile Island plant. Principally here we are talking about HPIS operation as a function of time, as well as makeup and let-down flow rates as a function of time.

These are not well established at this time and certainly to have a influence on the overall mass and energy amm

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balance in the system.

We took the information — the best information we had from the Nuclear Regulatory Commission and factored that into a test plan for these two-hour simulations. Here we are looking at a comparison of the system pressure as a function of time for approximately a two-hour period. As you can see, the agreement is generally pretty good between the Semiscale system and Three Mile Island. I think this is primarily attributable to the fact that we were able to simulate the mass discharge through the pile on the operated relief valve fairly faithfully with our orifice. (Slide.)

Here is a comparison of hot leg temperature in the two systems. The agreement is qualitatively fairly good; and notice especially that the time at which superheating is noted in the Three Mile Island plant was fairly closely simulated by the Semiscale system, following the termination of pump power. (Slide.)

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Here in this slide we are looking strictly at Semiscale data. This sort of summarizes the tail end of our simulations.

Here we are looking at the last few moments of the transient, if you will, culminating with core uncovery. We are looking at the pressurizer level in Semiscale, in this case multiplied by a factor of four to conform to the elevation in Three Mile Island.

The middle plot is a -- is the collapsed liquid level in the Semiscale core; and the bottom line is the mid core temperature measurement in Semiscale.

Now, you will notice that after the pumps were shut off in Semiscale, we see a termination of the oscillatory behavior in the collapsed liquid level in the core. It evens out and remains level for quite some time.

During this period of time, what occurred was a draining of liquid from the upper portions of the system; the steam generators into the hot legs into the core to balance the boil off that was occurring in the core so that the level stays fairly stable.

When this draining is complete, we then see a boil off, a fairly straight boil off of the core. We see the collapsed liquid level dropping. When the collapsed liquid level falls low enough in the core to uncover, the location associated with this thermocouple, you see the temperature take off. It is significant, I think, to note that during all of this voiding of the system in the core region, you see the pressurizer remains full.

The discussion of the validity of these results with respect to typicality of Semiscale and TMI was discussed earlier, so I won't touch upon that.

(Slide.)

It's interesting to look at what was going on in the core region of Semiscale during the time immediately adjacent to the termination of pump operation.

Because we have in Semiscale the external downcomer, we can make density measurements directly into the core. In this plot, what we are looking at is the density measurement at three different axial locations in the core. The lower part of the core shows the least amount of void, obviously. As we progress up the core, we see higher voiding.

Note that this -- this period incidently is the same period we showed a minute ago where the liquid level was fairly stable in the core.

Note that during the boil off, which occurs here, what we see is a progressive drop off in the density starting from the upper part of the core, where we see a rapid turnover in this density, indicating that fluid has passed by that axial measurement, and progresses up the core.

Now, this sort of a measurement enabled us in conjunction with the collapsed liquid level measurement to infer the relationship between a collapsed level and a froth level.

(Slide.)

That is shown on this next slide here. In other words, by noting the times at which the density takes the sharp drop at the various elevations, you can infer where the froth level was as a function of time. That's shown here. We can then compare that with the collapsed liquid level as shown here as measured by the delta p measurement of the core.

We not very interestingly that these two levels tend to come together. This is not totally surprising since as the boil off occurs, what we have is less and less net energy entering the fluid, and, therefore, less violent boiling as the boil off proceeds down the core.

So when we reach an elevation that's approximately slightly below mid core, we see that we have practically collapsed all of the froth, and we have perhaps a very small froth region. This has implications in terms of the coolability of those exposed core rods above that region.

We were able to look at the heat up data from the Semiscale core rods, and by performing inverse calculations, compute the heat flux at the various elevations above the collapsed level.

This plot illustrates the computed heat transfer coeeficient at one particular axial elevation as a function of its position above the collapsed liquid level.

Again, we could construct a number of such curves for each -- for each elevation to have its own curve.

This shows that as the collapsed liquid level drops down in relation to the position of this particular location of the core, that the heat transfer coefficient drops down.

Now, in order to calculate this heat transfer coefficient, we required the use of an assumed bulk fluid temperature at the same axial elevation. In this case, what we have done is assume saturated vapor. You will recognize the fact that the vapor will not be saturated, the fact it will superheat as that vapor rises through the core in the voided region. However, in the case of Semiscale, we are -- our measurements are insufficient in terms of measuring vapor temperature because of radiation.

In any event, we could take this heat transfer information and together with other data from Three Mile Island, namely data relative to the power and axial power shape and so on, together with liquid level data as infereed from the ex-core detectors in Three Mile Island and construct, if you will, a liquid level as a function of time in Three Mile Island and which, taken together with the heat transfer coeeficient measurement or calculations, I should say, from Semiscale, would

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yield a prediction for the heat transfer coefficient as a function of elevation in Three Mile Island.

That's illustrated on this slide here.

(Slide.)

We are looking at the period essentially from the time the pumps were -- the last pump was shut off in Three Mile Island to the time when the pumo was reactivated. These are the predictions, again, based on that methodology I have just described for the heat transfer coefficients at various elevations in the Three Mile Island core.

When these results were applied to a conduction calculation in Three Mile Island, which took into account the exothermic zirc-water reaction on the external portions of the rods, it yielded a temperature prediction that you are looking at right now.

(Slide.)

This showed or suggested that below about the 2.9 meter elevation in Three Mile Island, that the core reamined cool, but that above 2.9 meters, the temperatures reached elevated levels. Depending upon which side of the uncertainty band and the heat transfer coefficient that we computed you wished to use, you would either obtain temperatures that essentially went out of sight or leveled off at nonetheless very high temperatures where, in fact, the zirc-water reaction would be very important, and also where core damage could

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obviously result.

(Slide.)

Summarizing, then, in terms of what we think is significant from these tests, first of all we think it's rather significant that the overall hydraulic trends demonstarted in Three Mile Island were fairly faithfully reproduced in our system despite the fact that we were not designed to simulate that system.

I think that because this accident is predicated on mass and energy balances primarily, that we were able to achieve a fairly good simulation.

Secondly, as evidenced by the results I showed you earlier, it's fairly clear now -- and I don't think there is any argument on anyone's part -- that the pressurizer from Three Mile Island -- liquid level in the pressurizer was not a valid indication of system liquid inventory.

Thirdly, that the whole area of core uncovery heat transfer deserves a great deal of additional attention. Dr. Rosztoczy suggested this earlier, and we certainly agree. In fact, the heat transfer that we saw in Semiscale during the core uncovery period is very, very poor and gets worse as the level -- liquid level in the core drops.

This is an area that I think is particularly relevant in terms of small break calculations.

Again, the fourth point is really an indication of

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what I showed you a minute ago, that our results when projected on the basis of what I have described it to Three Mile Island and suggest signifcant core damage could have resulted. Again, it's -- there's probably no argument there.

Lastly, these tests were useful in terms of defining those particular areas in Semiscale that would require attention in the future for looking at very long transients; specifically what I mentioned earlier, the heat loss problem; pump leakage was another area Tommy Larson talked about earlier which became a factor in these tests.

That concludes my presentation.

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DR. PLESSET: I guess there are no questions. Thank you very much. We appreciate that.

Well, we are near adjourment time. I was going to see if our consultants wanted to make general remarks, or specific remarks, before we adjourn.

(No response.)

DR. PLESSET: I guess not.

MR. LIPINSKI: I have a general comment.

DR. PLESSET: Yes, please.

MR. LIPINSKI: In Dr. Rosztoczy's view graphs, he indicated there was a desire to have the reactors run some mild transients experiementally; is that correct?

DR. ROSZTOCZY: Yes, that's correct. I am not sure I characterized it as mild.

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MR. LIPINSKI: In the view graph prior to that, it addressed the LOCA events. I forget what the magic words were. I think confirmatory or something like that.

I don't think you specifically said you wanted to do any experimental work on small LOCA's on an actual plant; am I correct?

DR. ROSZTOCZY: Not on the last point. We have requested that they do experimental work on the small loss of coolant; but we have asked that at least the one test showing the normal behavior of the pressurizers as designed. So the pressurized sufficiently that the high pressure safety injection, the safety injection and the low pressure injection comes into play, a demonstration test for that.

We asked for a typical test for the case when the break is smaller than the size and the pressure hangs up on -- at the pressure level which is usually close to the second area system pressure, and then show the recovery from this condition.

Then we asked for a third test which is even smaller break size than this, where the pressure after an initial drop is atcually going to increase and repressurize to some higher level and the show how it should recover from that test.

MR. LIPINSKI: Is this per plant or per typical plant?

DR. ROSZTOCZY: Just one. One for each on the

Semiscale system.

Then there were some additional requests. We wished to demonstrate some of the basic phenomena which is presently being predicted to take place in some of the systems. We talked about nature circulation in the ones through steam generator. We were wondering if it were possible to set up some tests that may be for a given time period you can stay in a steady condition in this type of heat transferm demonstrate how it works, demonstrate how you get into it, and what might possibly interrupt this.

MR. LIPINSKI: Are you talking about tests or tests on a natural system?

DR. ROSZTOCZY: No. All of these tests are for Semiscale.

MR. LIPINSKI: I am talking about tests on a full size reactor. Om transients you specifically said these tests would be performed on an actual reactor system , a full sized reactor to get confirmatory results.

I was going to pose a question as to whether you are looking for equivalent information on small LOCA's on a full sized reactor system.

DR. ROSZTOCZY: I am sorry. I misunderstood the question. For all the LOCA type of tests, we are not proposing any LOCA type of test to be done on a full sized reactor. We a. oporsing all of those -- for transients where normal limit is that you do not go into DNB. We are proposing carefully selected typical tests on actual reactors.

MR. LIPINSKI: In vuew of the questions on confirming codes, experiments, comparison to experiments, three dimensional systems, multiple loops, is there any benefit to consider full sized tests where the risk is relatively small?

DR. ROSZTOCZY: A good example of that would be the General Electric tests. They had a code developed 20 years ago that has been used for the safety evaluation in the design of the boiling water reactors. Basically, the code simulated the transient and calculated the change in the critical power ratio during the transient.

The plants then are designed to have a sufficnet margin in the operating condition that they will be removed far enough from the point when you might go into a hot point DNB to cover this critical change during the transient.

It is an important part of the design of the plant. This coed has been used to predict the tests. As a matter of fact, it has been used for more than that. It has been used for the safety evaluation of the reactor for those tests before we could permit those tests in an actual reactor. We wanted an assurance that the concept of the tests are acceptable.

There is calculation based on that that permitted them to run the tests, They ran the tests, The data CPR changed for the three-date appointment they had planned. It was 100 percent up to either 300 percent. At the same time, the margin that was in that calculation was on the order of maybe 20 percent. So we found a huge difference between the code calculation which had been performed continuously for 10 years or so, the licensing of these plants, and within the physical behavior that one could observe an actual reactor which had all the feedback.

After obtaining this, they went into the detail to analyse where the difference was coming from. It has been tracked down to the time response of the void collapse. The void apparently collapsed in the actual reactor faster than in the calculation which provided the reactor with the feedback,and the activity feedback came somewhat earlier in time. There was no other position for that.

That resulted in a higher power excursion for a shorter period of time.

This is an example of a test performed on an actual reactor that can point out deficiencies that existed for a number of years, but there was no way to check on it. The code response was the type of response that you would expect. There was nothing in it that would say this is unrealistic. It was only delayed somewhat in time, and that caused the problem.

When the PWR's -- they can't -- point to a certain to a certain problem of here is something that might be wrong. Nevertheless, we would like to see some challenging test which goes through on a reasonably trusted transient, that it challenges the code. Obviously, it has to be within the safety limits of the reactor and see if it turns out the same way as the calculations.

The PWR's the dynamics of the steam generator stop that from performing these calculations.

They are usually evaluated for planned operations. There is no data for anything more drastic than that.

We feel it would be beneficial to have some selected tests in probably one reactor, or two typical ones and compare those against the calculations.

MR. LIPINSKI: No further comment.

DR. PLESSET: Any other questions?

(No response.)

DR. PLESSET: Let me ask a question, then, of Zoltan.

What you are planning is a series of tests in Semiscale in which you have a version which is B and W, a version which is CE, one which is Westinghouse, is that correct?

DR. ROSZTOCZY: We are doing calculations for each of those.

DR. PLESSET: Calculations? I thought you were going to have tests?

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DR. ROSZTOCZY: The tests, I believe, are being set up to simulate some of the basic phenomena expected in each of these reactors. I think the CE and the Westinghouse are the same, so I don't think we are talking about any different tests there.

DR. PLESSET: So the machine would be the same?

DR. ROSZTOCZY: I believe it would basically be the same.

For the B and W case, there would be some differences because of the ones through steam generator that introduces a kind of difficulty.

DR. PLESSET: My question, then, is somewhat reduced. Would you feel that it was significant to make the changes in the machine, Semiscale, for these changes relative to the plants? Would that be lost in the errors in the similitude which Semiscale has, of necessity?

In other words, they are all pressurized water reactors. That's the zero order approximation.

Now then, you get into a higher order. One is four loop. Westinghouse. U-2. One is a two loop. The other is once through. With all of these refinements -- will these refinements be different enough to justify changing the machines before you do the experiments? Do you see what I am trying to get at?

DR. ROSZTOCZY: I think so. Let me try to explain

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it through an example about the circulation problem.

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In the B&W design, the ones through steam generator, through analysis, through thinking and doing certain calculations, both B&W and us, we kind of arrived at a poiint where both sides agree. We found two ways of how the natural circulation can be interrupted in those sytems. One of them is the formation of a bubble in the top of the candycane which can prevent natural circulation. The second one is a condition when the water in the secondary side of the steam generator is at the low level but if there is no spraying of any more feed water, for example, because you can't control to that level, could prevent natural circulation in the loop.

The second type of actual -- stopping of the natural circulation we believe actually happened at TMI.

. To demonstrate these two, you need a once-through steam generator in the Loop. You would have to take out the steam generator which is there now and put in a different one.

At the same time through our thinking and this calculation, we could not postulate any other than this. The test might bring attention to something else. So we think it's important.

Now, when you go with the same question, the question of natural circulationg to a U-2 type of steam generator, both manufacturers who have U-2 type of steam generators describe a quite different process. They are saying the basic generator, even if there is just a small amount of water on the secondary side, then the steam has to go up on a small cooled section on the steam generator tubes.

Some of the steam will condense there. Water is going to drop down against the steam flow and then leak back to the top of the core and provide core cooling.

So they believe that actual stopping of the natural circulation will not happen in the steam generator unless they dry out the steam generator.

They have calculations to support this, but we have not seen any physical demonstration that it really works this way; and there will be enough water dropping back provided you have so much water in the secondary side of the steam generator. We would like to see a demonstration of this type of phenomena.

Whether I have 8,000 tubes in the steam generator, which the normal PWR has, or whether we just have a handful of tubes there, that's somewhat different. The phenomena, I think, can be demonstrated. We are asking for the demonstration. This alone would warrant the insertion of a different steam generator into the loop for one type of test as opposed to the second type of test.

DR. PLESSET: I see your concern. What I was trying to ask about are the atypicalities of Semiscale large enough so that you might not trust the results either way?

DR. ROSZTOCZY: I don't think so.

MR. ALLEMAN: Does it scale?

DR. PLESSET: That's the question. Does it scale? If it doesn't, what do you need to do?

DR. ROSZTOCZY: I used on my slide the word demonstration test. Demonstration is meant along these lines, that we intend to demonstrate through these tests that this type of actual circulation does work, does work in a system with a few tubes.

If we can demonstrate that, then I think we are in a much better position to pass a judgment as to how effective this phenomena is in the large system. If we have never seen any demonstration of this, we are in a much bigger position.

DR. CATTON: Wouldn't you be better off to run this particular test as a separate effects test? The steam generator U-2 refluxing?

DR. ROSZTOCZY: I am not sure what is the terminology that has been used here. You can think of it as a separate effects test. Part of our thinking was one of these can be run as more than a series. These are not tests -- if it works properly, this is not a test which has to be terminated. Some of the blowdown you have to terminate.

You run for a steady state and you see how this sets in. Then from this, you might transfer into a different mode of heat removal and you demonstrate that for a half an hour and transfer into a third mode and demonstrate that for a half hour.

DR. CATTON: This would not necessarily be in Semiscale?

DR. ROSZTOCZY: I am not as familiar with the details of how difficult it is to set up some separately — so-called separate tests as opposed to taking this equipment into Semiscale and running it in Semiscale when you have all the instrumentation all ready.

I don't know which is cheaper. I don't know which is easier to do.

Theoretically, yes, some of this could be run as somewhat of a separate test. It would require a full loop in the heated core.

DR. TONG: I would like to make a comment. In separate effects, you need another facility. If you wanted a high pressure, you have to have a separate facility. You fabricate a high pressure group to test the separate effect. So Semiscale, you can interpret it as a separate effect in the components.

If you have any thought about loop circulation

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time, then you can -- either you can incorporate it by calculation, say this data should be modified by — divided by, or you get all this circulation.

You do find out that a separate effect in the steam generator exists.

You can treat Semiscale —

DR. CATTON: You make the rest of the Semiscale just your high-pressure system then?

DR. TONG: You can do that. I really think that it's not quite economical or practical. To do it, many separate effects at high pressures would have to be done.

DR. CATTON: It's a matter of the view you take when you set up a test at the outset?

DR. PLESSET: Well, clearly we don't have enough in the way of experimental facilities that work at high pressure.

DR. TONG: Right.

DR. PLESSET: Do yu want to make any final remarks, Dr. Tong?

DR. TONG: Well, I think that the last statement is almost the same. The time schedule is so tight. We wish we could use all the existing facilities with modifications and satisfy the licensee's need; and also all the other people's need.

But in doing this, I urge my contractors to

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emphasize their analytical effort. I don't mean cold calculations; analytical analysis. Use the hardware, the facilities to analyze or to understand the behavior of the facility, what would come out, which is typical, which is not typical, and get advanced information before the test results come out and avoid a misunderstanding. That's all I wanted to say.

DR. PLESSET: Thank you.

Mr. Kaufman, do you want to have a last word?

MR. KAUFMAN: Very briefly. I would like to come back to the opening remarks that I made. We believe that our product here is to develop understanding of the processes and the system behavior that characterize postulated accident conditions. We think that the LOFT program and the Semiscale program and indeed the code development efforts focus on the areas that are of the greatest concerns now.

We believe that the programs we put together are responsive to the requests of NRC and are responsible in terms of approaching the important issues.

I think any one part of our program without the other would put us in a somewhat weaker position. Specifically, to conduct meaningful tests in LOFT, it implies that we have sufficient understanding of the phenomena that we can design the tests and locate the

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instruments and prepare our operators for what they will experience. To do that well, I think we need the tests and facilities like Semiscale. Whether one calls those separate effects or integral, I think that is somewhat moot.

The point is issues are identified, phenomena are explored; and that adds to and contributes to our understanding.

I could make the same remarks for the codes and code development because again that's another adjunct to characterizing the degree of our understanding.

I think that's the overall program that we have going on, and we appreciate very much your comments and observations.

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DR. PLESSET: Well, thank you. I think we have had a good meeting. We look forward to the next one.

With that, we will adjourn.

(Whereupon, at 2:40 p.m., the meeting was adjourned.)

Larson
8/28/79

SEMISCALE SCALING

T. K. LARSON
ACRS MEETING
AUGUST 27, 1979

EG&G Idaho, Inc.

SEMISCALE SCALING PHILOSOPHY

- APPROACH
- CRITERIA
- COMPROMISES

DEFINITION OF SCALING

THE TECHNIQUE BY WHICH A FACILITY IS CONSTRUCTED TO ALLOW THE IMPORTANT PHENOMENA IN A REFERENCE SYSTEM TO BE REPRESENTED IN A GEOMETRICALLY SMALLER SYSTEM

REDUCED SCALE EXPERIMENTS

- IT IS IMPOSSIBLE TO MAINTAIN GEOMETRIC, KINEMATIC, AND DYNAMIC SIMILARITY AT EVERY POINT IN THE EXPERIMENT FOR ALL TIME PERIODS OF CONCERN

- DISTORTIONS WILL EXIST IN THE EXPERIMENT RELATIVE TO THE LARGE SYSTEM FOR PHENOMENA WHERE MULTIDIMENSIONAL EFFECTS ARE IMPORTANT

POSSIBLE SCALING APPROACHES

- LINEAR
- DIMENSIONLESS NUMBERS
- VOLUME

MOD-3 SYSTEM SCALING

- VOLUME SCALING APPROACH
- WESTINGHOUSE TROJAN BASE PLANT FOR LOOP SCALING
- WESTINGHOUSE UHI BASE PLANT FOR VESSEL
- INITIAL SCALING DONE BY EG&G
- NRC, WESTINGHOUSE AND CONSULTANT REVIEW
- FINAL SCALING WITH NRC AND REVIEW GROUP

REFERENCE PWR AND MOD-3 VOLUME DISTRIBUTION

	REFERENCE PWR		MOD-3	
COMPONENT	VOLUME(M^3)	% TOTAL	VOLUME(M^3)	% TOTAL
DOWNCOMER	31.37	9.75	0.0222	10.66
UPPERHEAD	22.53	7.0	0.0140	6.75
UPPER PLENUM	17.49	5.44	0.0112	5.38
CORE	18.31	5.69	0.0106	5.08
LOWER PLENUM	27.52	8.56	0.0157	7.54
GUIDE TUBE	8.45	2.63	0.0015	0.74
SUPPORT TUBE	1.19	0.37	0.0004	0.19
TOTAL VESSEL	126.84	39.45	0.0756	36.34
INTACT LOOP	153.62	47.79	0.1037	49.86
BROKEN LOOP	41.00	12.76	0.0287	13.79

IMPLICATIONS OF VOLUME SCALING IN MOD-3

- MAINTAIN GEOMETRIC, DYNAMIC, AND KINEMATIC SIMILARITY IN THE CORE AND STEAM GENERATOR
- PIPING LENGTHS NOT MAINTAINED
- PIPING DIAMETER NOT MAINTAINED
- PIPING SURFACE AREA NOT MAINTAINED
- L/D OF SOME COMPONENTS IS LARGE

MOD-3 COMPONENTS

- DESIGNED WITH UHI IN MIND
- TRIED TO MINIMIZE DISTORTIONS
- HEAVILY INSTRUMENTED TO HELP QUANTIFY DISTORTIONS

MOD-3 CORE

- SCALING CRITERIA - VOLUME, LENGTH, POWER, AXIAL POWER PROFILE
- COMPROMISES - ELECTRICAL ROD, SPACER GRIDS, STRUCTURE SURFACE AREA
- TREATMENT
  - DESIGN
    - COMPUTERIZED POWER CONTROL
    - INSULATION
  - EXPERIMENTAL
    - POWER CONTROL TESTING
    - SERIES 7 RESULTS
  - ANALYTICAL
    - CORE POWER DETERMINATION
    - CONDUCTION CALCULATIONS

SEMISCALE MOD-3

INTACT AND BROKEN LOOP

SCALING CRITERIA:

HYDRAULIC RESISTANCE, VOLUME, PUMP SUCTION LEG DEPTH

SCALING COMPROMISES:

FLOW AREA, LENGTH, SURFACE AREA, INSTRUMENTATION

TREATMENT:

- EXPERIMENTAL:
  - HYDRAULIC RESISTANCE TESTS
- ANALYTICAL:
  - CONDUCTION ANALYSIS

SEMISCALE MOD-3

STEAM GENERATORS

SCALING CRITERIA:

- VOLUME, SURFACE AREA, PRESSURE DROP, LENGTH

SCALING COMPROMISES:

- INTACT LOOP (SCALED FROM LOFT)
- BROKEN LOOP SECONDARY SIDE VOLUME

TREATMENT:

- EXPERIMENTAL:
  - INVESTIGATION OF INFLUENCE OF BROKEN LOOP STEAM GENERATOR (TEST SERIES 7)
  - EVALUATION OF INTACT LOOP STEAM GENERATOR HEAT TRANSFER (MOD-1 TEST SERIES 1)

ANALYTICAL:

- RELAP CALCULATIONS

SCALING CONSIDERATIONS FOR LARGE BREAKS

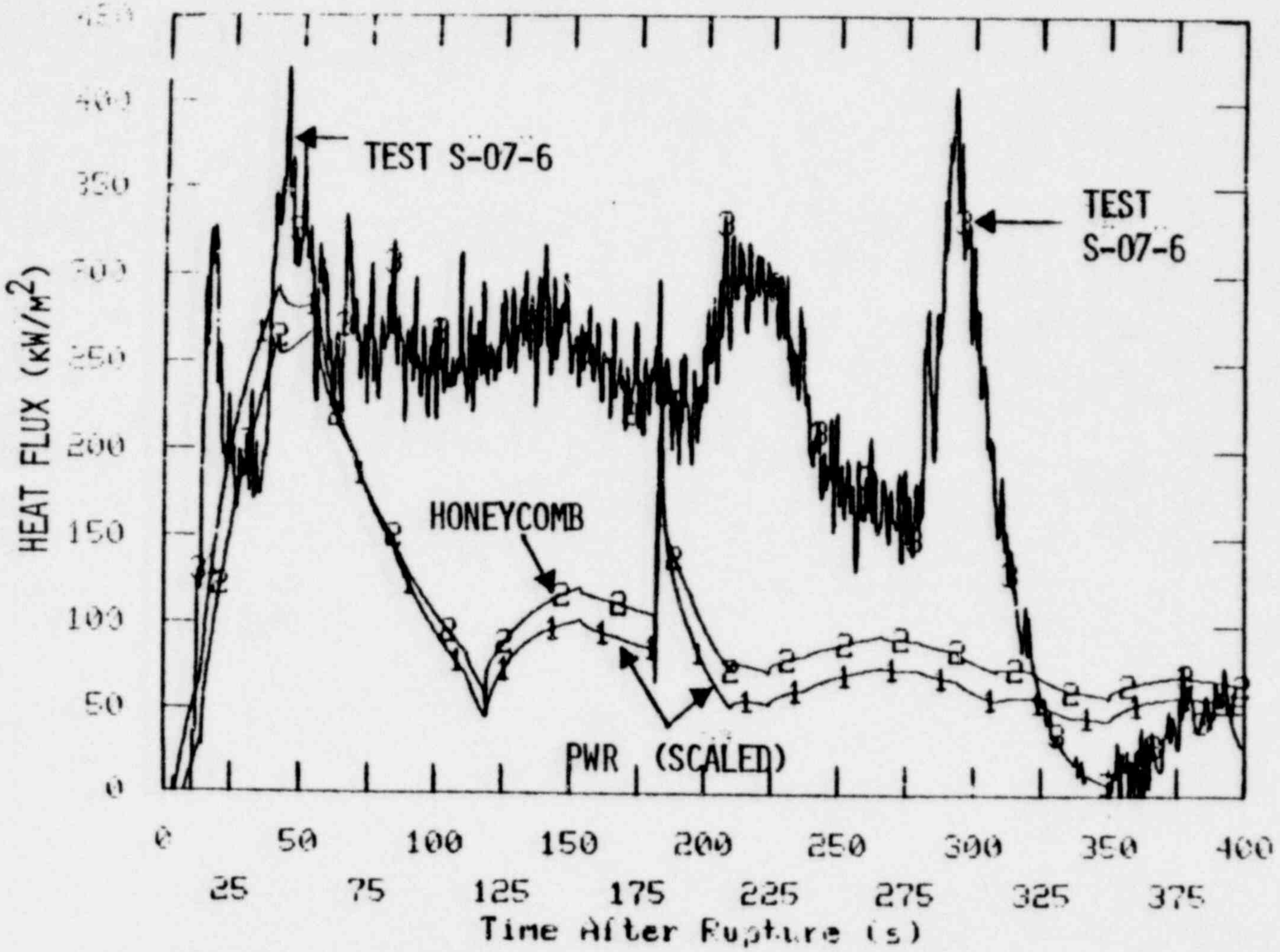
POSITIVE POINTS:

- RESISTANCE DISTRIBUTION IS CORRECT
- ENERGY AND MASS DISTRIBUTION IS CORRECT
- CORE GEOMETRY
- TIME SCALE
- RELATIVE ELEVATIONS MAINTAINED

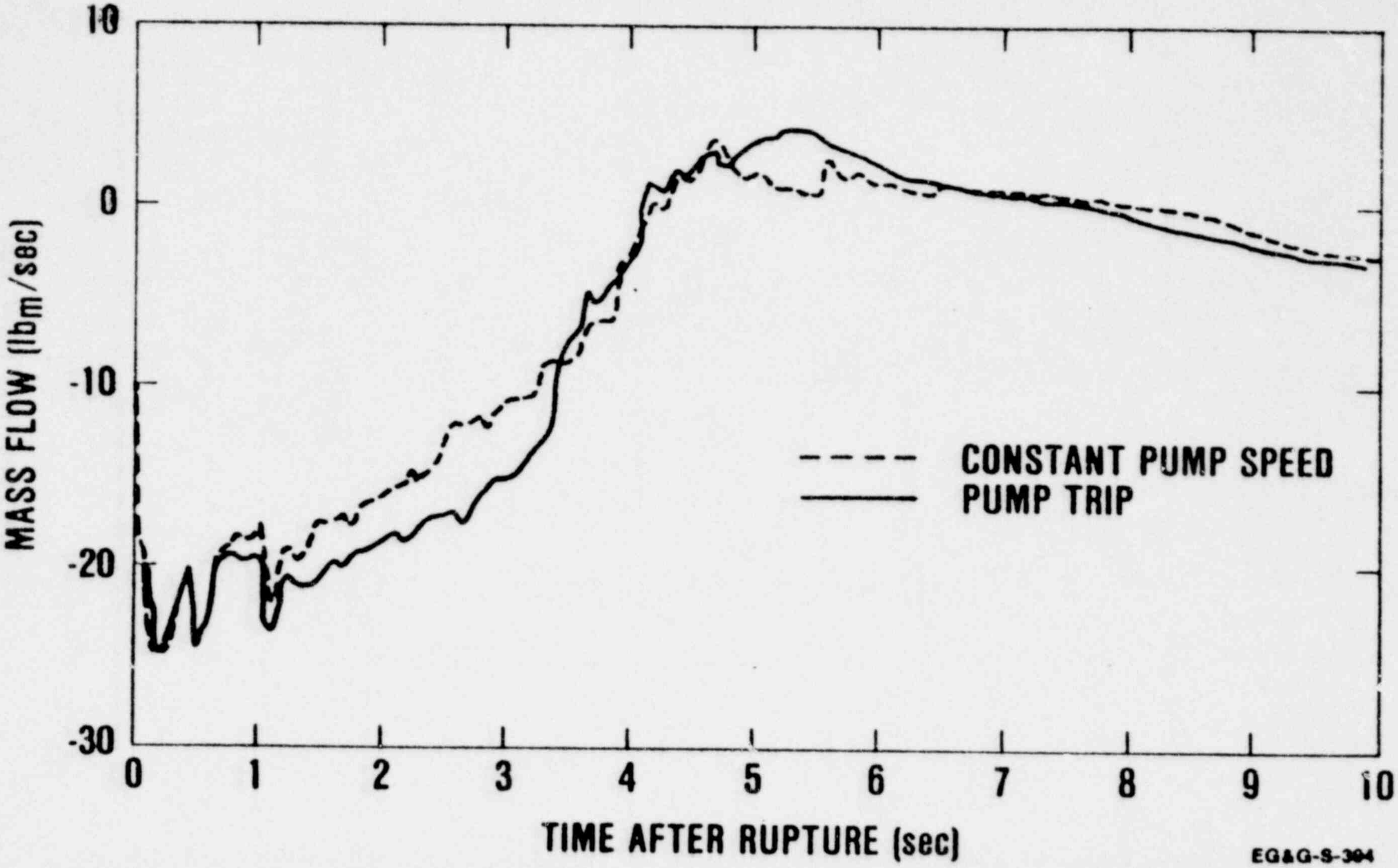
CONCERNS:

- METAL STORED ENERGY AND SURFACE AREA TO FLUID VOLUME RATIO
- LOOP VELOCITY
- PUMP CHARACTERISTICS
- STEAM GENERATOR CHARACTERISTICS

PROPERLY SCALED HEAT FLUXES FOR TEST S-07-6 AND CONDUCTION LIMITED PWR AND HONEYCOMB DOWNCOMER



DOWNCOMER MASS FLOW



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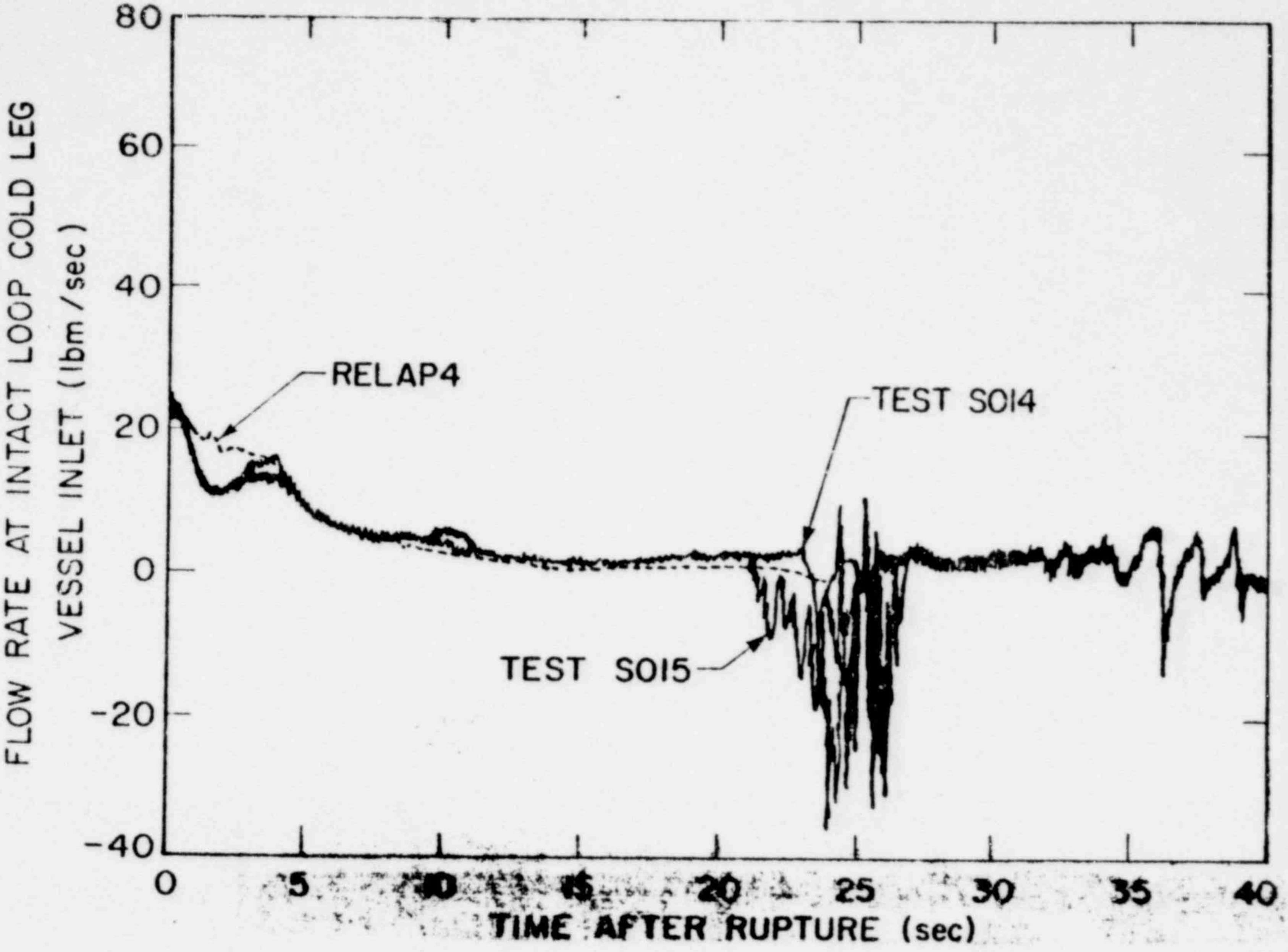
FLOW RATE AT INTACT LOOP COLD LEG



SCALING CONSIDERATIONS FOR SMALL BREAKS

POSITIVE POINTS:

- LOOP SEAL ELEVATIONS CORRECT
- ACTIVE BROKEN LOOP COMPONENTS
- CORE LENGTH AND GEOMETRY
- BREAK AREA EASILY CHANGED

CONCERNS:

- HEAT TRANSFER (LOSSES, PIPING THERMAL TIME CONSTANT)
- FLOW REGIMES / VELOCITIES
- PUMP BEHAVIOR
- CRITICAL FLOW
- STEAM GENERATORS
- DIMENSIONALITY

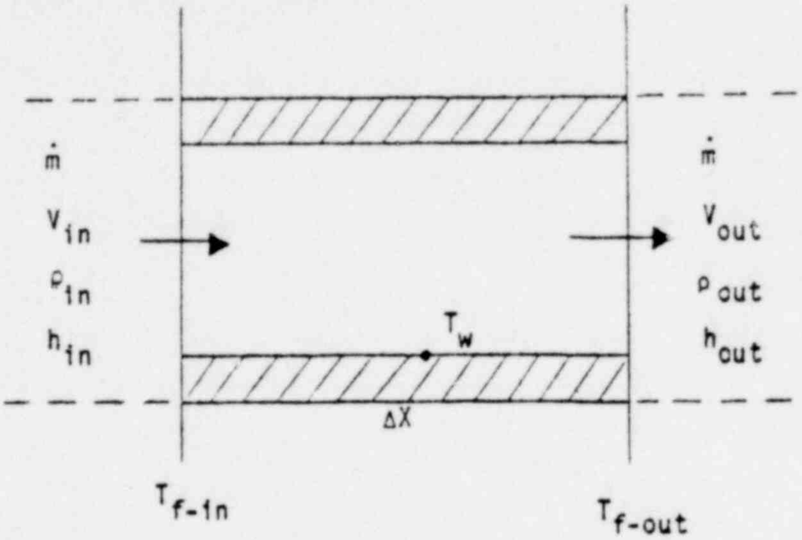
ESTIMATED COMPONENT HEAT LOSSES IN SEMISCALE

COMPONENT	PRESENT HEAT LOSS (KW)	RESOLUTION	ESTIMATED MINIMUM HEAT LOSS (KW) *
VESSEL	40	HONEYCOMB, EXTERNAL HEATING, QUALITY INSULATION	5
DOWNCOMER	13		5
LOOP PIPING			
BROKEN	18	HEAT TAPES QUALITY INSULATION	3
INTACT	25		3
INSTRUMENTATION	13	MODIFICATION IN APPROACH	7
MISCELLANEOUS	8	ATTENTION TO QUALITY OF INSTALLATION OF INSULATION	3
	117		26

* WITH THE EXTENSIVE USE OF HEAT TAPES

EXTERNAL HEAT LOSSES COMPARISON TO LOFT, PWR, SEMISCALE

	PWR	LOFT	SEMISCALE
TOTAL LOSS (kW)	1500 - 2000	257	120
% OF CORE POWER	0.07	0.5	6.



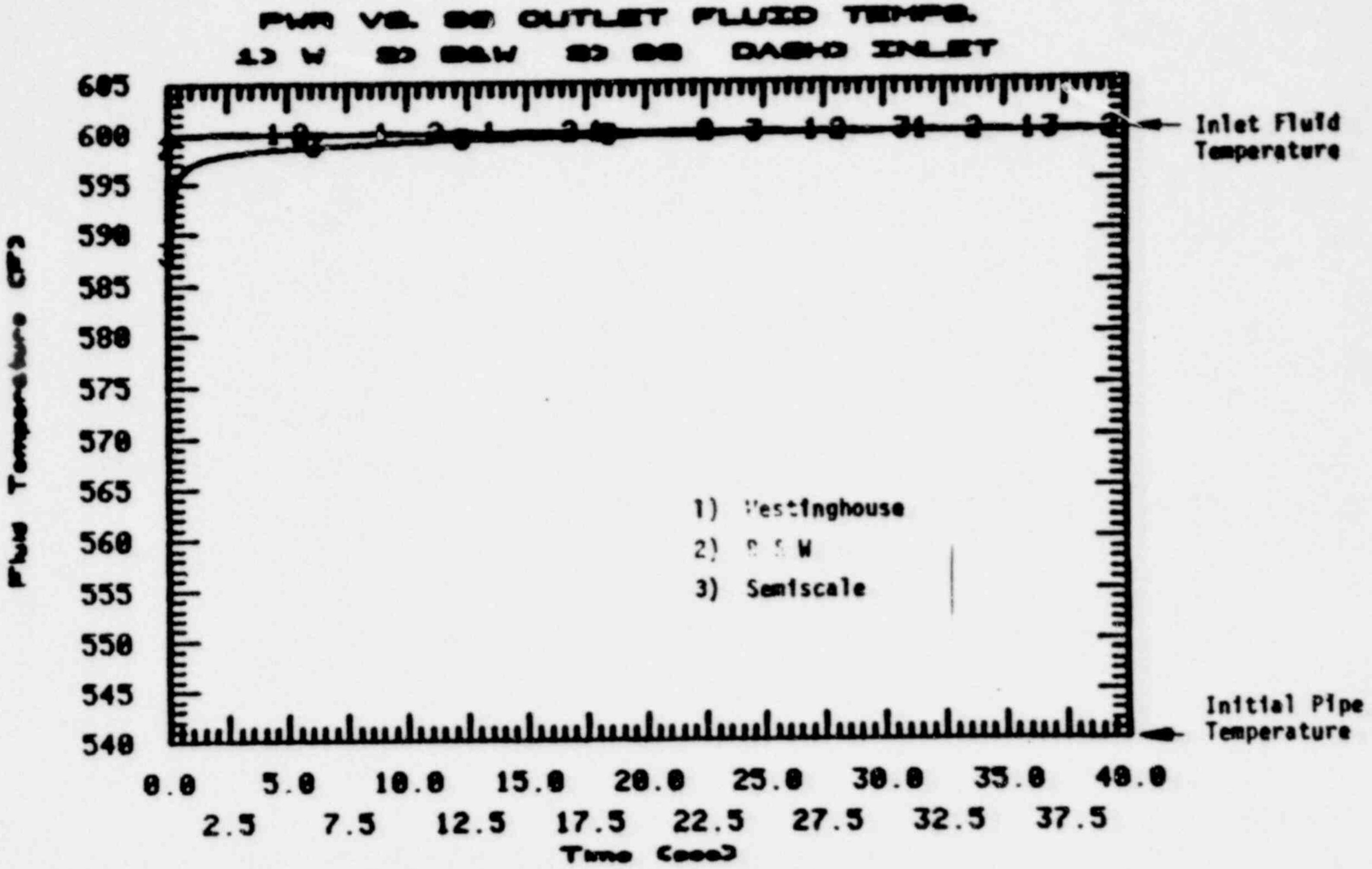
MODEL USED FOR PIPE WALL TRANSIT

TIME CALCULATIONS

POOR ORIGINAL

INFLUENCE OF PIPING ON FLUID TEMPERATURE

FOR A TEMPERATURE STEP AT THE INLET



CORAL PIPE MODEL CALCS.

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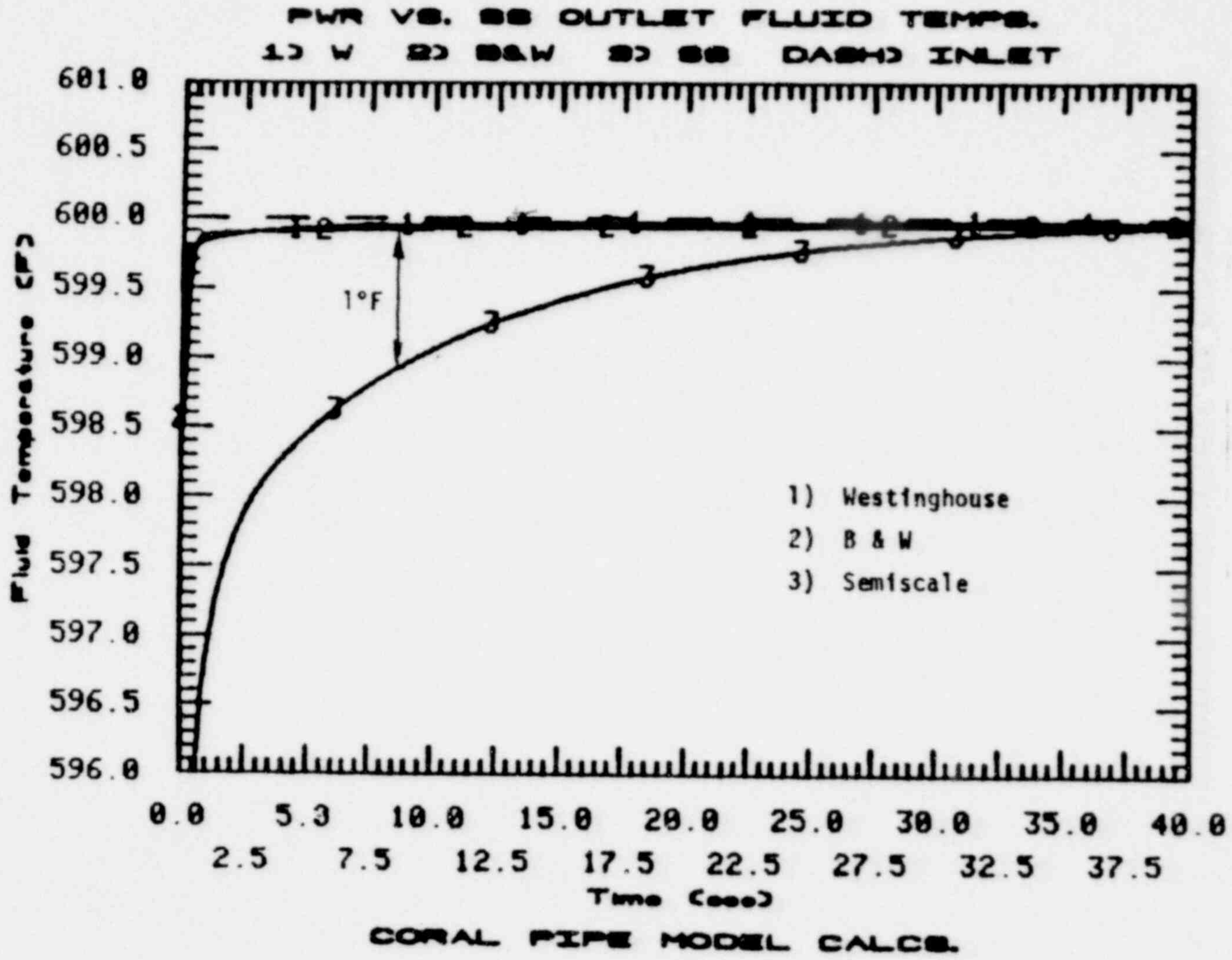
INFLUENCE OF PIPING ON FLUID TEMPERATURE

FOR A TEMPERATURE STEP AT THE INLET



FLOW REGIMES

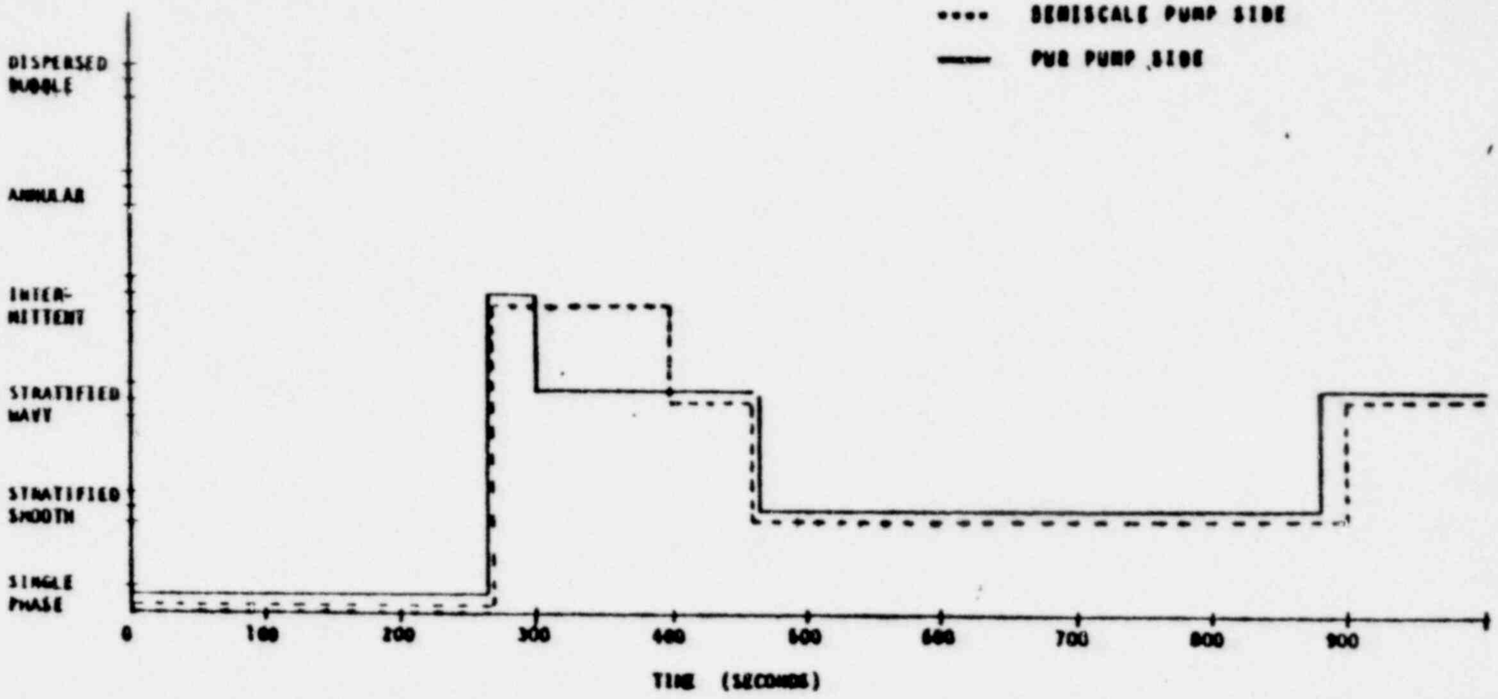
INFLUENCE:

- WETTED AREA, PRESSURE DROP, CRITICAL FLOW

TREATMENT:

- FLOW REGIME CALCULATIONS
- DATA COMPARISON TO LARGER SCALE

PREDICTED FLOW REGIMES IN SEMISCALE AND PWR COLD LEG PIPING DUKLER-TAITEL METHOD - PWR 4 IN. BREAK AUDIT CALCULATION MASS FLUXES USED



LOOP PUMPS

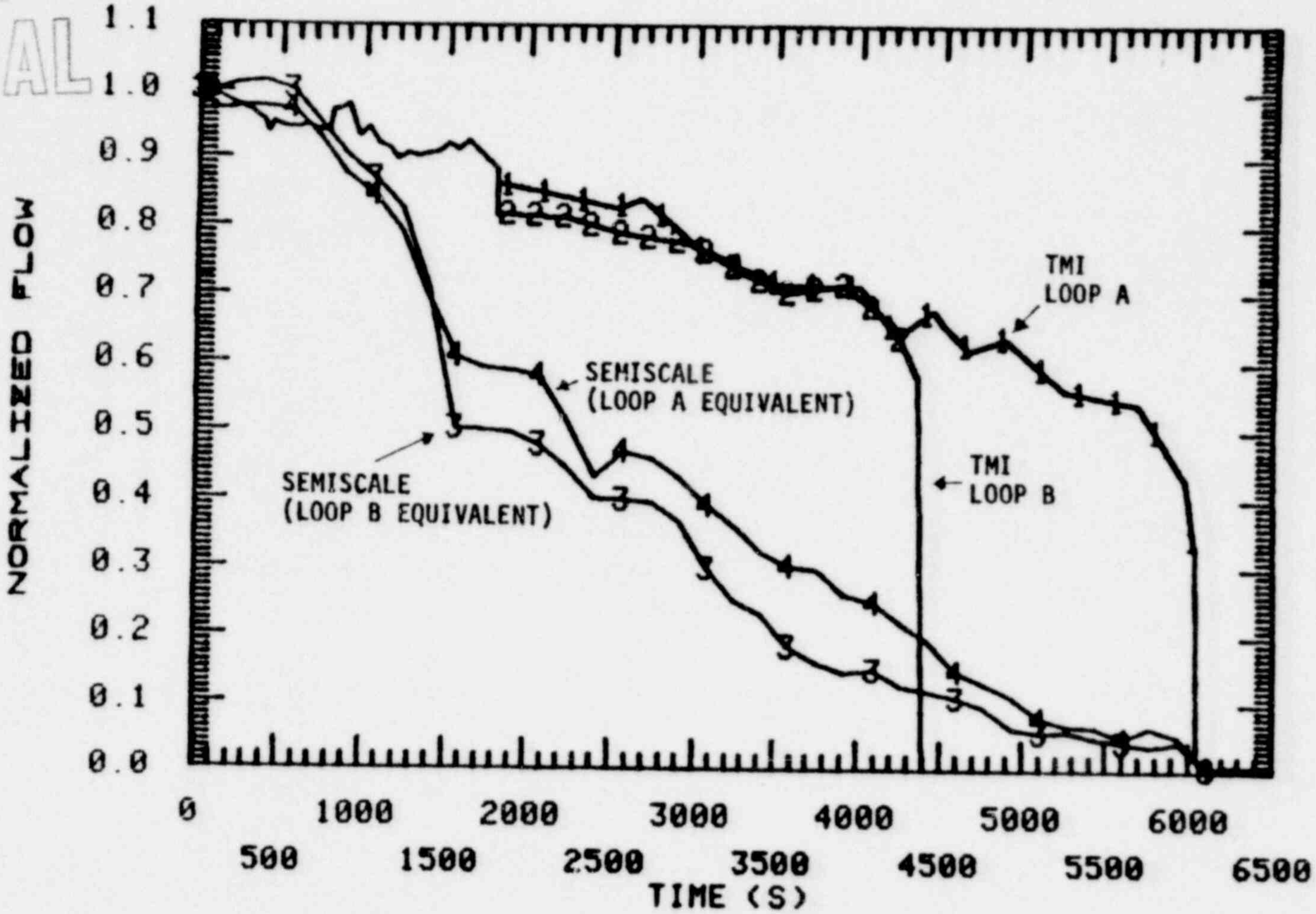
INFLUENCE:

- LEAKAGE
- TWO-PHASE DEGRADATION

TREATMENT:

- PUMP REPLACEMENT, REDUCE LEAKAGE, PUMP LEAKAGE BACK INTO SYSTEM
- QUANTIFY PUMP PERFORMANCE

COMPARISON OF NORMALIZED LOOP FLOWS FOR TMI AND THE SEMISCALE SIMULATION (TEST S-TMI-3I)



CRITICAL FLOW

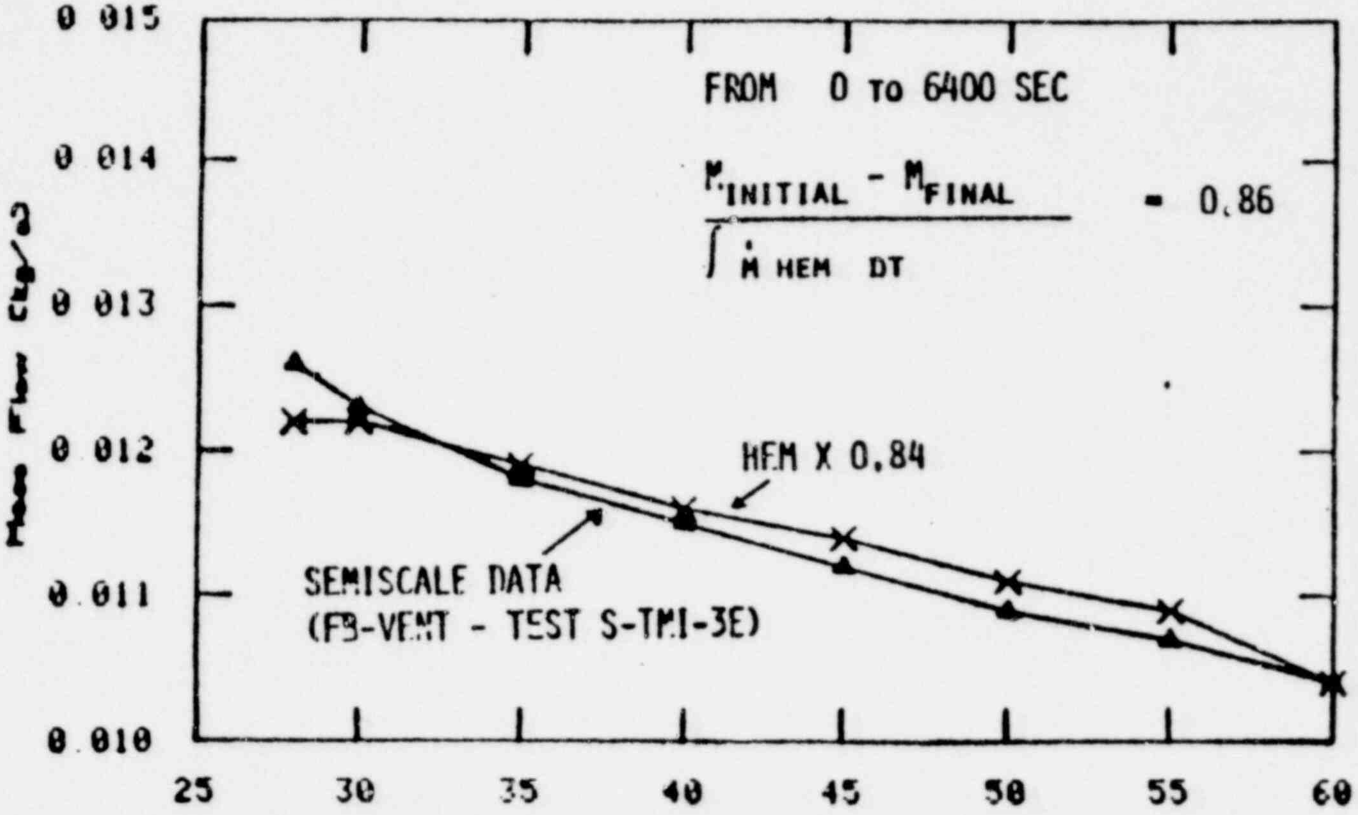
INFLUENCE:

- SMALL SIZE OF SEMISCALE ORIFICE
- BOUNDARY LAYER EFFECTS

TREATMENT:

- RELATION OF MODELS TO RESULTS FROM PREVIOUS TESTS
- TESTS TO BE CONDUCTED IN LOFT TECHNICAL SUPPORT FACILITY

COMPARISON OF POV FLOW FROM SEMISCALE TEST S-TMI-3E AND
FLOW PREDICTED FROM HOMOGENEOUS EQUILIBRIUM MODEL



STEAM GENERATORS

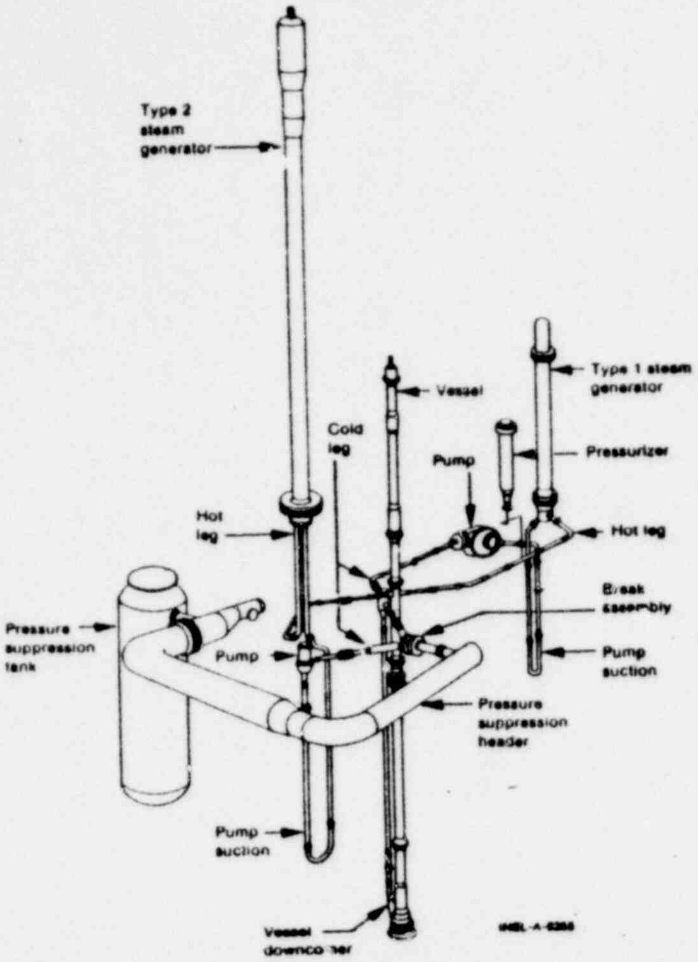
INFLUENCE:

- OVERSIZED SECONDARY
- ELEVATION

TREATMENT:

- LONG TERM - NEW STEAM GENERATOR, REDUCE SECONDARY VOLUME
- NEAR TERM - REDUCE SECONDARY LEVEL
  DRAIN SECONDARY
  QUANTIFY ELEVATION INFLUENCE
  DIMENSIONLESS NUMBER

SEMISCALE STEAM GENERATORS



	INTACT LOOP	PWR THREE LOOPS	BROKEN LOOP	PWR ONE LOOP
$\frac{\text{SECONDARY WATER VOLUME}}{\text{PRIMARY VOLUME}}$	0.62	0.43	1.79	0.14
$\frac{\text{SURFACE AREA}}{\text{PRIMARY VOLUME}}$	13.05	12.37	3.47	4.12
HEIGHT (M)*	3.5	11.1	11.1	11.1

* COLD LEG CENTERLINE TO LOW TUBE SPILLOVER

DIMENSIONALITY

INFLUENCE:

- 1-D VERSUS POTENTIAL 2-3D INFLUENCE

TREATMENT:

- ANALYTICAL

SUMMARY

- VOLUME SCALING MOST APPROPRIATE
- DISTORTIONS ARE PRESENT IN SYSTEM AS IN ALL SYSTEMS
- DISTORTIONS ARE MANAGEABLE FROM CODE STANDPOINT
- DATA WILL BE USEFUL FOR CODE BENCHMARKING / LOFT PHENOMENA IDENTIFICATION / QUANTIFICATION OF DISTORTIONS
- SMALL BREAK DISTORTIONS NOT FULLY QUANTIFIED - FUTURE WORK REQUIRED

FUTURE SCALING WORK

- JOINT SMALL BREAK SCALING ANALYSIS BETWEEN LOFT, SEMISCALE, AND AUSTRIAN GOVERNMENT
- IDENTIFICATION AND ANALYSIS OF SCALING CONCERNS FOR BOTH ANTICIPATED TRANSIENT WITHOUT SCRAM AND LOSS-OF-FEEDWATER EXPERIMENTS
- DOCUMENTATION OF SCALING EFFORTS
- ASSESSMENT OF THE EFFECTS OF DIMENSIONALITY

Rosztoczy
8/28/79

LOFT & SEMISCALE EXPERIMENTAL DATA NEEDS

TO BE PRESENTED AT
ACRS ECCS SUBCOMMITTEE MEETING

AUGUST 28, 1979

BY ZOLTAN R. ROSZTOCZY

UPPER HEAD INJECTION (UHI)

- NRR COMPLETED THE REVIEW OF UHI SYSTEMS IN 1977.
- CONCLUSION OF REVIEW: THE AVAILABLE CALCULATIONAL TECHNIQUES ARE NOT SUFFICIENT TO QUANTIFY THE PROBABLE BENEFITS OR DRAWBACKS OF ADVANCED ECCS, LIKE UHI. WE DO NOT KNOW WHETHER UHI REPRESENTS AN IMPROVEMENT IN TERMS OF PUBLIC SAFETY.
- BETTER, AND MORE COMPLETE EXPERIMENTAL INFORMATION IS ESSENTIAL:
  (1) FOR THE UNDERSTANDING OF UHI PERFORMANCE

  (2) FOR THE DEVELOPMENT AND VERIFICATION OF APPROPRIATE CALCULATIONAL TECHNIQUES.
- NRR, THEREFORE, REQUESTED A TEST PROGRAM TO DEMONSTRATE AND EVALUATE UHI PERFORMANCE (MEMO FROM E.G. CASE TO S. LEVINE, DATED NOV. 8, 1976).
- THIS IS STILL AN OUTSTANDING REQUEST. THE DEVELOPMENT OF ADVANCED ECCS IS A RATHER IMPORTANT ISSUE. THE TEST PROGRAM SHOULD START AS SOON AS POSSIBLE.

SMALL BREAK LOSS-OF-COOLANT ACCIDENT

- COMPARISON OF CALCULATIONS AND SMALL BREAK INTEGRAL TEST DATA (SEMISCALE TEST S-02-6) INDICATED LARGE UNCERTAINTIES IN THE CALCULATIONS.

- B&W LICENSING SUBMITTAL (SUMMER 1978) SHOWED THAT CALCULATIONS ARE RATHER SENSITIVE TO MODEST CHANGES IN THE MODEL.

- SMALL BREAK CALCULATIONS ALSO INDICATED THAT THE CONSERVATISMS REQUIRED BY APPENDIX K HAVE ONLY A LIMITED EFFECT ON THE CALCULATIONS.

- NRR REQUESTED A SET OF SMALL BREAK SEMISCALE TESTS TO
  - (1) EVALUATE THE UNCERTAINTIES OF SMALL LOCA CALCULATIONS.
  - (2) VALIDATE THE CALCULATIONAL METHODS.

- NRC REQUIRED THAT REACTOR SUPPLIERS PERFORM PRETEST PREDICTION FOR THE FIRST SEMISCALE TEST.

- FIRST TEST WAS COMPLETED IN DECEMBER 1978. FURTHER TESTS ARE OUTSTANDING.

SMALL BREAK LOSS-OF-COOLANT ACCIDENT

- TMI-2 BROUGHT ATTENTION TO THE FACT THAT:
  - (1) VERY SMALL LOCA'S ARE OCCURRING WITH A HIGH FREQUENCY (4 EVENTS IN B&W PLANTS DURING 30 REACTOR YEARS OF OPERATION).
  - (2) PLANT RESPONSE TO VERY SMALL BREAKS DIFFERS FROM LARGER BREAK AND REQUIRES DIFFERENT OPERATOR ACTION (NATURAL CIRCULATION, OVERPRESSURIZATION, HIGH PRESSURIZER LEVEL).
  - (3) CALCULATIONAL TECHNIQUES MUST BE REVISED TO HANDLE VERY SMALL BREAKS.
  - (4) THERE IS INSUFFICIENT EXPERIMENTAL INFORMATION TO VALIDATE THE CALCULATIONS.
- NRR REQUESTED DEMONSTRATION TESTS FOR
  - (1) THE ASSESSMENT OF PLANT BEHAVIOR FOLLOWING SMALL BREAKS.
  - (2) THE VALIDATION OF CALCULATIONAL METHODS.
  - (3) THE EVALUATION OF PLANT RECOVERY PROCEDURES.
- NRC IS REQUIRING REACTOR SUPPLIERS TO PROVIDE PRETEST PREDICTIONS FOR SELECTED SMALL BREAK LOCA TESTS (NUREG-0578).
- THE OUTSTANDING SMALL BREAK TEST REQUESTS ARE HIGH PRIORITY ITEMS AND SHOULD PROGRESS AS FAST AS PRACTICABLE.

SEMISCALE TEST S-07-6

- TEST RESULTS INDICATED MULTIPLE VOIDING OF THE DOWNCOMER REPEATED WITH A CONSTANT FREQUENCY. RESULT: A 400 SEC. DELAY IN CORE QUENCHING.
- CALCULATIONS PERFORMED WITH RELAP AND 1-D TRAC WERE UNABLE TO REPRODUCE THE OBSERVED PHENOMENON.
- NRR REQUESTED EXPERIMENTAL AND ANALYTICAL STUDIES TO DECIDE WHETHER THE PHENOMENON IS TYPICAL FOR PWR'S AND WHETHER IT IS SIGNIFICANT FOR LICENSING.
- PRESENT STATUS: (1) OBSERVED PHENOMENON IS REAL, IT CAN OCCUR IN LARGE SYSTEMS (PWR) AS WELL AS IN SMALL SYSTEMS (SEMISCALE); (2) LICENSING CODES AND RELAP ARE NOT APPROPRIATE TO HANDLE THIS PROBLEM; (3) PRELIMINARY 3-D TRAC CALCULATIONS SHOW DOWNCOMER VOIDING IN PWR'S DURING LOCA; AND (4) THE RELAP ANALYSIS INDICATES THE POSSIBILITY OF SUBCOOLED WATER BEING BYPASSED IN THE TESTS.
- AN UPDATE TO THE BOARD NOTIFICATION WAS ISSUED ON MAY 30, 1979 INDICATING NRC CONCERN.
- PWR VENDORS WERE REQUESTED EITHER TO SHOW THAT THIS PHENOMENON WILL NOT AFFECT THE SAFETY EVALUATION OF THEIR DESIGNS OR MODIFY THEIR EVALUATION MODEL TO ACCOUNT FOR THE PHENOMENON.
- DEVELOPMENT OF AN ANALYTICAL CAPABILITY TO PREDICT THE DOWNCOMER MASS DEPLETION PHENOMENON IS NEEDED. CONFIRMATORY EXPERIMENTS ARE ALSO NEEDED. THIS IS A HIGH PRIORITY ITEM.

TRANSIENTS & ACCIDENTS

- BWR TRANSIENT TESTS ON A NUCLEAR PLANT (PEACH BOTTOM) SHOWED LARGE ERRORS IN THE CALCULATIONAL TECHNIQUE USED FOR LICENSING.
- NRR ISSUED A BOARD NOTIFICATION REQUEST EXPRESSING CONCERN.
- CHALLENGING TEST RESULTS ON PWR TRANSIENTS ARE NOT AVAILABLE.
- BOTH BWR & PWR TRANSIENT ANALYSIS METHODS NEED IMPROVEMENT AND VERIFICATION.
- NRC IS CONSIDERING TO REQUIRE TRANSIENT TESTS ON TYPICAL BWR'S AND ON TYPICAL PWR'S.
- EPRI HAS ASKED FOR TRANSIENT TESTS ON LOFT.
- DEVELOPMENT OF A BALANCED TRANSIENT TEST PROGRAM UTILIZING NUCLEAR PLANTS AS WELL AS TEST APPARATUS (POSSIBLY LOFT) IS IMPORTANT.
- SAFETY ANALYSIS METHODS USED FOR STEAM LINE BREAK AND FEEDLINE BREAK ANALYSIS ARE PRESENTLY UNDER NRC REVIEW.
- ANALYSIS METHODS USED FOR STEAM GENERATOR TUBE RUPTURE HAVE NOT YET BEEN REVIEWED BY NRC.
- EXPERIMENTAL VERIFICATION OF THESE METHODS IS RATHER LIMITED.
- IMPORTANT ASPECTS OF ACCIDENT ANALYSES REQUIRING EXPERIMENTAL SUPPORT ARE:

  STEAM GENERATOR DYNAMICS

  MOISTURE CARRY-OUT

  REALISTIC PLANT DYNAMICS

8/27/79
Broadus

BEACON DEVELOPMENT PROGRAM

C. R. BROADUS
ACRS MEETING
AUGUST 27, 1979

EG&G Idaho Inc.

1004 165

BEACON

PURPOSE:

- TO PROVIDE A BEST-ESTIMATE CONTAINMENT ANALYSIS CAPABILITY TO PREDICT THE ACTUAL TRANSIENT.

LICENSING PHILOSOPHY

USE CONSERVATIVE CODES (HIGH P, T) WITH CONSERVATIVE INPUT

- DO NOT KNOW THE DEGREE OF CONSERVATISM
- MAY COVER UP IMPORTANT PHENOMENA/PROBLEMS
- HIGH PRESSURES AND TEMPERATURES ARE NOT CONSERVATIVE FOR SOME ANALYSES

BEACON PHILOSOPHY

USE BEST ESTIMATE CODES TO PREDICT THE ACTUAL TRANSIENT

- COMPARISON WITH LICENSING ANALYSES TO DETERMINE THE DEGREE OF CONSERVATISM
- PREDICTION OF PHENOMENA WHICH MAY BE COVERED UP BY HOMOGENEOUS, EQUILIBRIUM CODES
- MAY BE USED FOR EUROPEAN LICENSING PHILOSOPHY (B.E. ± SAFETY FACTOR)
- MAY BE USED AS A GENERAL LICENSING TOOL TO GET A HANDLE ON NEW PROBLEMS.

BEACON METHODS

- BUILD FROM EXISTING CODES AND MODELS WHERE POSSIBLE:
  - LASL KFIX NUMERICAL SCHEME (2-D, 2-PHASE, UNEQUAL VELOCITIES, UNEQUAL TEMPERATURES)
  - INEL HEAT 1 CONDUCTION SUBCODE
  - LASL/BNL WATER PROPERTIES
  - INEL DYNAMIC STORAGE ROUTINES
  - INEL INPUT AND PLOT PACKAGES

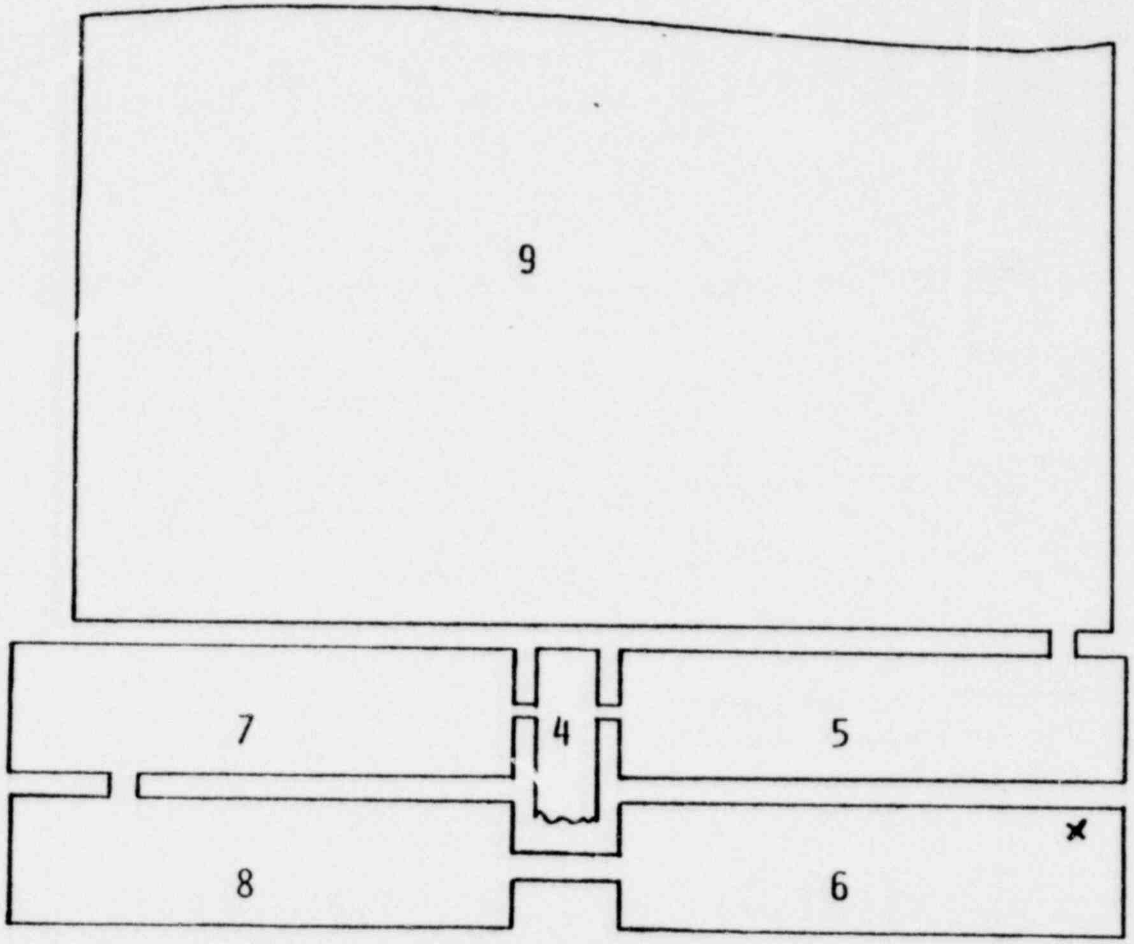
BEACON METHODS (CONTD . . .)

- DEVELOP NEW MODELS WHERE NECESSARY
  - AIR COMPONENT ADDED
  - MASS, MOMENTUM, ENERGY SOURCE
  - WALL FILM MODEL
  - HEAT TRANSFER CORRELATIONS
  - MESH COUPLING
  - VARIABLE MESH SPACING
  - PARTIAL FLOW BLOCKAGE
  - LUMPED PARAMETER MODEL
  - RESTART
- MODULAR IMPLEMENTATION

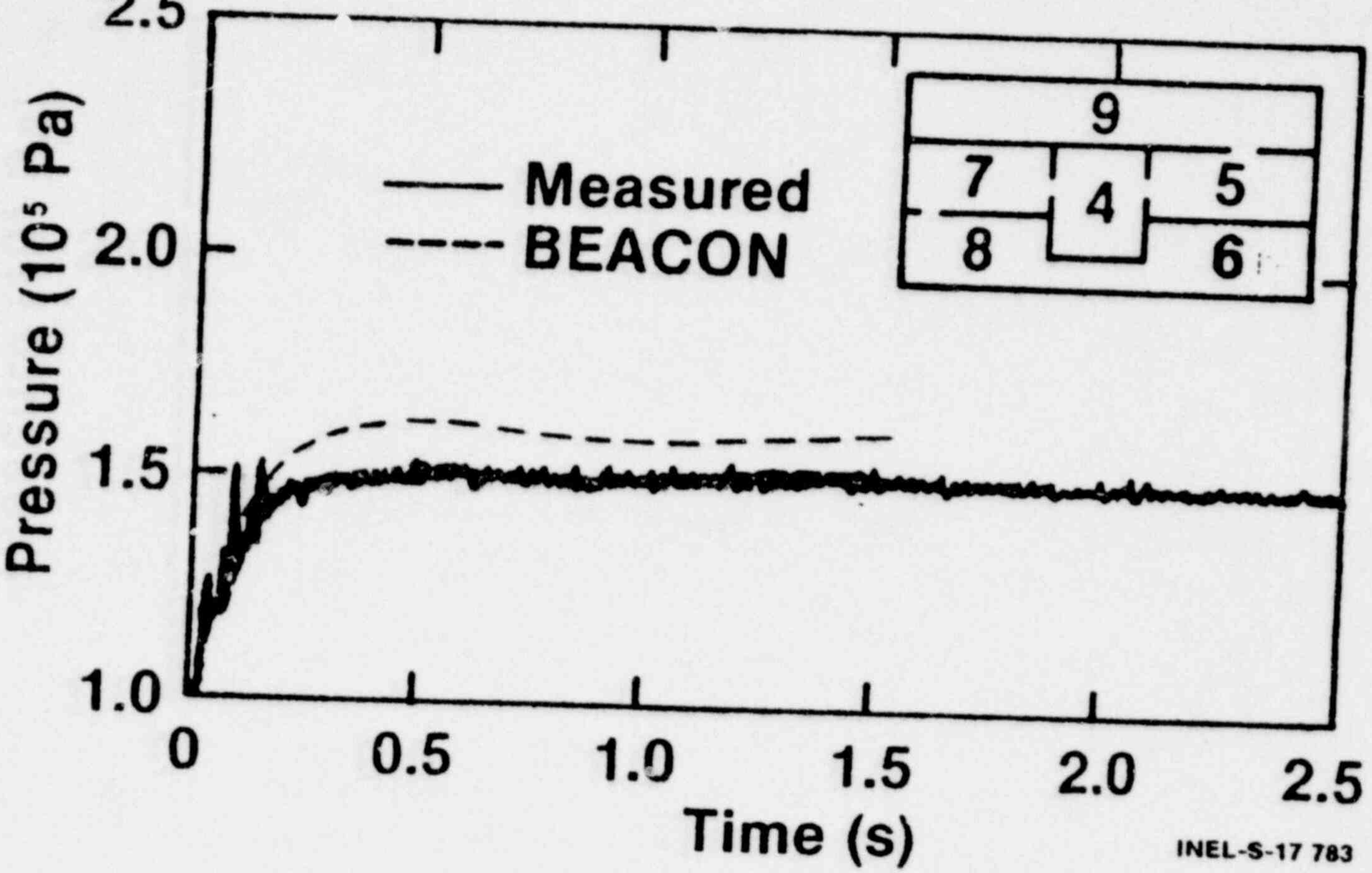
BEACON/MOD2A

- TWO - DIMENSIONAL
- NONEQUILIBRIUM
- TWO - COMPONENT (AIR AND WATER)
- TWO - PHASE (AIR/VAPOR AND LIQUID)
- COMPLEX GEOMETRIC MODELING
- SHORT TO INTERMEDIATE TERM
- HEAT TRANSFER
- WALL FILM

BATTELLE - FRANKFURT D-15 TEST FACILITY



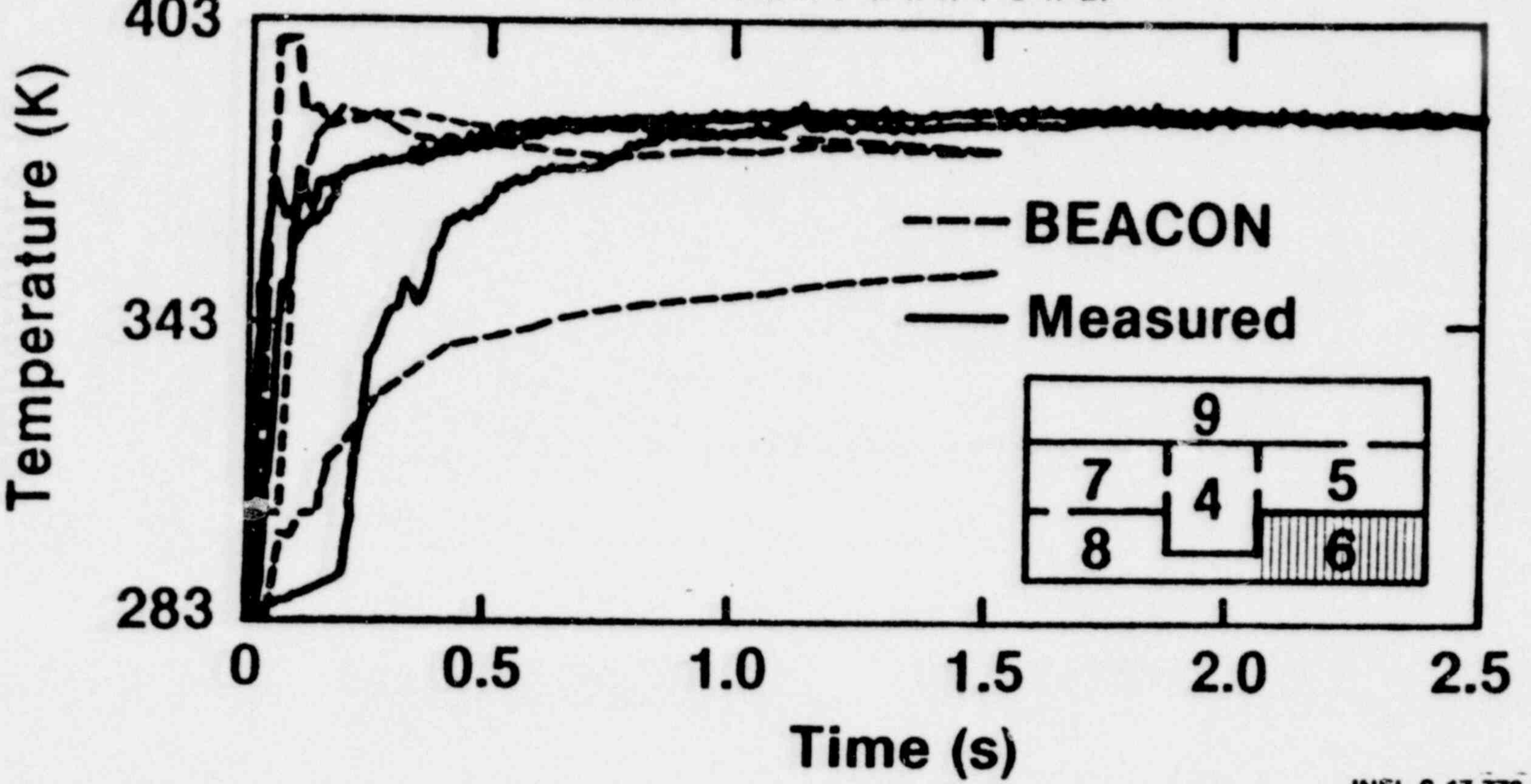
Pressure in Room 6



INEL-S-17 783

Temperature in Room 6

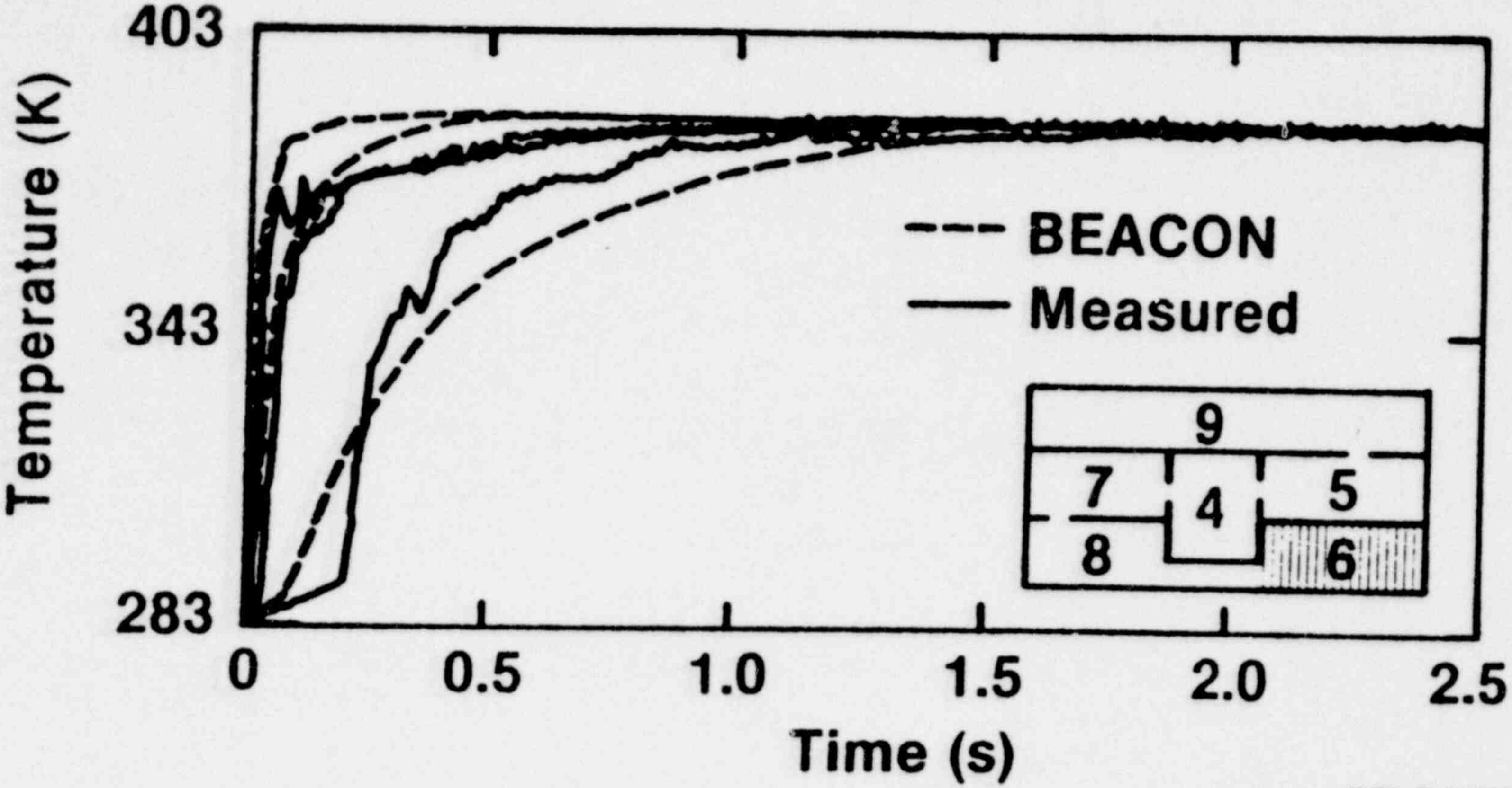
(GAS TEMPERATURE)



INEL-S-17 778

Temperature in Room 6

(SATURATION TEMPERATURE)



INEL-S-17 779

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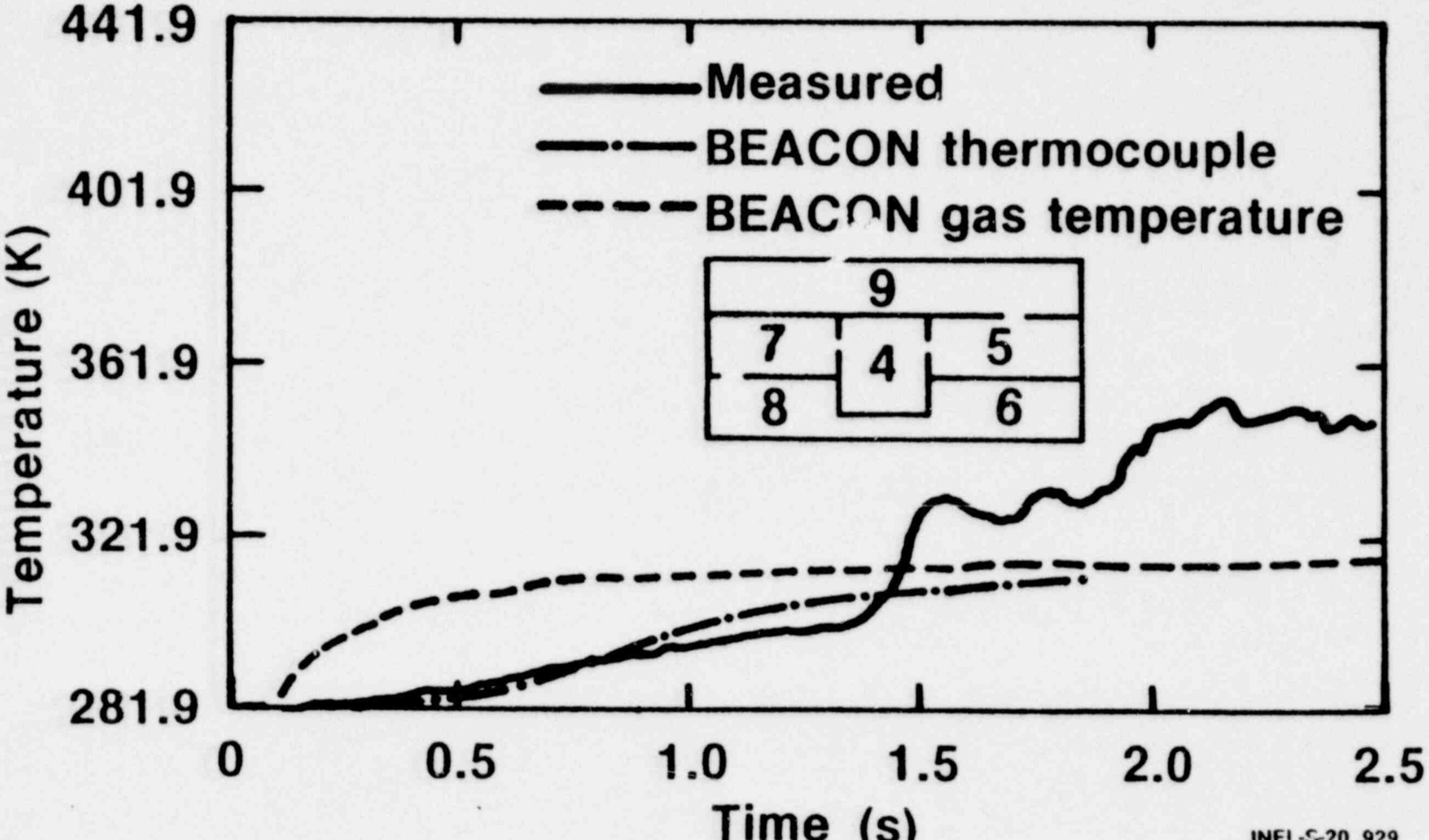
Temperature in Room 7
Thermocouple Comparison



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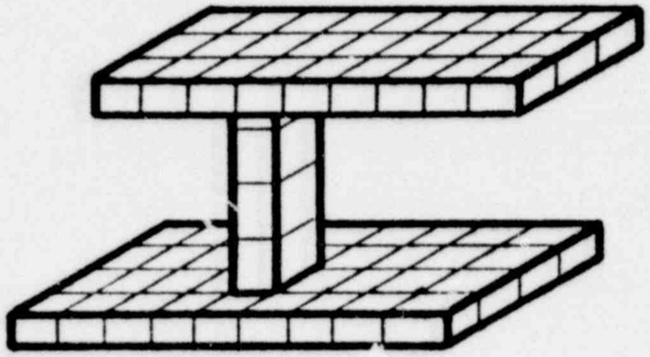
CURRENT WORK

BEACON/MOD3 - NEW MODELS

- BEST ESTIMATE CORRELATIONS PACKAGE
- FORM AND FRICTION LOSS
- OUT - OF - PLANE COUPLING



DEVELOPMENTAL ASSESSMENT PROGRAM

PURPOSE: DEFINE THE RANGE OF CONDITIONS/PROBLEMS TO WHICH BEACON IS APPLICABLE

METHODS: COMPARE TO DATA IN FOUR PHASES:

1. PROBLEM SETUP AND DOCUMENTATION
2. "BLIND" RUN
3. DATA COMPARISON
4. INPUT MODIFICATIONS WITHIN BEST-ESTIMATE INPUT LIMITS

PROBLEMS: SEPARATE EFFECTS PROBLEMS

- ENTRAINMENT/DEENTRAINMENT (DREXEL)
- 2-D SEPARATED FLOW (LAHEY)
- ETC.,

CONTAINMENT PROBLEMS

- BATTELLE - FRANKFURT C & D SERIES TESTS
- CVTR TEST
- ETC.,

8/28/79

Sahota

BEACON/MOD3
INTERPHASIC EXCHANGE RATES

M. S. SAHOTA
ACRS MEETING
AUGUST 27, 1979

EG&G Idaho, Inc.

BEACON/MOD3

INTERPHASIC EXCHANGE RATES

UNEQUAL VELOCITIES → INTERPHASIC DRAG

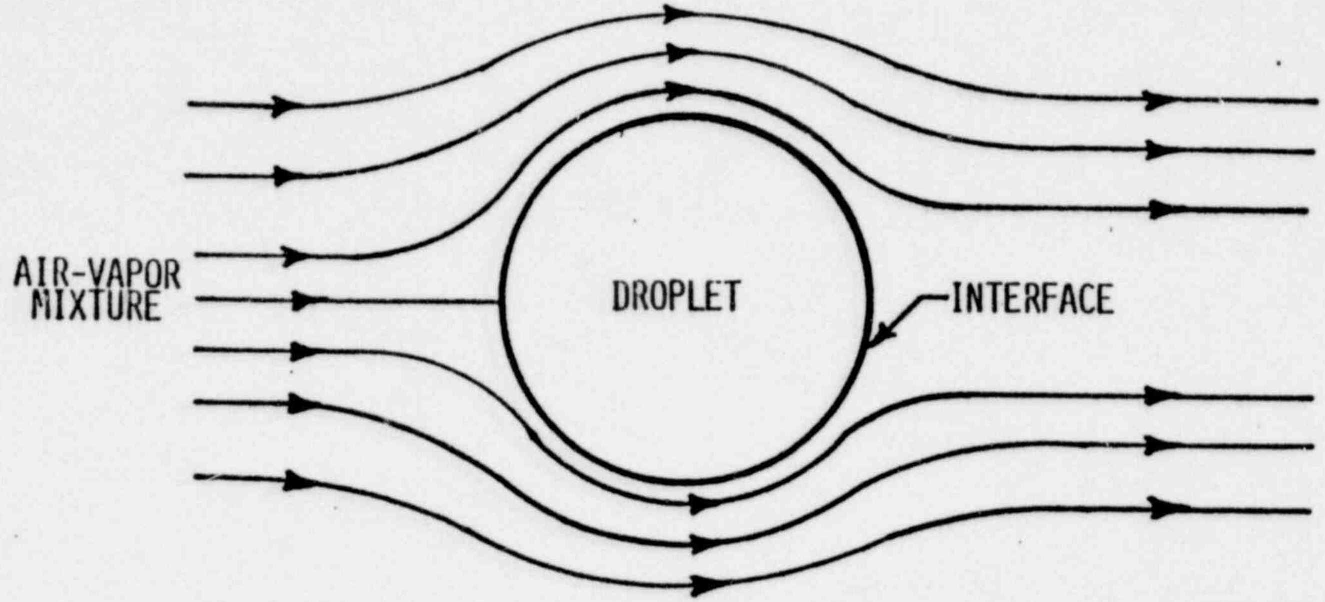
THERMODYNAMIC NONEQUILIBRIUM

→ HEAT AND MASS TRANSFER IN
THE PRESENCE OF INERT GAS

FLOW REGIMES

1. DISPERSED DROPLET FLOW ($\theta \rightarrow 1$)
2. BUBBLY FLOW ($\theta \rightarrow 0$)
3. VOID FRACTION CLOSE TO 0.5
4. OTHER FLOW REGIMES
   - (A) $0.5 \leq \theta \leq 1.0$
   - (B) $0 \leq \theta < 0.5$

1. DISPERSED DROPLET FLOW



(A) EXCHANGE RATES FOR A SINGLE DROPLET

$$\text{DRAG} = C_D \, 1/2 \, \rho_\infty \, U_\infty^2 \, \pi R^2$$

$$C_D = \begin{cases} \frac{24}{R_E} \left(1 + 0.15 \, R_E^{0.687}\right) & ; \quad R_E < 1300 \\ 0.4 \; ; & R_E \geq 1300 \end{cases}$$

MASS BALANCE AT THE INTERFACE GIVES

$$\dot{M} = G_M \frac{W_{VI} - W_{V\infty}}{1 - W_{VI}}$$

ENERGY BALANCE AT THE INTERFACE YIELDS

$$\dot{E} = G_H \; (H_{MI} - H_{M\infty}) + \dot{M} \; H_{MI}$$

$$(L_E = 1)$$

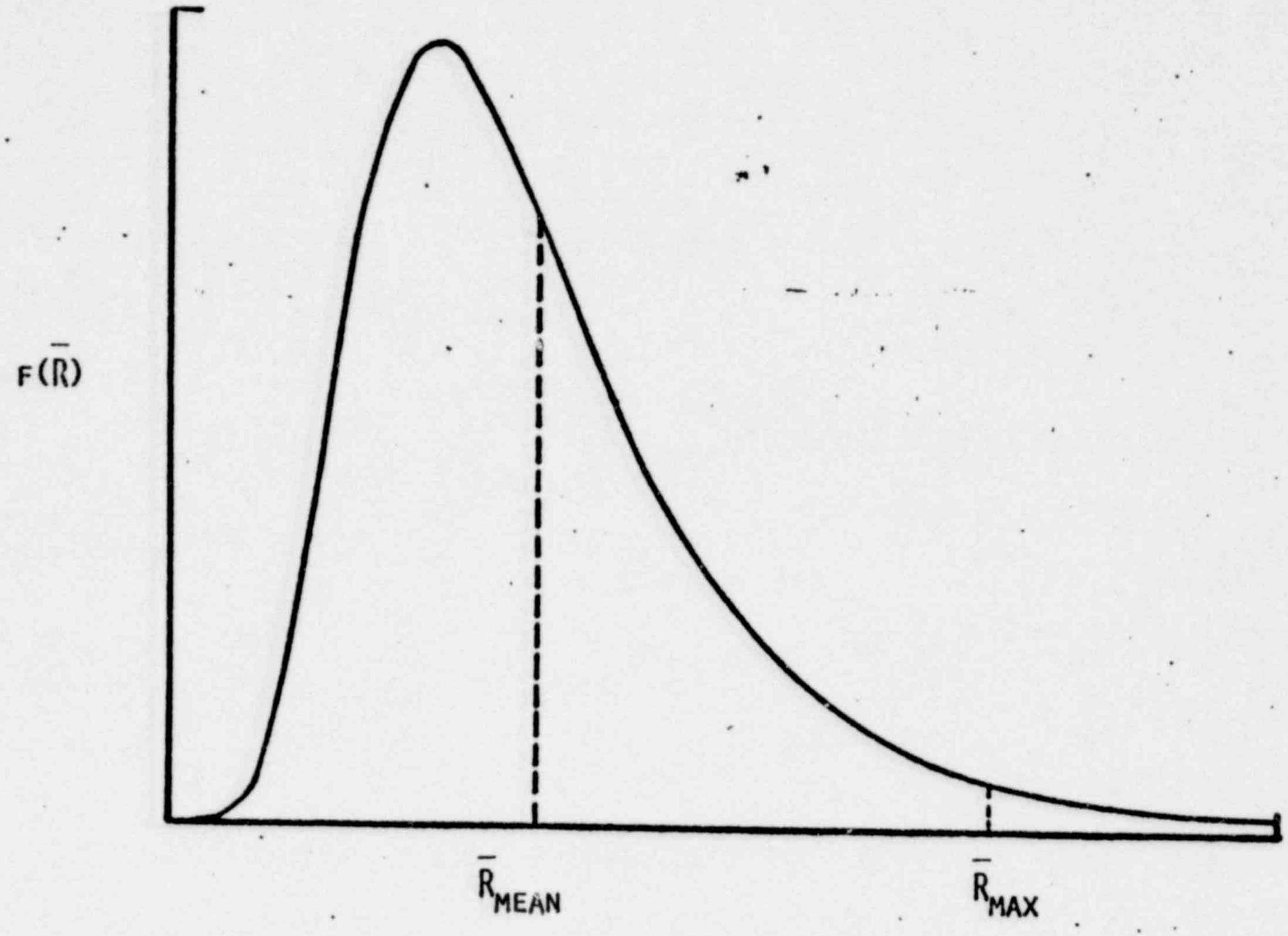
SPECIES EQUATION SOLUTION GIVES

$$\frac{G_M^*}{G_M} = \frac{G_H^*}{G_H} = \frac{1 - W_{VI}}{W_{VI} - W_{V\infty}} \; \mathrm{LN} \left(\frac{1 - W_{V\infty}}{1 - W_{VI}} \right)$$

FLOW PAST A SPHERE

$$Nu = 2.0 + 0.60 \; P_R^{1/3} \; R_E^{1/2}$$

(B) DROPLET SIZE DISTRIBUTION



$$\bar{R} = \frac{R}{R_{MEAN}}$$

(C) MACROSCOPIC EXCHANGE RATES

$$\text{TOTAL DRAG} = \int_0^\infty C_D \, 1/2 \, \rho_\infty \, U_\infty^2 \, \pi R^2 \, . \, N \, f(\bar{R}) \, d\bar{R}$$

$$Nu_{EFF} = 1/A \int_0^\infty Nu \, . \, 4\pi R^2 \, N \, f(\bar{R}) \, d\bar{R}$$

2. BUBBLY FLOW

(A) EXCHANGE RATES FOR A SINGLE BUBBLE

DRAG - SAME APPROACH AS FOR DROPLET

$$\dot{E} = G_\ell (H_\ell - H_{\ell I}) + \dot{M} H_{\ell I} = \dot{M} H_{VI}$$

$$\dot{M} = \frac{G_\ell (H_\ell - H_{\ell I})}{H_{FG}}$$

(B) BUBBLE SIZE DISTRIBUTION

(C) MACROSCOPIC EXCHANGE RATES

} SAME APPROACH AS FOR DROPLETS

3. VOID FRACTION CLOSE TO 0.5

LIQUID ↓ | ↑ AIR-VAPOR MIXTURE

(A) EXCHANGE FLUXES - DIFFUSION LIMITED IN GAS PHASE

$$\tau_I = 1/2 \, f \, \rho_\infty \, U_\infty^2$$

$$ST = \frac{(f/2)^{1/2}}{[5 \, Pr + 5 \ln (5 \, Pr + 1) + (f/2)^{-1/2} - 14]}$$

(B) MACROSCOPIC EXCHANGE RATES

- MULTIPLY BY AREA

4. OTHER FLOW REGIMES

(A) $0.5 \leq \theta \leq 1.0$

VOID FRACTION WEIGHTED DRAG, HEAT AND MASS TRANSFER COEFFICIENTS

(B) $0 \leq \theta < 0.5$

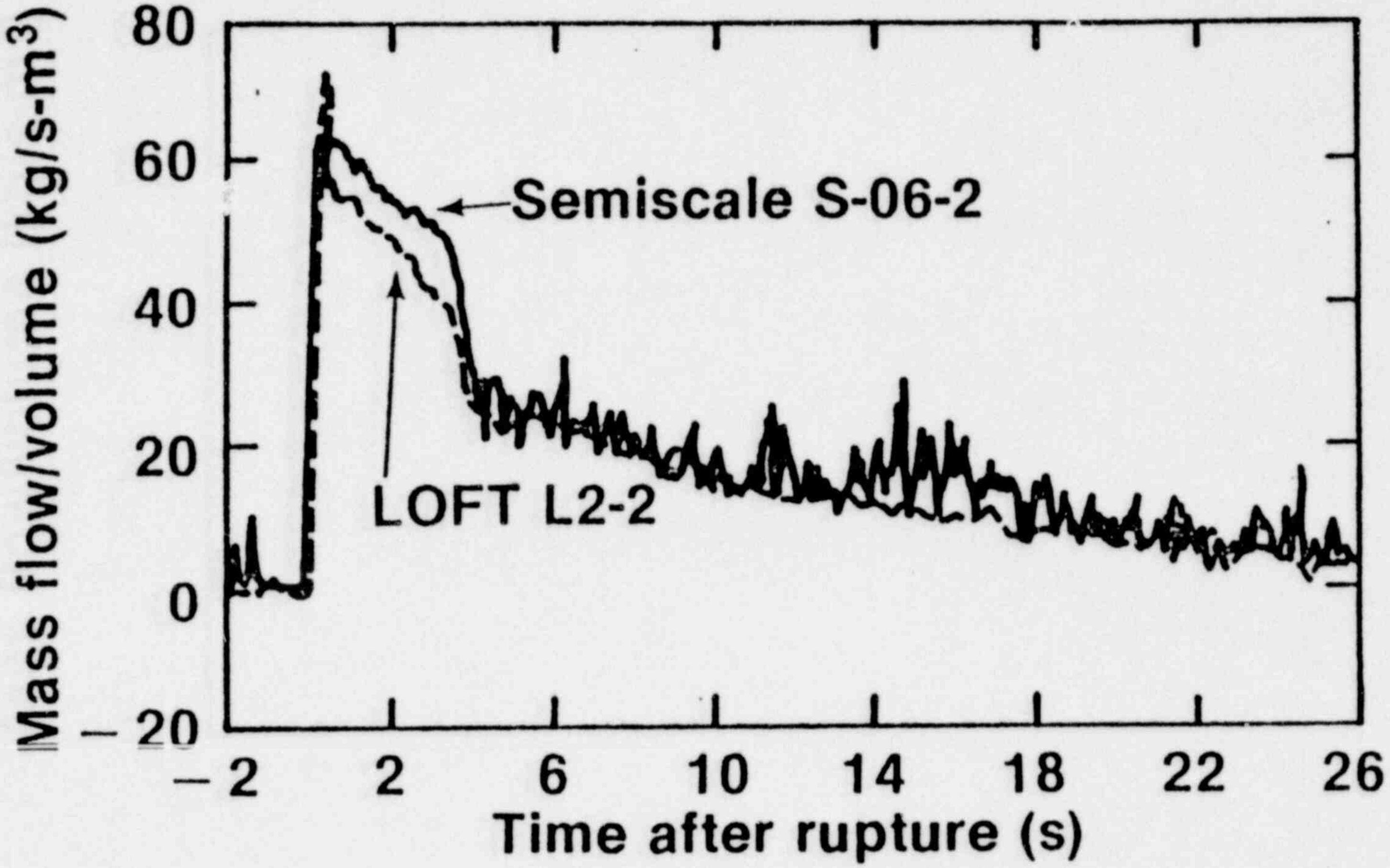
FORCE AND ENERGY BALANCES AT THE INTERFACE

$$\frac{1}{K} = \frac{1}{K_G} + \frac{1}{K_\ell}$$

Supplemental Larson 8/20/79

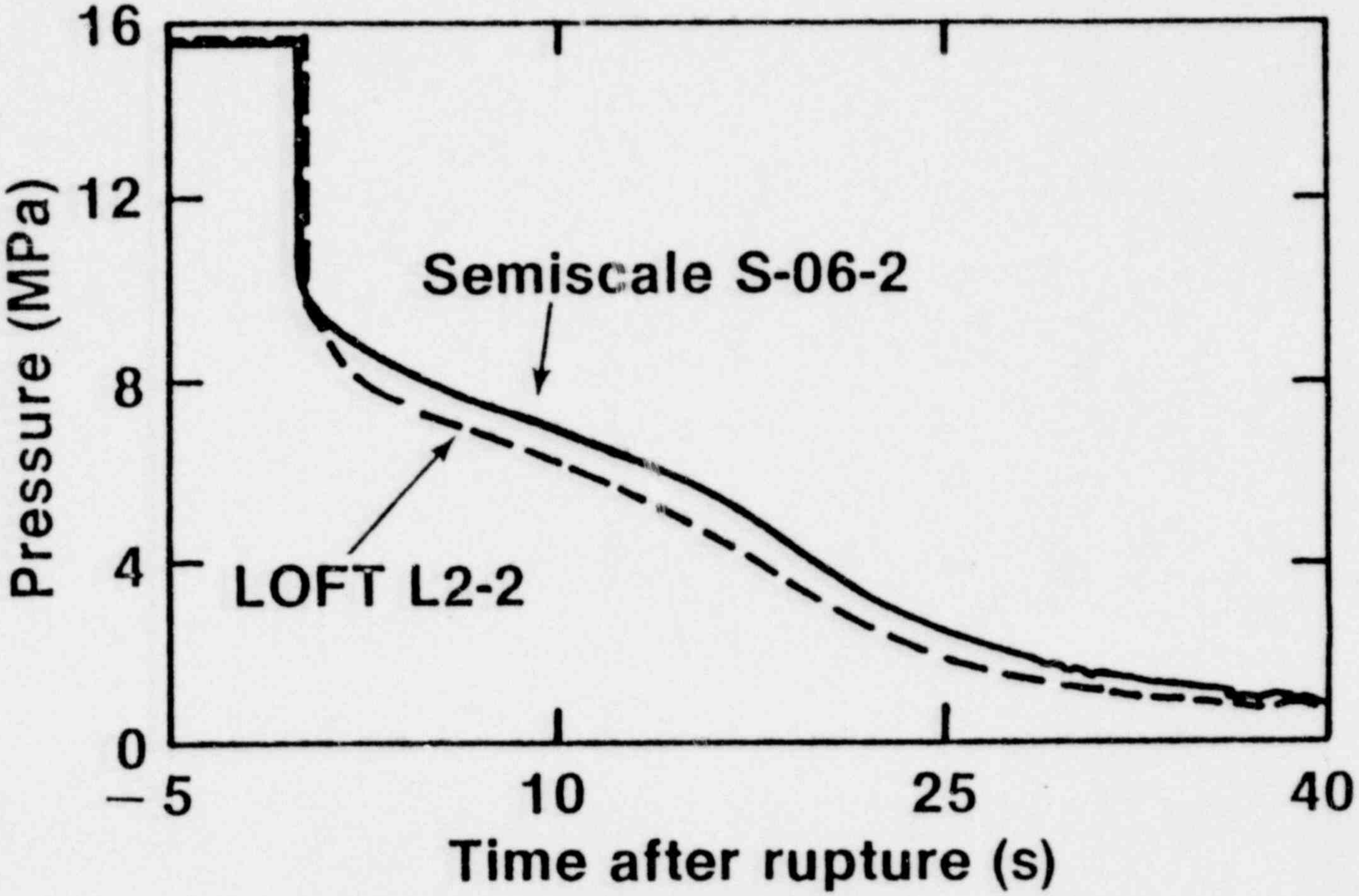
1004 190

Broken Cold Leg Mass Flow (Normalized to System Volume)



INEL-S-20 232

System Pressure



INEL-S-20 231

PRESSURIZER SURGE LINE

INFLUENCE:

- SIZE
- HYDRAULIC RESISTANCE

TREATMENT:

- SCALED RESISTANCE

COMPARISON OF PWR AND SEMISCALE SURGE LINES

	PWR[A]	SEMISCALE[B]
DIAMETER (IN)	8.5	0.37
AREA (FT^2)	3.941×10^{-1}	7.467×10^{-4}
$R'(LBF\text{-}S^2 / LBM\text{-}IN^2\text{-}FT^3)$	8.617×10^3 [C]	6.200×10^3 [D]
D^+	1.333×10^2	5.804
J_G (FT/S) [E]	4.206×10^1	2.15×10^1
$J_{G_{CRITICAL}}$ (FT/S)	1.135×10^1	2.369

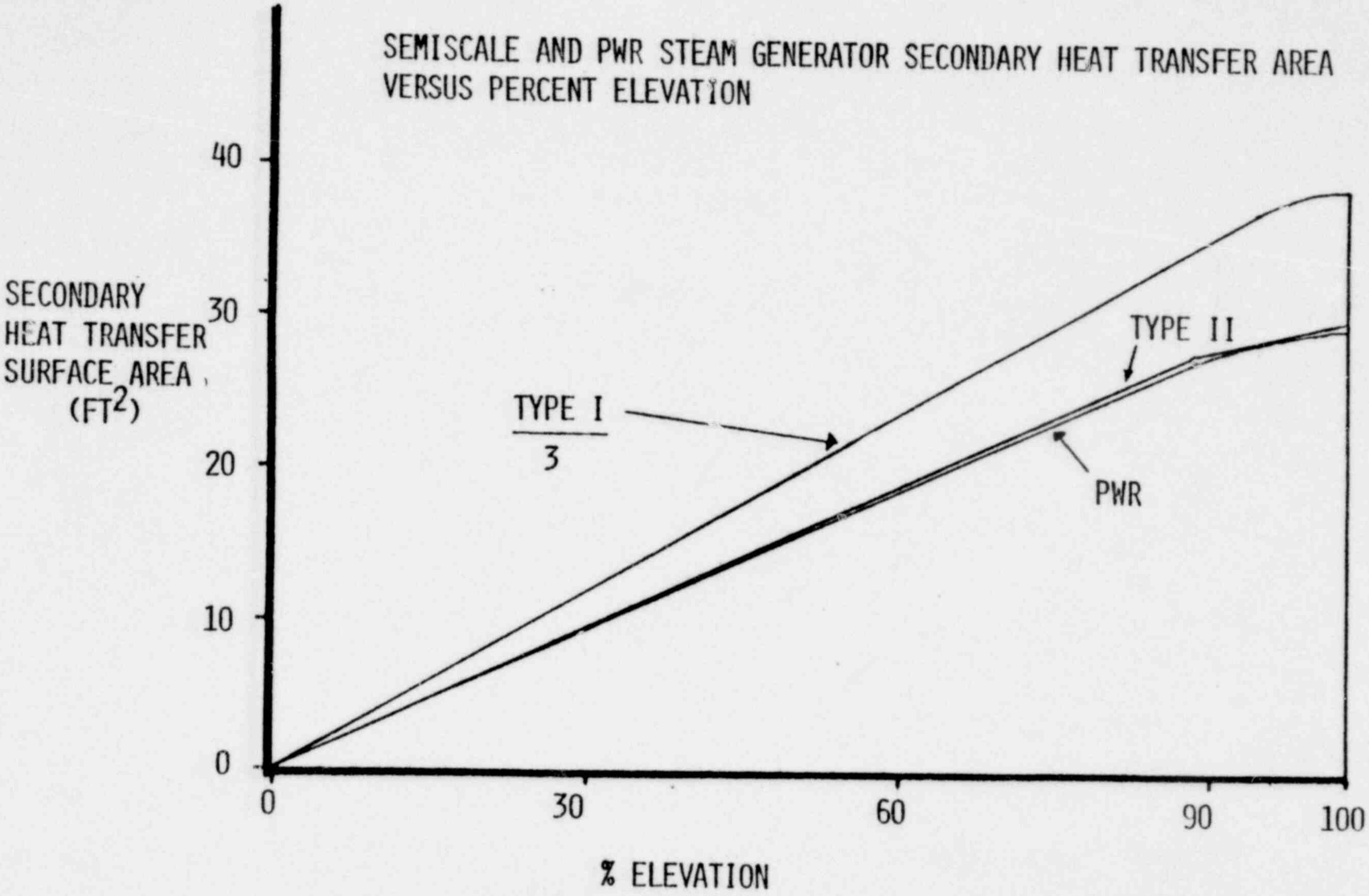
[A] B&W

[B] VALUES LISTED ARE FOR THREE MILE ISLAND SIMULATIONS

[C] SCALED TO SEMISCALE

[D] CALCULATED

[E] FOR CASE OF POV LIQUID DISCHARGE AT PRESSURE OF 1000 PSIA



SEMISCALE AND PWR STEAM GENERATOR SECONDARY HEAT TRANSFER AREA VERSUS PERCENT ELEVATION

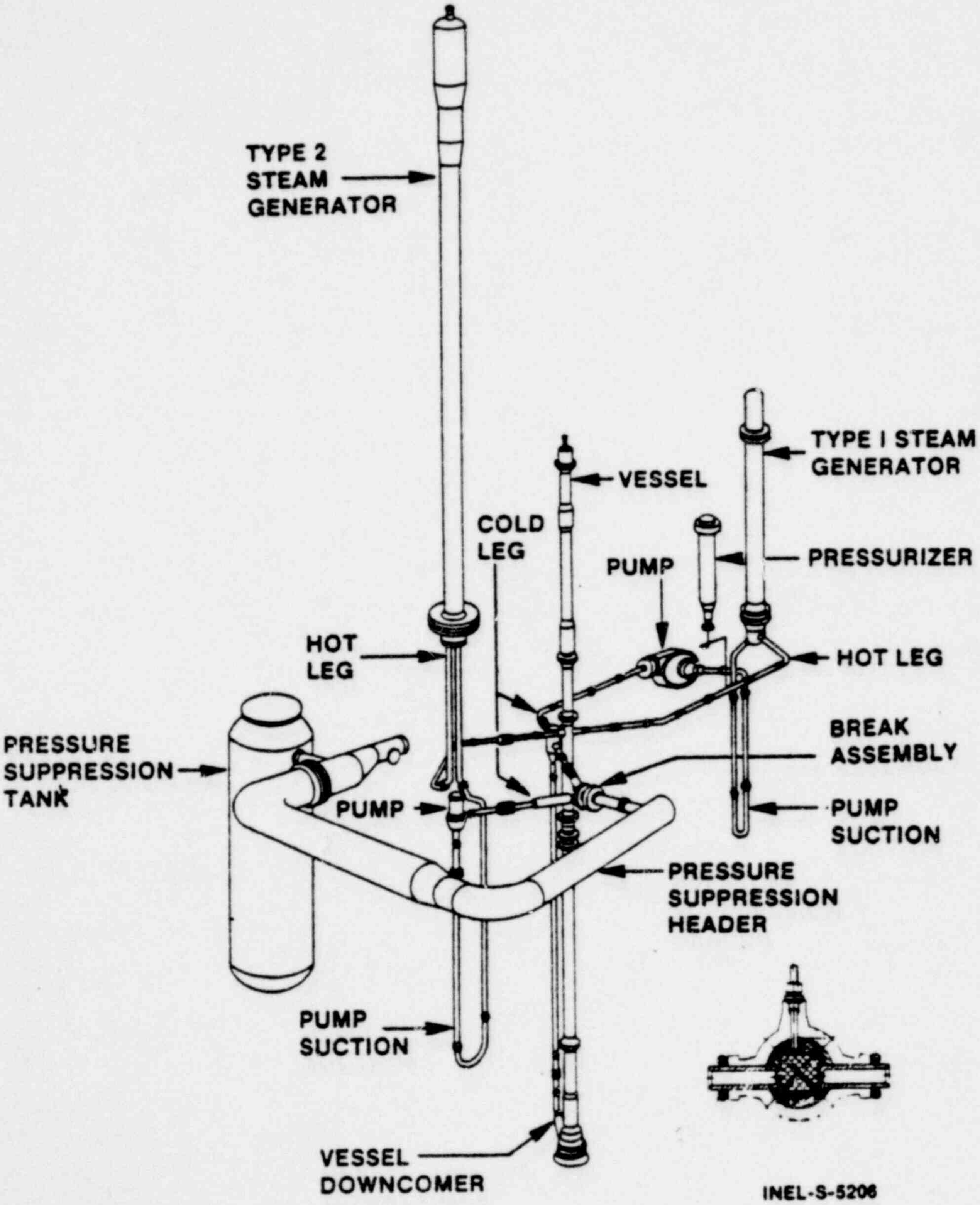
Johnsen
8/28/79

SEMISCALE PROGRAM ACCOMPLISHMENTS

G. W. JOHNSEN
ACRS MEETING
AUGUST 27, 1979

EG&G Idaho, Inc

MOD-3 SEMISCALE COLD LEG BREAK ASSEMBLY

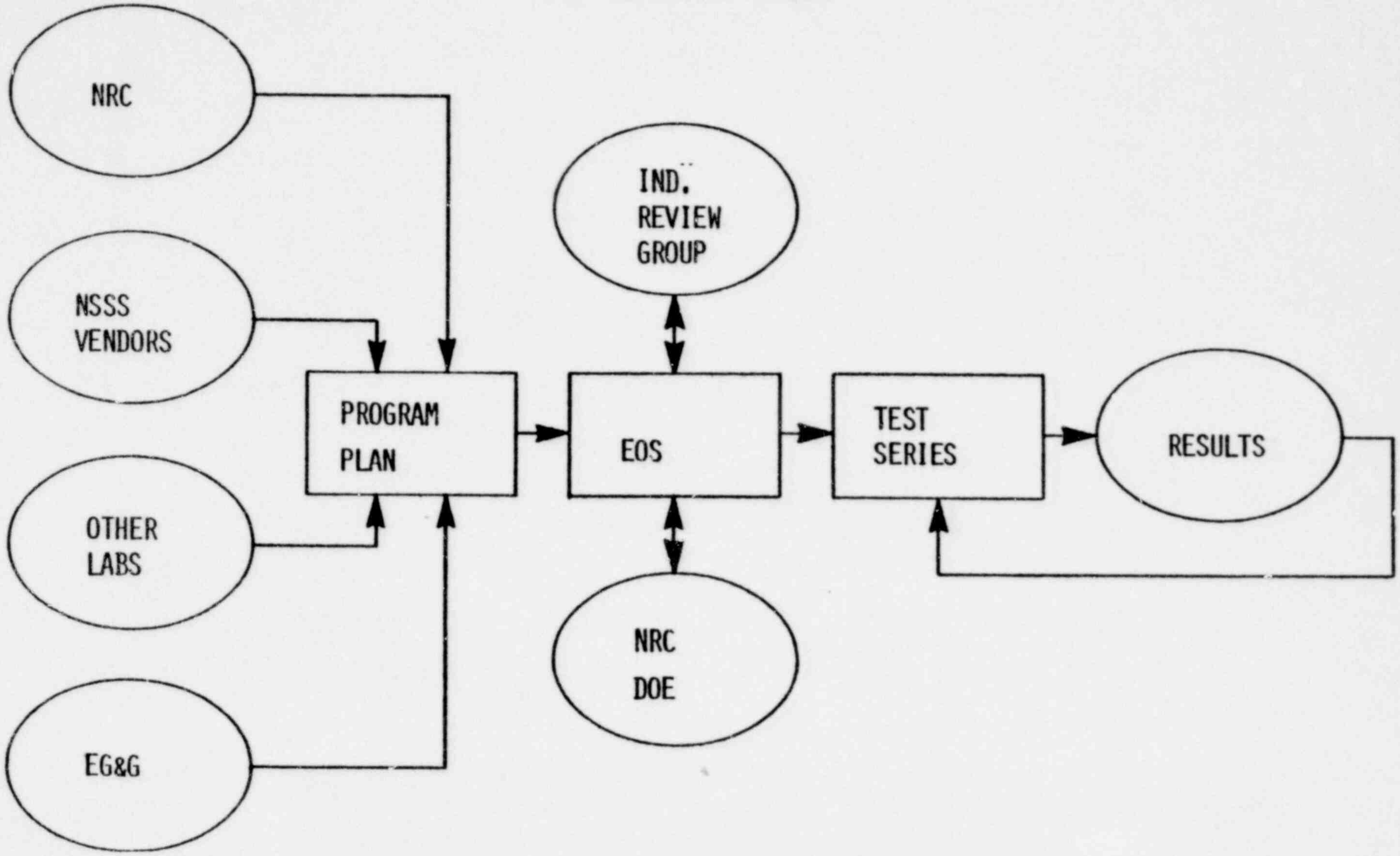


INEL-S-5206

EXPERIMENTS CONDUCTED

- SERIES 7 - MOD-3 BASELINE
- SMALL BREAK - STANDARD PROBLEM
- LOWER PLENUM INJECTION
- TMI SIMULATIONS
- PLANNING TESTS

TEST PLANNING PROCESS



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SEMISCALE'S ROLE IN SAFETY RESEARCH

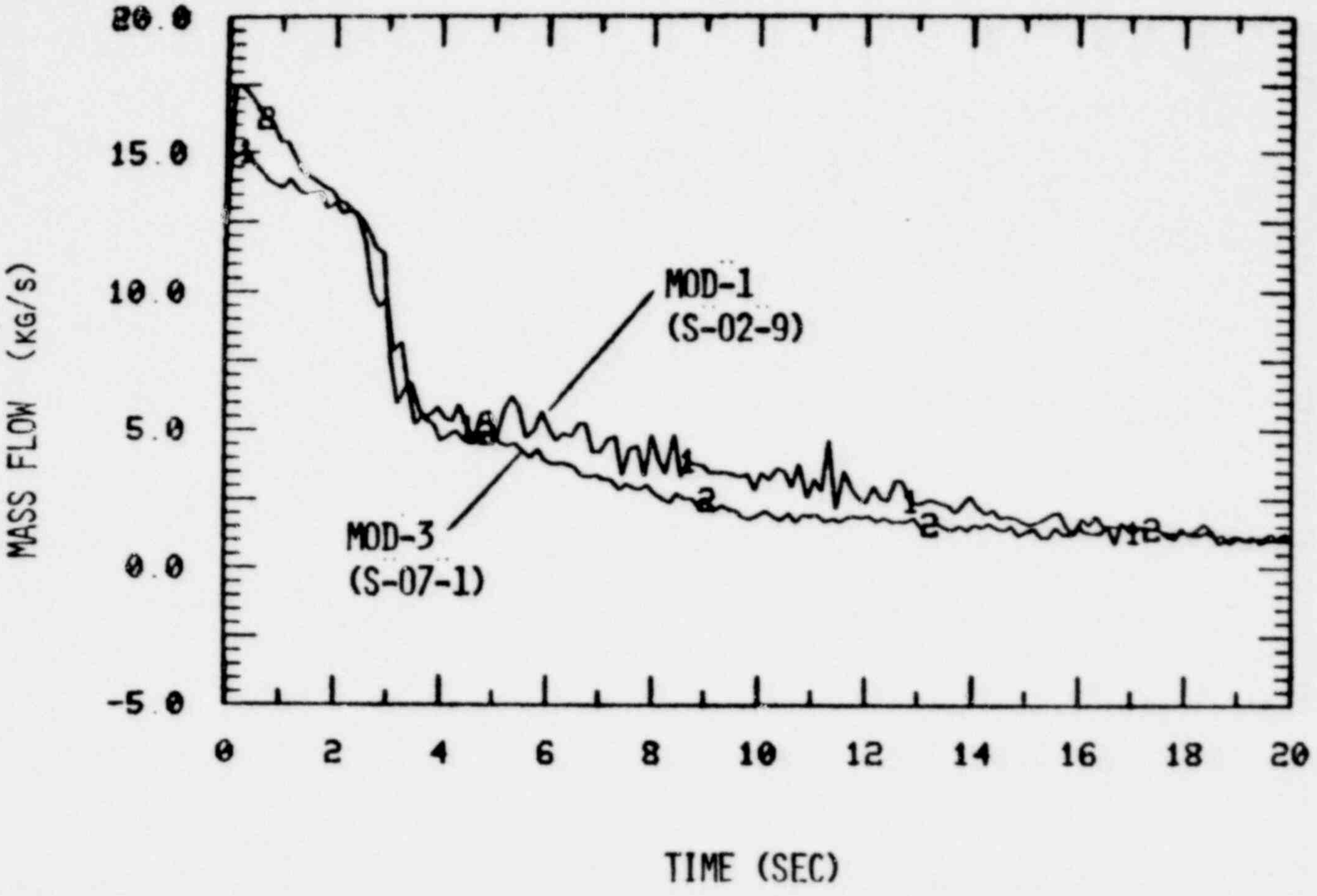
- CODE ASSESSMENT
- BASIC SYSTEM PHENOMENA
- EFFECTS OF SCALE

TEST SERIES 7 RESULTS

- SCALING (MOD-1 VS. MOD-3)
- MASS DEPLETION

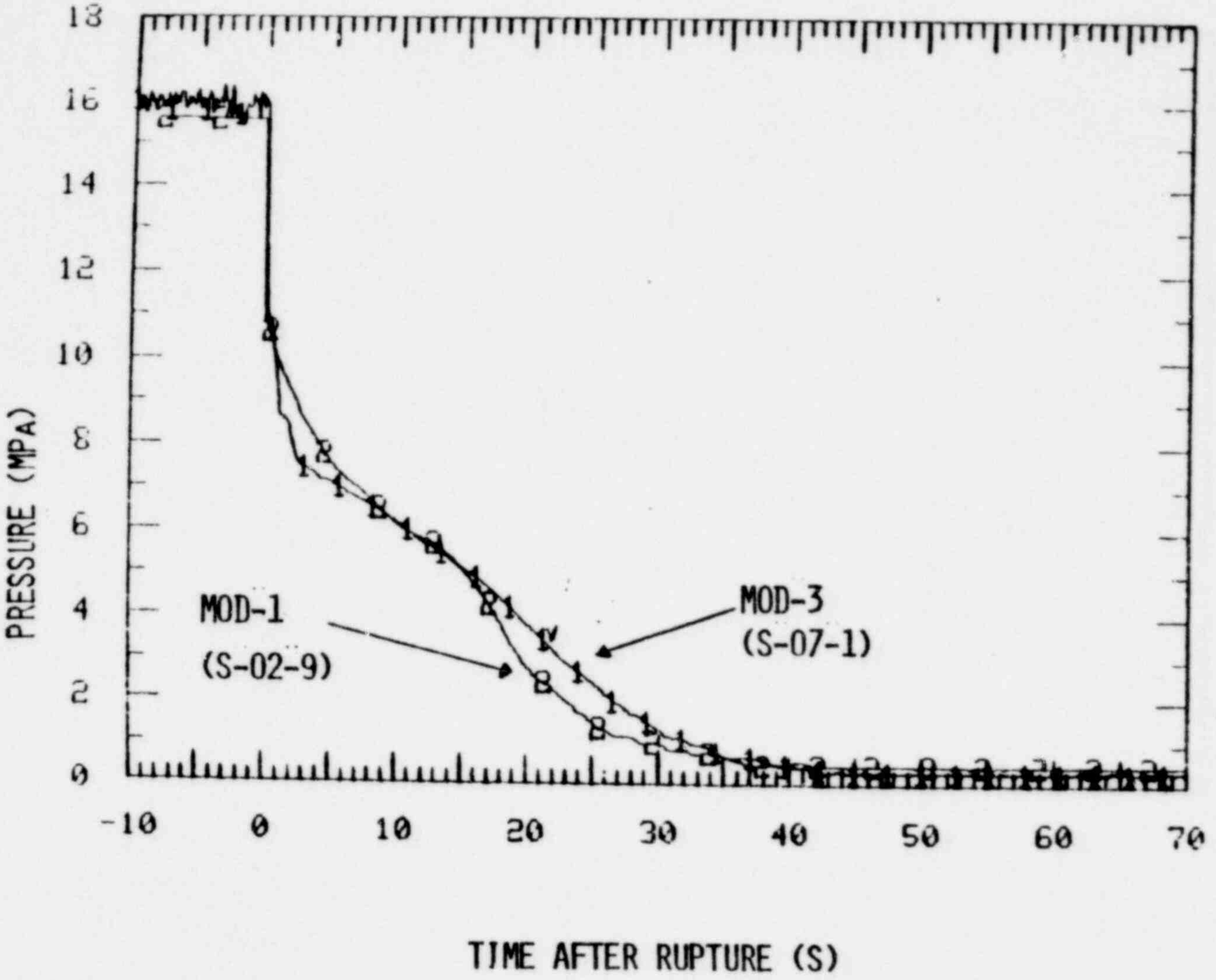
COMPARISON OF COLD LEG BREAK FLOW

MOD-1 VERSUS MOD-3



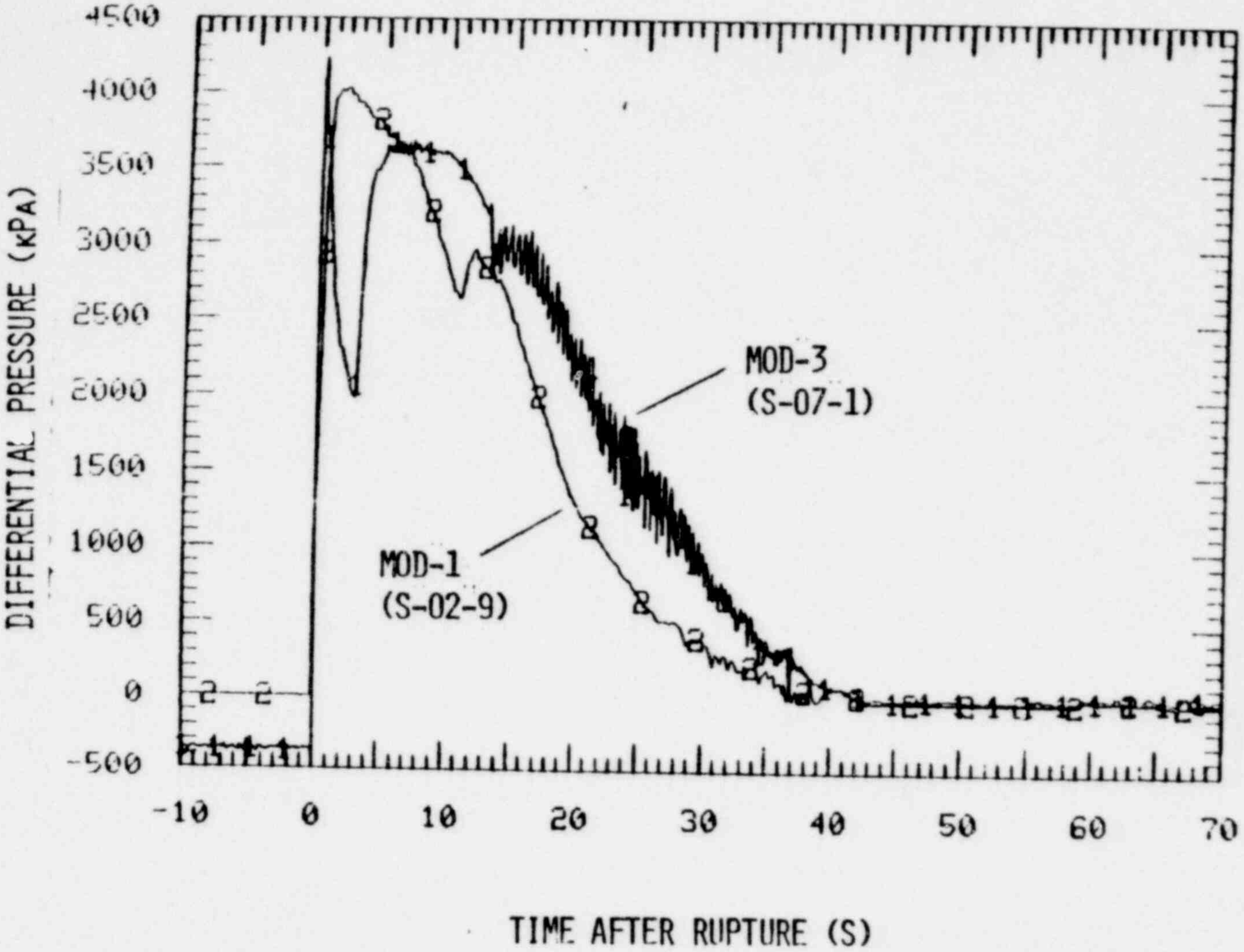
COMPARISON OF DEPRESSURIZATION

MOD-1 VERSUS MOD-3

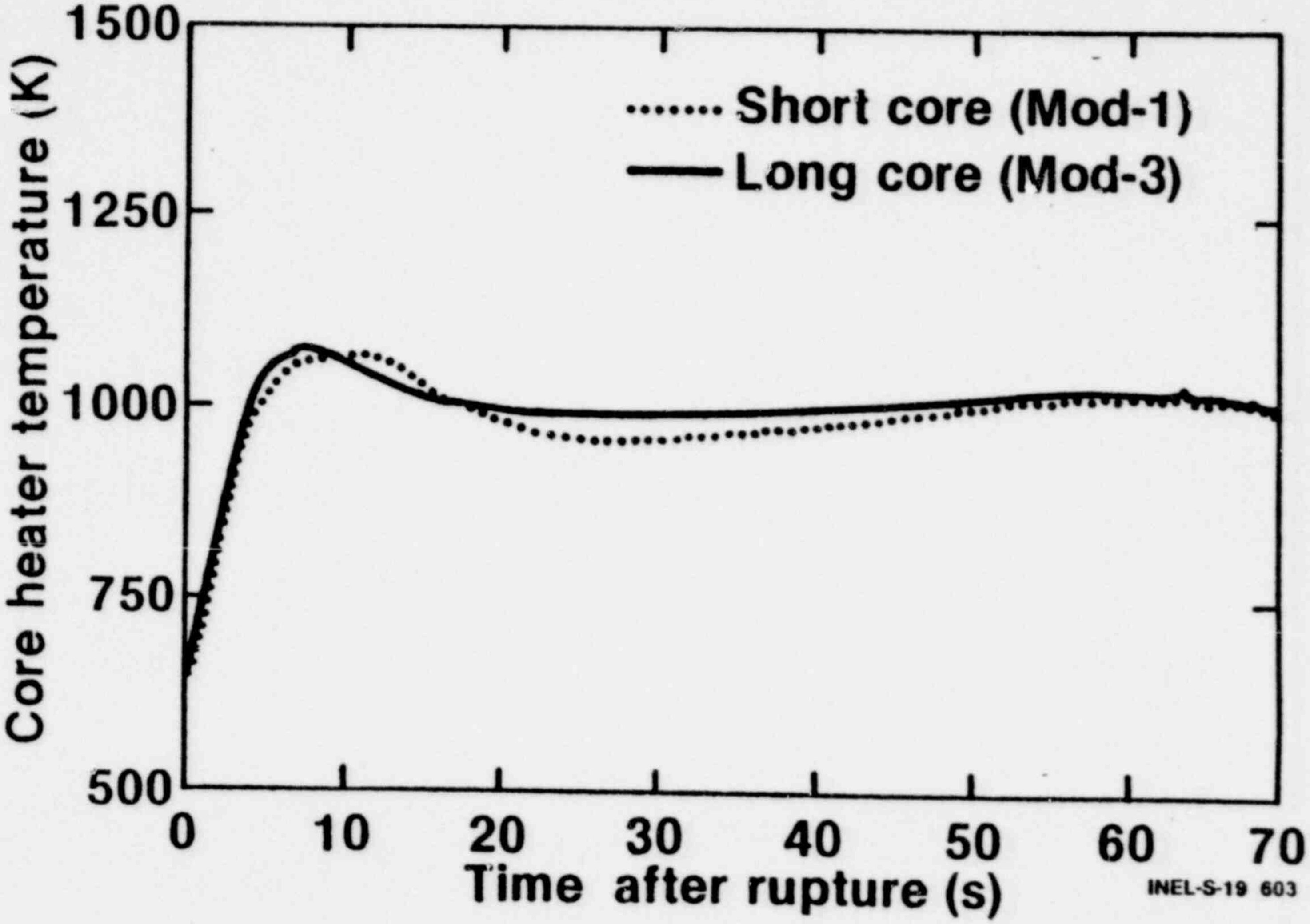


COMPARISON OF PRESSURE DIFFERENTIAL

MOD-1 BROKEN LOOP PUMP SIMULATOR VERSUS MOD-3 ACTIVE PUMP

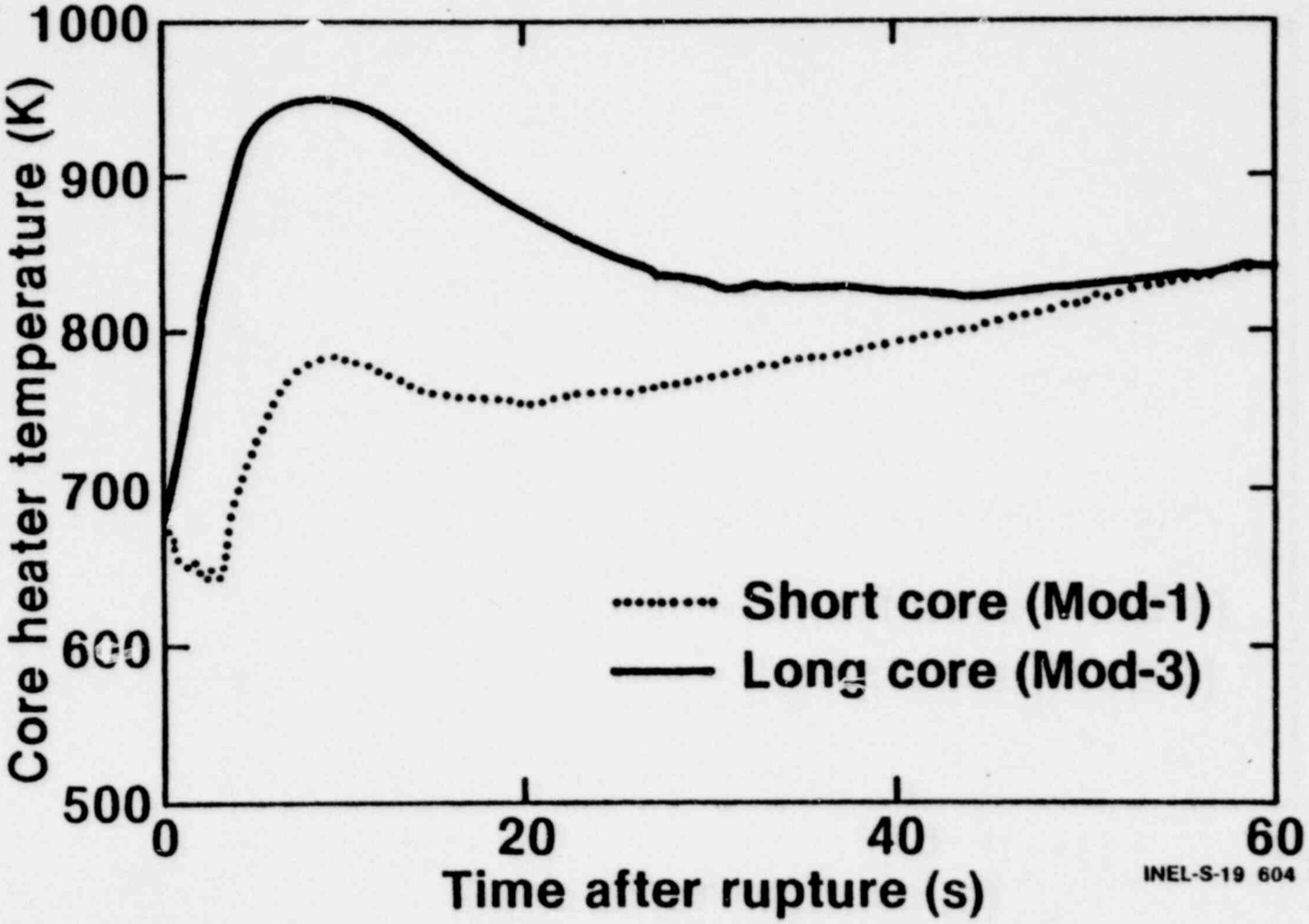


Measured Rod Temperatures at Midplane of Semiscale Core



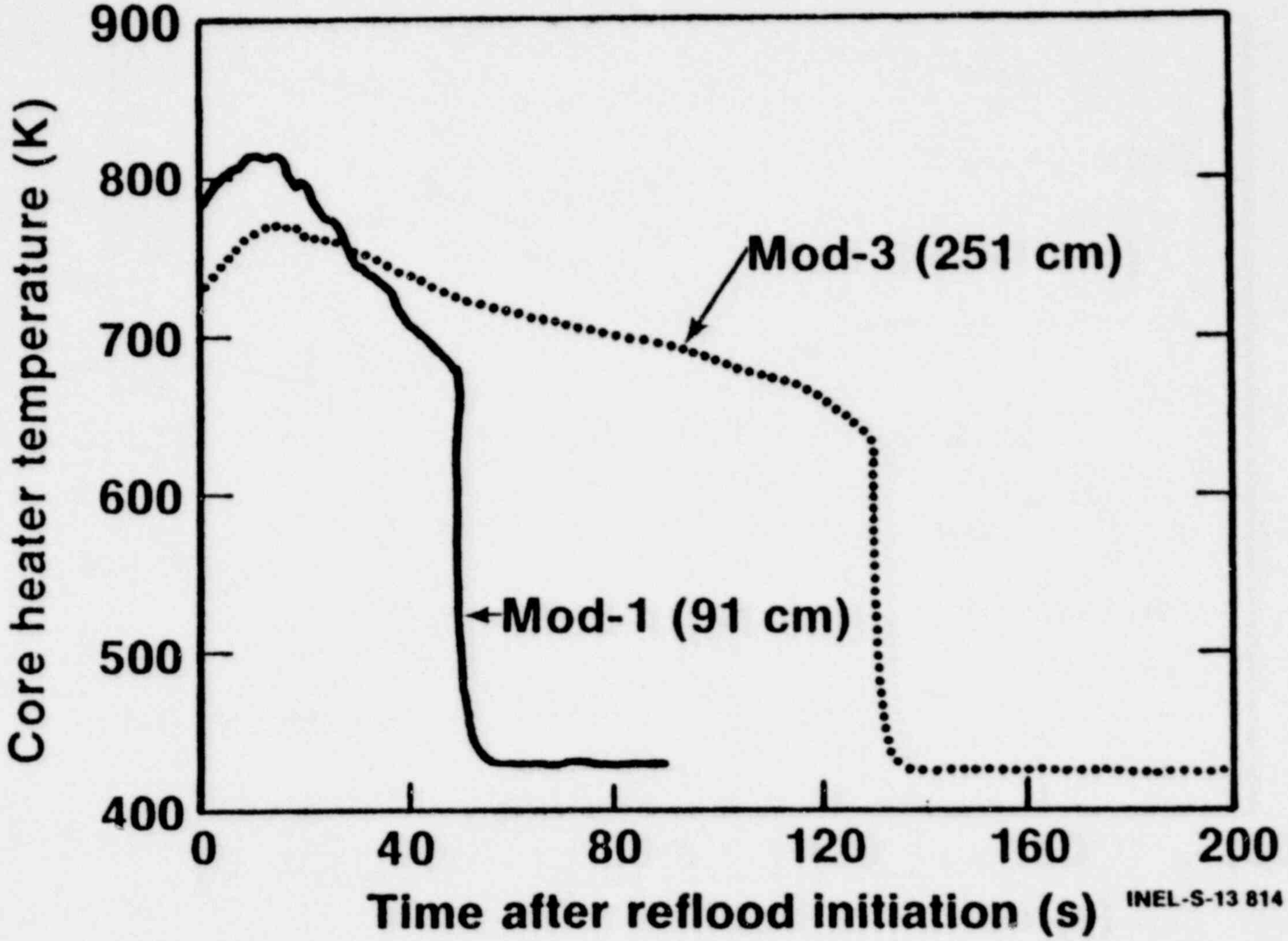
INEL-S-19 603

Measured Rod Temperatures In Upper Core Regions of Semiscale



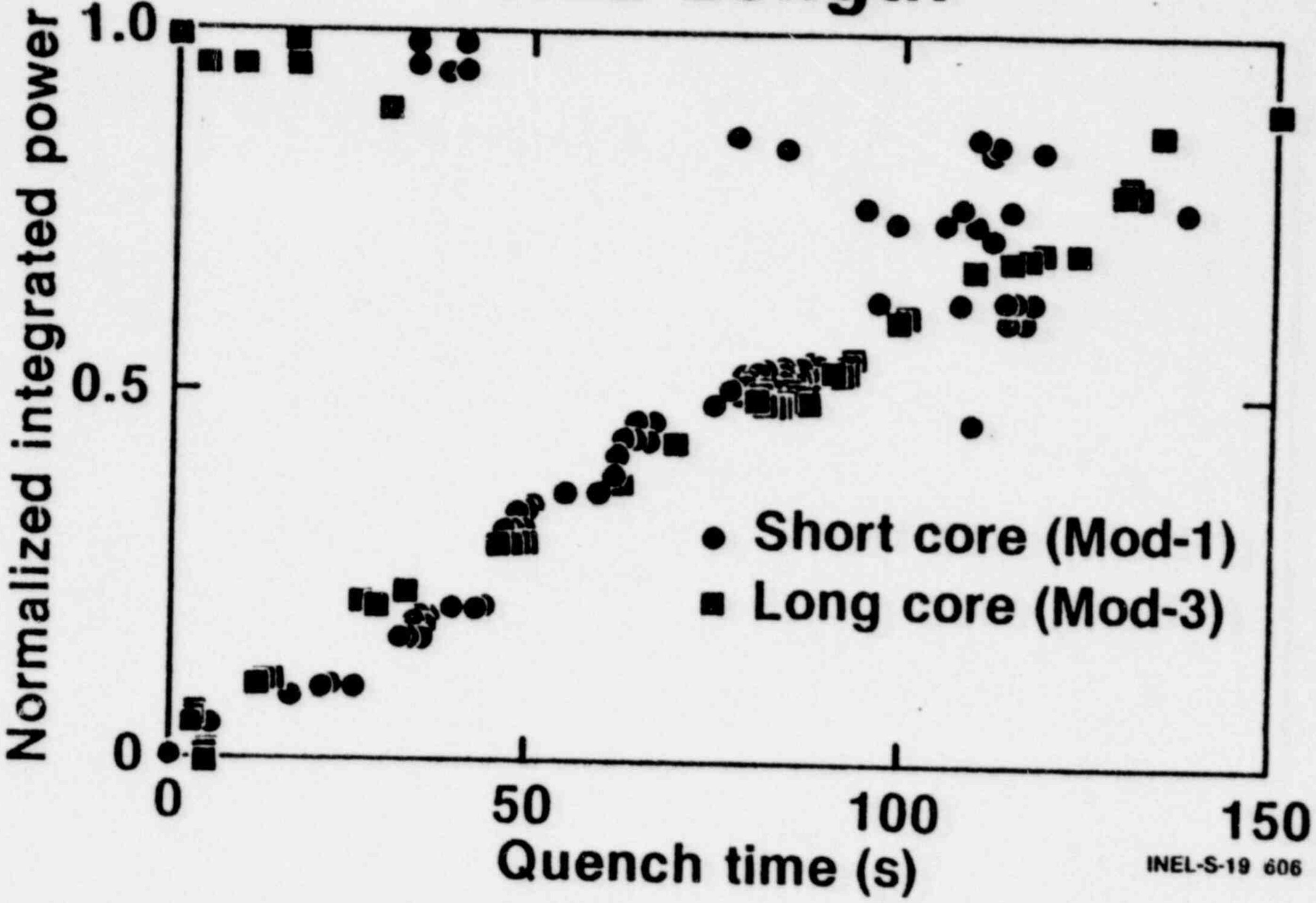
INEL-S-19 604

Comparison of Cladding Temperatures

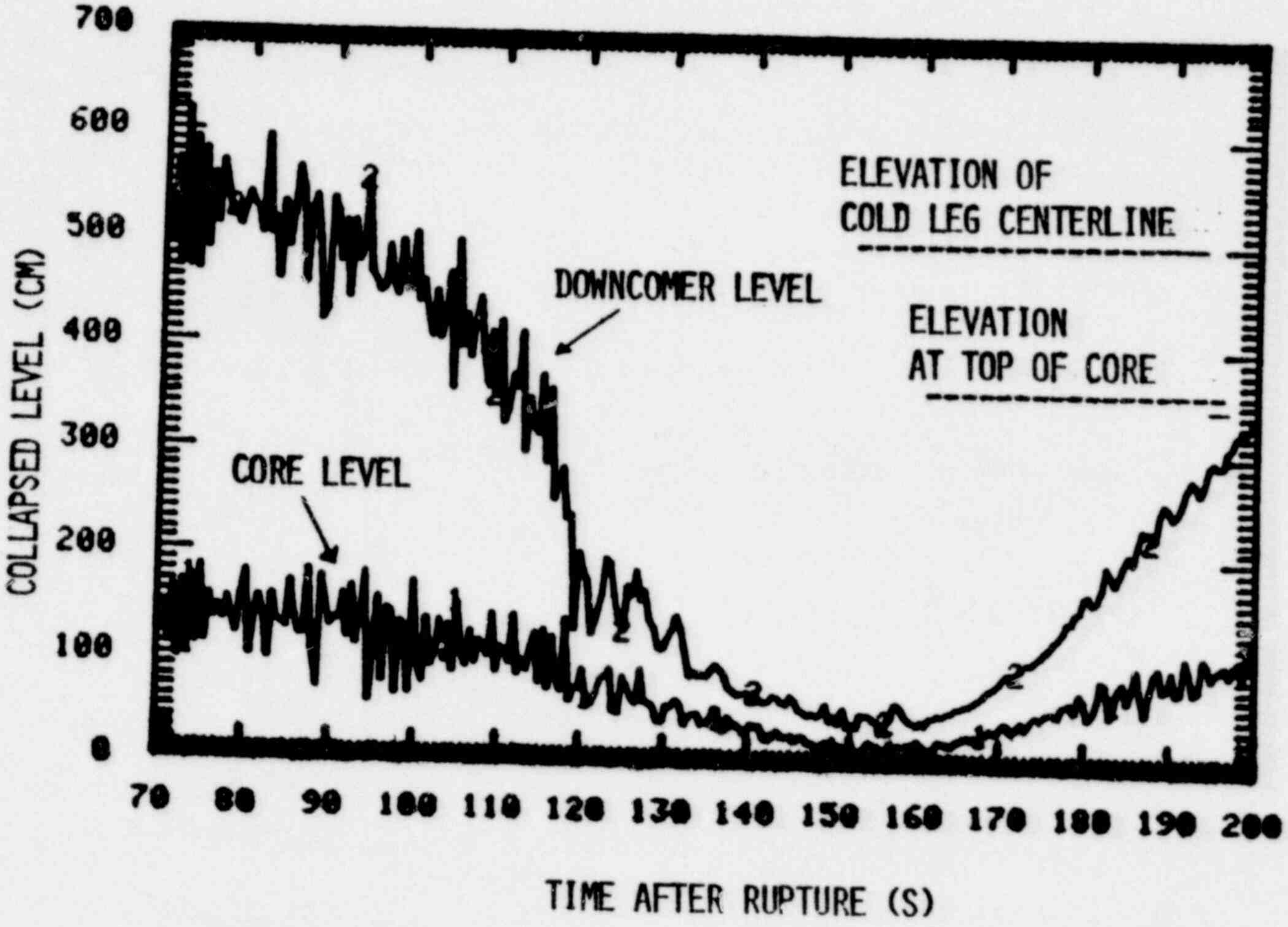


INEL-S-13 814

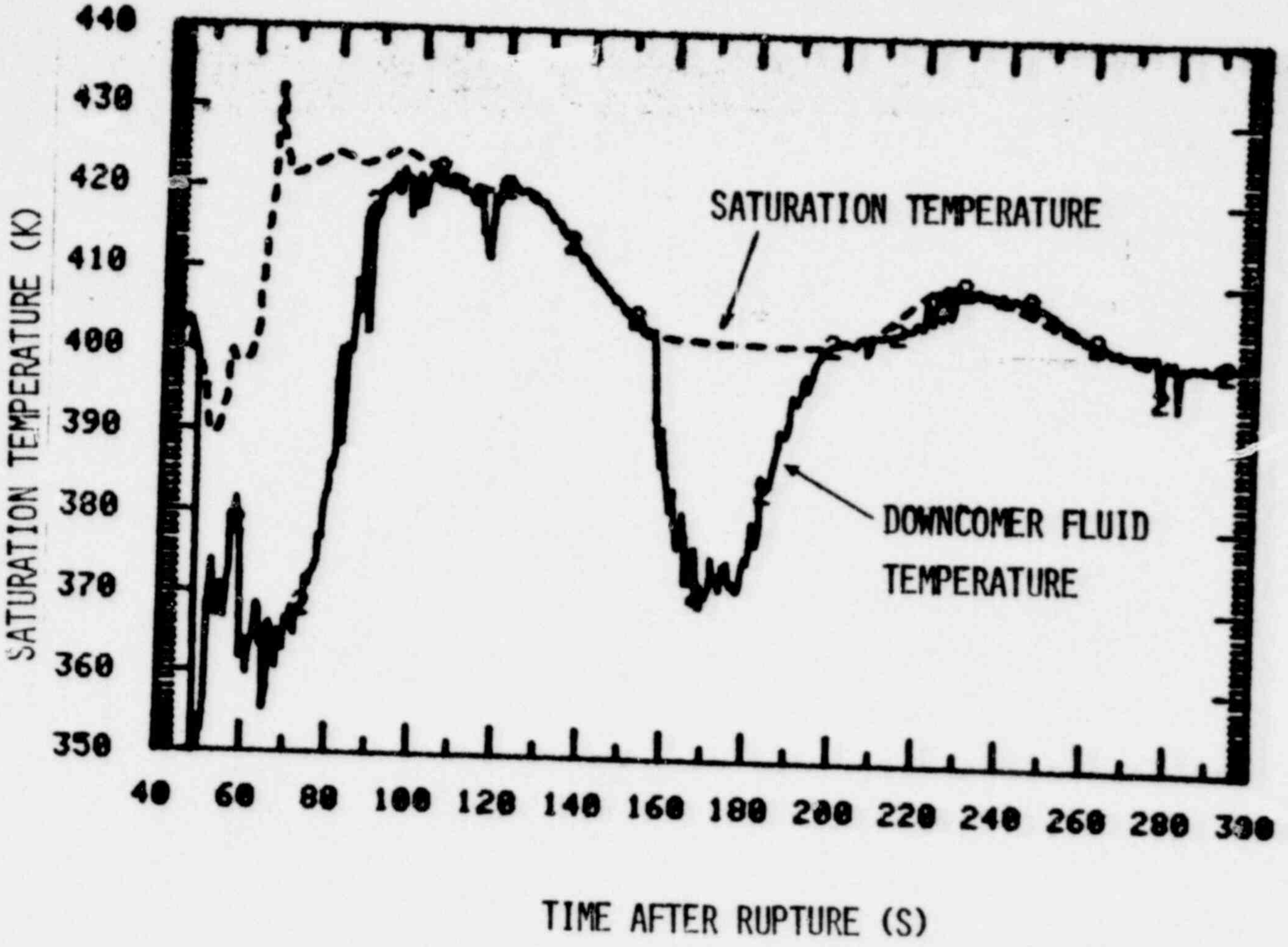
Quench Times Corrected for Rod Length



DOWNCOMER AND CORE MASS DEPLETION COLLAPSED LIQUID LEVELS DURING TEST S-07-6



COMPARISON OF SATURATION TEMPERATURE AND DOWNCOMER FLUID TEMPERATURE DURING MASS DEPLETION



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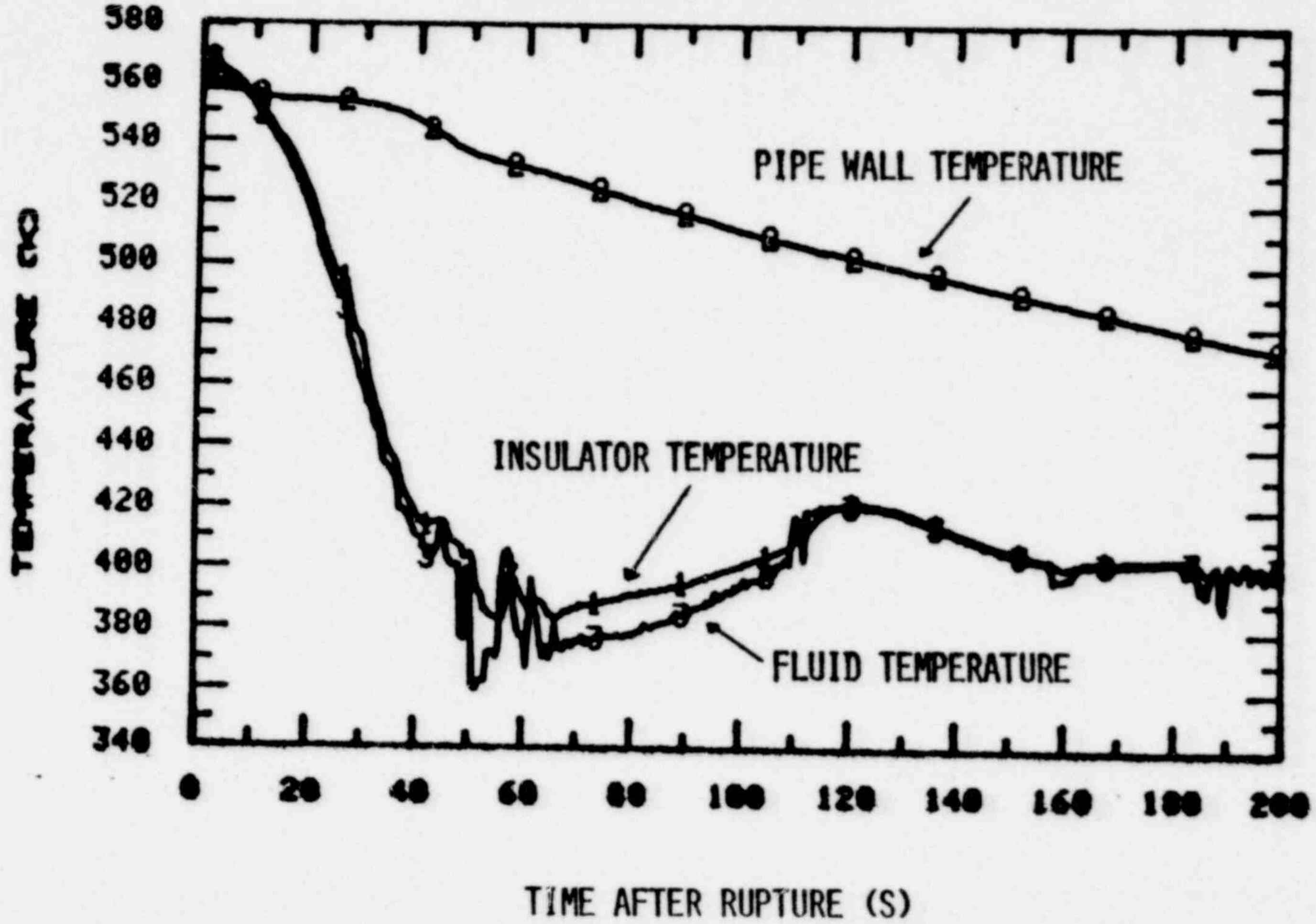
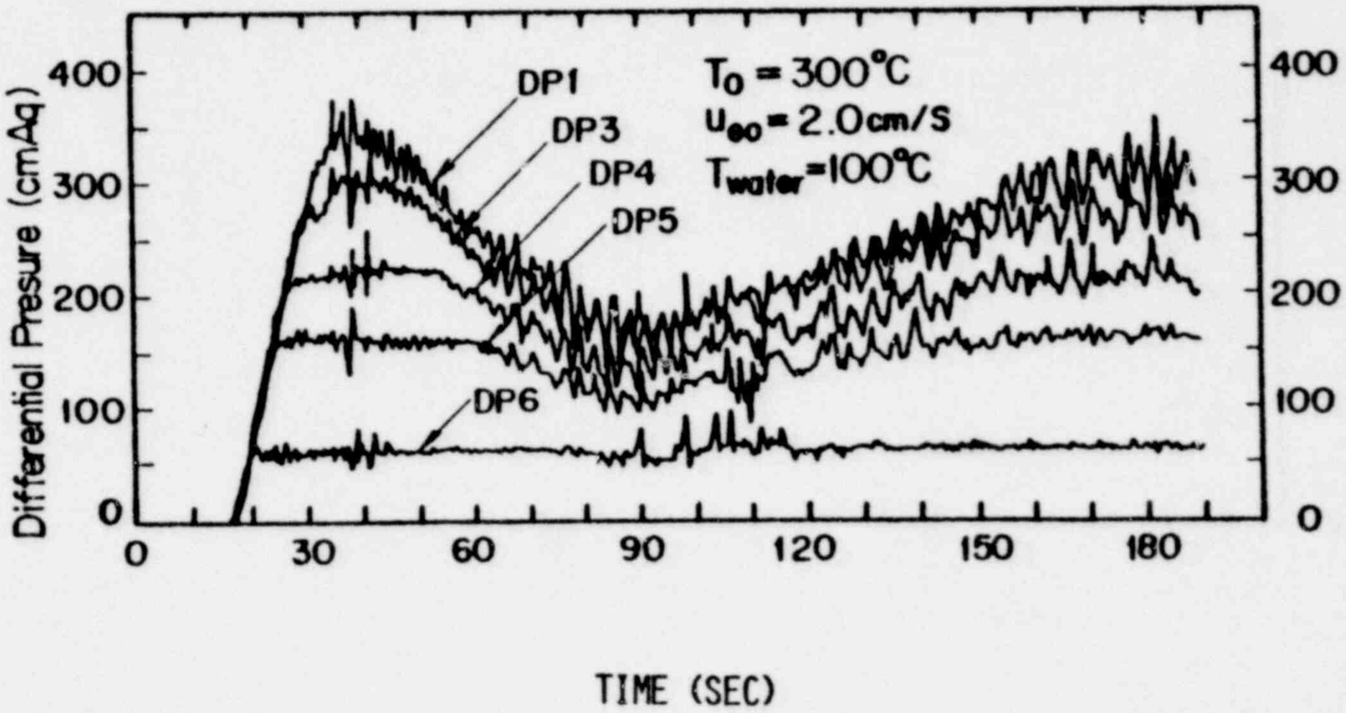
COMPARISON OF PIPE WALL, INSULATOR, AND FLUID TEMPERATURES IN DOWNCOMER

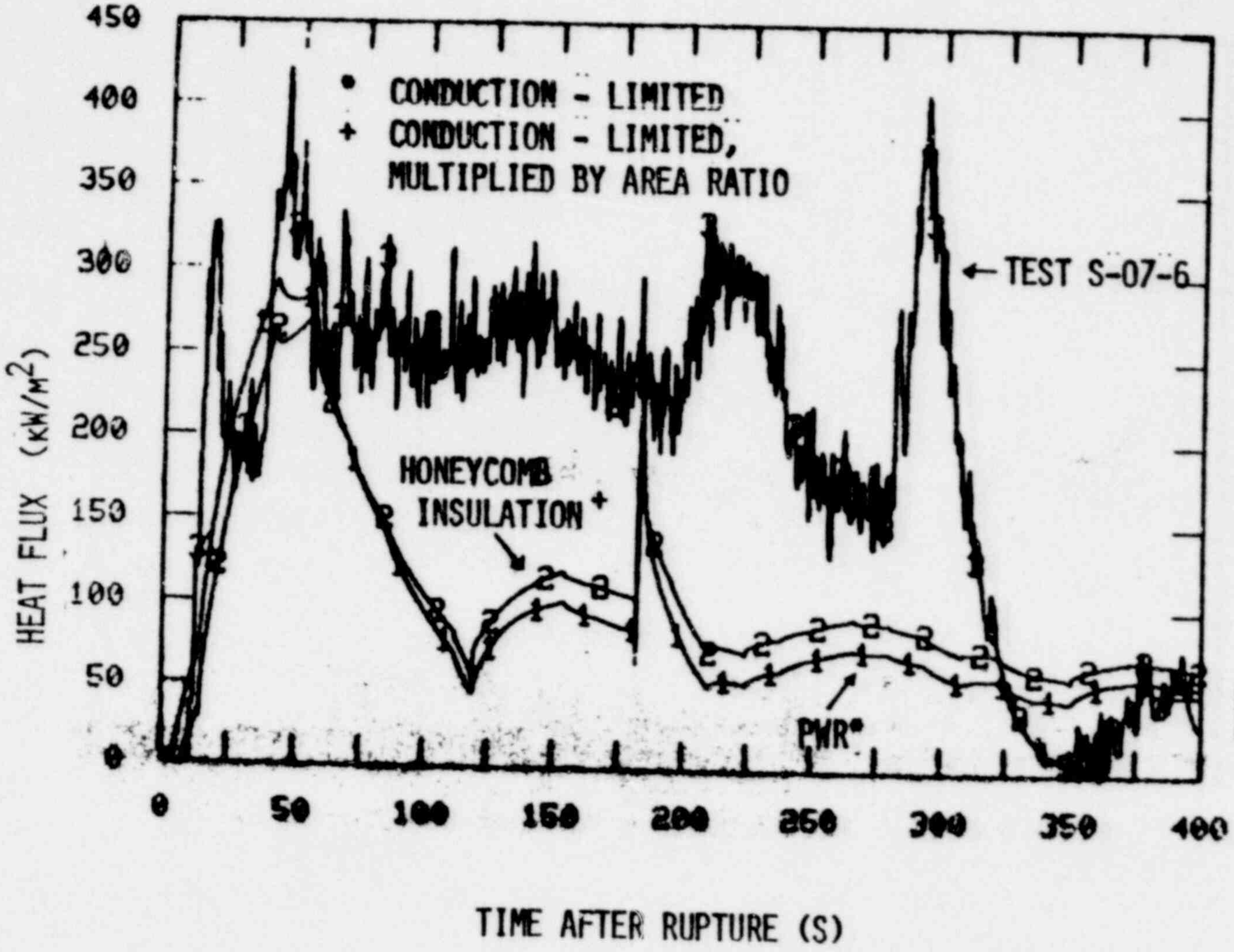


JAPANESE DOWNCOMER EFFECTIVE WATER HEAD TEST RESULTS

DOWNCOMER HEAD DURING FILL AND DEPLETION



COMPARISON OF DOWNCOMER WALL HEAT FLUXES



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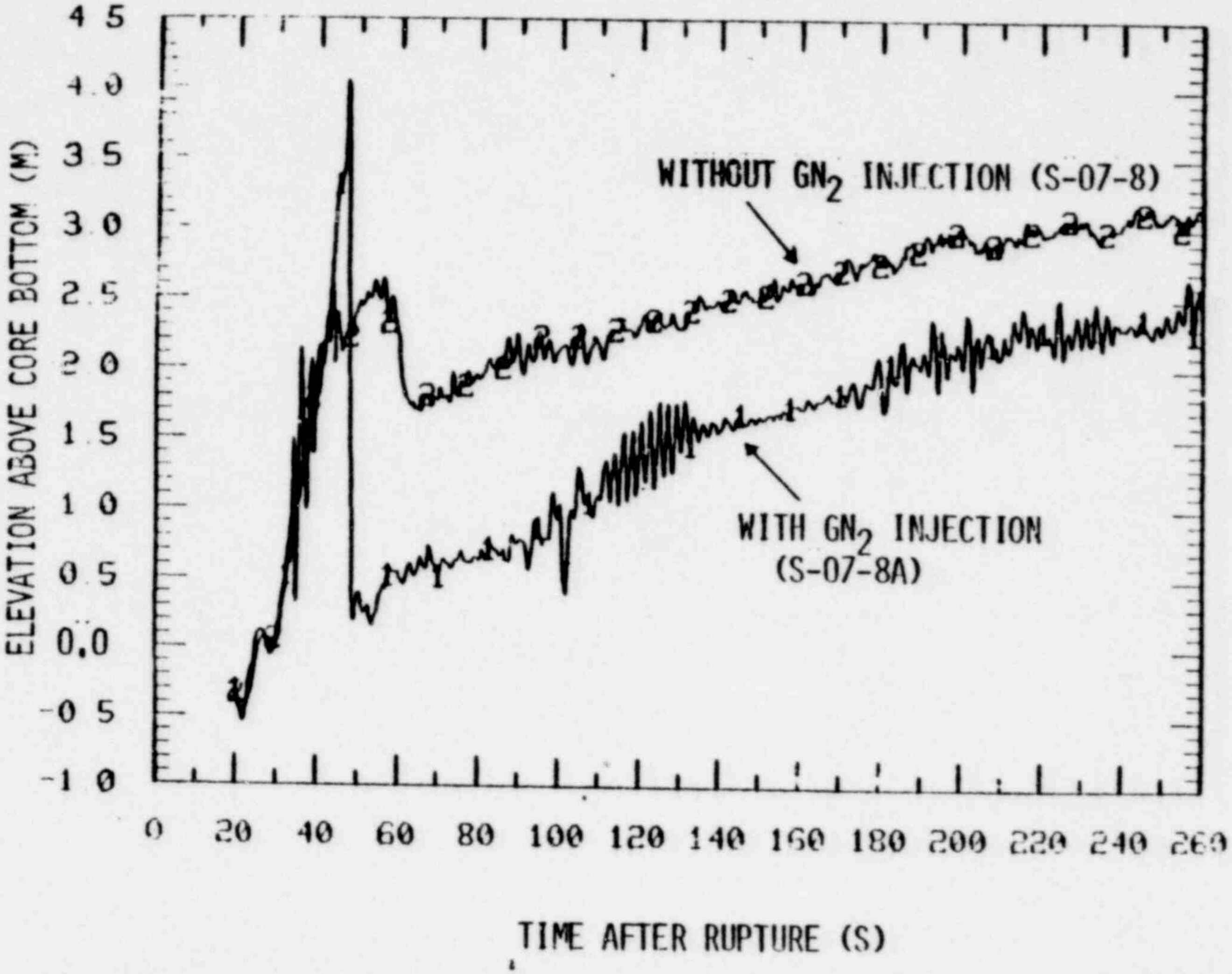
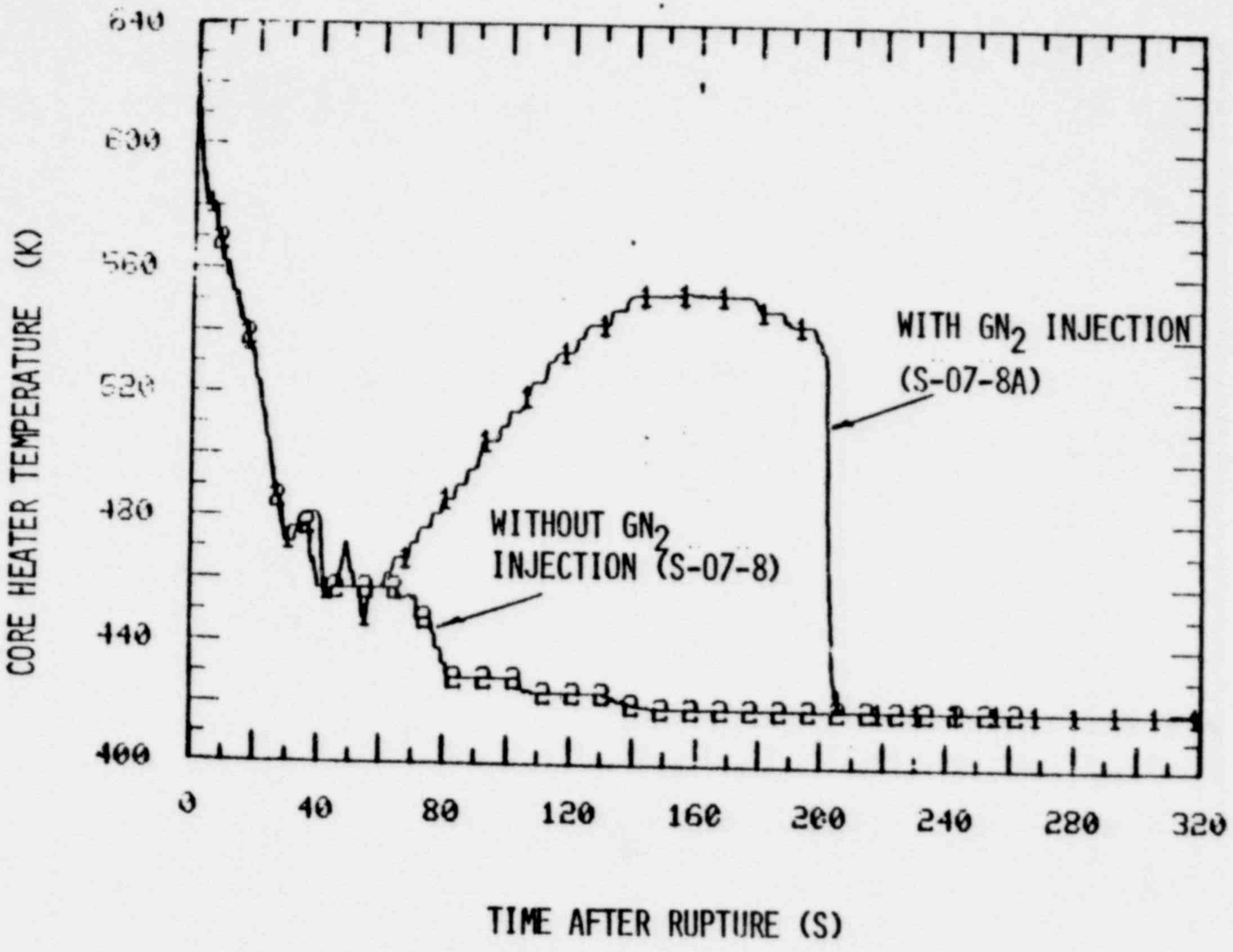
COMPARISON OF CORE COLLAPSED LIQUID LEVEL WITH AND WITHOUT ACCUMULATOR GN2 INJECTION



COMPARISON OF UPPER CORE TEMPERATURE RESPONSE
WITH AND WITHOUT GN_2 INJECTION



TMI EXPERIMENTS

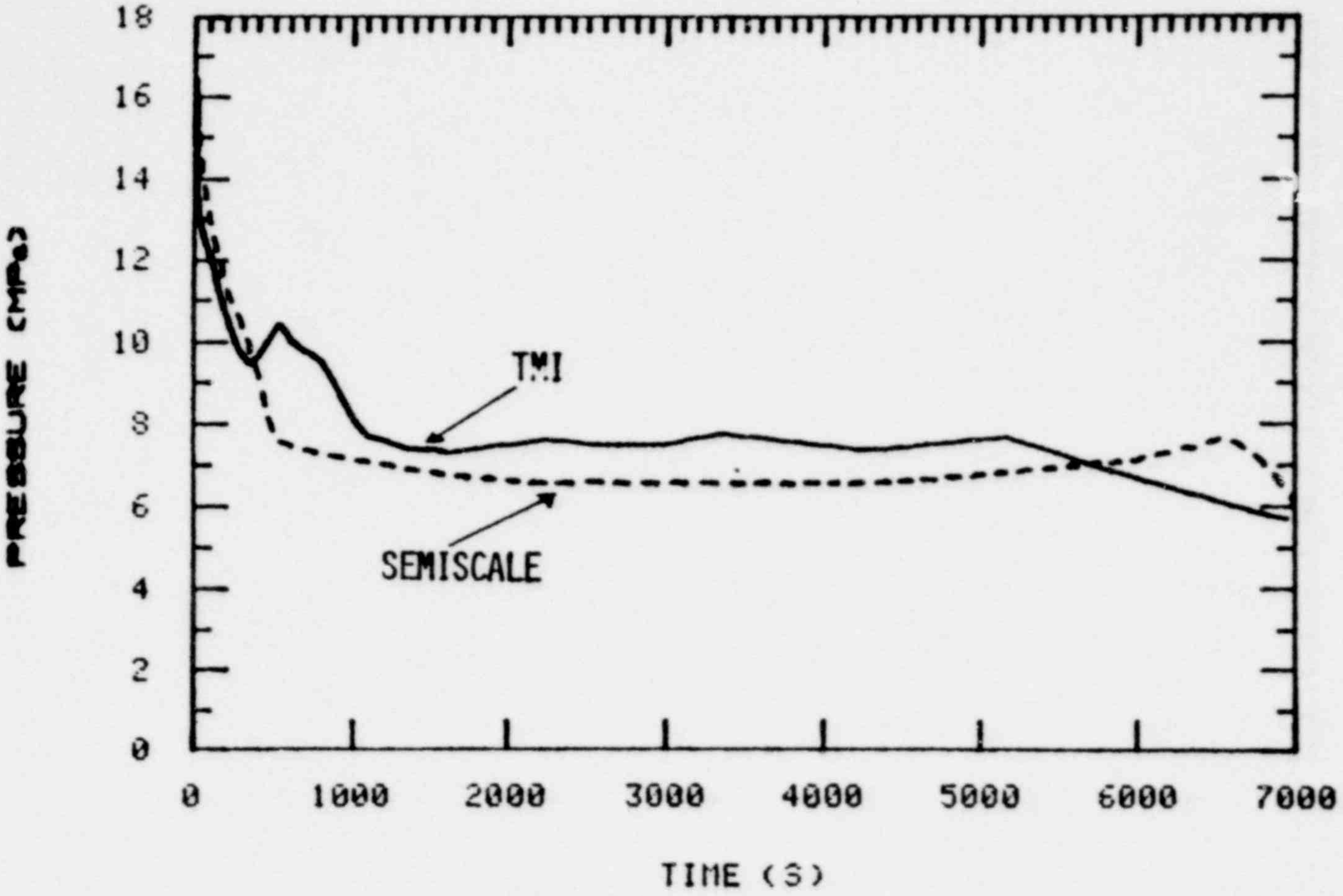
. NON-CONDENSIBLE BUBBLE TESTS

. TWO-HOUR SIMULATIONS

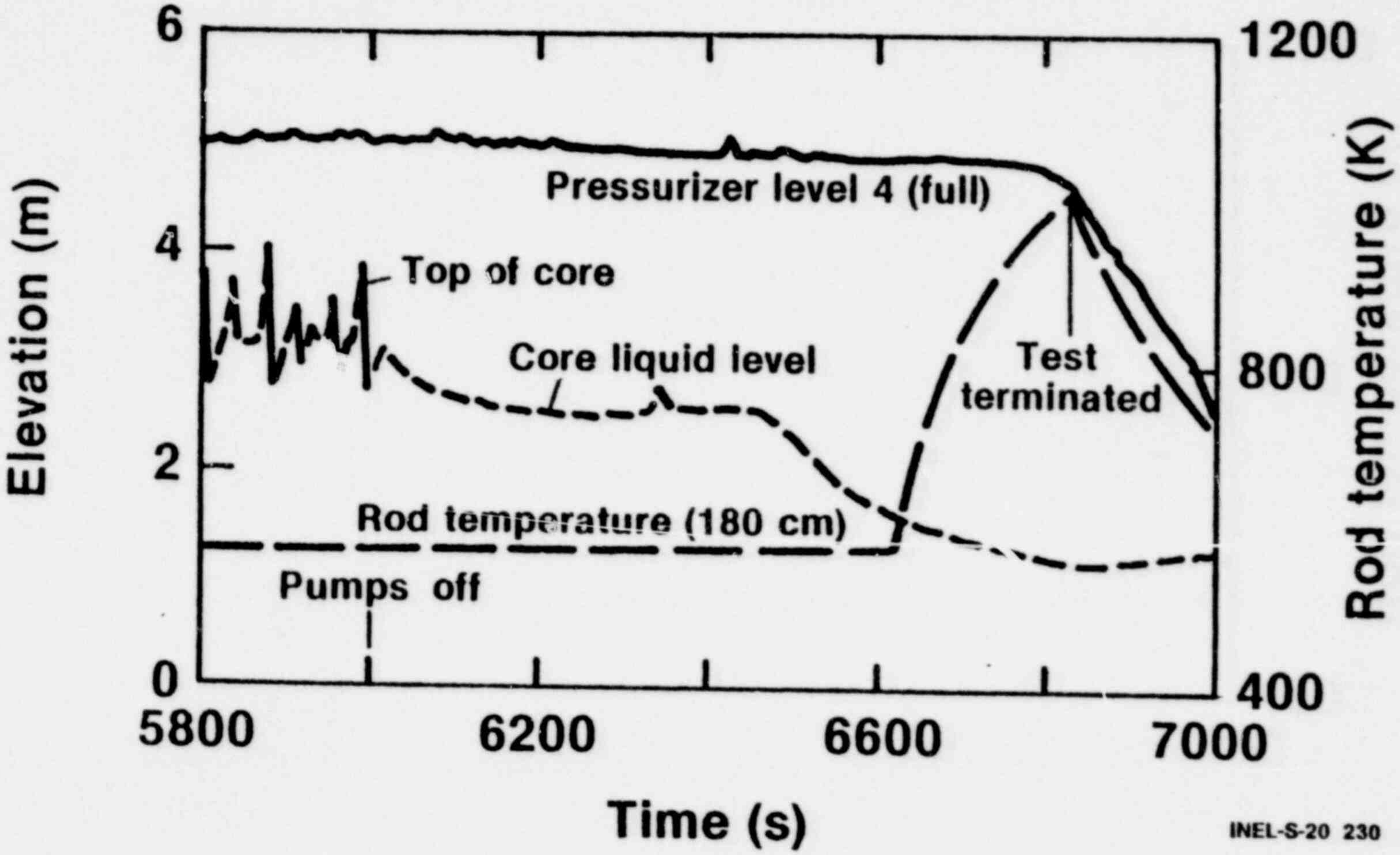
BUBBLE TEST RESULTS

- GAS TRANSPORT DURING DEPRESSURIZATION
- MULTIPLE PUMP OPERATION

COMPARISON OF SYSTEM PRESSURE FOR THE TMI AND THE SEMISCALE SIMULATION (S-TMI-3I)



Comparison of Collapsed Liquid Levels with Rod Temperature (Semiscale Test TMI-3I)



INEL-S-20 230

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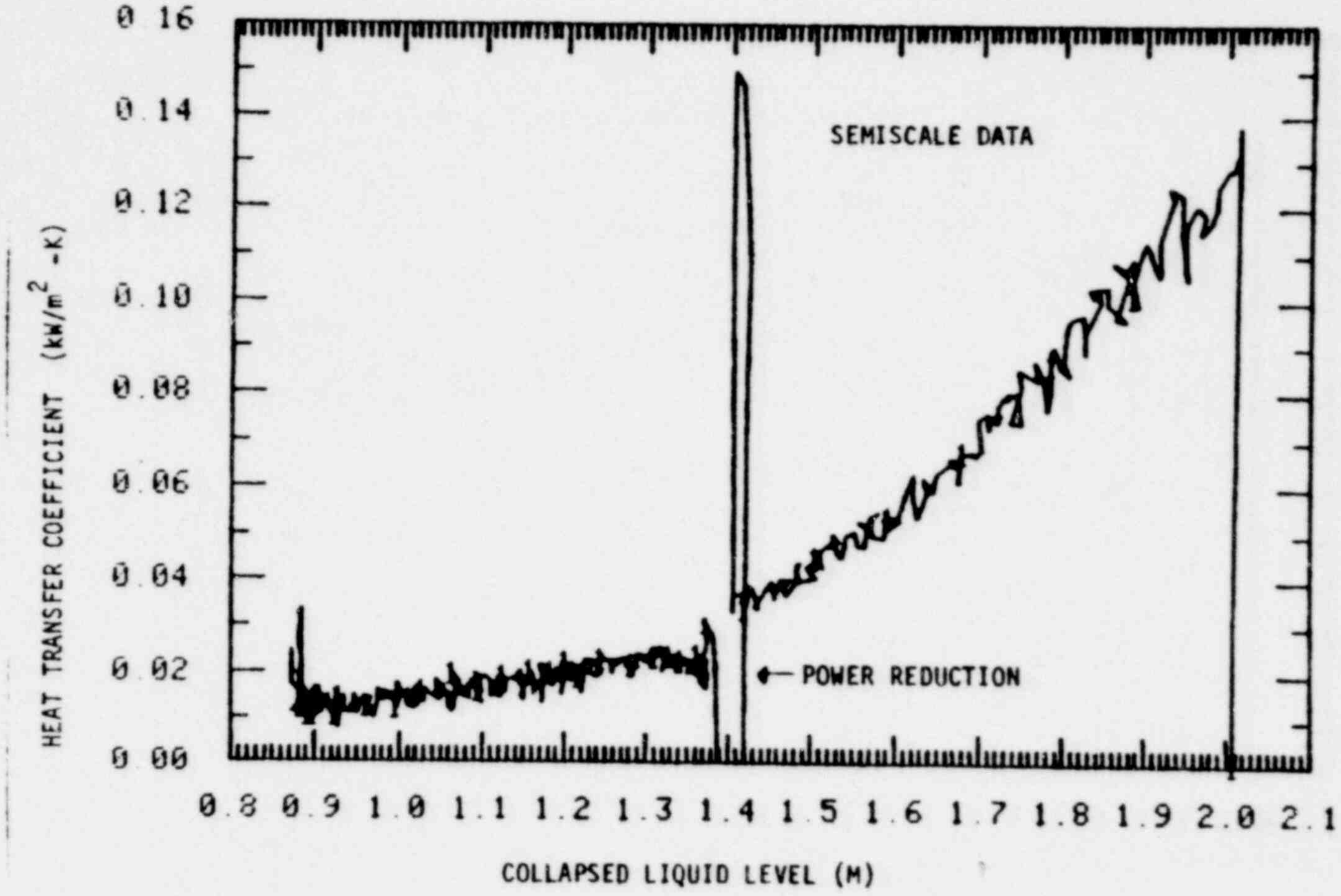
SEMISCALE HEAT TRANSFER COEFFICIENT VERSUS LIQUID LEVEL (TEST S-TMI-3C)



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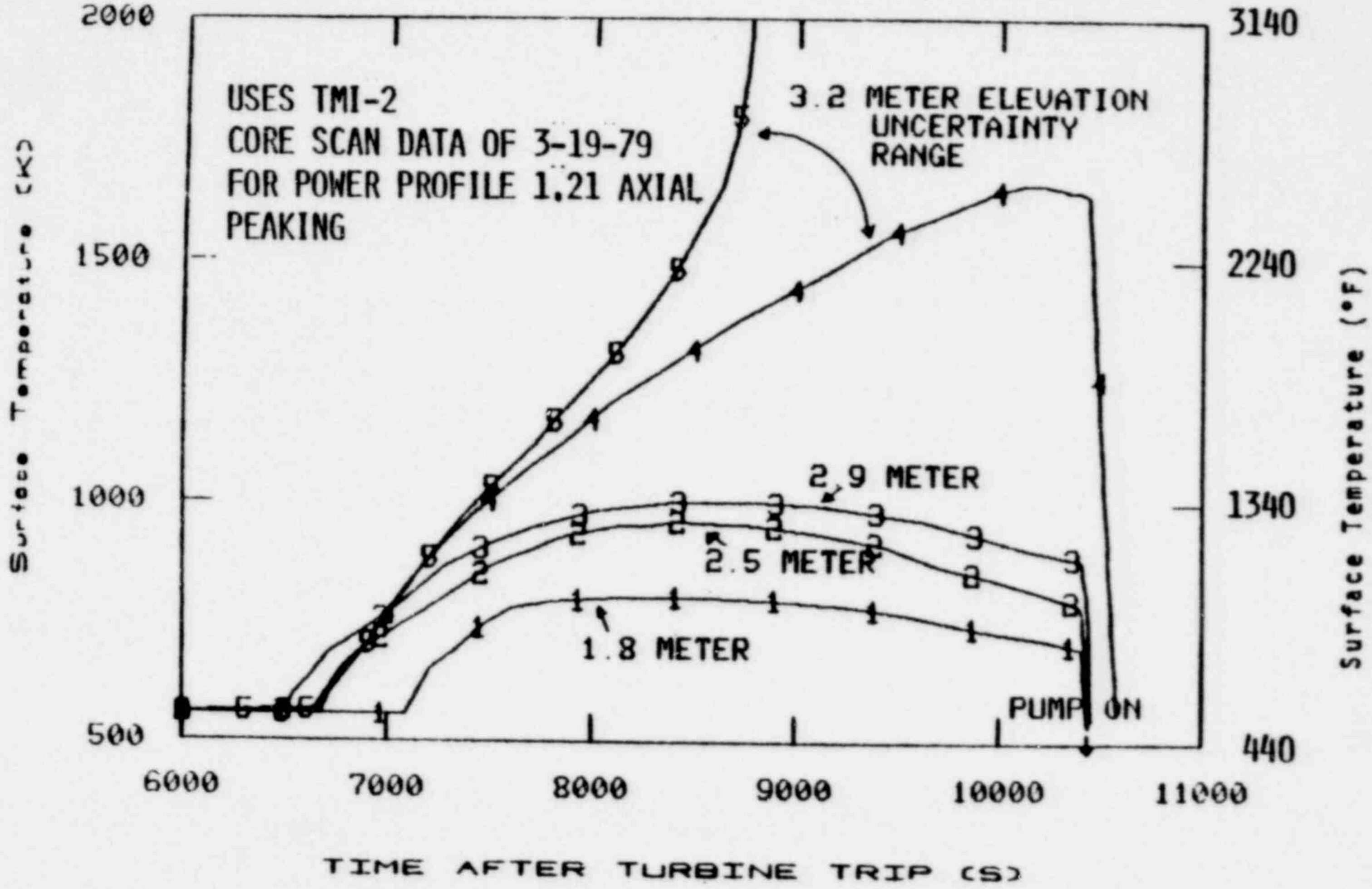
ESTIMATED THREE MILE ISLAND CORE THERMAL RESPONSE



NATURAL CIRCULATION
AND
UPPER HEAD DRAIN TESTS

- FLOW BEHAVIOR
- MEASUREMENT CAPABILITY
- CODE BENCHMARK

SUMMARY - RESULTS AND SIGNIFICANCE

. LOFT DATA ENHANCED

  - CORE LENGTH
  - INACTIVE COMPONENTS

. POTENTIALLY IMPORTANT PHENOMENA UNCOVERED

  - MASS DEPLETION
  - LOWER PLENUM INJECTION.

Olson
8/27/79

SEMISCALE PROGRAM PLAN

FY-1980 - 1981

D. J. OLSON

ACRS MEETING

AUGUST 27, 1979

EG&G Idaho, Inc

MOD-3 TEST SCHEDULE

FY-1978 AND FY-1979

- MOD-3 BASELINE TESTS (JUNE - NOVEMBER 1978)

- MOD-3 UHI SENSITIVITY TESTS (DECEMBER 1978 - APRIL 1979)

- TWO-PIPE CHARACTERIZATION TESTS (APRIL - JULY 1979)
  (TENTATIVE)

- MOD-3 TWO-PIPE SENSITIVITY TESTS (SEPTEMBER 1979 - FEBRUARY 1980)
  (TENTATIVE)

THREE MAJOR PERTUBATIONS

- TEST S-07-6 ANOMOLOUS BEHAVIOR
- UHI CALCULATIONAL CAPABILITY
- THREE MILE ISLAND

SEMISCALE SCOPE

(DECEMBER 1978)

- COMPLETE BASELINE SERIES (APRIL 1979)
- UHI DRAIN TESTS (MAY 1979)
- TEST S-07-6 REPEAT (JUNE 1979)
- SMALL BREAK TESTING (JULY - SEPTEMBER 1979)
- INTERIM TEST SERIES (SEPTEMBER 1979 - JANUARY 1980)
- UHI SENSITIVITY TESTS (JANUARY - MAY 1980)

SEMISCALE SCOPE

(AUGUST 1979)

- SMALL BREAK TESTING (AUGUST 1979 - MARCH 1980)
- TEST S-07-6 REPEAT (NOVEMBER 1979)
- SCOPING LOSS-OF-FEEDWATER TESTS (APRIL - OCTOBER 1980)
- UHI SENSITIVITY TESTS (NOVEMBER 1980 - MARCH 1981)
- CLOSED LOOP SECONDARY (MARCH - JUNE 1981)
- 2 X 4 LOOP SIMULATION (JULY 1981)

KEY PROGRAM REQUIREMENTS

- PERFORM COMPARIBLE SMALL BREAKS TO LOFT AS SOON AS POSSIBLE

- INSTALL NEW VESSEL INSULATORS, EXTERNAL INSULATION, NEW PWR SCALED STEAM GENERATOR PRIOR TO LOSS-OF-FEEDWATER TESTING

- PRIOR TO CONVERSION TO OTHER THAN FOUR-LOOP CONFIGURATION A REEVALUATION OF PRIORITIES IS REQUIRED

    - UHI

    - TWO-PIPE DOWNCOMER

    - FOUR-LOOP OPERATIONAL TRANSIENTS

Harvego
8/25/79

FUTURE SEMISCALE TEST
PLANS AND SCHEDULE

E. A. HARVEGO
ACRS MEETING
AUGUST 27, 1979

EG&G Idaho, Inc.

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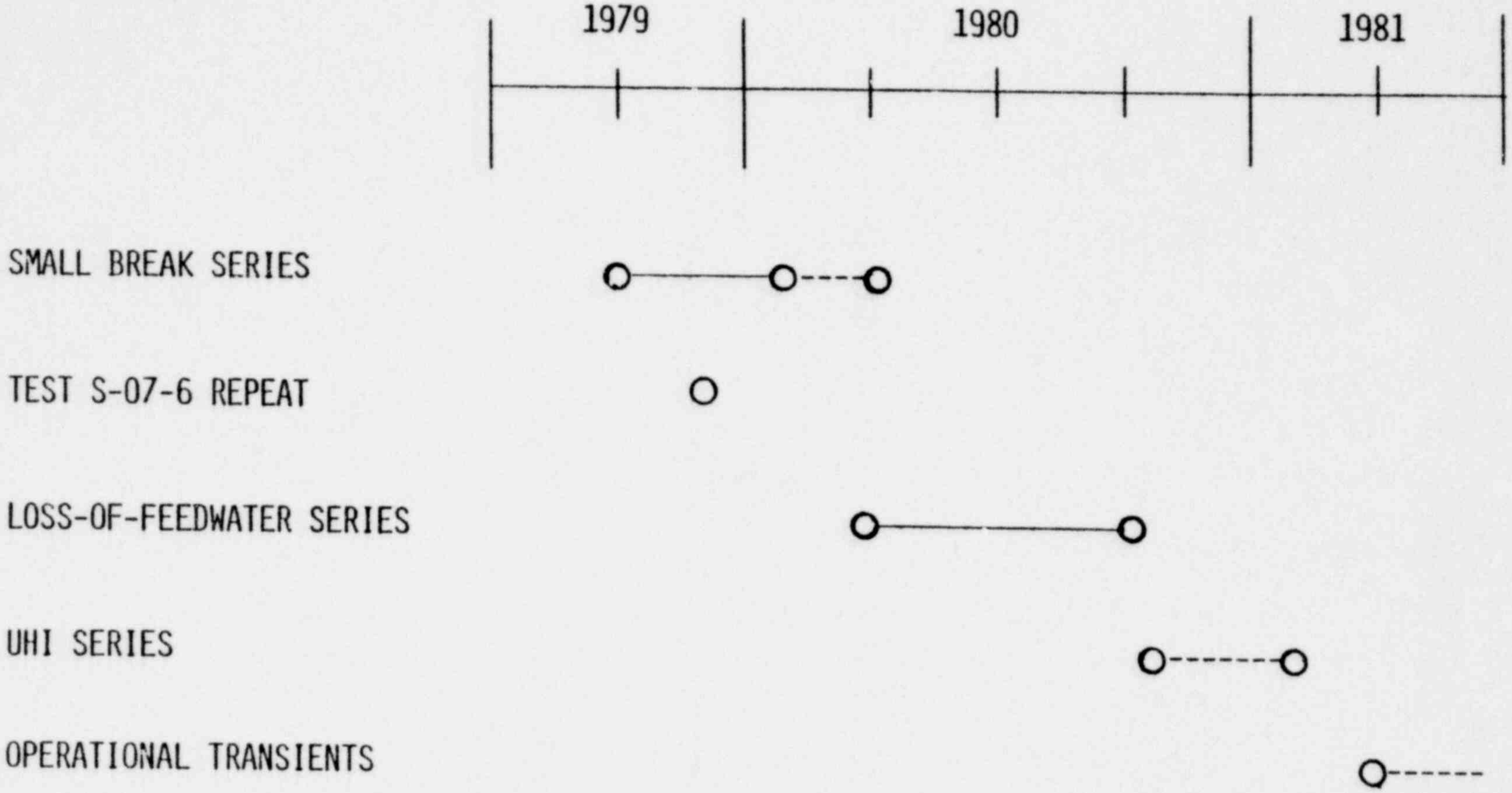
PROPOSED FUTURE TESTS

- SMALL BREAKS
- LOSS-OF-FEEDWATER
- UNDEFINED OPERATIONAL TRANSIENTS

FUTURE SEMISCALE TESTS



NEED FOR SMALL BREAK TESTS

- RECENT EVENTS
- LIMITED DATA
- CODE BENCHMARK DATA

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SEMISCALE SMALL BREAK TEST OBJECTIVES

- IDENTIFY IMPORTANT THERMAL-HYDRAULIC BEHAVIOR
- COMPARE WITH LOFT / AUDIT CALCULATIONS
- CODE EVALUATION

PROPOSED SMALL BREAK TESTS

TEST	BREAK SIZE	INITIAL CONDITIONS	CONFIGURATION
S-SB-2	2.5%	PWR AUDIT CALCULATIONS	MOD-3
S-SB-4	2.5%	LOFT TEST L3-1	LOFT
S-SB-3	2.5%	PWR AUDIT CALCULATIONS	LOFT
S-SB-5	0.16%	PWR AUDIT CALCULATIONS	MOD-3
S-SB-1	10%	TEST S-07-10	MOD-3 (UHI)

EXPECTED SMALL BREAK RESULTS

- CORE UNCOVERY
- NATURAL CIRCULATION PHENOMENA
- SECONDARY SIDE INFLUENCES
- SCALING EFFECTS (LOFT/SEMISCALE)

LOSS-OF-FEEDWATER RATIONALE

- RELATIVE PROBABILITY
- EVENT TREE WELL DEFINED
- LACK OF EXPERIMENTAL DATA

LOSS-OF-FEEDWATER OBJECTIVES

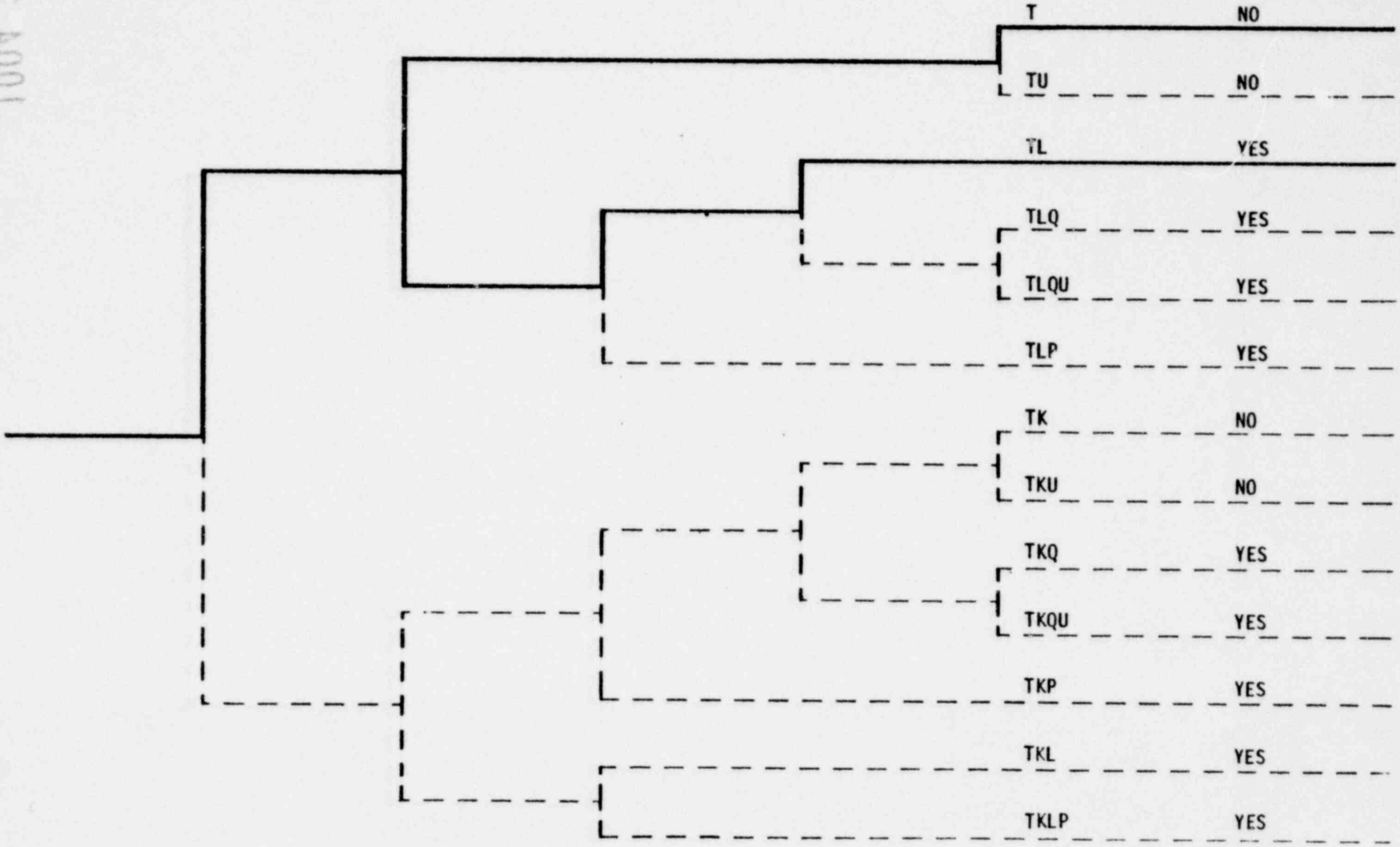
- SYSTEM RESPONSE
- MULTIPLE FAILURES
- CODE DEVELOPMENT

LOSS-OF-FEEDWATER TEST PLAN

- LOSS-OF-FEEDWATER INITIATES TEST
- EVENT TREE APPROACH
- TEST SERIES PROCEEDS FROM INITIATING EVENT TO LIMITING CASE

EVENT TREE FOR LOSS-OF-FEEDWATER TRANSIENT

INITIATING EVENT LOSS-OF-FEEDWATER	REACTOR PROTECTION SYSTEM (SCRAM)	SEC. STEAM RELIEF AND AUXILIARY FEEDWATER	PRESSURIZER RELIEF VALVE OPENS	PRESSURIZER RELIEF VALVE RECLOSES	HIGH PRESSURE INJECTION	CORE DAMAGE
T	K	L	P	Q	U	



EXPECTED LOSS-OF-FEEDWATER RESULTS

- IDENTIFY IMPORTANT THERMAL-HYDRAULIC EFFECTS
- IDENTIFY LARGE REACTOR MEASUREMENT CAPABILITIES / LIMITATIONS
- ASSESS CODE ABILITY TO PREDICT OUTCOME OF EXPERIMENTAL PATH

POSSIBLE FUTURE TRANSIENTS

- INCREASED SECONDARY HEAT REMOVAL (SHORT TERM)
- DECREASED SECONDARY HEAT REMOVAL (SHORT TERM)
- DECREASED REACTOR COOLANT SYSTEM FLOW (SHORT TERM)
- REACTIVITY AND POWER DISTRIBUTION ANOMALIES (MEDIUM / LONG TERM)
- INCREASE IN REACTOR COOLANT INVENTORY (MEDIUM / LONG TERM)
- DECREASE IN REACTOR COOLANT INVENTORY (MEDIUM / LONG TERM)
- ANTICIPATED TRANSIENT WITHOUT SCRAM (LONG TERM)

OBJECTIVES OF OPERATIONAL TRANSIENTS

- SYSTEM RESPONSE TO SINGLE INITIATING EVENT
- INFLUENCES OF MULTIPLE FAILURES
- LARGE REACTOR INSTRUMENTATION CAPABILITIES / LIMITATIONS
- DATA BASE FOR CODE DEVELOPMENT

TEST PLAN FOR OPERATIONAL TRANSIENTS

- EACH INITIATING EVENT BASIS FOR TEST SERIES

- TEST SERIES USES EVENT TREE APPROACH

- SERIES PROCEEDS FROM INITIATING EVENT TO LIMITING CASE

- FOLLOWUP TESTING DEVELOPED FROM EACH TEST SERIES (SYSTEM RECOVERY, OPERATOR INTERVENTION)

EXPECTED OPERATIONAL TRANSIENT RESULTS

- IDENTIFY POTENTIAL THERMAL-HYDRAULIC BEHAVIOR FOR A WIDE RANGE OF OPERATIONAL TRANSIENTS
- IDENTIFY POTENTIAL OPERATOR ACTIONS TO MITIGATE CONSEQUENCES
- IDENTIFY CODE ABILITY TO PREDICT LIMITING CASES AND ASSESS OPERATING MARGINS
- BASIS FOR DEFINING SEPARATE EFFECTS EXPERIMENTS

Supplement -
Johnsen
8/28/79

SEMISCALE PROGRAM ACCOMPLISHMENTS

SUPPLEMENTARY SLIDES

OBJECTIVES:

- o INVESTIGATE NONCONDENSIBLE GAS BUBBLE BEHAVIOR DURING PRESSURIZER VENTING AND QUALIFY SYSTEM RECOVERY PROCEDURES

- o ESTABLISH SYSTEM THERMAL-HYDRAULIC RESPONSE TO TMI SEQUENCE OF EVENTS

    - QUANTIFY PRESSURIZER LIQUID LEVEL BEHAVIOR

    - IDENTIFY SEMISCALE HARDWARE AND CONFIGURATION LIMITATIONS WITH REGARD TO EXTREMELY SMALL BREAK SIMULATIONS

HARDWARE CHANGES MADE TO SEMISCALE SYSTEM FOR TMI SIMULATIONS

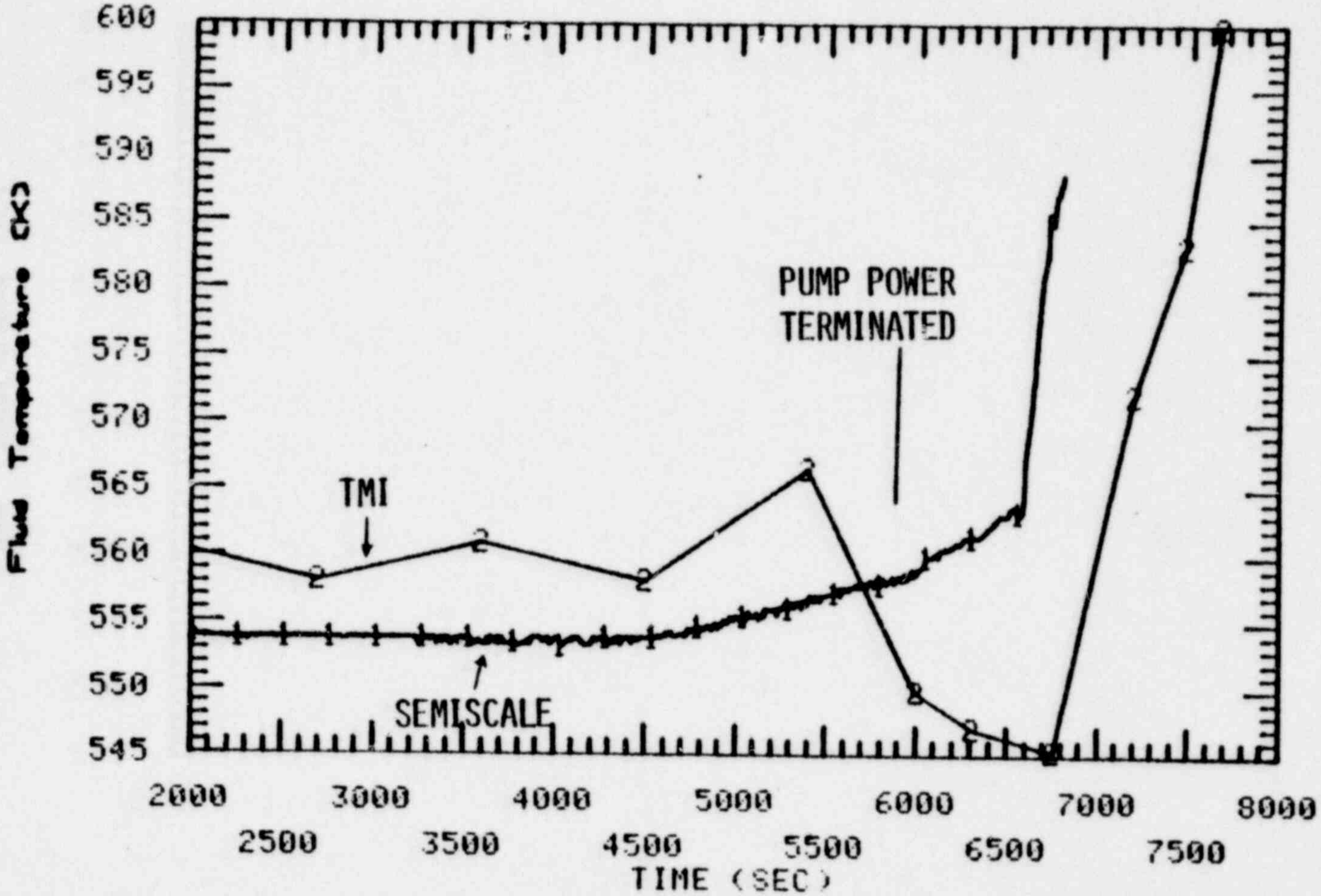
- PRESSURIZER SURGE LINE ELEVATION AND RESISTANCE MODIFIED
- ADDITION OF PRESSURIZER SAFETY VALVES AND POWER OPERATED VALVE
- ADDITION OF UPPER PLENUM TO INLET ANNULUS VENT LINE AND VALVE
- REDUCED LOOP RESISTANCE
- ADDED ORIFICE IN HOT LEG VESSEL NOZZLE TO PROMOTE SYMMETRIC LOOP RESPONSE

SEMISCALE SYSTEM OPERATING LIMITATIONS

- o EXTERNAL HEAT LOSS
- o STEAM GENERATOR DESIGN
- o PCP DEGRADATION

1004 249

SEMISCALE VERSUS TMI HOT LEG TEMPERATURE



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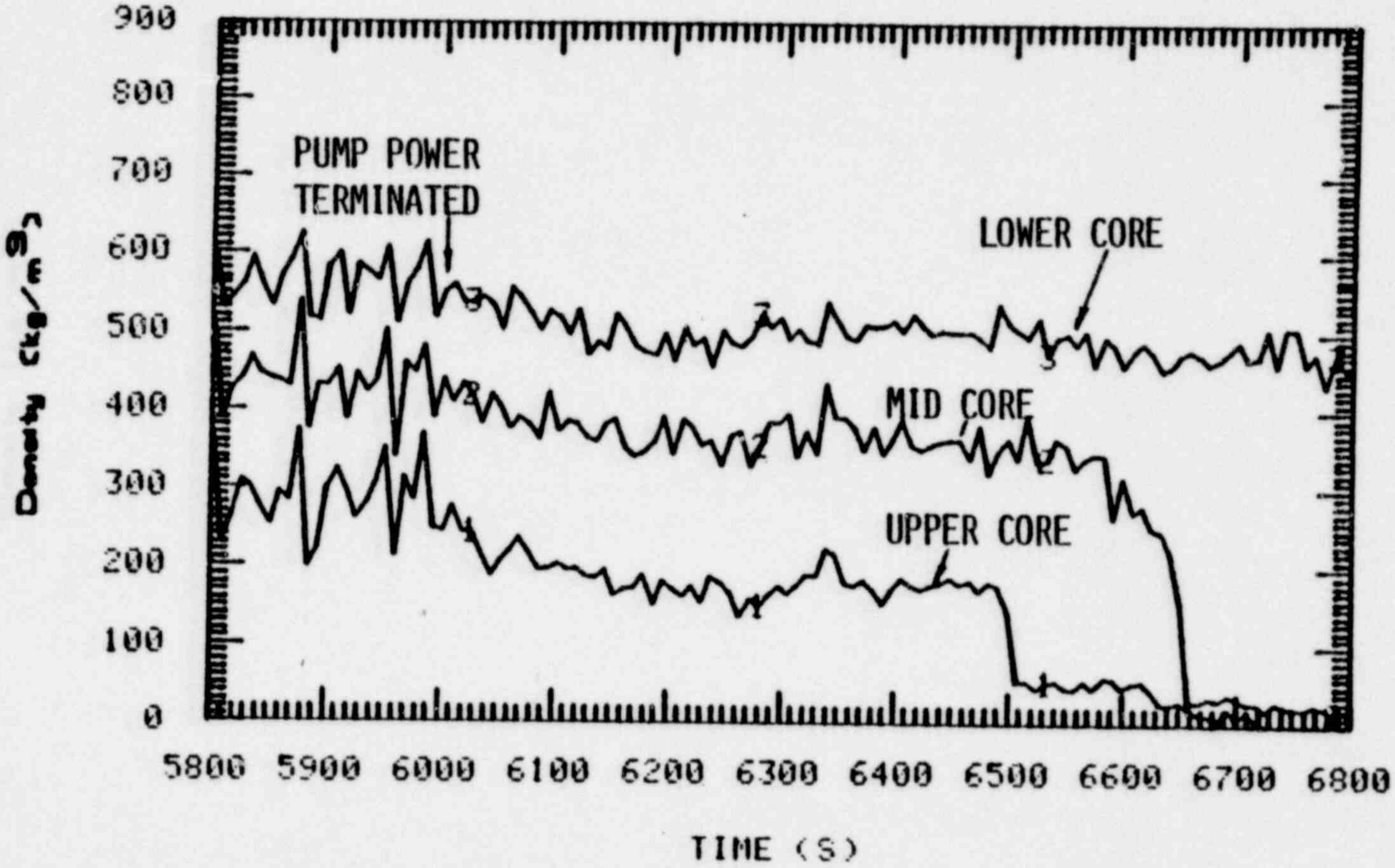
1004 250

CORE FLUID DENSITY DISTRIBUTION FOR TEST TMI-31



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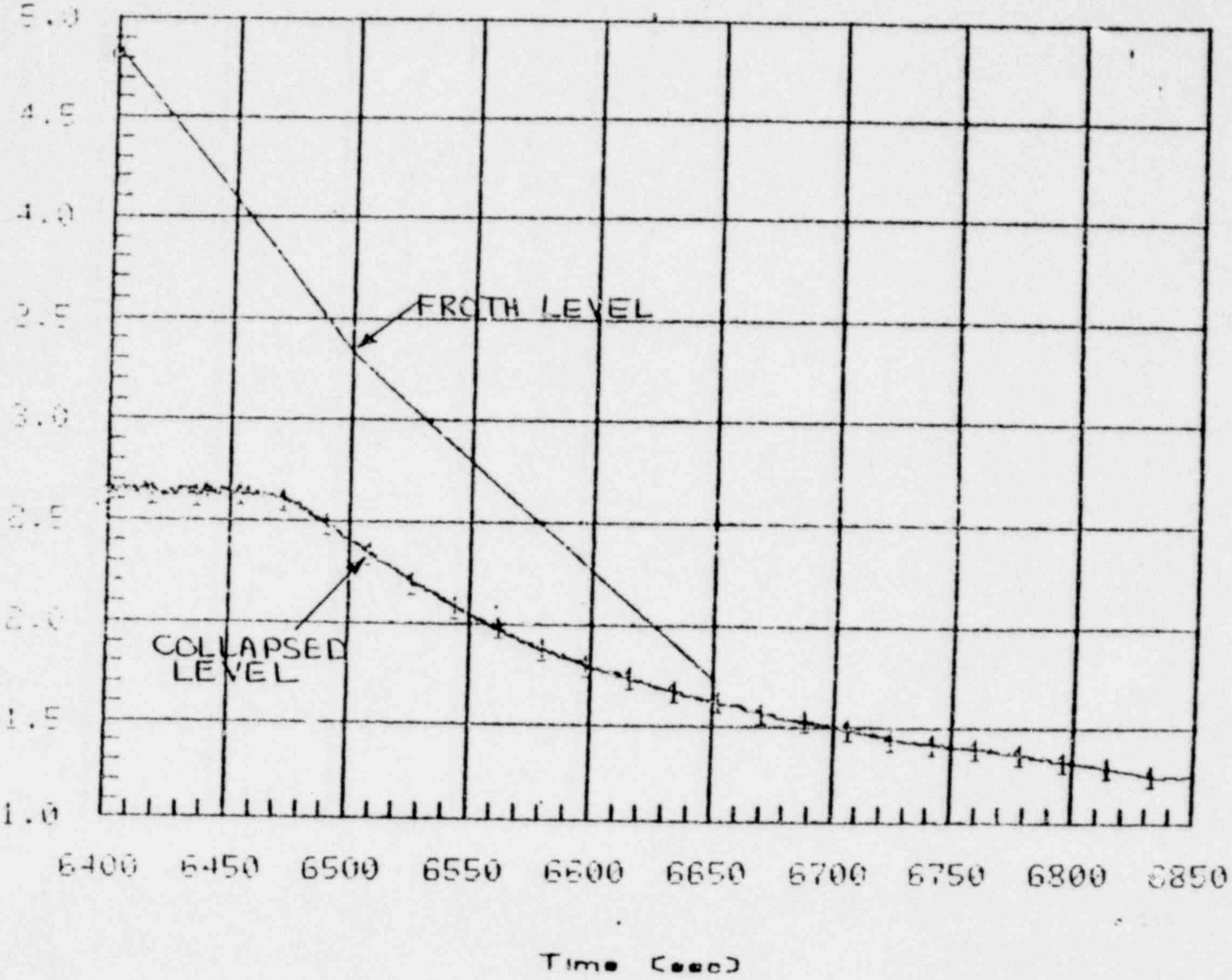
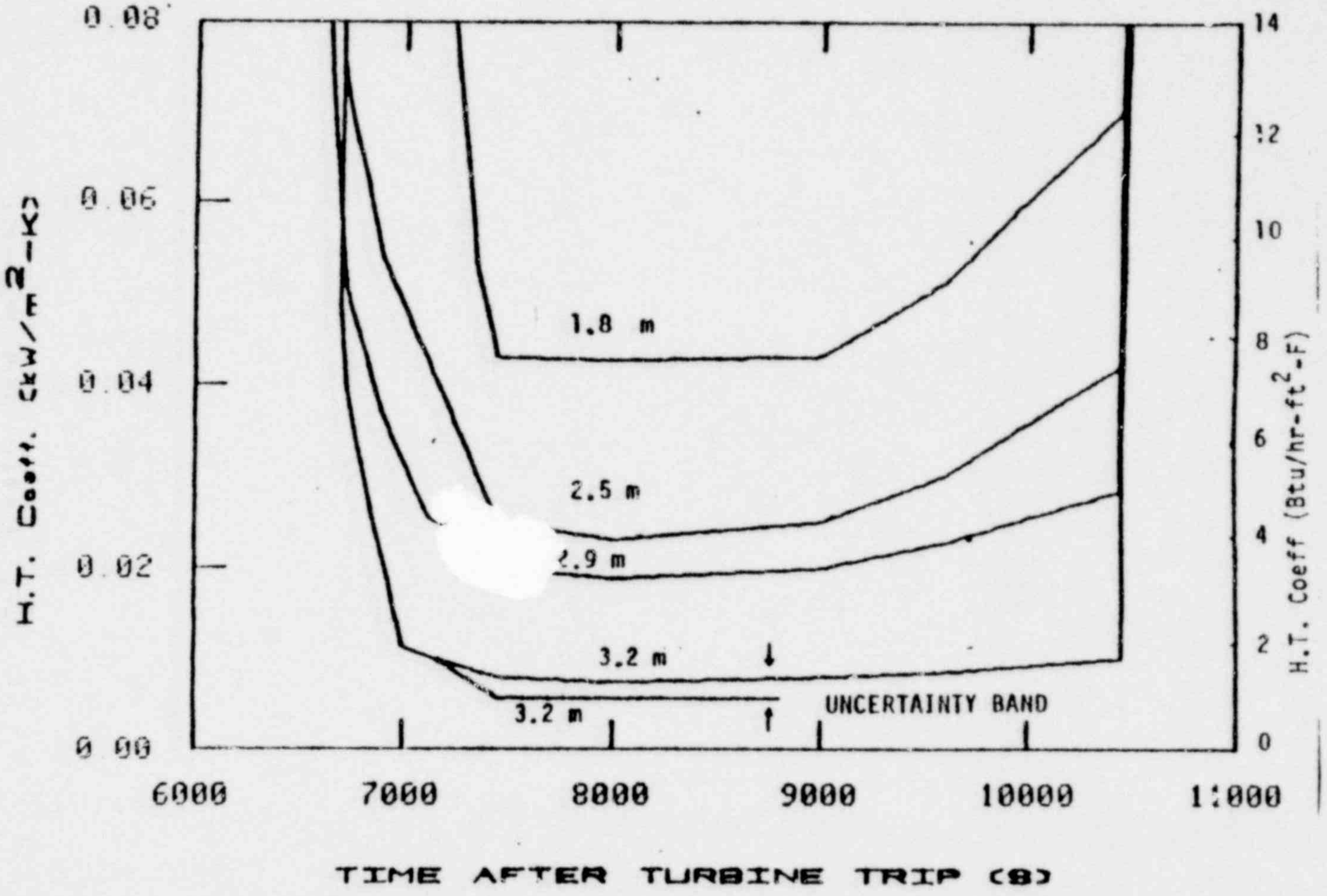
COMPARISON OF CORE FROTH AND COLLAPSED LIQUID LEVELS FOR TEST TMI-3I



HEAT TRANSFER COEFFICIENTS ESTIMATED FOR TMI BASED ON TSAT



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KEY POINTS

- OVERALL THERMAL-HYDRAULIC TRENDS OBSERVED IN THE SEMISCALE SIMULATIONS WERE SIMILAR TO THOSE OBSERVED IN THE TMI DATA

- SEMISCALE RESULTS TEND TO INDICATE THAT THE TMI PRESSURIZER LEVEL INDICATION WAS VALID FOR AT LEAST THE FIRST FEW HOURS OF THE TRANSIENT, BUT WAS AN INAPPROPRIATE INDICATION OF SYSTEM MASS INVENTORY

- SEMISCALE CORE RESPONSE AFTER PUMP SHUTDOWN INDICATES VERY POOR HEAT TRANSFER ON EXPOSED ROD LOCATIONS

- SEMISCALE RESULTS SUGGEST THAT SIGNIFICANT DAMAGE COULD HAVE OCCURRED IN THE UPPER SECTION OF THE TMI CORE

- SCALING DISTORTIONS MUST BE CONSIDERED WHEN CONDUCTING SLOW TRANSIENTS IN THE SEMISCALE FACILITY