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UNITED STATES NUCLEAR REGULATORY COMMISSION'S

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

DATE: Wednesday, November 8, 1989

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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

Thermal Hydraulic Phenomena Subcommittee

BWR T/H Stability Analyses/ABWR ECCS-LOCA Review

San Francisco Airport Hilton

Terrace Room

San Francisco, California

Wednesday, November 8, 1989

The above-entitled proceedings commenced at 8:30 o'clock a.m., pursuant to notice, Carlyle Michelson, committee chairman, presiding.

PRESENT FOR THE ACRS SUBCOMMITTEE:

I. Catton, Subcommittee Chairman

D. Ward

J. Carroll

C. Michelson

P. Boehnert, Cognizant ACRS Staff Member

ACRS CONSULTANTS:

J. Lee

M. Plesset

V. Schrock

H. Sullivan

C. Tien

PARTICIPANTS:

B. Shiralkar	Z. Rouhani
W.L. Weaver	G.E. Wilson
W. Wulff	D. Bessett
H. Scott	L. Phillips
J. March-Leuba	C. Sawyer
U. Saxena	G. Watford
J. Andersen	J. Shaug

PROCEEDINGS

[8:30 a.m.]

MR. CATTON: The meeting will now come to order. This is a meeting of the Advisory Committee on Reactor Safeguards, Subcommittee on Thermal Hydraulic Phenomena. I'm Ivan Catton, the Subcommittee Chairman.

The ACRS members in attendance are Carl Michelson and Dave Ward. We also have ACRS consultants, John Lee, Milt Plesset, Virgil Schrock, Mr. Tien. Virgil is not here yet. He's lost in the building.

The purpose of this meeting is to discuss the capability of the thermal hydraulics codes to model BWR core power and stability, and the key thermal hydraulic design aspects of the GE ABWR related to the ECCS and LOCA analyses.

Mr. Paul Boehnert is the cognizant ACRS staff member for the meeting. The rules for participation in today's meeting have been announced as part of the notice of this meeting previously published in the Federal Register on October 24, 1939.

A transcript of the meeting is being kept and will be made available as stated in the Federal Register notice. It is requested that each speaker first identify himself or herself and speak with sufficient clarity and volume so that he or she can be readily heard. We have received no written comments or requests to make oral statements from members of the public.

I have a few comments of my own before we start. There are two parts to the stability question. First, there is delineating the instability region. The AEOD special report, S-803 of March 1988, concluded that stability analysis methods were highly uncertain. This was based on their observation that the amplitude decay ratio calculated for the LaSalle incident was .6.

They also noted that similar results were found for Vermont Yankee. To further complicate matters, analysis of the LaSalle event by JAERI using RETRAN led to the conclusion that transient thermal hydraulics destabilizes the core. Hence, the quasi-static conditions do not yield proper bounds for the unstable region.

It's not an easy task to address such problems with a finite difference computer code. I'm looking forward to hearing how the various aspects of these problems are being addressed. In particular, I would hope that the owners group would share with us the results of the EPRI critical review, as EPRI chose not to do so themselves.

The second part of the stability study is to determine what the unstable behavior is like. This is a finite amplitude problem, and as such highly non-linear. Here are the existing codes I believe suffer more from cost of operation than lack of capability, providing that proper accounting of multi-dimensional neutronics are included.

This concludes my introductory comments. Are there any of the members who care to make further comments?

[No response.]

MR. CATTON: Consultants?

[No response.]

MR. CATTON: We'll proceed with the meeting, then. I call upon Dr. Shiralkar, the General Electric Company, to begin.

MR. SHIRALKAR: I am Shiralkar from GE. As the first speaker from GE, let me welcome you all to the Bay Area and the post-earthquake area. Let's hope we don't have any shaking events in the next few days.

Let me get on with introductory remarks on behalf of this morning's presentation. This morning, we're going to be talking about the TRACG code, which is a GE version of the TRAC BWR code and its application, particularly to stability. We'll be talking about some of the details of the models, the qualification, the application, and the limitations of the TRACG code.

With that, let me put up the proposed agenda.

[Slide.]

MR. SHIRALKAR: I'll lead off the brief introduction tracing some of the TRACG historical development and application. This could have been done probably better by the Idaho National Lab, but since we are leading off, we will try to provide a brief introduction.

After that, Mr. Andersen and Mr. Shaug will talk about the TRACG models that are significant to stability. Particularly, we want to talk about models that are different from other versions of TRAC. Specifically, the interfacial shear and sub-cooled boiling models which are different from the TRAC BWR code, and the numerical methods and kinetics models which are different from the TRAC-BD1-BF1 code that's maintained and developed by Idaho.

Following that, we'll provide a brief summary of the previous TRACG qualification of thermal hydraulics and kinetics and we'll follow that up by stability as a specific qualification that's partly in progress and that will be covered by Mr. Andersen on the numerical damping aspects, Jim Shaug on thermal hydraulic stability and on the transient application of the boiling transition actual correlation.

After that, Glen Watford will provide a description of the LEIBSTADT that we have and the LaSalle 2 unit. And then Jim Shaug will talk about the qualification of the same data with the TRACG code. Following that, I'll come back and summarize the capabilities, limitations and future plans for TRACG.

Now, we do have a lot of material and if you would like to emphasize certain aspects or deemphasize certain aspects, you can let us know and we'll try to follow your guidelines.

MR. CATTON: I'm particularly interested in how you qualify the code with respect to things like the mobilization in the vertical direction and the time step control.

MR. SHIRALKAR: We will be covering that.

MR. CATTON: Good.

[Slide.]

MR. SHIRALKAR: The TRACG code owes its origins, as does the BWR version of TRAC, to the Los Alamos TRAC-B1A code. Actually, we started with a version that was somewhere in between the P1A and PD2 in 1979. We collaborated with EG&G under a research program that was funded by NRC, EPRI and GE, and developed the BWR version.

The primary differences between the BWR and the PWR versions are, one, the addition or BWR components, such as jet pumps and separators; revising the structure of the code primarily because the core region in the BWR is associated with channels, whereas in the PWR it was associated with three-dimensional vessels, and that needed some restructuring of the code; and also, importantly, I think, the interfacial shear model, which is substantially different between the BWR and the PWR version.

So the collaboration with the cooperation EG&G and GE ended about 1984 at which time EG&G had the TRAC-BD1 version 12 code. The GE version was a little bit different because we had additional models in the two-step numerical method for the 1D and 3D methods, and an upper plenum mixing model.

Now, these models were made available to EG&G, but at the time they did not implement them into their version. So we started off with a somewhat different version in 1984. Now, beyond that point, we have incorporated proprietary models working by ourselves and those include the hot rod model, which is more significant for LOCA than for stability.

What it is is trying to account for variation void fraction. We've added 1D and 3D kinetics capabilities to the code. And we have made significant improvements in the numerical efficiency which improves the running of the code, but doesn't impact the results.

As far as TRACG applications, it's a general purpose BWR transient analysis tool because it has coupled thermal hydraulics and kinetics, control systems and balanced the plant components. So we have been using it for LOCA analysis, operational transients, the anticipated scram analysis, as well as stability, and we've applied it to operating BWRs and to advanced BWRs.

MR. CATTON: In the paper that somebody from GE presented at the meeting in Idaho on stability, they talked about explicit time-stepping.

MR. SHIRALKAR: Yes.

MR. CATTON: Is that the old method of time-stepping?

MR. SHIRALKAR: Yes. The explicit method was the one that was originally in TRAC and what we call the implicit is a refinement of that to make the method more implicit, the two-step numerical method and its refinements. We'll be covering that in more detail in the next session.

MR. CATTON: Okay.

MR. WARD: On the previous slide, if I understand then, the code that was available at the time of the LaSalle event was TRACB, or was TRACG available?

[Slide.]

MR. SHIRALKAR: This part of the development was performed jointly with EG&G and GE, but we both maintained our own version of the code. All the models that were developed by GE were made available to EG&G for incorporation into their code. So at this point, we had models that were reasonably close, but these models were made available to EG&G but not incorporated into their version.

So we ended up with a version of the code, B04, which is somewhat different from BD1 version 12. Now, beyond that point, then, the codes have varied somewhat.

MR. WARD: Okay. But the code you used --

MR. SHIRALKAR: Is TRACG.

MR. WARD: Was that available right after the LaSalle event?

MR. SHIRALKAR: Yes.

MR. WARD: Okay.

[Slide.]

MR. SHIRALKAR: In terms of specific stability analysis applications, we're trying to use TRACG to develop increased understanding of phenomena, particularly for the out-of-phase regional oscillations, because they do have the capability in TRACG for multi-dimensional analysis of kinetics and thermal hydraulics.

We are using it to demonstrate compliance with GDC-12, which has, basically, two parts to it, but one is that you either -- that oscillations in [illegible] and power should not be -- should be prevent, should not occur, or they should be detected and suppressed.

In order to prevent oscillations, we should be able to predict the onset of oscillations and demarcate regions on the power-flow map in which they will not occur.

In order to detect and suppress, we need to be able to calculate allowable applicative oscillations to prevent fuel damage or, more conservatively, boiling transition, and be able to calculate the continuation of localized oscillations with our instrumentation, the LPRM instrumentations. That's the detectability aspect.

We are also using the stability-analysis codes to look at the effects of design changes, work on fuel, and the operating strategies.

[Slide.]

Why did we choose TRACG for stability analysis? We feel that TRACG is our best shot and the best one availa^le to us for this purpose, and the reasons for this are the features that are inherent in TRACG.

First of all, we have inherent in the facial sheer model, whic' has extensively qualified for BWR void fraction predictions. Secondly, the three-dimensional inter-kinetics model is consistent with the GE design codes, and that means two things -- one, that was have all the nuclear data to use consistently, and secondly, that we have a model that has been used for many years to design and to monitor the plants. So, that's our qualification behind the model.

We had available numerical schemes for explicit or implicit integration, the cord of the modular structure that can represent components, facilities, and plants and can take advantage of the qualification at various levels of separate effects, components, facilities, and so on, and also, we had the possibility of exploring multi-dimensional effects in the lower plan and upper plan.

The cord has had extensive qualification. Previous qualification consisted largely of thermohydraulics LOCA-type qualification and some kinetic model qualification -- for example, the turbine trips at Peach Bottom and so on, and we are augmenting that now with stability-specific studies, both at the component level and the plant level.

So, we feel that, for us, this is the best choice to perform stability application analysis.

With that, I think I'd like to get into the meat of the topics and call upon Jens Andersen to talk about some of the modeling details, unless you have any questions on this part.

MR. ANDERSEN: My name is Jens Andersen, from General Electric.

I will present a number of subjects to you. First of all, I'll talk about some of the models in the TRACG Code that are particularly important for stability. I'll talk about the mathematical methods that are used in the TRACG Code and how we choose the time step and the integration technique. I will also give a brief summary of the previous qualification.

I have a lot of material, so if there is some of the material that you want me to go over lightly, please say so.

[Slide.]

MR. ANDERSEN: The models, in fact, that are particularly important for the capability are the basic models, such as the interfacial sheer model and sub-cool boiling model. These are the models that are particularly important for the predictions of the void fractions in the BWR bundle, and void fraction prediction is important for density vapor oscillations.

I will also talk about the numerical methods because of the inherent dissipation that exists in all numerical methods can cause a damping of the density weight.

[Slide.]

MR. ANDERSEN: Just as a brief introduction, let me just show you the equations in the TRACG model, which are the same for all TRACG code. We are solving the conservation equation for mass momentum and energy. The important terms for the interfacial sheer are the interfacial sheer term, which is this term here in the momentum equation, and what I'd like to describe is how we calculate that interfacial sheer.

MR. LEE: I have a question for you.

MR. ANDERSEN: Yes.

MR. LEE: In your TRACG Code, do you have the capability to go down to somewhat of a low-order model. Namely, instead of using six-equation model in its full glory, can you go down to four-equation, five-equation model?

MR. ANDERSEN: No, no. We don't have that option. It's always a six-equation model.

MR. LEE: So, when you mention the importance of this interfacial treatment, do you have any idea, other than relying on your code itself, to show, indeed, these treatments are so crucial? Could you touch upon that?

MR. ANDERSEN: Yes. I will try and talk with that in my presentation.

MR. CATTON: The equations that you just show, both the gas momentum and the liquid momentum, are not written in the conservative form itself. In the actual code, are they treated in conservative form or --

MR. ANDERSEN: No. That's the form of the momentum equation, which is the same for all the TRACG codes, both the P and the PWR version. It's really an equation of motion. It's not a conserving form. So, it doesn't perfectly conserve momentum.

MR. CATTON: The difference in technique does not?

MR. ANDERSEN: No, the equation itself is not on the conserving form of the momentum equation.

MR. CATTON: Well, one of the things that you have be careful about, in looking at a stability problem, is that you treat the inertial terms properly, and that, typically, means that you solve them in a conservative form.

MR. ANDERSEN: The continuity and energy equations are solved in a conserving form.

MR. CATTON: I see that.

MR. ANDERSEN: Now, the density vapor propagation is really not controlled by the momentum, and as we'll show you later in the qualification, we get good agreement with the data.

MR. LEE: I guess I don't understand your last comment, that density vapor oscillation is not controlled by the momentum conservation.

MR. ANDERSEN: It's not a dynamic -- it's not like an acoustic-wave propagation. The density vapor propagation is a disturbance in the void fraction that travels with the fluid velocity up to the boiling channel.

MR. LEE: Right, but the instrument itself is highly tied to and related to momentum concentration.

MR. ANDERSEN: The calculation of the pressure drop, the components of the pressure drop, such as the frictional and the static head terms in the momentum equation, yes, they are important. The convective terms in the momentum equations are not very significant for density wave oscillations and that's a term that's not on a conserving form in the momentum equation.

MR. LEE: Why do you choose this particular non-conservative form of a convective term?

MR. ANDERSEN: We haven't particularly chosen it. It was -- the code was developed that way originally, when it was first developed on Los Alamos. The momentum equation was put in this form. We have never had a need to change the formulation of that form. We have never observed any deficiencies in the capability of the code to putting the data.

MR. CATTON: Well, but you have not been looking at stability problems very much in the past, and what you are doing is you are propagating a bad practice. Stability problems -- you have to be very careful about these things.

MR. ANDERSEN: Well, we will show you some results later in the presentation.

MR. CATTON: I will be very interested.

[Slide.]

MR. ANDERSEN: Void fraction is very important. We have a flow machine map in TRAC that is roughly divided into a bubbly flow machine, and a dispersed annular flow. The bubbly is actually subdivided into bubbly and churn flow machine, and each of these regimes we have a separate correlations for the calculations of the interfacials here.

The model that exists in the code for the interfacials here was actually developed when we had a very close cooperation with ET&G in Idaho, and the model is virtually identical between the TRAC BD-1 code and the TRACG code.

[Slide.]

MR. ANDERSEN: The model is essentially based on the following assumption: that for adiabatic and steady state conditions, the two fluid model and drift flux model should be equivalent. We use the drift flux parameters to characterize the relative velocity and the flow distributions.

We then make the assumptions that the correlations that have been derived for the interfacial shear will also apply under transient conditions.

[Slide.]

MR. CATTON: I kind of missed a little bit. My mind wandered. How do you use the drift flux?

MR. ANDERSEN: Okay, we use the drift flux correlation to characterize the face and velocity distributions of the vapor and the liquid.

MR. CATTON: So then what do you do with the two-fluid model?

MR. ANDERSEN: We still use it in the two-fluid model and let me describe it to you. What we assume is that the interfacial shear is given locally by the relative velocity of the faces. The key assumption then is that when you have a cross sectional average equation, that wher you calculate the interfacial shear, you have to average the relative velocity over the channel.

The key thing is that the average of the relative velocity is not the same as the difference between the average velocity, because you have to account for the face and velocity distribution. That is -- this is the key assumption.

So what we do is that we describe the interfacial shear by the average of the relative velocity and what that does is that we use the expression from the drift flux model where you can show that having the C0 parameter that is the standard parameter in the drift flux model to describe the face and velocity distribution, you can show that the average of the relative velocity, which is really VGJ average divided by one minus alpha, is given by this expression where this is the average vapor velocity and this is the average liquid velocity.

That is what we use in the calculation of the interfacial shear. Now, the distribution parameter for C0, for the various flow regimes, we have taken that from the drift flux correlation. The other thing we do --

MR. LEE: I guess I'm still somewhat behind what you are trying to explain there. From the actual drift flux velocity or drift flux correlation -- which primary is used in your code?

MR. ANDERSEN: We used the C0 correlations. The other thing you do is that when you look at the momentum equation, you can easily show that the interfacial shear has to balance the buoyancy of the vapor face, so you get a relationship like this showing that the interfacial shear must balance the buoyancy.

Now, having the drift flux correlation and an expression for VGJ which was shown on the previous slide, we can use this expression here to determine the coefficient here, C, which is the friction factor or the factor that controls the interfacial shear.

Let me give you an example on how we do that. As I mentioned, we calculate the interfacial shear, the coefficient times the average of the relative velocity squared. Now, this coefficient; it's convenient to have this in this form where we have a direct coefficient times the density of the continuous face times the parameter that's either a particle size or an interfacial area per unit volume.

So, for example, for bubbly flow, we are using ISHII's recommendations which we are actually using for all the flow regimes. By balancing the interfacial shear and the buoyancy, we get this expression here. We assume that the interfacial area per unit volume is given by critical Weber number.

We use ISHII's recommendations for the relative velocity, and substituting these expressions into this equation here, we can come up with a relationship between the di- coefficient and the critical Weber number, which is just a function of the void fraction.

MR. CATTON: How important is this particular step? Once the instability has occurred, you have accelerating/decelerating flow, which means the Weber number criterion really isn't very good. It's okay for accelerating flows, maybe.

It certainly isn't for decelerating flows. You only need to --

MR. ANDERSEN: Well, the flow is still accelerating up to the channel. You are right that for very large oscillations you would get periods of deceleration.

MR. CATTON: Even just the flow slowing down;

certainly it's all in the upper direction, but you only have to imagine this room filled with fog, and what your code would calculate after one time step.

You'd have all the water on the floor. We all know that's wrong. So, I can understand the Weber number criterion for the accelerating flow, but not for decelerating flow.

But I don't know what this would do to you, because you are using drift flux that's based on correlations. So I don't understand why you are doing this at all.

MR. ANDERSEN: Well, we decided to do that because you have expressions for the di-coefficient that are good for very idealized flow schemes such as spherical particles. You don't have good expressions for what the di-coefficient is, for instance, for a churn flow machine.

MR. CATTON: We all know as well that in codes such as TRAC, that's one of the weaknesses.

MR. ANDERSEN: So, to overcome that, we used a rather pragmatic approach. We said that we have a very large base of void fraction data that have been correlated in terms of drift flux parameters and as I mentioned earlier, it can easily show that there is a correspondence between the parameters describing the interfacial shear and the drift flux parameters.

We used that to determine these parameters that control the interfacial shear. In the fashion I'm showing here, this is the parameter for the distribution parameter.

This is how we did it for the bubbly and the churn flow regime. I have a table here that briefly summarizes the key term for the other flow regimes.

[Slide.]

MR. ANDERSEN: Again, in all cases, we are using the recommendations that ISHII made a number of years ago, and we used his drift flux expressions to come up with equivalent expressions for the interfacial shear, where these are the di- coefficient controlling the interfacial shear.

We have one other modification to the interfacial shear which goes beyond ISHII's recommendations which is for the region of sub-cooled boiling. In sub-cool boiling, we apply a multiplier to the distribution parameter, C0, which is a function of the liquid and enthalpy HDL is the enthalpy for onset of net vapor generation.

HF is the saturation, so at the onset, C0, the distribution parameter is 0. When fully developed boiling has been obtained, the multiplier is one. The calculation of the enthalpy for onset of net vapor generation is given by the Saha-Zuber model.

Furthermore, in calculated the subcool boiling, we use a model that's due to Rouhani and Bowering where we essentially take the wall heat flux and just separate it into two terms; one term which heats the liquid and the other term that generates vapor.

This expression which is due to Rouhani and Bowering, shows hcv we calculate the separations of the heat flux. Again, at the onset of net vapor generation, all the heat flux goes to heating of the liquid, and as the liquid reads the saturation temperature, all the heat goes into vapor generation.

In summary, this is the model we have used.

[Slide.]

MR. ANDERSEN: First I have a couple of slides that shows the comparison between the TRACG calculated results and measured void fraction data for a tube. This is a pressure of 5.5 mega-Pascal. It shows void fraction as function of equilibrium quality.

MR. LEE: May I ask you a question at this stage?

I see that the agreement between the Christensen data and TRACG predictions for void fraction profile is very good, looks good, but what is this good agreement, apparent good agreements mean in terms of the interfacial treatment and other things that you have put into TRACG code?

MR. ANDERSEN: Well, what it means is that we are calculating the relative velocity between the phases and we are calculating the interfacial shear correctly.

It also means --

MR. LEE: Maybe 1 didn't make myself clear. Does the momentum equation play a role, a significant role, in the void fraction prediction?

MR. ANDERSEN: Yes, because it's from the solution of the momentum equation that we calculate separately the vapor velocity and the liquid velocity and the velocity difference between the two faces. Without an accurate calculation of the two velocities, you cannot accurately predict the void fraction.

MR. LEE: But you are using empirical correlations such as Saha Zuber's correlation, which in fact fits the sub-cooled information data exactly, so you are imposing your model to predict in a way what Saha Zuber's model of correlation represents -- so if you are off a little bit in pressure and other predictions perhaps you'll be getting the same answer.

MR. ANDERSEN: The Saha super model is a model for the onset and that vapor generation and sub-cooled boiling which basically would control this point here on the slide when you start generating void fraction.

Rouhani-Bowering model and the way we multiply the C-shear would control particularly this region here, the void fraction prediction.

MR. LEE: Suppose -- let me go back to one of my earlier questions -- go back and try to use the crudest of possible two-face representations, such as homogeneous equilibrium model.

MR. ANDERSEN: Yes.

MR. LEE: You might be able to predict the void profile fairly well with the exception of perhaps near the onset on the sub-cooled nucleate boiling.

MR. ANDERSEN: Well, if you used the homogeneous equilibrium model, first of all you wouldn't generate any void fraction until you reached an equilibrium void fraction.

MR. LEE: That's what I said, with the exception of near the onset of sub-cooled nucleate boiling you might be able to predict void profile fairly well.

MR. ANDERSEN: In this region here you would typically overpredict the void fraction because using a homogeneous model tends to overpredict the void fraction for two-face flow, whereas when you have relative velocity you get more face abrasion and you get lower void fractions.

MR. LEE: The homogeneous equilibrium model -- my question is do we really have a case whereby we can claim that the testing and validating this obfuscated interfacial momentum transfer models and things like that. That really is my concern here.

MR. ANDERSEN: The approach that we have taken is that we have taken models that have existed in the literature for the interfacial shear. We chose to use the drift flux parameters to characterize interfacial shear. We have taken models for sub-cooled boiling that existed in the literature such as the Saha Zuber and the Rouhani-Bowering model and we chose those because we felt that those were some of the best available models.

What we did was then that we implemented them into the TRACG Code and this is the type of agreement we get compared to data which to me is a fairly good indication that we accurately predict the interfacial terms.

MR. CATTON: In using all these correlations I would hope that you would.

One of the problems in the past with the TRACG Code was that if you predicted the void fraction or you somehow tuned the friction, which is what you've done, you've tuned the friction with the drift flux. The drift flux is known to do a good job in these kinds of problems.

Now you have your void fraction correct, but what about the heat transfer, because at least in the TRACG Code for the PWR's if you get the void fraction right you don't get the heat transfer right. If you do them both incorrectly, you get the right temperatures and heat flow.

You're showing us a piece of the picture. I think use of drift flux is eminent good sense because that's where the data is but I am a little unsettled about the other part of the problem.

MR. ANDERSEN: The only part of the heat flux that this slide would show would be how to calculate the heat flux in the sub-cooled boiling region because in the fully-developed nucleate boiling the wall superheat is not very large. Sub-cooled boiling region I think it does indicate that we do a reasonably good job in separating the heat flux into the term that heats the liquids and the term that controls the net vapor generation.

MR. CATTON: If you were to plot on that same graph the equilibrium you would have different values for the void fraction.

MR. ANDERSEN: Yes.

MR. CATTON: The difference between the two is an indication of the non-equilibrium in the flow.

MR. ANDERSEN: Yes.

MR. CATTON: That means you have interchange going on between the liquid and the vapor --

MR. ANDERSEN: Yes.

MR. CATTON: -- and all sorts of things.

MR. ANDERSEN: Yes.

MR. CATTON: It's that arena that gets all mixed up with these kinds of codes.

MR. ANDERSEN: But as you can see in this region here we have a very substantial amount of non-equilibrium.

MR. CATTON: That's right.

MR. ANDERSEN: And we still predict the void fraction fairly accurately.

MR. CATTON: Based on your correlations and an adjustment of friction factors.

MR. ANDERSEN: Based on the correlations for the interfacial shear and based on the correlations we used for how to calculate the wall heat flux.

MR. CATTON: So do you have your own heat transfer package is this code, or do you use the original TRACG PWR?

MR. ANDERSEN: No, the heat transfer packets that exist in this code is also more developed jointly between General Electric and EG&G in Idaho.

These models that controls the interfacial shear, the heat transfer particularly in the sub-cooled boiling were developed very early in the program during the refuel program we worked together with EG&G Idaho, and those are the same model more or less than exists in TRAC BD-1 as in TRACG. They differ from the models that exist in the PWR version of the code.

In this arena, primarily in the area of the sub-cooled boiling model, when we started on the code development there was no good sub-cooled boiling model in the TRAC code.

MR. CATTON: I think what would be convincing would be a graph like this one, also a graph showing the PIN wall temperature.

MR. ANDERSEN: Wall temperature is not going to be very large for this case. I have a couple of other slides later on where I show some temperature traces.

MR. CATTON: Maybe you are missing what I am driving at. I am interested in being sure that you just haven't built a bunch of compensating errors into what you are doing.

MR. ANDERSEN: I understand your concern.

[Slide.]

MR. ANDERSEN: I have a couple of other slides showing the qualification. This is the same tube data, slightly different pressure, 6.9. These are two data. We also performed comparison against void fraction data in bundles.

MR. SCHROCK: In that last one, it appears that there is a considerable difference between the Saha-Zuber, one set of boiling, and the data for the case. Is that something that you find typically?

MR. ANDERSEN: This one here, yes. You get some difference down here that we calculate a net vapor generation slightly earlier than this particular data. Let me show some of the other data which we have which had taken them in large bundles.

[Slide.]

MR. ANDERSEN: These are two of the test cases that are from the test facility which is a bundle with 36 heated lots. This one here has very little inlet subcooling. This case here has very high inlet subcooling, about 25 degrees celsius. In this case here, we actually -- the production is slightly in the opposite direction that we predict on that vapor generation slightly later than the data.

So in answer to your question, I think on average, we find that the Saha as a super-model generally predicts the onset of net vapor generation very well.

[Slide.]

MR. ANDERSEN: These two comparisons have very little inlet subcooling, but primarily shows -- particularly this one here, a comparison to void fraction data for very high values of the void fractions.

So as kind of a conclusion to my talk about these basic models that we believe are important to stability, I've described the models we used for the interfacial shear and for the subcooled boiling. Our conclusion, which is based on the qualification that we have performed, of which I've shown you a couple of examples, is that we accurately can predict void fractions.

MR. CATTON: Can I imply that you can also predict the heat transfer very well?

MR. ANDERSEN: When I come to the second part of my presentation where I'm going to summarize the qualification that we have done, I'll show you a couple of examples on temperature predictions.

MR. CATTON: And the heat flux.

MR. ANDERSEN: Not the heat flux, because you cannot measure the heat flux, but you can measure what temperatures.

MR. CATTON: You really need both to be sure. I can adjust the heat flux and match any void fraction data you might want to show me. So somehow that needs to be addressed a little.

MR. ANDERSEN: But in this case here, when you're talking about steady state measurements, the heat flux is whatever the power generation is.

MR. CATTON: That's true.

MR. ANDERSEN: The temperature applied is what will tell you whether the heat transfer is correct.

[Slide.]

MR. ANDERSEN: Another area of the code which we believe is important for the prediction of stability is the numerical methods in the code. Let me just put up this slide here, which gives kind of a history of the numerical method in the TRAC code.

[Slide.]

MR. ANDERSEN: The original, when we started the code development starting from the PWR version of the code, we had a similarly implicit formulation of the momentum equation, similarly implicit in order to be able to exceed the acoustic Courant limit, which is the time-step larger than the transient time of an acoustic wave across a node.

The margin energy equations were explicit, which means that the time step is limited by the material. During the refueling program, we developed and implemented a two-step method for the one-dimensional components, similar to what exists in TRAC-PF1, which allowed us to exceed the material Courant limit for the one-dimensional component and only have the limitation for the three-dimensional components that we had implemented in 1982.

At the end of the programs where we developed TRAC in cooperation with EG&G in Idaho, we had also implemented the two-step methods for the three-dimensional components, which was available in 1985. Since then, we at GE have continued separately on a development of the TRACG code.

We have further developed the two-step method in order to provide a fully implicit solution of the continuity and energy equations. That was implemented into the code in 1987.

And some of those results I'll show you later, they were also presented at the stability symposium in Idaho.

When we started looking at the numerical methods and its effect on stability, we also developed an experimental second order method to try and have higher order accuracy methods in order to quantify the accuracy of the other methods which are first order methods.

MR. TIEN: What do you mean experimental?

MR. ANDERSEN: I mean we have not implemented it into our production version of the code. It was a very separate effect, starting where we implemented higher order integration technique into the code. It has very limited applicability and it has not been extensively qualified, but we used it to compare the results to the lower accuracy methods.

MR. CATTON: What is the second order in time?

MR. ANDERSEN: The second in order in time and space. I'll show it to you later on.

MR. CATTON: Because usually when you exceed the Courant limit, you need to do some filtering or damping of some of the noise or your program won't run right.

MR. ANDERSEN: That is correct.

MR. CATTON: And now you're looking at a stability problem where you want things that are real to grow.

MR. ANDERSEN: Yes.

MR. CATTON: Yet, you may be filtering it if you exceed the Courant limit.

MR. ANDERSEN: You're taking my words away from me. That's exactly what happens with the implicit methods, and I'll show it to you.

[Slide.]

MR. ANDERSEN: The solution for conservative equations for mass momentum and energy. The momentum equation is a semi-implicit solution. We have either an explicit solution or a fully implicit solution available.

[Slide.]

This slide, briefly, shows the momentum equation. We have here on the lefthand side, the mesh is used for the velocities as given at the node boundary. Other properties, such as void fraction and pressures, are given at the node centers.

So this term here represents the time derivative at this node boundary here. In dealing with the convective term, we use a domiciled approach where we take this velocity here times this velocity difference. If the velocity is positive, using these two; if it's negative. This is standard for all versions of the TRAC program.

The pressure difference between the two, the new pressures are used and that's really the origin of the expression semi-implicit. Using the new pressures, you can exceed the sonic Courant limit.

MR. CATTON: Before you take this away. This is what we used to call upwind differencing.

MR. ANDERSEN: Yes.

MR. CATTON: And upwind differencing is known to be highly damping.

MR. ANDERSEN: It is.

MR. CATTON: You can look at a problem that you know is unstable and do a calculation with this differencing scheme and get nice laminar solutions. You can get solutions in a boundary layer and Reynolds numbers of ten million and it would be stable.

MR. ANDERSEN: Again, if you'd let me go on with my presentation, I think I'll answer most of your concerns.

MR. CATTON: I'm sort of warning you where -- this is the area that gives most of us a little bit of concern about the use of a code like TRAC. I think if you could, if you could demonstrate why, for the problems you're looking at, this doesn't matter. I think you have to do it more with analysis than with comparison with experiment, because there are too many tunables in the TRAC code.

MR. ANDERSEN: If you'll let me show the next couple of slides, I'd like to show that.

[Slide.]

MR. ANDERSEN: When you linearize the momentum equation just taking the time that goes for the new time-step, you essentially get an expression that relates the new velocity to the new pressure difference across the node boundary.

[Slide.]

MR. ANDERSEN: In the explicit formulation -- and just as an example, I'd like to show how it's done for the vapor continuity equation. The liquid and continuity in the energy equation are similar. You have here the change in mass in and out. Here you have the in-flow and, again, you use a domiciled technique or what you call an upwind differencing.

This is the inlet flow and you use the domiciled node to describe the flow across the inlet boundary, and similar for the exit. You have here vapor generation term. One thing that's important to recognize in showing this equation here is that this is the conservation equation for vapor mass written on a conserving form.

It does have non-linear terms in the equation. When we use the explicit formulation, the only non-linear term is this term here. We do solve the full non-linear equation and we solve for the new void fraction and density to enable the process. Essentially what you do is you take tne non-linear term, such as the part of the void fraction and density, and you devise an iterative scheme where you linearize around the late solution which you got insulation K, and you basically solve for the next iterative value by linearizing and changing void fraction and the primary dependent variables you have in track is temperature and pressure.

And you keep iterating on that until you get a converged solution. So one concern that has been raised about the numerical method in TRAC is do you actually solve the full non-linear equation. You do solve the full non-linear equation.

[Slide.]

MR. ANDERSEN: The other option we have is a fully implicit solution where the continuity equation for the vapor would have this for the convector term here. It's still a domiciled difference in space, but instead it's a new time-step property for both void fraction and density.

Again, here, you linearize these terms in the same way as we did, as I showed you before, and you use an iterative approach to solve the full non-linear equation.

It becomes a little more complicated for the --

MR. LEE: Excuse me. I'm somewhat lost. Where does this two-step iterative scheme come in and semi-implicit scheme come in? Could you comment on those relative to what you have? You have explicit scheme and then implicit.

MR. ANDERSEN: Yes. As a matter of fact, the two-step method was the method that was originally developed by Los Alamos, where you used a combination of new and old properties in the convective terms. Generally, you get a problem when you have new terms if you have new properties from other than the cell you are linearizing around.

So the old two-step method used a combination, where if you had out-flow out of the cell, you used the new time-step property; whereas, if you had in-flow from a neighboring cell, you used the old property.

Now, that would give you a mass conservation error. So later on, you needed a correction in order to correct for the mass conservation error. That was what we had when we originally implemented the two-step method. Now, later on, we have further developed a method to get a fully implicit solution so that both the in-flow and the out-flow are given by the new properties.

When you iterate on the solution until you get a fully converged solution, you actually don't need the second step. You can still include the convector step and you can bring the mass conservation error down to the machine accuracy. But in principle the second step is not needed.

MR. LEE: You are not using the second step.

MR. ANDERSEN: We are using it in order to control any small mass balance error we might introduce by not having absolutely perfect convergence. We usually converge until our error is smaller than a tolerance that's given by a parameter you specify and is input into the code. That would control the magnitude of the mass and energy balance you might commit.

By having the second step, you can actually show that you can perfectly satisfy mass conservation and you can show that you get absolutely no mass conservation error or that your error is given by the machine accuracy.

MR. LEE: So can you say that you are using two-step semi-implicit scheme or implicit scheme?

MR. ANDERSEN: We are using a fully implicit solution. We are using the second step still in order to get perfect mass conservation rather than having a mass conservation that is, say, ten to the minus four; what you would usually get if you had an iterative solution. You don't keep iterating on your solutions indefinitely. At some point you will stop.

MR. LEE: It's the two-step implicit scheme that you're using, then.

MR. ANDERSEN: Yes. You can call it that. We don't need, as I mentioned, we don't need --

MR. LEE: Right. Still you are using the two-step approach.

MR. ANDERSEN: Yes.

MR. CATTON: Have you tested this particular formulation for the numerical diffusion?

MR. ANDERSEN: I have a presentation later on where I will show that to you. As I mentioned, we have the fully-implicit method. The main advantage of the fully implicit method, of course, is that you can exceed the material Courant limit.

[Slide.]

MR. ANDERSEN: This shows a comparison from the PSTF test facility. This is a -- let me show you the previous slide.

[Slide.]

MR. ANDERSEN: This a simple vessel, where we had water and we pressurized at about 1,000 psi, blew down to a line here, and this shows the comparison on how well we can predict the pressure for various values of the time step size.

This is plotted in terms of the maximum coolant limit, which occurs at the choke flow plane, and shows comparison for coolant numbers ranging from 1 to 200, and you see very little sensitivity in the calculated results and, in all cases, good agreement with the data.

MR. CATTON: What about mass flux and non-equilibrium and all these other things? Do they get predicted well, too?

MR. ANDERSEN: You can't predict the pressure well if you don't predict the mass flux out to the break.

MR. CATTON: I don't know about that.

MR. ANDERSEN: We do have comparisons to the mass flux. We have made those comparisons. I did not include it in the presentation today. Mass flux is predicted. Well, later on, I have a slide I'd like to show you which also shows the void fractions inside the vessel.

MR. CATTON: We would like to see sort of all them together. In the past, everybody has used pressure.

MR. ANDERSEN: Yes.

MR. CATTON: It turns out it's pretty easy to get reasonable predictions of pressure. It's the other variables that give you a headache.

MR. ANDERSEN: I can show you a comparison of the void fraction distributions. We have made the comparisons for the mass flux, and they also agree well with the data.

These reports -- results are reported in the document we issued as part of the fuel reflux, and the first programs were TRACG. The BWR version was developed in cooperation with EG&G in Idaho. So, those comparisons have previously been published.

MR. SCHROCK: Is this test a single-phase discharge?

MR. ANDERSEN: This particular test is single-phase vapor during the entire test. There are other tests where the standpipe inside the vessel was removed, where you had two-phase discharge.

MR. SCHROCK: So that the pressure as a function of time is more determined by the relaxation of this liquid and not by the processing described in the equations that you're discussing.

MR. ANDERSEN: But let me just point out, the only purpose I had in showing the slide was as a demonstration of the capability of the implicit numerical scheme that we could exceed the material coolant number and still get accurate predictions of very little sensitivity to the calculated results. It was not purpose for showing this slide to demonstrate anything about critical flow model.

MR. SCHROCK: No, I don't mean critical flow model. I mean the relaxation of the liquid.

MR. ANDERSEN: Yes.

MR. SCHROCK: Okay. It's the flashing in the liquid.

MR. ANDERSEN: Okay. It was not the intent to discuss any of these models. The intent was to show that we have an implicit numerical scheme in the code. We can take very large time step and still have very little sensitivity in the calculated results to the time step size.

MR. CATTON: This is a rather limited demonstration, in that the problem is relatively simplistic, compared to what you have to deal with in the core of a reactor. I think we need a stronger problem to test the argument you're trying to make.

MR. ANDERSEN: Yes. That's true.

MR. CATTON: There are too many counter-examples of what happens to you when you use this upwind differencing, too many for this particular experiment to counter.

MR. ANDERSEN: But again, as I mentioned before, a little later in the meeting today, I have a presentation on the numerical damping, and I would like if we can defer the discussion until that point.

MR. CATTON: We will defer it only if you don't make great claims in your comparison of experimental data as you go along.

MR. ANDERSEN: Okay.

MR. CATTON: Fair enough?

MR. ANDERSEN: Yes.

MR. TIEN: You may work up some validation of data.

MR. ANDERSEN: I am planning to have two more presentations -- one which is an overview of some of the previous qualification of the TRACG program and another presentation which is a particular study of the effect of numerical damping on the code's capability in predicting density wave oscillations.

MR. TIEN: For your last point, did you use some other high-order to validate?

MR. ANDERSEN: Both that and exact analytical solutions.

This is kind of my conclusion on some of the presentations of our models. I talked a little about some of the basic models, such as interfacial sheer and heat transfer and numerical masses.

We have another part of this presentation that deals with the kinetics model, and Jim Shaug will be giving that presentation.

MR. TIEN: Could I ask one general question on the numerics? In terms of when you use different ionizing schemes, do you consider whether they are conserving, non-conserving, what kind of a TRACG would be on your results?

MR. ANDERSEN: The continuity in the energy equation is --

MR. TIEN: Not the equations, the differencing schemes.

MR. ANDERSEN: The differencing scheme that we use are conserving in terms of energy. So, as energy is conserved, the momentum equation in itself is not on a conserving form. So, there the question of whether the different scheme is conserving is kind of academic.

MR. TIEN: I'm not sure about that. Perhaps that how you deal with it, but there are finite differences, though.

MR. ANDERSEN: Yes, I agree. We are conserving, as I mentioned, on mass and energy. We are not conserving on one particular term in the momentum equation, which is the convective term.

Now, the density wave oscillations is not controlled by acoustic phenomena. We are talking about two different frequency scales. When you are talking about density wave, you are talking about oscillations that occur with time periods of about 2 seconds. When you are talking about an acoustic wave travelling that's reversing, say, a nuclear fuel channel, you are talking about time scales in the order of milliseconds.

MR. SHAUG: I am Jim Shaug from GE.

As part of the presentation, I'd like to give you an overview of the various kinetics models we have available in TRACG. The models we have available are applications of various design models that we have at GE.

[Slide.]

MR. SHAUG: We have a point kinetics model. In this model, the total power level will vary as a function of time.

The spacial distribution of power is held constant in this model. The thermo-hydraulics is then collapsed to provide a single core average parameter for reactivity feedback.

We also have available a 1-D kinetics model. In this model, total power and the core average axial power distribution is allowed to vary with time. In this model, the bundle-to-bundle radial power distribution in the core is held constant. From this model, the thermo-hydraulics is collapsed to provide a core average axial parameter for the reactivity feedback.

We also have an application of a GE 3-D kinetics model, in which the power level and the spacial distribution, both radial and axial, is allowed to vary as a function of time. In this case, the hydraulic channels provide characteristic response for a specified group of kinetics bundles.

MR. SCHROCK: It's limited, then, to symmetric distribution?

MR. SHAUG: No. You mean by the use of the term "radial"?

MR. SCHROCK: Radial and axial.

MR. SHAUG: Radial is actually X-Y. Okay? In terms that we commonly use, we think of bundle-to-bundle as a radial distribution. So, it is an X-Y-Z formulation.

[Slide.]

MR. SHAUG: Just a quick word about model consistency -- each of the models shows in the previous slide is formulated consistently with the GE 3-D BWR core simulator. Each model obtains its nuclear data and operating conditions from the BWR simulator.

Just a word about the simulator -- it's the basic tool used for core design. It's a 3-D coupled nuclear thermohydraulics code for analysis of a BWR core, uses a one-group diffusion equation in coarse mesh, one mesh per bundle, and cross-sections are derived from three-group cross-sections from last physics codes.

[Slide.]

Just to give you an overall view of how we've implemented the kinetics models, whichever one is chosen for a particular application and how we've interfaced it with the hydraulics, the kinetics models will calculate a power. That's then transferred to a fuel heat transfer or through direct moderator heating directly to a channel and bypass hydraulics.

The kinetics models obtain their input from the heat transfer package, which would determine the fuel temperature. It was also obtain some input from the hydraulics package and the channeling bypass through moderator density.

MR. CATTON: And I can replace thermohydraulics with TRACG?

MR. SHAUG: You can replace it with -- well, TRACG is the whole package, would be TRACG.

MR. CATTON: Okay.

MR. SHAUG: The thermohydraulics would refer to the in-channel as well as bypass region.

[Slide.]

Just a quick summary of the 3-D kinetics models, again, consistent with the GE design core simulator -- 3-D finite difference model, one neutron energy group. We use six delayed neutron precursor groups. We solve the equation on a mesh, one per bundle, in the X-Y plane, and we can solve up to 25 meshes per bundle in the axial direction.

MR. CATTON: Do these meshes match with the thermohydraulics or is this a separate --

MR. SHAUG: It's a separate mesh. Typically, for our application, we match the mesh axially, but we don't use this finite hydraulic channel grouping, as we do in the kinetics.

MR. CATTON: In several papers I have read, including one by Andersen, it's noted that you need meshes on the order of a couple of inches or less in order to get accurate predictions of the stability. How are you getting around this?

MR. SHAUG: Well, at 24 or 25, we're using meshes on the order of 6 inches. We have done studies where we have -- I think, probably, in the stability comparisons that you are referring to, we don't see that much sensitivity when we go below the 6-inch level. Okay? We see some sensitivity, but not a great deal.

MR. CATTON: Now, the Japanese study, based on retran, felt that they had to use 2/10ths of an inch -- 2/10ths of a foot, which is quite a bit less than 6 inches.

MR. SHAUG: I think, again, as Jens referred to earlier, we have stability qualification that we'll get into and show the sensitivity.

MR. LEE: There may be a distinction between the hydraulics calculation and the neutronics calculation.

MR. CATTON: Well, that's why I asked the question and they said that there was not.

MR. SHAUG: There typically is not.

MR. CATTON: There could be, but typically there is not.

MR. LEE: Right. So, for the calculation of power distribution, maybe 6 inches would be sufficient, but with thermohydraulics, one could go to a much finer mesh.

MR. SHAUG: That is a possibility. That's what I was trying to get at.

We also time-dependent positioning of control rods, and as far as geometry options, we can go full core through symmetry.

MR. LEE: How do you handle the interface between, I guess, the feedback calculations, temperature and density feedback on cross-sections?

MR. SHAUG: You mean how do we pass the hydraulic calculations into the kinetics?

MR. LEE: Right.

MR. SHAUG: When we have a different grouping of hydraulic channels and nuclear --

MR. LEE: Right.

MR. SHAUG: Okay. The heat transfer and thermohydraulics would be solved for a specified number of hydraulic channels. Their calculation of fuel temperature and density are then applied to a specific number of kinetics channels. Okay? So, we essentially map the kinetics channels and the hydraulic channels, and the hydraulic channels would get their power as the average of that particular group's kinetics response, and then those kinetics channels would get their hydraulic and fuel-temperature input as that specific hydraulic channel's response.

MR. LEE: In your neutronics model, you're using one energy group. So, basically, you are using infinite multiplication factor and migration area as two controlling primaries, and then you try to represent the fuel temperature and density or void feedback on those two parameters?

MR. SHAUG: That's right.

MR. LEE: And then you'd like to somewhat average of the thermohydraulic channel groups to calculate average void fraction in the average fuel temperature and then feed that back to the neutronics?

MR. SHAUG: No. We take the neutronics and, for a specified number of neutronics channels, the hydraulic response of those channels would be represented by one single-track hydraulic channel.

MR. LEE: Okay. I got it wrong. I got it opposite.

So, you have a much closer description for the density and fuel temperature. So, my question really should have been how do you then break that up into finite neutronics calculations?

MR. SHAUG: Each mesh or bundle in the kinetic calculation would receive the same density response. Its particular fuel type description and cross-section would determine its own unique response. So, we used the hydraulic channels to determine a characteristic density and fuel-temperature response.

MR. LEE: So when you have this density wave oscillation type of boiling boundary movement taking place, how well can you represent that in terms of neutronics feedback?

MR. SHAUG: Well, I think that really, when we say we are using a much coarser mesh on the kinetics, or on the hydraulics, that is an option that we have available for each simulation. Okay? In areas where we expect a very, very close coupling between a, well, a very dominant area of the core, we group the hydraulics channels much more closely to the kinetics channels.

In other words, for a dominant bundle in the core, we could simulate one kinetics channel with one hydraulic channel. And as we move further away from that dominant area of the code, then we begin to smear the kinetics channels into a single hydraulic response.

MR. LEE: Now, in the actual mechanism for accounting for void and fuel temperature feedback, do you use a single coefficient that accounts for these feedback mechanisms, or do you go through a table and look up, as a function of fuel temperature and density for these two neutronic parameters, introduce the multiplication factor, and migration area?

MR. SHAUG: Let me go to another slide here.

[Slide.]

MR. LEE: Are you going to get to it later on?

MR. SHAUG: Well, I am not sure I am going to get to it in the detail that you asked the question. But in the transient solution area, the time dependent change and basic equation terms, calculating the function of moderating density and control state, and the calculated function of fuel temperature, those will use the same fits that are available for our design calculation. And those are a function of exposure, and essentially quadratic fits, in terms of density and control state.

MR. LEE: Thank you.

MR. SHAUG: So it is not table lookup.

MR. CATTON: Do you inc'ude heating of the fluid?

MR. SHAUG: Yes, we do.

MR. CATTON: When you decide that you are going to do a stability calculation, how do you decide how many channels, and where do you place them? Do you pick a particular mode of instability, then ask yourself if it will occur, or what do you do?

MR. SHAUG: I think to this stage, we are still in the qualification phase, or assessment phase. And we have been, in our track application, grouping it based on the experimental response that we have found in either a test or in a reactor event.

I think we have under development some methods that we think will aid us in grouping the hydraulic channels by showing us what form os oscillation is most likely to occur, and the position in the core that will be the most dominant during the oscillation.

MR. CATTON: I'm not quite sure I understand that. From what I've seen, you worry about whether you have just a sort of a one-dimensional instability; then you can say gee, I may have the first radial mode, first azimuthal mode. Maybe you have a combination of the first azimuthal and the first radial, plus the one-dimensional.

In particular, if you go to finite amplitude, you have to worry about bifurcation from one to the other. And one doesn't go away because you are looking at a different one. They are all sort of there. It seems to me that that is going to be a very tricky aspect of how you do your calculations. And I would be very interested in hearing about your strategy.

MR. SHAUG: And I think, yes, as you say, the nodalization, or how you group the channels, will to some extent determine what kind of response you get.

MR. CATTON: You bet. And that is not what you should be doing. You should be looking for the physical response of the system, and do whatever you have to do to allow it to manifest itself. That is where the strategy becomes kind of difficult.

MR. SHAUG: Now, as I mentioned, we do have some methods under development to identify for a given reactor condition what is the most likely oscillating condition.

MR. CATTON: That is where I have a little bit of concern. Because you are sort of implying that you have seen it, and you are just going to try to reproduce it. Right? Or else, how would you do it? If you guess what kind of an instability is going to be there, you certainly could look for that one. But that doesn't exclude the others.

MR. SHAUG: Well, then, I think given that problem, it is a matter of analyzing all possible conditions to determine which will result in the largest amplitude.

MR. CATTON: It is a matter of strategy. And I would very much like to hear how you are going to do it.

MR. STIRN: Dick Stirn from GE. I just want to answer that tomorrow we are going to address that.

MR. CATTON: Okay.

MR. STIRN: That will be in the proprietary session tomorrow.

MR. CATTON: Okay. Good enough.

MR. SHAUG: To continue on with the transient solution. Again, the basic equations are similar to the equations we used in our normal design process. The transient solution utilizes a flux factorization method in which we break up the space-and-time-dependent flux into a time-dependent amplitude function and a space-and-time-dependent shape function, the amplitude function representing the magnitude of the neutron flux over the core and the shape function representing the spatial distribution of the flux and the core.

To give us flexibility in the calculation, we allow different time steps to be used in the solution of the amplitude and shape function.

[Slide.]

MR. SHAUG: Just to give you a typical calculational sequence for a time step.

Our first calculation is a prediction of the amplitude function, using a quadratic extrapolation of the necessary equation parameters, which are functions of the notable cross-sections and the shape function.

Having solved the amplitude function, we also estimate the shape function from a new time step, using a linear extrapolation. With our shape and amplitude function, then we solve the thermohydraulics equations, your basic track equations. And at this point, if the solution requires a smaller time step, because of convergence or rate of change considerations, then we'll back up and resolve our amplitude and shape, using a smaller time step.

MR. LEE: How do you tell if you need a finer time step for shape function calculation?

MR. SHAUG: For shape function?

MR. LEE: Yes.

MR. SHAUG: At this point? At this point, we are just extrapolating.

MR. LEE: That I understand. But in your third step, when you check the convergence, to see if the time step is fine enough, what do you do?

MR. SHAUG: Well, the convergence here would be strictly on the thermohydraulics, using the amplitude and shape function that we have obtained under the first two steps.

MR. LEE: So you don't check at all if the shape function was calculated accurately or not?

MR. SHAUG: No. Not at this stage, no.

MR. LEE: Do you do that at all?

MR. SHAUG: The shape function step is in this portion. Let me get through these.

MR. LEE: The quota study method that you are using here in separating shape function from the amplitude function depends heavily on your accuracy with which you calculate the shape function. So that is why I am a little bit concerned here.

MR. SHAUG: The basic assumption in the model is that the shape function is linear from one shape function calculation to the next, okay? And so that is the basic assumption that is used in the calculation. And it is the reactivity step that must be essentially converged to the shape function step.

MR. LEE: But if you use fairly crude time step for the backward difference and for the time derivative shape function, your shape function is not going to be terribly accurate.

MR. SHAUG: True. Again, the size of the shape step can vary as the problem dictates.

MR. LEE: But do you choose that shape function time step manually? Is that what you are saying?

MR. SHAUG: Manually.

MR. LEE: But how can you tell, unless you go through and repeat the calculation, whether the shape function calculation is accurate or not?

MR. SHAUG: Well, in particular for our stability calculations, we are using reactivity in shape functions on the order of the hydraulic time steps. And so I think clearly, for that application, that is fine enough.

MR. LEE: Do you have somewhat of a better perhaps, or much more affirmative verification than that?

MR. SHAUG: No, not at this time.

To continue on, having solved the hydraulics within the amplitude parameters, and then we'll go ahead and solve the 3-D precursor equation. If we're only do a reactivity step, we then would continue on back and repeat the process for a new time set.

If we are performing a shape calculation, then we solve with shape step, using the latest amplitude function and cross sections. Having obtained the latest shape function, then we go back and rather than using our extrapolated or the linear extrapolation of shape function, we use the actual calculated shape function and recalculate the amplitude function and various amplitude parameters that have been used prior to the new shape function step.

And so it's this step that then updates the amplitude and parameters consistent with the actual calculated shape function.

MR. LEE: What about the cross section dependence as a function of time over the shape function time step? Do you linearly extrapolate the feedback component of the cross section dependence or is there iteration involved?

MR. SHAUG: There's no iteration involved. The cross section parameters, say, from the amplitude function, are calculated at the end of the shape step and then the parameters are presumed to be linearly varying from the previous shape function step to the current one, as far as back-calculating the amplitude function.

MR. LEE: So the feedback is assumed to be -- is basically linearly extrapolated? Can I say that?

MR. SHAUG: Yes.

MR. LEE: Over the shape function time step?

MR. SHAUG: That's correct.

MR. LEE: When you said density vacillation type of calculation, you said the shape function time step equal to the thermohydraulic time step. What order of magnitude time step are you talking about?

MR. SHAUG: In our calculation, we're using amplitude steps on the order of 20 milliseconds, shape function steps on the order of 40 milliseconds, so we take one intermediate reactivity step relative to the shape function.

MR. LEE: Thank you.

MR. CATTON: For those of us who are not neutronics types, could you distinguish an amplitude function and a shape function? I'm kind of lost in what you're doing.

MR. SHAUG: I'll go back to this slide.

[Slide.]

MR. SHAUG: In other words, to facilitate the solution of a time-dependent flux equation, we have factored in an amplitude function which gives us essentially the power level, and a separate function that we use to describe the distribution over the core. This is very similar to the point model where you only get the amplitude of a flux.

Only, in a point model --

MR. CATTON: You normally to get your shape function and then you just have to multiply it by an amplitude?

MR. SHAUG: Yes. Now, in the point kinetics model, it would be as if the S function or Shape function were constant. In other words, this would never change. Now, in our model, the amplitude as well as the distribution of the power over the core is allowed to vary.

MR. CATTON: If I were doing an exact solution, I would turn that into some kind of a series, right?

MR. SHAUG: Yes, you would.

MR. CATTON: The R is really a vector?

MR. SHAUG: The R represents the spacial dependence.

MR. CATTON: X,Y,Z.

MR. SHAUG: X, Y, Z.

MR. CATTON: Okay, thank you.

[Slide.]

MR. SHAUG: Just a quick word about qualification. We've shown consistency with our 3 BWR core simulator for steady state and scram response. We've assessed the transient capability against turbine trip data. We'll be showing you that later, and we're in the process of qualifying a couple of calculations for stability as well as rod drop analysis.

I think I'm --

MR. LEE: I have one more question for you. You said you're using 20 millisecond time step typically for amplitude calculation and 40 millisecond for essentially linear calculation of reactivity feedback.

Do you feel that those time steps are fine enough for rapid reactivity related transients? Over a period of a second, power level can change by, let's say, hundred percent to two percent to two hundred percent of rating.

MR. SHAUG: I believe so, as far as our comparison with test data. Again, if we need a finer or a smaller time step, then I think we would also need a finer calculation of the hydraulics that would be providing the parameters for the power calculation.

I still think we would be tied to the hydraulic calculation as far as defining our reactivity step.

MR. CATTON: Isn't the time constant associated with the neutronics much shorter than the time constant associated with the hydraulic.

MR. LEE: That was exactly the question I was going to raise.

MR. CATTON: If it is, why do you have to solve the neutronics as a transient problem?

MR. SHAUG: To get the spacial distribution of the flux.

MR. CATTON: Can't you do it as if the neutronics are quasi -- I don't know if you can or you can't. It seems to me that if the time constants are grossly different, the hydraulics doesn't know that the neutronics is a dynamic problem and you could do it as quasi-steady and my friend out there is shaking his head no, so I'll just stop.

That's all I have on this. The next area --

How long is this summary going to take? Two days? It looks like you've got an awful lot of slides.

MR. CARROLL: I will try to make it short and skip a couple of the slides.

MR. CATTON: I don't think we want you to do that. It's 10:15, so why don't we take a 15 minute break and then you can give us a summary at 10:30.

[Break.]

MR. CATTON: Just trying to get your attention. It's your turn to summarize.

MR. ANDERSEN: Thank you.

I'd like to give a brief summary of some of the type qualification. It's not an exhaustive summary of everything we have done. I have selected a number of examples on our qualifications. Some of the examples I have chosen particularly because I thought they might be relevant for stability.

[Slide.]

MR. ANDERSEN: In the development of the TRAC Code we followed a step-wide approach where we first when we started on the development back in '79 we concentrated on the models for the more basic phenomena such as the interfacial shear, the heat transfer, and we tried as far as possible to develop first principal models where we could.

We tried to qualify it against separate effects tests.

The next thing we did was to develop models for the typical BWR components, again as far as possible using first principal models. The BWR components would be models such as the jet pumps, the separators.

We qualified them against component defects tests from those particular components.

The next step would then be to perform qualification against system effects tests such as scale simulations of an entire BWR and plant data where available.

Subsequently, having done all that we feel we can apply the Code for the BWR calculations.

Now in the basic model development the areas we concentrated on was the interfacial shear, which we felt was critical for void fraction predictions, heat transfer, which not only affects void fraction but also temperature prediction.

[Slide.]

MR. ANDERSEN: I've shown you a number of examples when I talked about the interfacial shear. I showed you a couple of examples on our predictions.

Some of the slides are repeated here and I'd like to just skip those.

One slide I showed you was the pressure response from the PSTF tests showing the comparison of pressure as a function of time.

[Slide.]

MR. ANDERSEN: I have one slide here which was the same facility which is a four foot diameter vessel at four different times during the transient at 2 seconds, 5 seconds, 10 seconds, and 20 seconds into the transient shows comparison between the measured and the calculated actual void fraction profile inside the vessel where the void fraction jumps from a value of about in most cases around .6, .7 up to 1 is where the location of the two-faced level is.

MR. LEE: Excuse me. What is the mechanism behind this jump?

MR. ANDERSEN: You have a two-faced level. If we go back to -- see, what happens is that you have a two-faced level initially here. You blow down through this pipe. As you depressurize the fluid flashes and the level swells up. I think at some point you have a two-faced level.

MR. LEE: Thank you.

[Slide.]

MR. ANDERSEN: I have an example here on a temperature prediction and this is a comparison against one of the THTF thermohydraulic test facility test that was conducted in Oak Ridge National Laboratory.

It is a combined blowdown and power excursion. Anyway, at about 10 seconds into the transient you get a boiling transition. You get a very rapid temperature increase and the final temperature is in very high temperature, film boiling heat transfer. It shows a comparison between the measured temperatures and the calculated temperatures.

MR. CATTON: How well did you do in this case in your predictions of the void fraction?

MR. ANDERSEN: The void fractions -- I don't believe that we have any data for the void fractions.

MR. CATTON: Well, THTF at Oak Ridge did make combined runs where they measured void fraction along with temperature.

MR. ANDERSEN: The void fractions I do remember were very high in the upper 90's.

MR. CATTON: See, this is an opportunity of you to show that in an integrated sense you have put the program together right, so you really should show us the void fraction along with this temperature data.

You may have to choose different runs because it was only at the tail end of the program where they measured void fractions.

MR. ANDERSEN: Yes. Maybe we should look at some of the othe. tests.

MR. CATTON: I think you should if you are trying to demonstrate that your code does a good job on these kinds of problems. You need to show integral results.

MR. ANDERSEN: In some of the other tests I will show you in a little while comparisons of data from our fifth test facility. We do have measurement of fluid inventories and temperatures corresponding measurements.

MR. CATTON: It's not quite the same though. At Oak Ridge they actually measured the void fractions.

MR. ANDERSEN: Yes.

MR. CATTON: You have to infer it from other measurements.

TRAC can give you as good a prediction of the temperature but the void fraction's wrong. I am not referring to your TRAC but the TRAC PWR will predict those temperatures but not the void fractions.

MR. ANDERSFN: Let me show you a couple of slides a little later.

Anyway, we feel that the code has the capability to predict those void fractions and temperatures.

[Slide.]

MR. ANDERSEN: As an example on some of the component models I'd like to show you an example on some of the qualification we did for the jet pumps and the steam separators for which we developed separate component models.

[Slide.]

MR. ANDERSEN: This slide shows the comparison against a one-sixth scale jet pump but tests that covers all six flow regimes that might occur in the jet pump, the schematic shows here this would be the normal operation where this arrow indicates the drive line. This is the suction flow and here we have the discharge flow. This is normal operation. During accidents and abnormal situation you can get into the other modes of operations where you have reverse flow in the jet pump.

Comparison shows this is the data. You plot the data in terms of the M and the N ratio, the M ratio being the ratio between the suction and the drive flow and the N ratio represents the pressure difference between the discharge and the suction relative to the pressure difference between the drive line and discharge.

When you plot it this way you can plot the data from several different flow rates in the same curve. These are the data. The solid line with the black triangle represent corresponding TRAC calculations.

We have data covering all the flow regimes. This shows the corresponding comparison for full scale jet pump. The solid line is the type calculation and the circles are the data. This is, unfortunately, for the full scale jet pump, we only had the data for the first quadrant of this curve.

MR. LEE: Excuse me. Could you say a few words about actual model that goes into your jet pump description? Do you use two fluid models?

MR. ANDERSEN: It's a two-fluid model. It's the same model as we have everywhere. In the jet pump, what we have implemented into the model is we have -- it's essentially a combination of two models. One model that describes the conservation of momentum for the mixing process when you mix the drive line and the suction line, the flow coming in. The second model is a number of models for the various losses you may have in the jet pump.

There are certain losses associated with the mixing process and then there are the form losses that you might have in the system. You have pressure drops that discharge from the nozzles. You have pressure drops in the vent, in the drive line. You have pressure drops, frictional pressure drops in the diffuser sections of the jet pumps.

So it's a combination of a model for the conservation of momentum for the mixing process of the two streams, the drive line and the suction, and models for the various irreversible losses that you may have.

MR. LEE: There's a momentum mixing model, there's a time dependent model.

MR. ANDERSEN: Yes, it is. We essentially describe -- we solve the transient momentum equation.

MR. LEE: Thank you.

[Slide.]

MR. ANDERSEN: Another model, component model that we developed was the steam separator model. The important characteristics of the steam separator is to be able to predict the carryover and the carry-under in the pressure drop. The model briefly describes all the axial momentum equation, as well as the angular momentum equation in the separator in order to calculate the fact separation.

This is an example for a three-stage separator, comparison of carry-under. The circles are the data. The dotted line with the squares on it represents the TRAC calculations comparison of carry-under.

[Slide.]

MR. ANDERSEN: For the carryover, the comparison looks like this. The solid line represents the data and the dotted line represents a TRAC calculation. The normal operation is typically around the quality of about 12 percent inlet quality to the separators.

[Slide.]

MR. ANDERSEN: This shows the comparison between calculated and measured pressure drops for a three-stage separator. The model predicts slightly higher, but quite close pressure drop.

[Slide.]

MR. ANDERSEN: So in conclusion, we feel that the performance of typical BWR components is well predicted by the models we have in the TRAC code. Having developed a more basic model in the component model, we went down and qualified the code by comparison of integral system effects tests and the tests we used were typically tests like the TLTA and the FIST tests.

I would like to show you a couple of examples from the FIST test series. We have also made comparison to plant data and I would like to show you an example of the comparison on the Peach Bottom turbine trip test and later on you will see lots of comparisons on stability.

[Slide.]

MR. ANDERSEN: First, I would like to show you a large break LOCA from the FIST test facility. This is the comparison of the calculated and the measured temperature or pressure response from the FIST test facility and you can see the agreement is quite good.

MR. SCHROCK: A calculation of that type uses the point kinetics model?

MR. ANDERSEN: This is the test facility where you have electrically heated fuel lots to simulate it. So the power as a function of time is known.

[Slide.]

MR. ANDERSEN: I'd like to put the next two figures on top of each other. It's two figures because the limitations in our graphics package that doesn't allow us to plot more than six curves on the same figure.

But what it shows is a recording at a given elevation which is about 71 inches from the bottom of the bundle. It shows all the measured temperatures from the thermocouples and the fuel lots. What you find is that most of the temperatures follow the saturation curve. Some of the fuel lots heat up and the fuel -- in this case here, you have one -- you have a sustained heat-up for a long period of time and then finally quenches.

What we find with the TRAC calculation having a one-dimensional hydraulic model for the flow into the channel is that we tend to predict the average response of the fuel lots because we do not account for cross-sectional variations inside the bundle. And the average temperature of TRAC is like this. It's kind of in between the ones that do heat up and the ones that don't.

In order to provide a bound for the peak temperature, we put in the hot rod model which is an estimate for bounding temperature calculation, and this is what we get. However, we feel that in order to get the right hydraulic response, you also need to have an average heat transfer which means that you predict kind of an average performance of the individual lots.

MR. TIEN: How did you get the difference, the average in hot rod?

MR. ANDERSEN: Essentially, what we did was that we made an assumption about the amount of cross-sectional variations in the void fractions and we assumed that for the hot rod calculations that there would be certain rods that would be starved of liquid having much less liquid available.

As a result of that, they would have a worse or lower heat transfer corruption.

MR. TIEN: How do you assume that condition or on what basis?

MR. ANDERSEN: It's a simple empirical correlation where we assume --

MR. TIEN: So the bounds is just kind of qualitative. It's not really -- higher bound.

MR. ANDERSEN: It gives a fairly accurate estimate of the maximum temperature that is obtained. The actual correlation from the average void fraction to the most limiting higher void fractions you'd have was one we developed by comparison to data.

I'd like to show you another example.

MR. WARD: Can we go back? I still don't see how you would just avoid assuming the answer there in the selection, the parameters or the relative parameters for the hot rod.

MR. ANDERSEN: No. We're not assuming the answer. We developed a model which is proprietary. That describes the hydraulic characteristics, the limiting hydraulic characteristics seen by an individual rod compared to the average hydraulic conditions.

MR. SCHROCK: Aren't these excursions initiated by boiling transition?

MR. ANDERSEN: They are initiated by boiling transition.

MR. SCHROCK: So it isn't clear to me yet what you've done to make the calculation do that. You've made a modification, then, in the boiling transition or you've modified something that produces thermal hydraulic --

MR. ANDERSEN: We made a modification, we made a bounding. The entire calculation is based on the average hydraulic. You have to realize that TRAC in the fuel channel uses the one-dimensional model. So it has an average hydraulic condition at a given elevation. So all the fuel rods which typically have very close to being the same power. If you see exactly the same hydraulic conditions in the calculation, you'll experience boiling transition at roughly the same time.

Now, in a real bundle, you do not have a uniform hydraulic condition, as hydraulic conditions, such as void fractions, typically, how much liquid do you have available. That varies across the bundle. You get some variation at the time you get the boiling transition, depending on how much liquid you have available close to the surface of a particular rod.

There may be differences on how soon the rod will lever again. Now, that kind of variation you cannot capture with a one-dimensional model. So what we did was we made an assumption, assuming that if you knew, say, an average void fraction in the bundle, we would assume that a limiting rod would see a slightly higher void fraction.

As a result of that, it may get a boiling transition slightly earlier and it may lever it slightly later and it may heat up to a higher temperature.

MR. CATTON: I guess the question is how do you quantify slightly.

MR. ANDERSEN: That we have turned to a number of data. How much higher that void fraction should be. That's a proprietary correlation.

MR. WARD: What data are these? Data from what?

MR. ANDERSEN: Data from several test facilities, such as this. Not only this, but TLTA and also the FIST test facility.

MR. CATTON: But you didn't measure void fraction in any of those facilities.

MR. ANDERSEN: No, but we calculated it. We also measured the pressure drop, the actual pressure drop which, for those flow conditions, are a very good indication of what the void fraction is.

MR. CATTON: But you have to -- it's just an indication of an average void fraction across a channel. You still have to address the question of -- so this is tuning. You adjust the void fraction until the heat transfer relationships give you roughly the right temperature. But they both may be wrong.

MR. ANDERSEN: The average condition is very well predicted. This was strictly a model that was put in for bounding calculation.

MR. CATTON: You missed the thrust of my statement. I understand that the average is good because that's just saturation. But you were trying to find -- well, no, it isn't. But you're trying to find the peak. If you're going to adjust the void fraction, on what basis do you adjust it? The only measurement you really have is that pin temperature.

MR. ANDERSEN: Yes.

MR. CATTON: So you can adjust it until the heat transfer relationships give you the right peak, but then your heat transfer relations and the void fraction may both be wrong. You don't know.

MR. ANDERSEN: You're right. We don't have --

MR. CATTON: The only way you can do it -- if you're going to do things like that, you ought to be using the THTF data where they actually measured the void fraction.

MR. ANDERSEN: But they didn't measure void fraction in individual sub-channels.

MR. CATTON: Yes, they did.

MR. ANDERSEN: They did?

MR. CATTON: They had pin -- there weren't very many pins, but they had pin-to-pin --

MR. ANDERSEN: Okay.

MR. SCHROCK: But you don't use a sub-channel analysis.

MR. ANDERSEN: No, we don't use sub-channel analysis.

MR. SCHROCK: Well, it's not in TRAC, I know, but you don't use that in order to make the choice that you're making in this particular kind of calculation.

MR. ANDERSEN: No. As I mentioned, it's a very empirical correlation.

MR. TIEN: Coming back, the hot rod calculation is still not binding because what you did is really based on the experiment that they did on the temperature. You back-calculate and then -- so it's really still based on -- you are talking about the same thing. It's not necessarily really the binding, but it is an indication.

MR. ANDERSEN: It is an indication of the upper bound.

MR. TIEN: Yes.

MR. ANDERSEN: Let me show you another example.

[Slide.]

MR. ANDERSEN: This is from a small break loss accident. It shows the measured and the calculated pressure response. For a small break, you get -- you don't depressurize. Initially, you isolate. You then pressurize and at some point you open the ADS valve and that causes depressurization.

[Slide.]

MR. ANDERSEN: This is a comparison between the calculated and measured flow rate to the ADS relief valve.

[Slide.]

MR. ANDERSEN: In this case here, you get a more sustained heat-up of the rods. Again, you see here a comparison. You see for the same axial elevation several temperature traces for the individual thermocouples. This is the hot rod calculation and the average rod calculation is somewhere here.

MR. CATTON: Why does it go below the saturation, the average rod?

MR. ANDERSEN: Because when you re-plot the --

[Slide.]

MR. ANDERSEN: This here is the calculated temperature. It doesn't go below saturation. It follows the saturation curve, but it's slightly lower because pressure is slightly under-predicted. So the corresponding saturation temperature is slightly lower.

MR. SCHROCK: Is the point of these comparisons that the comparisons are better than they would have been with the EG&G version of TRAC because you have a better interfacial?

MR. ANDERSEN: The interfacial shear model is the same in the EG&G version and our version. So I don't believe that that would be a substantial difference. The EG&G version doesn't have the hot rod model, which is really only important if you are talking about loss of coolant accident calculations. To stability it's not important at all.

MR. SCHROCK: I guess what I'm trying to ask is what motivated this particular selection of comparisons in terms of what you're trying to establish about TRACG as being well-qualified for stability studies.

MR. ANDERSEN: This part here was really just a summary of some of the general qualification that we have done.

The subsequent presentation which will follow this presentation will show all the qualification we have done for stability. So this is kind of just a quick summary of some of the previous qualification.

MR. CATTON: We had asked about the ECCS and LOCA type things as well. So this fits.

[Slide.]

MR. ANDERSEN: So far, we have compared the integral scaled system effects test where we have plant data. We have also compared the plant data and I'd like to show just one example which is a comparison to the Peach Bottom turbine trip test.

[Slide.]

MR. ANDERSEN: The first slide shows a comparison of the axial measured power profile in the reactor core. In the calculations, we had a total of 24 axial nodes in the core. There are really three curves shown here. The solid line is the output from the plant process computer, which is an indication of the axial measured actual power profile.

The circles here represent the calculated powers from using the one-dimensional model in TRAC. The squares represent the calculated axial profile using the three-dimensional kinetics model.

The bottom plot shows the transient response following the turbine trip shows the total reactor power. The solid line is the measured plant data. There are two dotted lines. This line here represents the calculating using the one dimensional kinetics model and the other line, which is slightly closer to the data, represents the results using the three dimensional kinetics model.

In both cases, the transient power response is very well predicted.

MR. CATTON: Doesn't this just say that you nodalized your sceam properly?

MR. ANDERSEN: The power response, when you get the pressure increase is, as you -- caused by the void collapse that comes by the pressure wave you get as you close the turbine control valve, but it depends on a lot of other parameters than just being able to calculate the pressure response.

It also depends on how much void fraction change you get as a function of that pressure response. That void fraction change controls very much the initial void reactivity you get here. At this point here, where the power turns over, turbine trip one was very interesting; that the power actually turned over before the scram occurred.

The scram occurred something like out here in time.

MR. CATTON: Was that a doppler?

MR. ANDERSEN: That number of effect of the controls is the turnover. There is the -- some doppler effect. There is some effect just due to the delayed response of the delayed neutron precursors. You also see an effect on the peak, on the direct moderator heating, because in this timeframe, you do get a small effect of the direct gamma heating of the hydraulics. It depends on other phenomena.

MR. LEE: Could you please comment on the previous reference again; the comparison between the TRAC and the computer calculation by axial power diffusion?

MR. ANDERSEN: This is the output from the plant process computer.

MR. LEE: Do you feel that the agreement is acceptable?

MR. ANDERSEN: Yes.

MR. LEE: I thought the TRAC model uses the same cross sectional database and basically the same neutronics model as the process computer?

MR. STIRN: The process computer does not use a neutronic model.

MR. CATTON: It's just measurements.

MR. ANDERSEN: That's a measurement.

MR. CATTON: This is the plank computer.

MR. STIRN: Dick Stirn from GE. The process computer just used direct measurements from in-core detectors. Our LPRX system is normalized through our traverse and in-core probe. That's basically a direct measurement. These are fission changes.

MR. CATTON: Okay.

MR. STIRN: That's not using the neutronics method.

MR. LEE: Well, there is some kind of correlations that you use to process the detector readings into power diffusion. In that sense, you use a certain amount of neutronics database. But you're right, process computer is not exactly calculating the results, but it's not necessarily directed to take the readings either.

You average LPN data and then convert the detector reactivity into power, so there is a conversion process involved. My question is; can you compare with your best, the core model.

MR. ANDERSEN: That is essentially the three dimensional calculation because the three dimensional calculation is the same model as we have in our steady 3-D core simulator as Jim Shaug mentioned earlier.

MR. LEE: Are you then satisfied with this few percent error in predicting the peak power?

MR. ANDERSEN: I'm satisfied.

[Slide.]

MR. ANDERSEN: I think it depends to a large extent on the particular application. Now, to summarize our qualifications, we qualified the code against system effects test and plant data and we feel that the integral system performance is well predicted.

In conclusions, we have gone through this stepwise approach, first qualifying against individual phenomena, then to component performance and finally the integral system effects and plant data, whenever we had data.

We feel that TRACG captures all the major phenomena in the BWR.

MR. LEE: Mr. Chairman, I have a question on the material, not presented today, but related to TRAC validation and this report we have received on -- I guess it's a TRACBD02 or something like that. I wonder when is the best time to raise such a question?

MR. CATTON: Well, I think we should ask the speaker.

MR. ANDERSEN: You can ask me the question.

MR. CATTON: I guess if it's proprietary, you would discuss it with us tomorrow?

MR. LEE: Is this report proprietary?

MR. ANDERSEN: It's TRACB02 qualification?

MR. LEE: I don't think it is proprietary. B22049?

MR. ANDERSEN: No, it's not proprietary.

MR. LEE: This is related to the Oak Ridge thermohydraulic test facility simulation.

MR. ANDERSEN: Yes.

MR. LEE: In one particular case you're showing -- I don't know what you mean by axial vapor temperature, but some kind of temperature of vapor, I guess.

MR. ANDERSEN: Yes.

MR. LEE: There is a considerable oscillation in your calculations which do not exist in the ephemeral data. I was wondering if you could comment on it?

MR. ANDERSEN: I would have to see the particular figure, because I don't remember it off the top of my head.

MR. LEE: There are a few other oscillations like that that I have seen in that particular report that do not exist in the experimental data, but this is one example.

MR. ANDERSEN: Okay, that is correct. What you see in the data is that the vapor temperature for this particular test most of the time was close to saturation temperature, and during short periods of times, you got a -- some superheated vapor.

This particular calculation TRAC predicted a boiling transition and departure from saturation temperature that happened a little earlier. The code would intermittently try and return back to saturation temperature which means you got a vapor temperature which would get back close to saturation temperature.

Now, the fuel rod temperatures were saturated -- were superheated during the entire calculation, indicating that there was boiling transitions on the fuel rods. I believe that these oscillations controlled by fluctuations in the Doppler concentrations calculated in the code and causing corresponding fluctuations in the vapor temperature.

MR. CATTON: That's not a very good explanation. Could it be that the code is beginning to go unstable?

MR. ANDERSEN: I don't believe that there is an instability in the calculation. There might have been some fluctuations in the interfacial heat transfer given by the fluctuations in the void fractions. I do not recall the exact details of this particular calculation, so I'll be happy to research that and to give you a more detailed answer.

MR. CATTON: It looks like somebody in the audience is trying to help you out.

MR. RUHANI: I would like to mention that it is extremely difficult to measure vapor temperature when there are some droplets of liquid. Those who are familiar with these kinds of temperatures know that we have to go to extreme lengths to promote instrumentation to measure the vapor adequately when it goes beyond the saturation point.

Just one small droplet of vapor which comes out of the thermocouple makes that temperature to give as the saturation temperature, while the steam may be quite superheated. So, the measurements are to be suspected in this case.

MR. CATTON: You may well be right, but it sounds to me like the calculation is a little bit malignant as well.

There's no reason for the calculation to give fluctuating temperatures. I don't know where in the equations it comes from, unless you're taking a little bit too big a time step.

The other thing that these codes do is that they divide the heat transfer between the liquid and vapor. They split it and that split is kind of non-physical. That may be the source of your problem as well.

MR. ROUHANI: It could be that the code is not doing a good job, but I just wanted to mention that.

MR. CATTON: You are absolutely right.

MR. ANDERSEN: Well, in this case, there's really not a split in the wall heat flux and the vapor because when you have a boiling transition, all the heat flux goes to the vapor. The oscillation is caused by oscillations in the facial heat transfer which could occur due to oscillations in the Doppler concentration.

MR. CATTON: There's nothing in the code that would give you the Doppler concentration oscillation, unless maybe there's something that's coming unstuck with your means of estimating the drop diameters.

This gets very complex and non-physical. It leads to all kinds of headaches. It's the numerical instability and then you have to go back to the modeling, because it certainly is non-physical.

MR. ANDERSEN: This is, by the way, also an older calculation. I do not believe that we see the same oscillations in code that -- in calculations that we conduct today. You are right that this is one of the comparisons that's not as good as we would like to see it.

MR. SCHROCK: It seems to me that Mr. Rouhani's point is very well taken, and maybe the problem here is more that it wasn't a good choice of data comparison for code qualification; am I right there?

MR. ANDERSEN: I believe that you are right in that.

MR. LEE: I can easily see fluctuations, but in the calculation again, if I understood Ivan's comment correctly, I don't see where the fluctuation could come in.

MR. ANDERSEN: That I can easily see where you get fluctuations, because in a test like this, you have void fractions that are very close to hundred percent void fractions, but you oscillate -- particularly the flow, if you look at the flow, is not very steady, particularly the void fraction concentrations.

You oscillate between void fractions that could range from, say, 94 to 98 percent. Now, if you have that kind of fluctuations, you get fluctuations in the liquid concentrations and the interfacial area that controls how much superheat you get as a vapor. That easily could be an order of magnitude.

So, when you have slightly lower liquid concentration, you get more interfacial area and you get closer to the saturation temperature.

It is the balance between the wall heat flux to the vapor and the heat flux from the vapor to the droplets that controls the vapor temperature, and so, if you get oscillation in the droplet concentration, you will also get oscillations in the vapor temperature.

MR. CATTON: Then I guess we'd have to question the source of the droplet oscillation in the calculations, because I don't see -- at least, near as I can tell, I don't see a source for those kinds of oscillations in the equations that are in TRACG.

MR. ANDERSEN: If you have velocities that are such that the liquid velocities is low, it's very frequent that you see oscillations in the droplet concentrations.

Anyway, I don't recall the exact details of this particular calculation. It was not made by me. It was an independent qualification of the code, which was one of the processes we used that we, as co-developer, we developed the code and did our own assessment.

This report is an example on a person that was completely independent of the code development, that did his subsequent assessment. He got large number of good results and some results like this one that was not as good. I think if you look at the entire report, you would see that most of the comparison are quite good.

MR. LEE: I agree with your last comment, but it made me uneasy when the report does not even mention the presence of this kind of oscillation, which may be spurious, which may be real -- I have no idea -- and just to gloss over, because in this qualification and validation effort, these are the minute details that one needs to pay attention to, because indeed, the code might be misbehaving, and you may not be catching these kind of behaviors in most of the situations.

MR. CATTON: I think we should continue.

MR. ANDERSEN: We will now go on to the qualifications against stability, and the first subject I'd like to talk about is a study which we undertook to evaluate how well you can apply time-domain code for stability analysis and what sensitivity the numerical method is.

[Slide.]

MR. ANDERSEN: I will show the analytical study which we undertook. Jim Shaug, who will follow me, will show the comparison to the data.

[Slide.]

MR. ANDERSEN: Essentially, what our concern is when we use a time-domain code is the numerical dissipation that can occur, the damping that may exist for the various numerical method, some method, like the implicit method, have much more numerical damping that other methods, and we tried to quantity that by comparing some of TRACG with exact solutions. We have also done the comparison to the stability data.

[S?ide.]

MR. ANDERSEN: Essentially, to just illustrate the problem, if you have a -- this is just an qualitative illustration. If you have a heated channel flow coming up and you apply heat, you get a void fraction profile that looks something like this, only a qualitative plot.

[Slide.]

MR. ANDERSEN: If you look at the sensitivity of that void fraction profile to variations in the flow, if you increase the flow, you get slightly lower void fractions, the void fraction on this scale and the axial elevation. If you reduce the flow, you get higher void fraction.

Now, if you oscillate the inlet flow, you can set up an oscillation in the entire flow and in the void fraction profile, and if you choose the time period of that oscillation such that the half period is very close to the transit time for the vapor or the kinematic wave moving up to the channel. You can get a void fraction oscillation that 180-degree out of phase at the exit or the inlet or with the point where the oscillation first starts.

Now, how do you calculate the movement of that density wave up to the channel? How is that sensitive to the numerical method, and that was clearly the intent of the study.

[Slide.]

MR. ANDERSEN: To illustrate what numerical dissipation does, if you have -- let's say you have a wave or a response of a certain parameter -- it could be anything that looks like this solid line -- and you have a differential equation like this that describes a travelling wave.

Now, if you look -- if the velocity is constant, what his curve here would look like after time period delta-T, it would just be shifted to the right, at a distance that is equal to the velocity times delta-T.

Now, that is what you would expect for an exact solution to traveling wave with no dissipation.

Now, what happens when you apply a finite difference technique? You divide your channel into nodes, and for each node, you calculate an average value of your particular property.

So, if you use an expression, what you would do is that you would have an in-flow to this particular node that's given by this value of the property and an out-flow that's given by the initial value of the property in the cell which is zero.

So, after a certain time step delta-T, if that is less than the material coolant limit, your response would look something like this, while the exact solution looks like this, if the time step is maybe as shown in this example here, like two-thirds of the coolant limit.

Now, what happens if you use an implicit method? Since the flow into the cell is given by the inlet property, but the flow out is given by the property in the cell at the end of the time step, in the explicit technique, nothing would flow out of the cell. With the implicit, you get flow out, and this is what the solution looked like using an implicit method with a large time step.

The exact solution looks something like this. The property has been kind of dissipated up along the entire channel, and that is, in a nutshell, the course of the numerical diffusion and that can have a strong effect on stability.

[Slide.]

MR. ANDERSEN: So, the evaluate the numerical stability, we looked at, say, what happens if you have a channel and you have a travelling wave that's travelling and damped, moving down to the channel? This is an exact solution to travelling that wave.

Now, if you take a channel and try and solve that using an explicit difference technique, "C" would be the material coolant limit and this is what the difference equation becomes if you a domicile difference technique to give an explicit technique, and what you find, that if you try and look at a situation like this, you get an expression like this for the damping, from which you can calculate the damping of the travelling wave.

[Slide.]

MR. ANDERSEN: You can do the same thing if you have an implicit integration technique where the property that's convected is calculated at the end of the time step. Again, you can substitute this travelling damp wave into this equation and you can calculate what the exact damping would be from the implicit technique.

[Slide.]

MR. ANDERSEN: And what you find is that if you look at the channel -- and this is what you would calculate using an explicit technique if you have, say, a long channel that's divided into a number of nodes. You have a property at the inlet, which could be flow or void fraction, you oscillate. What would that oscillation look like at the exit of the channel?

Now, if there was absolutely no damping in the system, you should get exactly the same amplitude at the exit as you get at the inlet, which means that the ratio of the exit amplitude to the inlet amplitude should be one.

Now, when you use an explicit technique, you get a solution that's something like this for the damping, showing that the explicit technique has no damping. If it shows a time step that was exactly equal to the material coolant limit, the implicit method has substantially more damping, and that's what allows you to use large time step in the implic.: integration technique. That's what makes it stable, that you have obtained the stability at the cost of more numerical dissipation.

This is what you get with the implicit.

MR. SCHROCK: Why are they both wrong for small values?

MR. ANDERSEN: Okay. That's a good question.

You have, when you talk about the integration of partial differential equations, you get truncations that are both due to spatial nodalization and due to the nodalization in time.

Now, here, what is happening is that when I make the core number smaller and smaller, I make my spatial disposition smaller and smaller, which means that the truncation that I obtain due to disposition in time goes to cell, but my spatial nodalization is still the same, and I do retain the truncation in space, all the second-order terms that you have left out, second-order and higher-order terms.

So, what this represents is the truncation error which you get for a given node size for the limit of very small time step.

MR. CATTON: Could I interpret this graph as telling me that for stability calculations, explicit would be more economical than implicit?

MR. ANDERSEN: You are taking the words out of my mouth.

MR. CATTON: I have a habit of doing that.

[Slide.]

MR. ANDERSEN: In answer to the previous question, this shows the same set of graphs. This is not in the handout, but a backup slide I had, in case this question came up.

If I make the node size smaller, go to smaller and smaller nodes -- this is for a channel where I had 24, 48, 240 nodes for the same length. As I make the spatial nodalization smaller, the spatial truncation error also becomes smaller.

In each case, the explicit and the implicit for small time step converged to the same solution. As I make the nodalization smaller and smaller, it gets closer and closer to the case where you have absolutely no damping.

Going back, this was the exact solution to what kind of damping you would obtain. This represents an example that we generated. This was particularly chosen to have large node, to make the numerical damping large. We ran a calculation where we took TRAC, took a pipe, had a constant velocity of flow through the pipe and oscillated the wide fraction at the inlet.

And we looked at what was the calculated oscillation in the exit void fraction relative to the inlet void fractions. The triangle represents the calculation, using the implicit integration technique. The solid line is the exact solution for what you would expect the numerical damping to be.

The circles representing the technique using the explicit integration technique, and as you concluded, the explicit is better, and ideally you would want to have time step as close to the material limit of one as possible. And that is what we have chosen to do in all our subsequent qualification with data.

MR. TIEN: What do you mean, the exact solution for the implicit and explicit?

MR. ANDERSEN: It is an exact solution of what numerical damping should be for a traveling wave.

MR. CATTON: Based on your previous --

MR. ANDERSEN: This is if you assume that you have a traveling wave, and you use an implicit integration technique, you can solve for the damping corruption and you get this one. And that is what the comparison is.

So we feel that we do understand what controls a numerical diffusion. And based on these results, --

MR. CATTON: On the graph you just took off, what are the crosses?

MR. ANDERSEN: Okay. Let me get back to that. So based on the result, comparing the explicit and the implicit methods, we decided that the explicit was the best, had the least amount of damping, and ideal should be used with time steps as close to the material core and limit as possible, to minimize the effect of the numerical damping.

Now, the answer to your question is that you can -- let's go back.

[Slide.]

If you use a second order integration technique, where you use essentially a central type differencing, rather than an open differencing, if you do that correctly, you can get second order accuracy in your integration technique.

Second order accuracy means that your truncation errors are sort of out of turn, and you get less numerical damping. And we, as I mentioned in the beginning, we develop an experimental second order technique.

MR. CATTON: Is this for the time advancement or for the spatial?

MR. ANDERSEN: Both. It is central, both in time and space.

MR. CATTON: Oh.

[Slide.]

MR. ANDERSEN: In this, the curve 3 here, if you do solve the -- going back to this slide here.

[Slide.]

MR. ANDERSEN: If you use a second order integration technique, you can show analytically that there should be, you should be able to calculate exactly a traveling wave, with no damping at all.

We tried to put this method into tne TRAC program.

[Slide.]

MR. ANDERSEN: And this is the type of results we got for the damping for the same case, using a second order integration technique, showing that you can get much less damping.

Now, it does turn out that the second . .der integration technique --

MR. CATTON: Doesn't this also approach the limit of one as you decrease the spatial differencing?

MR. ANDERSEN: Ideally, the exact solution is, independent of the spatial differencing, is at one.

MR. CATTON: But in reality, that never works out.

MR. ANDERSEN: In reality, we didn't get exactly that. We got more like .95.

MR. CATTON: Did you do the spatial --

MR. ANDERSEN: It is the same type of difference between the calculation with TRAC and the exact solution as we saw for the other difference techniques.

MR. CATTON: But you didn't do a spatial differencing study for this case?

MR. ANDERSEN: Not for this case. When we did the comparison to the data, we did some variations in the spatial nodalization. And I think Jim Shaug will be showing some of those results. And similar studies have been done also at EG&G

in Idaho.

Now, in turns out that the second-order technique has other unwanted features such that it does allow information to be propagated against the direction of the fluid flow which often gives you numerically-induced triples in your calculations. So we decided to do all our subsequent calculation using the explicit methods.

I would like to just show one example --

MR. TIEN: Could I just, and maybe it is not particularly relevant, in your second-order differencing, did you try to study the difference in terms of spatial differencing and time? You are doing that for both, second-order?

MR. ANDERSEN: Yes.

MR. TIEN: Did you try to see which, maybe a factual difference --

MR. ANDERSEN: No.

MR. TIEN: -- and so on?

MR. ANDERSEN: No, no. I didn't try the combinations of the two.

MR. TIEN: Okay. Because that would perhaps improve a lot in terms of the computational efficiency and so on.

MR. ANDERSEN: Yes. That is a possibility.

MR. CATTON: Also, there have appeared recently a number of methods of doing what is equivalent to upwind differencing that is nondissipative. There is some Japanese work that is an AIAA journal. There was the work that was done, I guess for EPRI, on the COMEX Code, and there are probably a half a dozen other examples of this.

MR. ANDERSEN: The COMEX use is skewed up in differencing, but that is for three-dimensional effects.

MR. CATTON: Well, but they found that there was a one-dimensional algorithm that worked very easily.

MR. ANDERSEN: Yes.

MR. CATTON: When they went to two-dimensional, they got a headache. But you are dealing essentially with a one-dimensional problem in a bundle.

MR. ANDERSEN: Yes.

MR. CATTON: It seems to me, going to the central differencing, that is nice, but it is academic. You would have been better off to have taken the approach that I guess has been used by Japanese and others for this particular problem, because you can control the instability, while also controlling the diffusion.

MR. ANDERSEN: Yes. I don't agree that -- I don't disagree that there are higher order methods available where you get better approximations on the spatial derivative. It is all a question of having time to put it into the computer program.

MR. CATTON: More than that, it is a question of how, it is a question of what kind of results you want to get, and how certain you want people to feel about what you have done.

[Slide.]

MR. ANDERSEN: This is the type of results we get. This is a comparison to one of the particular thermohydraulic stability experiments. And this is just taken out from the set that Jim Shaug is going to show.

But for this particular test, I looked at the implicit, the explicit, and second-order methods. And this particular test, which was at five mega-PASCAL, the onset of instability was at about six and a half megawatt. Now, using the three different methods, the implicit method calculated the decay ratio that was .51, now .61, which is consistent with the fact that the implicit has a large numerical dissipation.

The explicit method gave .97. And the second-order gave 1.08. Both the explicit and the second-order gave quite close agreement with the data. And what we have found is that, even the explicit in general tends to be conservative, compared to the data. The second order technique tends to be highly conservative, and that is another reason that we did not use it.

The explicit methods, as you will see, generally predict the data very well, but on the conservative side.

MR. CATTON: What was the delta G that you used for this calculation?

MR. ANDERSEN: In this case here, we used a -- I believe it was somewhere around 24, 30 nodes, actually. I don't remember the exact number, but it was in that number of nodes for the entire channel.

MR. CATTON: How long is the channel?

MR. ANDERSEN: The channel was on the order of -- probably in the order of 4 meter. I don't -- five meters.

MR. CATTON: And you used 30 nodes?

MR. ANDERSEN: Between 24 and 30 nodes. So that gives you a node size on the order of six to seven inches; in that range.

MR. CATTON: Well, if I use 30, I get what, .13 meters, which is about four inches, isn't it? I don't know. I can't do the division.

[Laughter.]

MR. ANDERSEN: Okay. In this case it was quite small nodes that we used. Same size of nodes that we have used in all our subsequent calculations.

Basically what we find is that the first order has too much numerical damping. The first order explicit methods have a small amount of numerical damping, and as you will see, we find that it generally predicts the data very well.

The second order technique tends to have no numerical damping, and be insensitive to the time step size, but it also tends to be very conservative compared to the data.

MR. TIEN: Insensitive to the time step size means that you don't need to do a second order.

MR. ANDERSEN: Well, I think the effect of that is more an effect on the cost of the calculation. That second order technique, if you decide to use that, will allow you to use logic time step and cut down on computational costs.

MR. CATTON: Yes?

MR. WULFF: Wolfgang Wulff from Brookhaven.

I think the second order term approximation should really be investigated with higher order terms, even though the second order damping is zero, there are fourth and higher order terms which may be of opposite size. That is, to get excitation, rather than damping, you need to carry out your truncation error analysis on to the next two terms, if you use the second order. I'm not simply saying that the number is zero and, therefore, I am -- on the previous graph, where you saw the staff with a line marked with a 3, you will see you have a difference that is explained by the higher order terms that are part of the truncation error analogy.

So you could have excitation with that second order term if you have several differences. And your decay ratio of 1.08 may be a computational instability, rather than a real instability.

MR. CATTON: I would agree with you. The only way you can get greater than 1 is computational instability.

MR. WULFF: No, it could be the system -- it is could be to the right of the imaginary axis.

MR. CATTON: But he is calculating a damping ratio; right? You're putting in something at one end of the pipe, and you're taking a look at what you get out the other end. And your damping ratio should be 1, because you have taken out all your --

MR. ANDERSEN: That's --

MR. CATTON: If it's not 1, there's a reason.

MR. ANDERSEN: That's exactly what I get in this calculation here, and I get a slight amount of damping when I do the calculation.

The other plot I showed here, not to be confused, this is not the same damping ratio I am showing here.

MR. CATTON: Okay.

MR. ANDERSEN: This is the decay ratio, which is the ratio between the magnitude, the amplitude of two subsequent periods, and the oscillation. So if the subsequent period is less than the first, you have a decaying solution -- you get a decay ratio less than 1. If you have an unstable situation, your oscillation goes, you get a decay ratio larger than 1.

MR. CATTON: When you look at an experimental case like you have here, do you first take a look at what the code would do under circumstances where it's not an instability, just to make sure that everything is done properly?

In other words, repeat your experiment that you show on the graph earlier; do you do that?

MR. ANDERSEN: I'm not sure I understand your question.

MR. CATTON: Well, you showed us an experiment, a numerical experiment, of the amplitude ratio as a function of the log of the Courant number.

MR. ANDERSEN: Yes.

MR. CATTON: Do you do that kind of an experiment with your experimental facility before you try to compare it with data?

MR. ANDERSEN: This is not an experimental facility. This is an exact solution. The FRIGG test facility, which this one is here, that's an experimental facility.

MR. CATTON: I understand.

MR. ANDERSEN: Yes.

MR. CATTON: But maybe you don't understand what I'm trying to put across.

You showed us a numerical experiment.

MR. ANDERSEN: Yes.

MR. CATTON: Where you compared your exit-inlet amplitude ratio with the log of the Courant number for the various methods.

MR. ANDERSEN: Yes.

MR. CATTON: Now you are going to try to compare your calculations against experimental data.

MR. ANDERSEN: Yes.

MR. CATTON: Do you repeat the numerical experiment with the particular geometry before you try to compare it with the experimental data?

MR. ANDERSEN: The way the experiment was conducted was not set up in this fashion. They didn't conduct this type of experiment. What they did in this experiment here was that they had a test facility that was in natural circulation, and they gradually increased the power until you got an onset of instability, at which point they stopped the testing.

MR. CATTON: I understand how they did the experiment; I'm just curious how you choose your time step and your spatial discretization in order to be sure that you have something reasonable as far as a damping ratio associated with the numerics.

MR. ANDERSEN: That was chosen from the numerical study we did compared to the exact solution. That was where we made the choice that we should use the explicit integration technique, we should use time steps as close to the Courant number as possible, and that you need to have, since when you get an oscillation in the channel, you get a density wave with a 180 degree face shift from the inlet to the outlet, and you need at least on the order of 10 to 20 nodes actually in order to get a reasonable resolution on the half period of that cosine wave you set up.

So the numerical technique, how to choose the time step and how to choose the spatial discretization, was guided by the numerical sensitivity study we did where we compared to the exact solution.

Furthermore, when we did the comparison to the data, as Jim Shaug will show, we did do also a sensitivity on the study on the number of actual nodes. Particularly what we looked at was at the number of nodes, at the inlet to the channel where the boiling boundary occurs, to see how sensitive we were to that.

And what we found, as you will see later on, was that we found a small, but not a very large, sensitivity.

MR. CATTON: Okay.

MR. ANDERSEN: That concludes my part of the presentation.

MR. SHAUG: I'm going to go through some TRACG calculations for the FRIGG natural circulation stability tests.

As I turned out, Jan's went through a lot of the presentation. I'll quickly go through some set-up. If there are no questions I'll just touch briefly on them.

[Slide.]

MR. SHAUG: This is a diagram of the FRIGG tests, the heated test section opening up to a steam separator region maintained at a constant pressure, a liquid reservoir then refeeds a recirculation loop with an inlet for water injection to control the sub-cooling at the channel inlet.

There is also a flow-meter to measure the flow rate and detect any oscillations that might occur.

[Slide.]

MR. SHAUG: There were some handouts in the presentation about the geometry. I'll skip those unless there's a question and we can come back to them.

[Slide.]

MR. SHAUG: Just to touch briefly on how the tests were performed, the loop was initialized to the desired steady state conditions, at power is well below the expected onset power of instability and then the power was increased and held constant until steady state was observed. This is the portion where the natural circulation versus power level could be determined and the power continued to increase until stability onset indicated by oscillations in the downcomer flow was detected.

We followed basically a similar scenario as we set up the test cases.

[Slide.]

MR. SHAUG: For the tests we compared the natural circulation flow versus data and this comparison is at 30 bars and the crosses of the data so at specified power levels they will let the loop go in natural circulation and record the flow rate.

We did a similar calculation with TRAC and the predictions are shown as the squares. Quite a good prediction of natural circulation flow.

[Slide.]

MR. SHAUG: As far as the actual onset predictions, you'll see on the comparisons with data we used two different nodalizations in the TRAC calculations, what we refer to as the X1 nodalization, a fairly coarse, ten-inch nodalization. We followed that by an X5 nodalization. Here we subdivide the node at the bottom of the channel where sub-cooled boiling occurs or boil initiation and we subdivide it down to less than an inch. You see the sensitivity.

MR. CATTON: I take it there was an X2, X3, X4 and arriving at X5?

MR. SHAUG: Yes. By the time we got down to X5, I think we had seen the kind of sensitivity we were going to get for nodalization around the boiling boundary.

MR. CATTON: At least for the FRIGG geometry you found this.

MR. SHAUG: For the FRIGG geometry.

[Slide.]

MR. SHAUG: Okay, on this comparison, which is at 40 bars, again like sub-cooling to 5 degrees, let's first walk through the data and identify where that is. Again if we remember how the tests were performed, we have several measurements here at the lower powers for which we constructed the natural circulation response and when we hit this leftmost dashed line the tests were still predicting a stable operation.

At the rightmost dashed line the test facility registered an oscillation in the downcomer flow indicating the onset of instability, so the actual onset of the loop would be found somewhere in between those two dashed lines.

Now we did a similar calculation with TRAC, again stepping up in power, performing our natural circulation tests and then performed a calculation at the last stable point in the data.

Now we see our two nodalizations, the square representing the coarse, the cross representing the fine nodalization around the boiling boundary. You can see that the coarse was giving us the stable calculation at the last stable calculation in the data.

The fine nodalization at the boiling boundary had begun to show a small oscillation in the downcomer, very close to the onset using the very fine nodalization. We then stepped up the power to the first point in the test data that showed oscillation or actually the final test point and they both predicted virtually the same amount of oscillation in the downcomer flow.

We continued on and the oscillation increased in magnitude. What we see here is low calculation, making a very good prediction of the onset, and very little sensitivity as far as the onset position as a function of whether we nodalize to a very fine degree around the boiling boundary or we use a very coarse node.

[Slide.]

MR. CATTON: So the point is that the coarse nodalization leads to an overprediction of amplitudes. What is happening in the core?

MR. SHAUG: Overprediction of the amplitude --

MR. CATTON: Am I reading the downcomer flow rate, fraction of average?

MR. SHAUG: Okay, again remembering that what we are interested primarily for this test setup is the onset. Okay, as far as the amplitude, we do overpredict the recorded amplitude in the test data. It is not clear that the test data is as steady at that point. They just recorded an oscillation that's maintained.

MR. CATTON: Wait a minute. Maybe I don't quite understand your graph then.

What you are doing is you are slowly increasing the downcomer flow rate?

MR. SHAUG: We are slowly increasing the power.

MR. CATTON: And you are looking at the downcomer flow rate.

MR. SHAUG: Looking at the downcomer or change on oscillation indicated by change in --

MR. CATTON: And this is the amplitude of the output.

MR. SHAUG: This is the amplitude of that change. Now the test was terminated as soon as they indicated an oscillatory condition in the downcomer.

MR. CATTON: Well, they didn't hold it and take an average or anything?

MR. SHAUG: No. They just registered this point. They got an instability stop-test.

But I think we have seen and would expect with an electrically heated channel, you would expect to see once an oscillation begins very little damping and so we get in our calculation very large oscillations in the channel flow.

I think if they took another increase in power, you know, we would see the same kind of response in the test. I think -- it's hard to know whether the data would have been this curve or this curve, but again I think --

MR. CATTON: I understand. I just didn't understand the graph.

MR. SHAUG: The data was shown on there just to indicate where the oscillation was first recorded in the test.

[Slide.]

MR. SHAUG: Here again, another example following the same steps. Here, you notice very small amplitude at which the test was stopped, and here you notice that that last step was very small. At the left-most dashed line, they were stable.

In this case, they incremented the power a very small amount, and so they got a very small amplitude in their recorded downcomer flow. Again, making the TRAC calculations using a two nodalizations, we find at the last stable point in the data, both calculations are predicting a steady flow.

We went -- I take that back. Both taking and calculating a stable flow. We went to the first recorded unstable point. There, again, both calculating a stable flow. Again, very small power increase. We then took it a little bit further and then first the fine nodalization took off, and we get --

MR. CATTON: Your graphics people don't do a very good job. If I extend those lines, they don't predict the same onset.

MR. SHAUG: Well, we took the fact that there was a difference in amplitude for those two points to indicate that the onset was somewhere in between this point and this point, and was closer to the stable point for the fine nodalization since it had gotten up to a more -- to a larger amplitude.

So I think during this period, we expect to see them very close as far as slope. Again, I think the important indication is that they're both predicting oscillations of very close to the onset and there is very little difference as far as onset prediction between the two nodalizations.

[Slide.]

MR. SHAUG: What we have here is for one of the cases at an unstable condition, these right-most points are the two amplitude of oscillation at the channel exist using our explicit calculation, Courant limit of one. Again, showing a small difference here in oscillation amplitude.

We then performed calculations where we reduced the Courant limit to see how sensitive we were to time-step size, and we can see from the fine nodalization, we see virtually no sensitivity here. There's a slight slope to it, but not anything significant.

We see close to the same situation for our course nodalization as we go smaller time steps. We see some increase, but, again, nothing that's really significant.

[Slide.]

MR. SHAUG: The next chart is a summary of all the tests that we performed. What we see here is essentially increases in pressure, 20, 30, 40, and 50 bars. The box indicates the actual conditions.

What we see from this is that certainly in the range of 40 bars and above, we're doing an excellent job as far as onset power prediction. When we compare against the test to onset power, in all cases, we are conservative relative to the test onset power.

MR. CATTON: Which mesh was this?

MR. SHAUG: This is the --

MR. CATTON: X-5?

MR. SHAUG: The X-5. But we've seen from the actual comparisons that if you plotted the X-1 on there, I'm not sure you could tell the difference between the two.

[Slide.]

MR. SHAUG: Just to summarize what we've done with FRIGG, we have a very good prediction with natural circulation flow versus power. We get a very good prediction of onset of instability above 30 bars and get lower pressures where conservative. We find the oscillation amplitude and onset is slightly sensitive to the nodalization at the boiling boundary and we find very little sensitivity to the time-step size using the explicit numerics.

[Slide.]

MR. SHAUG: Having completed the FRIGG testing, we also had access to some parallel channel data. So we wanted to give this a try as well. Again, just a quick view at the test loop. Here we have the two identical channels. In this case, they're fed by a prescribed flow at a prescribed inlet subcooling, and at the exit of the channel the constant pressure is maintained by a steam drum.

[Slide.]

MR. SHAUG: I've also shown you the rod bundle.

We'll skip that, TRAC nodalization.

[Slide.]

MR. SHAUG: Test procedure, very similar to FRIGG. Nothing too special about it.

[Slide.]

MR. SHAUG: I did show you a couple indications of a stable condition and an unstable condition. Here we see a -- what we're looking at as far as a stable prediction. Again, if you notice the scale on that. And a sample of an unstable calculation. Here, A and B are representing the inlet flows to the bundles.

MR. CATTON: Do you have to put a disturbance in to kick it off?

MR. SHAUG: No. Well -- no. The process of starting the calculation, what we do is -- I'm not sure exactly how to describe this. We prepare an input deck from a steady state calculation and the input deck itself only has so much accuracy in the input itself. So there's a built-in disturbance when we then take that input deck and supply it back.

We've watched a little of the accuracy on the pressure distribution up the channel. Now, that disturbance, you notice in the stable case, causes a very small disturbance in the mass flux. Now, under a stable condition, that disappears very quickly.

In the unstable case, that small disturbance is enough to trigger the oscillation if the conditions are correct.

MR. CATTON: And the two channels are out of phase.

MR. SHAUG: The two channels are out of phase, maintaining a constant pressure drop across the two and a constant total inlet flow.

MR. CATTON: WHere did you fix the pressure? Did you fix it --

MR. SHAUG: You fix it in the -- by supplying a constant flow. We supplied a break at the top and then the test was run by forcing a desired inlet flow at constant conditions.

MR. CATTON: You've modelled the pipe length and everything else.

MR. SHAUG: Yes.

MR. LEE: But this out of phase oscillation is entirely the consequence of constant inlet flow.

MR. SHAUG: That's right. Again, the way the test was run. The test of the pump supplying constant inlet flow. The split between the two channels was then determined by the channel conditions.

MR. LEE: And as a result, from the data, you could also tell that the pressure drop is essentially constant.

MR. SHAUG: Yes.

[Slide.]

MR. SHAUG: What we see here is a summary of tests performed at 70 bars and, if you notice in the system diagram, there are two -- a set of inlet valves at the bottom of each channel. Two sets of tests were run, one with those valves completely open, one with those valves partially closed.

This is the full open case. What we see here is the onset indicated in the test data at given inlet subcoolings. And what we did in our TRAC simulation was we ran a case at the given condition and then, depending on whether a TRAC calculation was stable or unstable, we made a second calculation just to see whether we were in the ballpark of predicting the onset. And in all cases, I think we're reasonably close.

MR. SCHROCK: Do you know in the computation if the resistance in that downstream pipe has much influence on the stability?

MR. SHAUG: It will. Here again, providing resistance to the flow oscillation, but there are some losses in that pipe.

[Slide.]

MR. SHAUG: Here we show a summary of the results at the 20 percent setting from the inlet valves. Again, fairly good agreement with the test data at various settings of inlet subcoolings.

MR. CATTON: Where is your TRAC calculation? Is it on here?

MR. SHAUG: Yes. The data is the box. The TRAC calculation, we performed one calculation to see if it was unstable or stable, and then just made another calculation to see if we were close to the onset in the data. Again, not the detailed study that we made with FRIGG. We just wanted to see if things that we had learned from FRIGG we could then apply to the parallel channel tests.

[Slide.]

MR. SHAUG: As far as conclusions for our hydraulic stability testing, we've got good agreement with instability threshold power, with data, and, in summary, we've qualified against single channel and parallel channel test data.

The next topic is a little bit of a change of pace.

MR. LEE: Excuse me. May I go back to the previous set of materials that you had. Was there a measurement of oscillation periods in this two-bundle?

MR. SHAUG: There was some indication of frequency and we spot-checked several of the calculations and the period did agree quite well. We did not confirm all the test points, but the frequency did agree quite well for the ones that we compared.

MR. LEE: Thank you.

MR. SHAUG: Again, we would expect that. We would expect a very good agreement as far as void fraction and transient time up through the channel.

[Slide.]

MR. SHAUG: GEXL application to stability analysis. We wanted to provide some information that are critical quality correlation was applicable under oscillatory conditions. So just to summarize some testing that's been performed, I guess it's ten years ago, they performed thermal hydraulic stability tests in the Atlas C transfer test loop using electrically-heated, full-sized BWR bundles, inlet peaked axial power shapes, at conditions simulating natural circulation.

Two basic types of tests were performed. One is referred to as a limit cycle, critical power test. Here, very similar to what we looked at for FRIGG and for the two-bundle test, again increasing power beyond the instability threshold. This time in the Atlas loop, it was increased until boiling transition was detected in the channel, and the power level was held constant at that point.

So what you find at that point is a cycling boiling transition rewet, boiling transition rewet. They ran a similar type of test, only this time as the power was being increased to find the transition, was also oscillating 10 to 20 percent to see the effect of an oscillating power on a boiling transition.

During the test, bundle power, pressure inlet subcooling, inlet flow rate were recorded and the number of boiling transition cycles experienced by the bundle was determined.

[Slide.]

MR. SHAUG: To confirm the GEXL application, the tests were analyzed to determine the critical power performance during oscillations. The analysis was performed using a single channel transient design code and the code was driven with measured boundary conditions, flow, pressure, inlet subcooling, and power.

To summarize, first, boiling transition was predicted for all the test conditions as observed in the test.

[Slide.]

MR. SHAUG: Moreover, if we look at the -- again, comparing measured and predicted -- in this case, number of cycles of boiling transition during the test. So here is a question of whether we can pick up the rewet and subsequent boiling transitions.

Again, I think you see very close agreement with the data for all the tests. The twos indicate that there are two test points with those same number of boiling transitions. Again, quite a good agreement.

[Slide.]

MR. SHAUG: Noting that the tests were analyzed using a design code, we wanted to confirm that our application in TRACG was also consistent with the conclusion that the GEXL is applicable. So we wanted to essentially rerun a couple of the test simulations using TRAC. So we picked a couple of them.

[Slide.]

MR. SHAUG: This chart kind of gives you a summary of what the conditions were. This is a limit cycle test, so you notice the constant power. This is the recorded channel exit pressure and the channel inlet flow rate. Those three, plus the inlet subcooling, were fed into TRAC and the solid lines are the TRAC predictions.

The little circles are the previous design code calculations and I think you see a very close agreement between the two calculations. Again, the characteristics are picked up by both codes and if you notice the number of times the thermal margin drops below one is the same in both calculations.

MR. LEE: I'm afraid I don't really follow what you are showing here. The pressure is the test data?

MR. SHAUG: Yes. The power, pressure and inlet mass flux are measured from the test. Now, those were supplied at boundary conditions to attract channel calculation. So all the boundary conditions are fixed. All we want to do is predict the thermal margin as a function of time.

MR. LEE: Is there any primary that you could compare with other than the boundary conditions?

MR. SHAUG: Well, the data that it was compared with was the number of times in the test that boiling transition occurred, so indicating a drop below 1.0. So there were various cycles recorded in the test over a given period of time indicating how many times boiling transition had occurred.

MR. LEE: Those are the experimental information, the dots.

MR. SHAUG: No. The experimental information is that there are, say, ten boiling transitions that occurred over maybe 50 seconds. The original comparisons that I showed you were run using a design code. Those are the black dots. So its prediction travelled a very similar pattern to a TRAC result and the summary chart that I showed you indicated a comparison of the number of times the design code predicted boiling transition compared with test data, and it agreed quite well.

MR. LEE: So you have some quantitative indication.

MR. SHAUG: Yes.

MR. LEE: That indeed --

MR. SHAUG: That the design code is predicting a very good response as far as boiling transitions, indicating that the GEXL is giving us a very good indication under the oscillatory conditions when boiling transition would occur.

This was just to confirm that if we used that same GEXL correlation in TRAC, that we have put it in there correctly, that we're getting, again, a very similar response. We just want to extend the conclusions of the prior study to

TRAC without having to redo all the calculations.

MR. SCHROCK: I don't understand how that can come out that way in view of what Mr. Andersen was showing us earlier, with the neglected subchannel effects you had no boiling transitions predicted and --

MR. SHAUG: No, no, no.

MR. SCHROCK: You had to stick those in and that -- did I misunderstand that explanation?

MR. SHAUG: I think what Jens was indicating was that specific rods may or may not go into boiling transition and, if they do, the temperature increase may under-predict the data. Now, GEXL will give you the limiting condition as far as boiling transition.

MR. SCHROCK: If you have the right thermal hydraulics locally on the rod.

MR. SHAUG: Well, GEXL is correlated to average conditions and so it will predict based on the average conditions.

MR. SCHROCK: Okay. I see.

MR. SHAUG: It's the actual individual rod response --

MR. SCHROCK: It's not identified.

MR. SHAUG: -- that's a function of the local conditions.

[Slide.]

MR. SHAUG: Again, this is just another calculation. This is a power oscillation test, so it's a little more well defined. Again, boundary conditions in the calculation based on the test data; oscillating power, pressure, flow, and subsequent calculation, both TRAC, solid line, dotted line the prior calculations.

Again, essentially no difference between the two, indicating that what was concluded about the GEXL correlation in the design code could equally as well be concluded about GEXL in TRAC, that GEXL is applicable to conditions during oscillations and TRACG using the GEXL correlation is applicable for calculating critical power during those oscillations.

MR. SCHROCK: Could I pursue the question just one more step so I understand it fully. GEXL is based on average bundle conditions, but at the same time, the parameters are for specific power distributions among rods and geometry of the bundle. Don't those features have to be specified when you make a comparison of this kind?

MR. SHAUG: You mean --

MR. SCHROCK: What is the configuration of the bundle that this comparison is made for?

MR. SHAUG: Yes. I didn't go through all of that. It's a GD-BWR bundle and --

MR. SCHROCK: It's a specific enrichment distribution.

MR. SHAUG: It's, again, electrically-heated and the test was run at a specific rod-to-rod power distribution. That power distribution was then used to calculate the parameters for the GEXL.

MR. SCHROCK: Thank you.

MR. SHAUG: That concludes what I've got.

MR. CATTON: Could I have a little help in estimating where you're at with respect to --

MR. ROUHANI: We're an hour behind.

MR. CATTON: An hour behind. You have an hour left. If you're -- then you're thirty minutes behind. That's no problem. I think we're all right.

MR. WATFORD: My name's Glenn Watford from GE. What I'd like to talk about is the actual plant data that we are current using for the qualification of TRAC for stability purposes. So we've moved from the separate effects through to the single channel test, and now I want to talk a little bit about the actual plant test that we'll be using in the TRAC qualification.

Some of the test data is going to be discussed as Jim Shaug later talks about the qualification. What I want to spend a little time on is talking about, in a little bit more detail, how the tests were actually run, some of the observations from the testing. We're going to also talk about two tests, the Leibstadt specific test, and I shouldn't refer to LaSalle as a test -- it was actually an event -- and talk about, one, some of the problems you encounter when you're using a total plant integrated tests and events and qualification, and also how we spend as much time as possible understanding the test data first before we put it into the qualification database.

The two tests that we've chosen for a lot of reasons -- some of the better reasons for choosing these: One, the LaSalle event was a core-wide in-phase oscillation. The Leibstadt test conditions were all what we call regional oscillations where the core neutronically was oscillating with a line of symmetry across the core, and a 180 degree out-of-phase oscillations from one side to the next.

Both Leibstadt and LaSalle provided data on operation beyond the onset of oscillations in a full-scale reactors, and were also run at a variety of power and flow, power distribution conditions. So they really provide a good variation in conditions and actually quite a challenge to TRAC to try and match these conditions and do as good a job as possible.

[Slide.]

MR. WATFORD: I want to first talk about the Leibstadt stability test. There was a series of tests that were done during the start-up of the plant. This is just kind of to put you in perspective of BWR power flow map.

The region we're talking about in stability is this little corner down here, relatively low flow between natural circulation, and very low minimum forced circulation conditions. And in the higher powered regions, there's two lines on this curve, two rod lines. This would represent operation that rated power-rated flow, holding control rod pattern constant, reducing flow. You'd follow this line.

Some plants operated with what's called a maximum or an extended operating domain, where, at lower flows, there is operation at powers above the rated rod line, but bounded by the rated power conditions.

For the type of plant Leibstadt is, this is a plant that operates with the maximum extended domain; goes down as low as 75 percent flow at rated power. So, really pushes the corner here. Considerable amount of testing was done in this region to understand the effects.

MR. CATTON: Are each of those black dots a point at which an instability took place?

MR. WATFORD: All except for one.

MR. CATTON: The one above the line?

MR. WATFORD: That's correct. And I think we do a little -- well, no, the -- let me go back. Of these test conditions, we do ourself a little bit of an injustice, I think, when we show these things. We don't show all the points that were stable. There are a large number of tests that were done in here where no oscillations occurred at all. These were in general inception points. I'll talk a little bit more in detail.

There's two points right here -- one of them is stable; one of them is unstable. There were two different sets of conditions, and I'll get into more detail about the differences between those.

[Slide.]

MR. WATFORD: The testing was really done in three stages. Most plants, in their normal course of their start-up test program, initial cycle, will go through some tests that will, in a limited way, look at the stability of the plant. Leibstadt was performing these tests, and actually modified the test to the point where normally, the purpose of the test is to go to a certain point relatively bounding on the conditions that the plant would operate at, determine if there is any instabilities at that point.int.

The regulatory in the utility decided they wanted to find out how far they could go, and where exactly oscillations would occur, and test conditions one and two that I've indicated here are those two such tests where they basically went to a given flow, withdrew control rods, until they established limit cycle oscillations. In fact, in this condition, this one is almost four or five percent above the licensed condition at which they would be allowed to operate, and, in fact, they started running into their rod block lines that would not allow them to go any further under those conditions.

Those two tests were done in not as controlled a manner as we would like to be done for tests where we're going to be taking data and qualifying models. The amount of data recorded, the information about the plant conditions, are not as precise under these two tests as we would normally like.

After these two tests at the reqeust of the regulator, we did additional testing a month later to more precisely look at the oscillation characteristics. There were basically two sets of test done, one with normal feedwater temperature, and a second set of tests done with a reduced level of feedwater temperature.

The basic test procedure would be to go to a certain flow rate, withdraw control rods. I've blown this up just to make it illustrative. In reality, this test went all the way up to the maximum allowed rod line for operation. Core flow was then reduced until the onset of oscillations were observed. There are a lot of intermediate steps where flow was held constant, data was recorded, and things were evaluated.

At this point, test condition four indicates the onset of oscillations for the set of tests that were done with normal feedwater temperature. These tests were done extremely precise with very small changes in flow. This whole range of flow is probably only four or five, six percent in flow, and we took almost an entire day to traverse down that line.ne. So we, as closely as possible, crept up on oscillations for that test condition.

We then continued to reduce the flow at different steps, down to the minimum flow. This plant operates with constant speed recirculation pumps and a flow control valve. There are actually two pump speeds that they can operate at. This was operated at the low pump speed, where the flow was varied by closing the flow control valves, and, in fact, at low speed, with the valves to their minimum position, the flow rate is indistinguishable from natural circulation flow rates. So it gave us the capability to vary the flow in the region where we really would like to see the variation.

After the minimum flow was reached -- call it test condition 4A -- the flow control valves were slowly opened back up, and the plant basically traversed back up to this line.

Soon after that, a second set of tests were done in which power was reduced, flow was reduced somewhat, and then a series of bypassing of feedwater heaters was performed to effect a known amount of feedwater temperature reduction from the feedwater system.

A specified amount, 20 degrees C, had been chosen. After that temperature change had been performed, the plant was still stable under these conditions. Some control rods were withdrawn a little bit more, and a point was reached at which it was decided to reduce core flow.

A same type of procedure was used. Five, again, is the best defined onset of oscillations that is possible to do at a plant. It turns out that when you cross the area between stable conditions and unstable conditions, at least in a plant it's not a nice clean break. You can watch the neutron flux monitors, and under all of these conditions, there is some level of coherence in the signals, and very slowly, over actually a band here, is when you begin to see pure, sustained oscillations at which you would conclude that you're at a limit cycle and a decay ratio of one.

Again, we then went further beyond that point, reduced flow all the way to minimum to determine the impact as you go beyond the point of inception.

A month later, to look at the -- these were very controlled tests for the specific purpose of really looking at the plant response. The regulator requested a two-pump trip into basically the same region to look at the effects of probably a more normal entry into that condition, when and if you ever do get into this condition, and also present more of a tangent condition to the plant to observe the plant response under those conditions.

[Slide.]

MR. WATFORD: Basically, the conditions were started from very near the maximum rod line, not quite at rated flow conditions or the flow at which you would be at rated power, 90, 92 percent power. both pumps were tripped to off. And the core flow coasted down to natural circulation conditions.

What happens in a BWR is. under these conditions, when you initially trip the pump, there is still stored energy in the fuel. And the heat flux stays high, the steam flow initially stays high, your feedwater heating at least initially stays at the same level it would be under these conditions.

As your heat flux slowly begins to decay after the flow comes down and the neutron flux, the neutron power comes down, the steam flow and resultant feedwater heating begin to reduce, and there is a gradual increase in feedwater temperature until some steady state conditions are reached at the new power flow point.

We have kind of graphically shown it as you come down initially lower. In fact, the flow coastdown is within a matter of seconds. So very quickly you come down to a lower power, and slowly the feedwater temperature reduces as the steam flow goes down, and your temperature, your reactor power will slowly increase.

This test was terminated before feedwater temperature had completely reached an equilibrium, but it had slowed down considerably, and the power increase was very low at this time.

The main point on this test was this was a test that ended up well within the region where oscillations had occurred a month before. Yet they waited and they waited and they waited, and thy were very disappointed because they didn't get any oscillations.

[Slide.]

MR. WATFORD: One of the aspects of the test that was looked at quite a bit was the oscillation modes that were observed in the neutronics. Both of these, all the test conditions were what we call a regional oscillation, where the harmonics and the nuclear solution were excited. Both were what we call half-core oscillations, where the dominant lines of symmetry across which you would have a detector on this side of the core and compare it to a detector on this side of the core, and you would clearly see a 180-degree out-of-phase oscillation between those two, indicating that the inlet flows were also oscillating 180 degrees out-of-phase.

As you moved towards the center, you actually crossed a point where detectors along this line really showed very little, if any, oscillations. And then, as you continued, you saw basically a mirror image of the oscillation that was occurring on this side of the core.

There were two distinct sets of conditions based on the time in the cycle when they were performed. And what we saw were two different lines of symmetry, basically the first azimuthal mode. But during the test, the dominant line of symmetry for these tests was along this axis, and for these tests was basically along the North-South axis.

After we went back and looked at the conditions in a little bit more detail, it became very clear, these tests were all done in what is called a B-sequence rod pattern, where you do not have octant symmetry in the rod pattern. The rod patterns are half-core symmetric and quart er-core symmetric. But at that point, they lose their symmetry.

These tests were done in what is called an A-sequence, where the center control rod and its symmetrical rods are part of the rod pattern. It is quarter-core, half-core and eighth-core symmetric.

So the rod patterns were clearly playing a role in determining where the dominant symmetry lines were resulting.

[Slide.]

MR. WATFORD: Jim Shaug is going to go over this a little bit more when he talks about the actual TRAC calculations. I have summarized all the test conditions here. Basically, we have been, in the qualification, concentrating more on the tests where we had very good control data, the four and the five tests. We have looked at Test Condition 2. We see a varying range of power and flow conditions that the tests were performed at.

As we expect, there is a sensitivity to the power and flow in the frequency. The frequency changes with different inlet flows, inlet velocities. Change in the void distribution can also affect the frequency of the oscillation.

I note on here, on Test Condition 6 -- this was the final conditions of the recirculation pump trip -- basically no data, because it was stable.

There were a range of oscillations with the LPRMs, or the local power range monitors, where in the center of every 16 bundles there is located a string of four axially-placed fission detectors that measure the neutron flux locally. So in a core the size of Leibstadt's, there is probably 140, 150 of these. We recorded maybe a fourth of those, at least one from each string in the reactor, and looked at the relative magnitude of the local oscillation relative to its average. And then the APRM is a spatial average of all the LPRMs, or a selective number of LPRMs, to get a measure of the core average behavior.

As expected, because of the out-of-phase oscillations, there is some cancellation that goes on. And so the APRM in most cases is some fraction, as far as the magnitude of its oscillation.

The tests went anywhere from, at inception we were seeing 10 percent of point. 4 and 5 are tests where it was really the inception of oscillation; 1 and 2 were the ones done during the initial startup test program. During this period of time, control rods were still being withdrawn. And it is very hard to define where inception occurred during those tests. 4A was the case where flow was reduced from this initial inception point, and as flow was reduced, we watched the oscillation magnitude reach a higher steady state limit cycle value as we went further beyond the initial inception.

Most of these tests, at least 4A and 5A, were done where flow would be reduced, the plant would sit there for ten minutes at steady state to make sure that the increase in the limit cycle oscillation had leveled out. We would take ten minutes of data and then we would reduce flow again to a new point.

MR. CATTON: The difference between 4 and 4A is just very minor. Yet the amplitudes are quite different.

MR. WATFORD: These are rounded-off numbers. The difference is probably 2 percent flow, on the range of 2 percent, 1-1/2 to 2 percent, within the accuracy of measuring that. And yes, this is -- Now, this is percent of average. When everything was said and done at the end, the actual magnitude of the LPRM oscillation on its scale was 30 percent peak-to-peak. Now it is operating down at about 50 percent of its scale.

But yes, there is an increase. There is a sensitivity as you reduce flow, and other conditions. That is one of the things that we have always seen in stability tests and calculations, is there is sensitivity to different parameters. And so you need to look at those in the qualification.

This gives us a set of test conditions where we have looked at sensitivity to power, to flow, to power distribution. These two were run at a lower inlet feedwater temperature, so there is a wide range of different conditions.

It is interesting in Test Condition 5 to 5A that we made basically the same amount of flow reduction as from 4 to 4A, yet the change in oscillations was very minimal.

In fact, you can look at the data at different points in time and conclude that this point is almost more stable than the initial point.

So these present a good, wide range of data to look at under varying conditions, and it is a real challenge for the codes to be able to predict that.

MR. LEE: Excuse me. Does anyone of the local power range monitored data represent the maximum of the amplitude?

MR. WATFORD: They represent an average of the region in which they reside. So they would not directly measure a peak bundle. If it was located adjacent to the bundle that was oscillating at the largest magnitude, it would also be averaging the response of other nearby bundles. So the peak bundle could very well have some ratio higher than that oscillation.

MR. LEE: Is it the reading of one detector, or average of the four?

MR. WATFORD: This is one detector. These numbers are taken from one detector. Now, one of the things that we have observed, and this has been fairly consistent among all the tests, is that when you represent the values this way, the peak-to-peak oscillation magnitude relative to its average value, these numbers are not sensitive to the level that you are in the core. So there are four levels, A at the bottom, D at the top. The peak-to-peak over average, for the B, C, this usually would be we would just take it from the A level, because it is usually the largest average value, so that the absolute oscillation is larger, but the normalized oscillation is very within the accuracy of taking a trace and taking it off of there. You get 66 percent of the average at each of the points.

MR. LEE: But you do also sort of phase shift among these four levels?

MR. WATFORD: Yes, we've also, from the data we can also calculate the phase shift of the density wave. And that is another bit of information that Jim will talk about, and how TRAC calculates that phase shift, also, relative to the rest data.

MR. LEE: But if you calculate an average of the LPRM data at a given point in time, you may not see 66 percent.

MR. WATFORD: That is correct.

MR. LEE: Much less.

MR. WATFORD: Not much less. It is about 80 percent. The phase lag is about 90 degrees from the A level to the D level.

After the [illegible]lle event, we looked at the effect of that and how that averaging affects, say, the APRM signal, even when the core is in phase. And that reduces the peak by about 80 percent, compared to a case where you had all four levels oscillating in phase.

MR. LEE: To about 80 percent.

MR. WATFORD: I'm sorry. That 80 percent -- If you took the four levels and oscillated them at some magnitude, say 100 percent, and put them all in phase, the average would be 100 percent.

If you gave them the 90-degree phase shift that they truly have, and averaged that, the peaks would be about 80 percent of what the peak would be if they were all in phase.

MR. LEE: Thank you.

[Slide.]

MR. WATFORD: I have a couple of examples of the actual recorded data from the tests. These are from A-level or detectors that are 18 inches from the bottom of the core, very close to the boiling boundary, the closest detector to the boiling boundary that we have.

I have represented what we will call the radial distribution of those four detectors throughout the core. This is for Test Condition 4, which was the onset. And as you see, for detectors 1 and 2, which are on this side of the core, they are basically in-phase, very small amplitude oscillation. These weren't neccssarily the peak LPRMs. These were the ones that we had the best recordings at the time.

You go to the opposite side of the core, and you can clearly see the 180-degree phase shift between the two sides of the core.

MR. LEE: This particular figure indicates peak-to-peak to 5 percent.

MR. WATFORD: Five percent or less. But this is absolute. This is not of its point. And these are not necessarily the peak LPRMs. The peak LPRM for these tests, I believe, is one that is located down here, that we just did not have nice plots available in the short time that we put it together.

But what you see is that as you move from the peak condition on either half of the core towards the center, there is a reduction in the magnitude of the oscillation. And these would both be in say a contour around that peak that would be somewhat reduced.

[Slide.]

MR. WATFORD: As we reduced flow further and reduced down to test condition 4-A, here are three of the four detectors we showed from the previous slide. Again, the magnitude is now larger. We have used the same scale and I have indicated 16 percent. Again, this is an absolute oscillation that was not normalized.

What we have also shown on here is what an APRM would look like under these same conditions. One of the characteristics that you see in an out-of-phase oscillation is what we call a double-hump type shape on the APRMs. This peak is clearly being dominated by the peaks from this side of the core. There is a second peak that's showing up here that is from the LPRMs that it averages from this side of the core. The APRMs are not perfectly symmetrical. The LPRMs are not perfectly symmetrical. So, the APRMs aren't a perfect average of the data. So, this is one of the characteristics that lets you look at the APRMs very quickly and determine the type of oscillation that you are seeing.

MR. LEE: I guess I fail to understand your last point. These small infrastructures that you observe is useful to identify a particular type of oscillation?

MR. WATFORD: That's correct. I could talk about it later, or I'll go ahead and talk about it now.

The LaSalle event did not have any LPRMs recorded during the event. Okay? The question obviously arose, what sort of oscillation? If it is a core-wide oscillation, you basically can determine the response of the -- locally by the core average. Okay? Things respond relatively the same, such as if the core average is going up a certain percent, locally the same thing is happening, just on a higher absolute basis.

If, on the other hand, it is an out-of-phase oscillation, you lose a lot of information in the APRM as to what's happening locally. By looking at the APRM traces from the LaSalle event, it was very clear that there was no presence of out-of-phase LPRMs. The APRM signal was very clean. I'll show it a little bit later. No indication whatsoever.

Since it's an average of a distribution of LPRMs, it was then very easy to conclude that all the LPRMs were oscillating, basically, in phase, as opposed to a side-to-side type oscillation

MR. LEE: If I may pursue this question a little bit more, if you have two half houses at the core, oscillating completely 180 degrees out of phase, the average will indicate no oscillation whatsoever.

MR. WATFORD: If the oscillations were perfectly linear sine waves, that's true. As soon as you get about 3 or 4 percent peak-to-peak oscillations, they already begin to lose their linearity, and very quickly, you begin to pick up the oscillations, even on the APRMs, because the peak of these oscillations is farther from its average than the minimum is.

So, when you take this peak, average it with this minimum, the average of those two values at that point in time is above the average, because this is further from its average than that is below it. It can't go below zero, so they very quickly begin to get skewed towards the top end.

MR. LEE: But to me, this is an indication of inherent lack of perfect symmetry in any operating reactor.

MR. WATFORD: No. I think we're talking about the non-linear system that we have. It's very non-linear. You don't get a sine wave out of this when it goes unstable. Maybe at very low magnitudes, you do.

MR. LEE: It doesn't have to be sine waves. If two halves of the core are completely out of phase, the average would be zero, regardless of how non-linear, how much of departure you have from sine waves.

MR. WATFORD: No. If you have perfectly symmetrical detectors, also -- say, we have two detectors that are measuring the average power in this half of the core and that's measuring the average power in that half of the core and look at them in time, and let's say they're 180 degrees out of phase, and I'm going to skew this for the specific purpose of making it very obvious when you look at it.

Okay. When you take the two halves of the core and you look at the summation of these two at this point, you average those two, you get a point above the axis. Okay? The average power at that point, of the total core, is greater than zero, and it occurs because of the fact that these become very non-linear. When you're averaging the peaks over here with the valleys over there, the peaks are further from their average than these valleys are, and there is a total core oscillation that appears to be twice the frequency. Okay?

Jim can -- may -- I'm not sure if he has got that specifically in his TRAC cases, but you should see that, that in TRAC it will show the exact same thing.

Now, what happens is, because the APRMs aren't perfect, depending on which side of the core has the dominant number of LPRMs, those two peaks do not show up the same. If you take the TRAC-calculated power, which you would see is a double peak here, where all peaks are exactly the same size, but in this case, at the actual plant -- and you can even look at different APRMs that take LPRMs from different strings. They'll all look somewhat different. Some of them are very close to being symmetrical and will show almost a perfect -- what we call a double hump. Others will show less of one, depending on which LPRMs they measure radially.

[Slide.]

MR. WATFORD: These are some, unfortunately, somewhat more crude plots. We still do not have nice, beautiful ones available. Those were plots that were taken, actually, real time during the testing. So, the sampling rate wasn't as high as we would like it to be, but the illustration here is to show the out-of-phase nature, detectors 1 on this side of the core, and 2, 3, and 4 on the other side of the core, relatively small magnitude oscillation amplitudes at these conditions, as close to inception as we could get.

[Slide.]

MR. WATFORD: These are oscillations, somewhat lower flow after that, where we reduced the flow from the same conditions, again the same basic relationship between the phases. What occurred under this test condition -- unfortunately, this is something that happens in the plant -- is you can't hold all your boundary conditions constant, and you have plant control systems that you have to live with. Looking at the data after the test, there was a very slow hunting in the feedwater control system that was varying feedwater flow 4 percent, peak to peak, with about a 20- or 30-second period.

Most feedwater controllers have an inherent, almost a historecis at conditions other than where they are really tuned to operate and will have a little dead band where they'll hunt for an exact level. So, there was a little bit of -- it wasn't a nice limit cycle, because unfortunately, the conditions of the plant were not nice and set, as we would like to have.

MR. TIEN: How about your number 2 trace? What kind of true waves are there?

MR. WATFORD: There is basically a 30-second period in the core flow, the level, the inlet feedwater temperature that's imposed on the average conditions, and as the core flow would increase or the core flow would decrease, you would see points that would have higher limit-cycle oscillations and points that would have lower limit-cycle oscillations.

MR. TIEN: It looks like you have two oscillations.

MR. WATFORD: Yes, there is. That's what I'm saying. The feedwater flow was oscillating at approximately a 30-second period, and if you look at enough of these traces, there is a nice, 30-second bead on top of it, because the inlet conditions were slowly varying at that frequency.

MR. CATTON: I think if you're going to go the LaSalle now, it might be a nice point to break for lunch.

MR. WATFORD: Okay.

MR. CATTON: Some of my colleagues are looking hungry.

When were these tests run?

MR. WATFORD: When?

MR. CATTON: When?

MR. WATFORD: September, October, November of 1984.

MR. CATTON: I have exactly 1 o'clock. So, we will reconvene at 2 o'clock.

[Whereupon, at 1:00 p.m., the hearing recessed for lunch, to reconvene this same day at 2:00 p.m.]

AFTERNOON SESSION

[2:00 p.m.]

MR. CATTON: We are going to try to stop at 6:00 p.m. today. So could we get started.

We are going to hear about the Lasalle event. The speaker is not here? There he is.

[Slide.]

MR. SHAUG: The second set of data for core total plant response that we are currently using in the qualification for TRAC for stability is the LaSalle event from last year, March of 1988. What I want to do is to spend some time going through the sequence of events.

I mentioned earlier that in an actual plant event, because you're not controlling the boundary conditions, the system response is very important and is very crucial in ensuring that you have interpreted the data correctly and when you do your comparison with your model, that you're comparing to the right thing, and that you're drawing the right conclusions.

I want to spend some time going over the sequence of events, what was observed during the approximately 6 to 7 minutes following the initiation and also to talk about some of the data that we believe is very important and critical in the qualification to this specific event.

LaSalle was operating very near to the rated rod line, 84 percent power, 75 percent flow. They were operating at this partial condition because one of the feedwater pumps was out of service and the capacity with the two remaining pumps was at this point.

There was some surveillance testing going on in the water level instrumentation and as a result of several valving errors by an instrument technician, a spike was seen on some of the level instruments -- not a true change in level, but just an inadvertent spike from the valving that was performed.

The Atlas logic for recirculation pump trip was initiated. Both recirculation pumps tripped to off and very rapidly -- well, within a minute -- the recirculation flow coasted down to natural circulation flow rate. We have 29 percent here.

There's obvious measurement uncertainty in the plant, but somewhere in this range is where natural circulation tends to fall. Because of the fact -- again, as I mentioned before -- in the Leipstadt test, heat flux lags the neutron flux. Your steam flow and therefore your feedwater heating initially stays near its initial level. Your feedwater temperature remains very close to the initial level, and so the power initially after a coastdown is lower than what you'd expect it to be.

[Slide.]

MR. SHAUG: Very quickly after the event, it was somewhere between 39 and 41 percent of rated.

MR. MICHELSON: Could you explain to me a little better, the valving area. All you saw, I assume, was a level instrument generating.

MR. SHAUG: It was a level instrument.

MR. MICHELSON: How does a valving error cause the noise in the level instrumentation?

MR. SHAUG: It's basically a pressure spike. The level instrumentation is a Delta-P transmitter.

MR. MICHELSON: What valving was involved?

MR. SHAUG: They were valving in -- they had valved out one of the pressure transmitters that had a common leg.

MR. MICHELSON: They were valving the instruments --

MR. SHAUG: Right, they were doing instrument testing.

MR. MICHELSON: I thougnt this was a process and I couldn't see --

MR. SHAUG: After the initial flow coastdown, several things occurred. One, the operators began to get alarms that the feedwater heater level, the level in the feedwater heaters was not normal. Under some cases, they begin to get some isolations of specific heater strings and for the first one to five minutes after the event, part of the operators' actions were directed towards trying to restore the heaters to their normal configuration.

The use of bypass to bypass some of the strings and try to maintain the normal amount of feedwater heating that would be under these conditions. Sometime later in the event, the last two minutes of the event, there were several attempts ot restart the recirculation pumps to recover from the natural circulation conditions.

On relative time scale, we'll be talking about some other events, but the reactor scram occurred approximately 6 minutes and 50 seconds after the initiation of the two-pump trip.

[Slide.]

MR. SHAUG: The next two slides are concerned with a sequence of events that really are one of the main things you're comparing to, and one of the focuses of the indications that we had of neutron flux and what neutron flux was doing, and also an underlying system effect was occurring in the feedwater system in that there was a variation in the feedwater flow and sometimes, at least when it was finally measured, a very large variation.

I want to go through some of the sequence of events that were occurring in the last few minutes and then try to show you how these events all worked together to result in the type of response that was observed at the plants. Somewhere very close to five minutes after the event -- 4 minutes 48 seconds -- the presence of LPRM downscale alarms begin to occur.

This was the first evidence that there was probably oscillations going on, at least from things that are recorded, such as alarms. At this time, in the middle of the event, the transient recorder was not operating. It was set in a mode that would automatically turn on when certain parameters were exceeded.

That occurred in th first minute of the event and occurred at the last minute of the event, but during the middle, there is no data recorded on the transient recorder. During this time period, about 15 to 20 seconds later, there was a low flow alarm on one of the reactor feed pumps. Soon after this, the first LPRM upscale alarms begin to occur.

The upscale alarms typically are at about 100-105 percent of the scale on the LPRM meters. The downscale alarms are typically down around 5 percent of scale. To put it in perspective, the LPRMs, the peak LPRMs are probably in the range of 40 or 50 at these conditions.

The average LPRM is probably reading about 25, so it's much closer to its downscale value and later simulations just using empirical correlations for what an LPRM oscillation would look like, also predicted that the downscale alarm would occur before the upscale alarms, because the LPRMs were just closer to that alarm level.

After the high alarms came in at 5:15, about seven seconds later, the last of the upscale alarms had cleared. There was basically every two seconds, given the resolution of the alarm recorders -- about every two seconds you could see the upscale alarms coming in, consistent with the period.

Seven seconds later, about the same time that the alarms cleared, a high water level signal was also reached. This isn't the Level 8 where the trip point would be, but there's a normal band, Level 4 to Level 7, where 7 is the higher level, that the plant tends to operate within, and it was the upper end of this band at that time.

Fifteen or sixteen seconds later, upscale alarms begin again. Very soon after that, or almost concurrent with that, was the presence of some low feedwater flow from one of the feed pumps. A little bit later, they cleared, and very son after that, we had high water level. This continued for the next minute of the event where we would see basically -- I don't want to call it oscillating at this point, but the alarms in the LPRMs would occur, very similar to the times when low water level alarms were occurring.

They would begin to clear and sometime in the same time period, the high water level would occur. It was real clear during this portion of the event -- actually, this part is on the recording and will show what's happening here. One of the challenges of the simulation is to try and simulate and as closely as possible, replicate what was occurring during these several minutes.

One of the things that happens with feedwater flow variations is that the effect on the core comes in several different ways. One, you get an effect on the water level as the feed flow is integrated in time. The water level can go up or down. This can result in a higher or lower natural circulation flow.

The second effect that occurs -- it's not as apparent and is not really recorded -- is that the higher feed flow for a given core flow will result in a change in the heat balance and a change in the core inlet temperature. Now, this takes a -- at least in the period of an oscillating of the neutron flux -- two seconds, we're talking about -- this change in feedwater temperature at the inlet is much slower for these variations that we're seeing.

There's a certain delay that occurs. You get a higher feed flow. That flow mixes with downcomer flow, goes through the jet pumps and then comes in through the inlet of the core, so there's a lag that's associated there. It's not easy to just look at the plots and say, oh, yes, at this point this must be higher feedwater temperature.

It's an effect that really has to be simulated to try and understand the combination of effects.

[Slide.]

MR. WATFORD: There was measurement of the feed flow and the feedwater temperature itself. The other thing that -- I'll get to that in just a second, I guess.

Data was recorded for the first minute and the last minute of the event, and during the middle of the event, there were one-minute averages that are taken by the process computer and that were recorded, and so you would have an idea what the average value was in the middle of the event, but you wouldn't necessarily pick up any variations that were going much faster than that.

The feedwater temperature response during a pump trip early on, I think some of the initial communications that came out characterized this event as a loss of feedwater heating event, and that's what caused the reactor scram.

It turns out afterwards when the data was examined much more carefully that the feedwater response was very similar to what we expected it to be. With any power reduction, there is a reduced amount of steam flow, reduced amount of feedwater heating, and a reduction in feedwater temperature.

It's relatively slow, there is a time constant associated with that process. It's relatively slow for these type of events. It turns out that the final temperature that was measured at the time of the scram, the feedwater temperature, was approximately 340 degrees Fahrenheit. If the plant had been running under normal conditions at those power flow points, the steady state feedwater temperature would have actually been about 7 degrees lower, about 340 degrees. So in reality, the final temperature of the event ended up at that time a little bit higher than what would have been expected under those conditions.

This is just a plot of the plant data, where the beginning and the end, you see we have some recorded time period, and here's recorded time period at the end, and we have some one-minute averages during the period. But this is a fairly typical exponentially decaying time response that you see from the feedwater temperature that scram occurred at this time, so you don't really have any data to determine what would have happened in the long run.

But basically the operators had restored at least sufficient amount of heating, had taken whatever actions were necessary, such that this was not a much more severe loss of feedwater heating than during that time.

So the temperature was really very close.

MR. CATTON: Why is that the temperatures measured continuously first and last minute, and not in the middle?

MR. WATFORD: These were the transient recorder recordings. It triggers on a certain exceeding of a parameter that would indicate an event is occurring. Because of the capacity of the system, the number of signals, the amount of memory, it can record so much, and that's about a minute for the system that they had.

It's set up to do that, so that some time later it can pick up some more data.

Something else was exceeded here. It was either level or flow, and it recorded again. These are more -- a different computer system that is a much slower sampling, like once every 10 seconds, and so it takes six of those and averages them and gets these values.

After the event -- and I think either as part of the testing prior to starting up after the event, or even maybe during the next start-up, an actuator valve, actually a control valve in the feedwater flow system, was discovered to not be functioning properly. It would basically get stuck at either the open or the closed position. It would have a demand to open, and it would stay shut, and eventually it would open up.

By this time, the level error that was sensed by the control system would say now you haven't put enough feedwater flow in, flow would go very high, you'd overshoot the level that would say close, close, and the valve wouldn't close; and then all of a sudden, it would close.

So there was a very erratic response of this valve during the event, and the result, at least in the last minute when we have specific recorded data, large swings in the feedwater flow were definitely observed.

Now there are a lot of different effects of the feedwater flow variations. I talked a little bit before about the water level, higher feedwater flow, you integrate the level up. A higher level gives you higher core flow, and then there is also the core inlet temperature variations. And to really treat this in a qualification or a comparison, you have to be able to pick up these different responses, both how they get integrated and also the types of lags that you would expect between the different responses.

What I have plotted here is basically the last two minutes of the event where the trace that's in red was the recorded feedwater flow variation for the last almost 60 seconds, 57-1/2 seconds or so. This would be approximately the average value during the time period and these are the type of variations that were actually measured during the last minute of the event. It went as low as 80 percent below its average to as high as around 60 percent above the average.

If we look at the period where we have the measured data, some very interesting things occur. One, very soon lagging somewhat, where a minimum is expected, we would begin to see high levels. Sometime after that, lagging again, the peak would be high water level alarms that were occurring.

Very soon after the minimum feedwater flow that was measured, we had a low level alarm, as you would expect. Sometime, almost at the same time, was when the LPRM high alarm occurred. Again, the flow went back up, the level went up also, and gave you a high alarm, and as the flow is coming back down, you would get LPRM high alarms.

And what we see prior to this time, when we have recorded data, we are at least two more -- I'll say oscillation periods where the same type of response was observed. There were LPRM high alarms followed, and actually would clear some time when a water level high alarm would come in.

At this point, a couple of points down here, we have some low feedwater flow alarms that occurred, indicating that level was probably coming back down. Some time after the level would come down, there would be another high alarm, and then a high water level alarm.

To be able to simulate this event, it's not sufficient just to have this one oscillation. This here is a span of about 60 seconds. To really get the integrated effects of what's happening to the feedwater temperature in the inlet of the core, it is really very difficult just to use this small amount of data that was recorded to accurately simulate what was happening. Because oscillations in the feed flow back under these conditions could very well be affecting the core out here where you are starting to see the delay in the lag and the change in feedwater temperature.

What we have used this data, the most data we have from the plant, is to try and construct the type of oscillations, trying to simulate as best as possible what was occurring before the final oscillation occurred, right before the scram.

I guess I didn't mark it on here, but on this scale, I think 65 seconds is the scram. So just like seven or eight seconds after the recording ended is when the scram occurred.

So we have taken the data that we have had recorded, the alarms that we have had, some knowledge of what happened to the system, to try and reconstruct what was occurring earlier to as closely as possible simulate the event.

Now we do have the last minute of recorded data. Fortunately I was able to squeeze all of them on here, except for the feedwater flow. We have seen the feedwater flow from the last chart. The water level is very similar, at least in its shape, in that the feedwater flow had peaked somewhere around here, was actually starting to come down, was at a minimum. The minimum in the level, since the level integrates to feed flow, was some time after the minimum feedwater. Feedwater flow was coming back up. Again the level was integrating that, feed flow started coming back down towards the end of the event, and here what you see is the level leveling off, and very possibly even beginning to go back down again.

Superimposed on top of the very slow feedwater-induced transient is basically 2.2 second variation in water level that was a direct response of the water level to the actual neutron flux and core power oscillations that were occurring.

The other thing that you can see from the water level is as the water level would decrease, you get less driving head at natural circulation, you'd see your core flow decrease, water level come back up, core flow would basically follow that directly.

Steam flow that we have here is really not as much a measure of these total peaks, but what's the average doing under these conditions, and there is a very slowly varying average also.

You still see the 2.2 second peaks on top as the heat flux basically integrates this neutron flux oscillation.

I put some relative peak-to-peak values on here. From the top of this level swing to the very bottom is about 20 inches. If you take it at the middle here, between the top and the middle between the bottom, it's more like 15, 16 inches. These were going several inches for the small oscillation.

The core flow from the peak to the minimum here was oscillating about 3 percent peak to peak. These values were much smaller, maybe only 1 percent peak to peak, as a response to the actual neutron flux oscillation.

What you see in the APRMs, and what you can infer from the previous minute or two of data, is as the core average neutron flux begins to increase at this time, first you would get LPRM downscale alarms, and if the peak got high enough, you would also get LPRM upscale alarms.

You can go back from here and find the occurrence of LPRM upscale alarms here. You can go back another period of the feedwater system and find LPRM high alarms again, and you can basically follow that in time, and if you go out here seven seconds more, you will find the peak that finally went above the scram setpoint.

But clearly between the core flow, less stable, less flow, you see the growing magnitude of the oscillations as the core flow went down. What you don't see is what is the effect of the feedwater temperature.

One of the things that you can see is even before the core flow is coming down significantly, you are starting to see some increasing peaks here.

What is hidden is what's the feedwater temperature at the inlet of the core doing?

MR. MICHELSON: Where is the core flow measured?

MR. WATFORD: This one is measured at the jet pumps. This would be the sum of a certain number of instrumented jet pumps.

MR. MICHELSON: Now the recirc pumps had already been tripped?

MR. WATFORD: They are tripped, so at this point you wouldn't be using their drive flow.

MR. MICHELSON: What's driving it to the recirc pumps to give you a measure of flow?

MR. WATFORD: Well, this is just the natural circulation flow from the density being less in the core than in the downcomer region.

MR. MICHELSON: It's a fairly small driving force, isn't it?

MR. WATFORD: Well, no, not really. I mean this is 30 percent of rated flow, approximately, that you get under these conditions. It's a substantial -- you've got --

MR. MICHELSON: That's right, near time zero you have power --

MR. WATFORD: We're up -- this average power level is like about 45 percent power, and there is a substantial driving head at that point.

[Slide.]

MR. WATFORD: In summary, we have got a fairly wide selection of data available that covers a lot of the different types of conditions that we are interested in, the different modes of oscillations, corewide versus regional. Feedwater temperature reduction cases, such as the Leibstadt case. The pump trip response with a lot of varying system response. We've got a real wide variety of conditions. But you've got to be careful that the underlying system response during the test and events is understood first, to make sure that you properly choose inputs for your simulation. It's nice when all your boundary conditions are defined and measured in a test. It takes a lot more work beforehand to take this type of data from an event or even a large-scale test like the Leibstadt test, and make sure you have pulled off the right information.

As I said, the Leibstadt test provided good range of data, fairly high power density plant operates aggressively as far as high power low flow.

The Lasalle event really gives you a look at the total integrated system effects from the core all the way out to the feedwater control system.

MR. MICHELSON: Why is the core flow going through its minimum when the power is going through its maximum?

MR. WATFORD: It's being driven by the level. It's the dominant control group. Or are you talking about in the oscillations?

MR. MICHELSON: Yes, in the oscillations.

MR. WATFORD: What you're seeing here is neutron flux. This isn't power generated to the moderator, okay?

MR. MICHELSON: It's not too far from it.

MR. WATFORD: Oh, yes, it's about 90 degrees out-of-phase with the heat flux. The fuel is a very effective filter to the neutron flux.

MR. MICHELSON: So the power then is 90 degrees out-of-phase with the thermal effect?

MR. WATFORD: That's correct.

MR. MICHELSON: That's why the --

MR. WATFORD: That's why -- I don't know exactly if you're going to get 90 there. It looks like 180 almost. You'd have to look at --

MR. MICHELSON: It's still puzzling, why it seemed to be out-of-phase 180 degrees.

[Slide.]

MR. SHAUG: Okay. We're now up to a final section on plant stability qualification, and I'm going to take a look at, first, a simulation of the LaSalle stability event using TRACG. You notice the preliminary indication on the slide. Again this work is still in progress. Take a quick look at the reactor vessel nodalization we're using. We're using a vessel with eleven axial levels, three radial rings, two to simulate the region inside the core shroud, and then the third ring to simulate the downcomer region.

The model has components for reject pump, separators, guide tubes, models steamline, feedwater, and the recirculation loop. In this particular simulation, we're using eight fuel channels in the hydraulic model.

[Slide.]

MR. SHAUG: Look at the detail of the fuel channel model. We're using 24 equally spaced axial cells, 90 heated cells at the top, and two unheated at the bottom, and including losses for the inlet orifices, lower tie plate, spacers, and upper tie plate. The nodalization, in numerical method, implying the explicit calculation, is consistent with the hydraulic stability testing.

[Slide.]

MR. SHAUG: The next slide is a little hard to make out. t's an indication of the channel groupings we used in our 3D kinetics simulation, which gives us a discreet calculation for each bundle kinetically. We've coupled it to eight hydraulic channels. The grouping is basically by radial power factor, so we grouped bundles with like radial peaking into a same hydraulic group, for a total of eight. We've also coupled the kinetics calculation through a control system to simulate the LPRM and APRM calculations.

MR. CATTON: Where are the eight channels? Could you point to them?

MR. SHAUG: It is a little hard to distinguish. Every bundle, say, in the solid black, would be treated as one hydraulic channel in the TRAC calculation.

MR. CATTON: All of those black channels are one?

MR. SHAUG: All of those are one, hydraulically. Kinetically, they're each treated individually.

MR. CATTON: So this doesn't allow for any side-to-side variations, does it?

MR. SHAUG: Not in the particular simulation of

LaSalle.

MR. CATTON: Okay.

MR. SHAUG: Again, for qualification purposes, we wanted to see how the model would calculate with the optimum distribution of channels.

MR. CATTON: Did you make the comparisons with Leibstadt?

[Slide.]

MR. SHAUG: Yes.

MR. CATTON: There you had to do it differently, didn't you?

MR. SHAUG: There, we did it differently.

[Slide.]

MR. SHAUG: As far as event simulation, we initialized the TRACG to the period just prior to the scram. We utilized the 3BWR core simulator wrap-up to provide our nuclear data and power shape information. And we wanted to use plant data to characterize the hydraulic conditions, but, of course, as Glenn has just told us, the plant was not in a very steady condition. I've made up a separate chart showing the oscillation that Glenn showed as far as core flow. We can see the oscillation characterization of the power oscillation, with two second period. We also see the oscillation generated from the feedwater flow with a period of about between 30 and 40 seconds.

So we've opted to make two starting points in our calculation. One, I picked the average condition, which gives me about 27 percent flow. I've made another calculation more typical of the minimum position, which has an initial flow of 26 percent.

MR. MICHELSON: Now, this is the calculated data?

MR. SHAUG: No, this is the final data.

MR. MICHELSON: Why doesn't it look like the slides you showed me before on core flow?

MR. SHAUG: I believe it does.

MR. MICHELSON: Is it the same. Oh, I see. It's stretched out.

MR. SHAUG: Yes.

MR. MICHELSON: Just checking.

MR. SHAUG: I think it will show that same 30 to 40 seconds.

MR. MICHELSON: Yes, it's the same. It's the same.

[Slide.]

MR. SHAUG: Okay. First, I'll show you what kind of result we got when we opted for conditions representing an average flow situation. Again, we started our transient calculations, no perturbation, and our oscillation, starting from our 44 percent power initiated, and when it appeared to peak out, it's about 15 percent peak to peak after 90 seconds. Again, it is oscillating ,and I think it's a situation typical of the minimum points in the power oscillation that was experienced at the plant during the beats.

[Slide.

MR. SHAUG: Here is the calculation where we picked conditions representative of a minimum flow point in the core flow transient, again starting out at our 44 percent power. This time, when the oscillation began, they grow to a quite high value, up to 90 percent of rated power, peak to peak, 70 percent. Again, quite representative of the power oscillations at their maximum conditions.

[Slide.]

MR. SHAUG: Having made those two calculations, we actually looked at the transient events that were going on, and we can see what Glenn showed here as far as the Feedwater flow transient that was taking place just prior to a scram.

[Slide.]

MR. SHAUG: The next slide indicates the transient that was occurring as far as feedwater temperature, again dropping at a rate of about two degrees per minute.

[Slide.]

MR. SHAUG: Again, as Glenn indicated, swings in the water level against -- I'll ask you to again remember that the water level was at a minimum, at about 20 seconds on this part.

[Slide.]

MR. SHAUG: We see here again, similar to what Glenn showed, core flow at a minimum, and a representative piece of the power transient as represented by the APRM. Again, at the conditions where you had the minimum core flow, you get the high peak-to-peak APRM signals.

[Slide.]

MR. SHAUG: To simulate what was going on during this period we took our initial case, which I'd rung out to 120 seconds and then imposed a representative feedwater transient on it, simulating what was going on in the plant data with approximately the 35 second period in the oscillation.

When we do that we get the corresponding oscillation level, again keep the peak very similar to what we see in the plant data. Again we get the minimums in water level.

[Slide.]

MR. SHAUG: If you remember where those occurred -- again what we see happening to the core flow, begin to get appropriate slings as far as core flow oscillation, again minimums occurring corresponding to the water level.

[Slide.]

MR. SHAUG: What Glenn had alluded to, which we can't get from plant data but TRAC will show us, is that because of a lag in the downcomer and through the jet pumps the core inlet temperature is showing minimums, again the same frequency as we saw but the delays causing the minimums in the core inlet temperature to occur at the same point that we had minimums in the core flow, very close.

MR. LEE: Do I understand correctly that in this portion of your simulation the feedwater temperature is calculated but the feedwater flow is imposed upon?

MR. SHAUG: The feedwater flow is imposed --

MR. LEE: Used as a boundary condition?

MR. SHAUG: Yes. The feedwater flow is being imposed to simulate what was going on in the plant. We were also taking a 2 degree drop in feedwater temperature over the last portion of the transient.

MR. LEE: But this temperature's calculated?

MR. SHAUG: No. This temperature is supplied as well, based on the plant data that indicated that the feedwater temperature was dropping at about 2 degrees per minute.

[Slide.]

MR. SHAUG: What we see here is the period after the feedwater transient was applied. Again we get an initial increase as the core inlet temperature is dropped and we get a minimum in the flow. However its initial value is up pretty high so this one we'll kind of discount.

What we see after that is the same kind of response that we saw in the plant occurring when you have the minimum core flow and it goes with the delays in the recirculation, minimums in the core inlet temperature.

I think if we look at this again the range of amplitudes in the minimum portion and in the maximum I think agree quite well with what we see in the plant data.

[Slide.]

MR. SHAUG: Now that slide was the calculated total core power. We have simulated what the APRM signal would be and here because we have a core-wide oscillation, we are in very good agreement with the calculated total for power so we see very little difference, maybe in the peak a couple of percent, but again good agreement with the sequence of events we see at the plant.

If we compare the frequency of the oscillation we see from the data that the oscillation's occurring at about a .4 hertz and the TRAC calculation is coming up with .42 hertz, again quite good.

MR. LEE: What would it take to actually calculate this feedwater flow and temperature transient with the TRACG Code?

MR. SHAUG: I'm not sure that we could calculate whatever -- the stuck valve that caused the feedwater flow to have the big swings. I mean we could -- it would be the same thing as inputting the feedwater flow. During the early part we were allowing TRAC to calculate the feedwater flow to hold the level constant but once the valve ceased acting in a normal fashion, that we really have to impose on the calculation.

MR. LEE: Is that, the mode of oscillation, also unstable?

MR. SHAUG: No, I don't believe so. I think that was just a mechanical problem with the valve.

MR. WATFORD: It's an open and closed type of response. The valve stays shut and at some time for whatever reason it opens up and it double overshoots and controller tells it to shut. It's stuck. Eventually it shuts, basically a dead-man type of response. It was an indeterminate time at which it's going to respond. It had at least in the last few minutes of data some were between a 30 and 40 second period between the time when it would go from a high to a low flow.

But as far as simulating that, there's really no way to simulate what that valve was doing. All you have is the measured data from the last minute and you have some our two previous minutes that tell you when the level was high, which would indicate the value had stayed open longer than it was supposed to, based on what the controllers tell us.

MR. MICHELSON: The valve wasn't going full open to full close, was it?

MR. WATFORD: It might have been. As the level goes down the feedwater controller is demanding more flow. The valve is not responding. It's very likely that when it finally did open it would see so much demand that it would open --

MR. MICHELSON: Full open but not full closed -- or did it go full closed, or do you know?Y

MR. WATFORD: It appeared to go full closed. The flow in one of the feed pumps that this valve was controlling its flow went to zero during at least one of those oscillations.

MR. MICHELSON: So there is a demand signal to tell it to go to full close, you are saying, there can be?

MR. WATFORD: There were two pumps operating. One was operating in a --

MR. MICHELSON: Normally it doesn't do that.

MR. WATFORD: Well, because it doesn't see that type of a demand.

MR. SHAUG: Again noting that the calculations are . ill in progress and we still are reviewing the calculations, I would make these conclusions.

MR. MICHELSON: Excuse me, one more interpretation. There were two feedwater line control valves operating at the time and only one did this?Y

MR. WATFORD: There was a turbine driven feed pump -- I'm sorry, a motor-driven with a valve -- scratch all that.

The valve that was not functioning properly was the valve for the turbine-driven feed pump.

MR. MICHELSON: But how many feeuwater lines were functional at the time?

MR. WATFORD: There was a second pump, motor-driven pump, that has a much reduced capacity -- that was also running at the same time and it was also very -- trying to compensate to its capacity --

MR. MICHELSON: But it was working, functioning properly.

MR. WATFORD: It was functioning properly.

MR. MICHELSON: Was the demand signal that was going to the feedwater turbine-driven, also going to the motor-driven, single demand signal? Or do they have separate controls?

MR. WATFORD: I don't know how that configuration specifically works.

MR. SHAUG: So the conclusion from the LaSalle simulations, TRACG predicts core-wide oscillations at LaSalle event conditions with no external forcing perturbation. The oscillation amplitudes are consistent with the plant data. The frequency of the power oscillations agrees well with the data. And we would conclude, as Glenn already has, that the feedwater transient played an important role in the event, and more than likely caused the reactor scram.

I think if we looked at the early calculation that was made where we simulated the low flow conditions, we noticed that the peak power oscillation got up to about 70 percent peak-to-peak, but had pretty much leveled off at that point, and was increasing very slowly, and it more than likely would have taken quite a while to get up to 120 percent level under those condition without --

MR. MICHLLSON: What is the highest it could get if you finely tune your simulation for the worst possible case? What kind of peak-to-peaks are we talking about? Have you searched the simulator to see what you could find as the worst case?

MR. SHAUG: The calculation that you are seeing has probably been done within the last week. And so we are still, I think each --

MR. MICHELSON: It could be higher, you are saying though, but you don't know how much higher?

MR. SHAUG: There could be conditions where it could be higher.

When the feedwater transient is simulated, TRACG predicts the system response, including the reactor power, in good agreement with plant data. We get the appropriate sequencing of events, and the oscillation amplitude from plant data and TRAC are quite sensitive to small system changes. As you notice, the 3 percent in core flow prompted a big change in oscillation amplitude.

MR. TIEN: How about the feedwater transient? How sensitive, what system changes? Did you do some calculations?

MR. SHAUG: No. Essentially, what we have had time to complete is our base simulation. I think that is essentially why we have labeled them preliminary. I think we do want to proceed on and do some sensitivities, and just look over our simulation in a little more detail. But I think what we see from it is very encouraging as far as the ability to capture all the events and sequence of events that went on during the event.

MR. TIEN: But your simulated feedwater transient is not exactly the same as from the data input.

MR. SHAUG: Not exactly. And again, we just have a small period.

MR. TIEN: Yes, period, and it seems to work out all right.

MR. SHAUG: Yes. I think, again, it produced responses in the system that closely resembled what we saw in the actual plant.

MR. WARD: Jim, your first conclusion was that the TRACG predicted the full core oscillation. But didn't your modeling constrain it to that?

MR. SHAUG: That's right. I mean, it predicted the full core because we grouped the channels to only allow a core-wide oscillation.

MR. WARD: Yes.

MR. SHAUG: But we did not force it to oscillate. If there had been too much damping in the code to where it would not oscillate, it didn't matter how we grouped the channels, it still would not oscillate.

So it is just the indication that we were able to predict the oscillations and in this case we essentially nodalized to produce the core-wide.

MR. WARD: Why did you do that? I am just curious.

MR. SHAUG: Again, from a qualification basis, our first effort had to be to show, you know, given our best representation of the plant conditions, could we predict the plant response. Because we knew it was a core-wide, we nodalized it to our best simulation of a core-wide event. Okay?

MR. WARD: It sounds like going in a circle a little bit there to me.

MR. SHAUG: Well, I'm not sure. Again, we did not force the oscillation to occur. That is the big thing that we have shown by the calculation. Now, we can take that one step further and group the channels into what would be a regional model, and see if it still predicted the core-wide.

MR. CATTON: That would have been a little bit of proof.

MR. SHAUG: As a second calculation, I think, or as a sensitivity to our base calculation. I think we needed this calculation as our best shot at predicting the plant data. We also, as I mentioned during the kinetics --

MR. WARD: I don't want to prolong that. But I mean, the best shot at predicting what happened in the plant, you know, it is the best shot at reproducing what happened, what you know happened. But it isn't a good shot at all at predicting what might happen in another case.

MR. SHAUG: I see your point as far as the channel groupings. I think the rest of the simulation --

MR. WARD: That's all --

MR. SHAUG: -- inconsistent.

MR. WARD: That's all I meant.

MR. SHAUG: Now, as I indicated in the kinetics presentation, we have been working on models to predict the type of oscillation that is likely to occur at a plant under given conditions.

So we would use that information to determine what kind of hydraulic grouping we would use in our calculation.

Now, it is also possible that, rather than using eight channels, if I wanted to group such that I could pick up multiple modes of oscillation, I could go to 16, 20, whatever it would take for that particular application.

MR. CATTON: I guess you would have to do that, wouldn't you, if you are trying to establish your exclusion region? You would have to do it both ways.

MR. SHAUG: I think we would have to either model it both ways or have some reliable way of predicting what the mode is going to be.

[Slide.]

MR. SHAUG: Okay. We move now to Leibstadt. Again, the qualification is in progress, so we mark this preliminary.

[Slide.]

MR. SHAUG: Quickly, the reactor vessel looks the same as LaSalle. This time, rather than eight channels, we are at 20 channels, hydraulically.

[Slide.]

MR. SHAUG: The fuel model is the same as we saw for LaSalle.

[Slide.]

MR. SHAUG: Now, if you look at the hydraulic grouping, here you see the big difference, again using the 3D kinetics for our discrete power calculation. Out 20 channels, in this case, are arranged relative to what we know from the data was the area of peak LPRM signal. So we identified that from the test data. It turned out to be in these two regions of the core. So an oscillation centered around this axis.

We identified in that region two dominant bundles, high radio peaking, very high axial peaking, at the bottom of the bundle.

From that, then, we grouped, geometrically around that, to get us the best representation of the power shape during the oscillation, again coupling the kinetics of the control system to get our LPRM and APRM.

[Slide.]

MR. SHAUG: Test simulation, very similar to LaSalle, as far as initializing to the test conditions, using the simulator wrapups and initializing to this time what was a more steady-state hydraulic information.

[Slide.]

MR. SHAUG: This first chart is a TRAC calculation of the power. In this case I have shown the core average power in the center, and then the out-of-phase oscillation of two channels, the dominant channels located as shown in the figure in the upper left.

What we see is, without any applied perturbation, that we go into an out-of-phase oscillation, and we see on the total core power what Glenn referred to as the doubling of the frequency where we get nearly perfect cancellation of the two halves of the core.

If we look at the peak amplitude, it is about 24 percent peak-to-peak, and minimal oscillation of the total core power.

MR. TIEN: Did you try instead of 20 channels, if you use ten what happens?

MR. SHAUG: I think if you wait for about four slides.

MR. TIEN: Okay.

[Slide.]

MR. SHAUG: This is a simulation of the peak LPRM shown with the X location on the A-level. And this is the peak LPRM shown in Glenn's data. In the data, we got a peak-to-peak of 14 percent. In the TRAC calculation, we got 19 percent peak-to-peak for our simulation.

[Slide.]

MR. SHAUG: To get an idea of how we are doing axially, picking up the characteristics of the oscillation, we again for that same LPRM location looked at the phase lag from the A to C and D levels.

You can see from the test data that from A to C we get a 60-degree lag. TRAC is calculating a 56-degree. From A to D, data shows 87 and TRAC is predicting 83.

Again, quite good agreement with what we find from the test data.

[Slide.]

MR. SHAUG: This is a plot that attempts to compare the shape of the power oscillation between TRAC and the test data. What we have are LPRMs located along a vertical, just off the center of the core. If we take the spacing of the LPRMs about the center line, we have one located one bundle away, one at three, five, seven, nine, eleven, and thirteen.

If we flip about the access of symmetry, we can pick up a half-core profile of the power oscillation, peaking again in the location of the peak LPRM shown in the data and then decreasing toward the center of the core, indicated by zero.

What we see is good agreement with the test data as far as shape of the oscillation. We also notice that in the data, it kind of tails off here down at low LPRM signals at the center of the core, where we know from the TRAC calculation that it will go virtually to zero as it cross the axis.

This is in the data down at about the noise level. So we just don't get any difference once we hit this level. Now, this is going to impact us when we calculate our APRM signals because in TRAC we'll get some cancellation on the two sides of the axis of symmetry, whereas in the data that won't be occurring. So we would tend to over-predict the -- under-predict the APRM signal.

And if we look at the --

MR. MICHELSON: Excuse me. This is still a natural circulation situation.

MR. SHAUG: That's right.

MR. MICHELSON: You're going to have 20 different channels, each with their own power generation. The flow rate, then, through each of the 20 is going to be a little bit different, but there's a driving force of natural circulation that's kind of a mixed-mean driving force. That's all built into our codes?

MR. SHAUG: Yes. The driving pressure for all the 20 channels is set up from the upper -- from the lower plenum to the upper plenum. We don't specify any --

MR. MICHELSON: But the actual flow through the channels will be a little different according to what power they individually are generating.

MR. SHAUG: Yes.

MR. MICHELSON: That all works out.

MR. SHAUG: Like a charm. If you've got the time to wait, essentially every ten seconds you see on here or every thirty seconds is about 12 hours of waiting around.

[Slide.]

What we see here is our simulating APRM. We see very small amplitude, about three percent peak-to-peak, and we see here a very similar shape as what Glen had seen in the plant data where you're not picking up. You have a perfect doubling of a frequency, but some in between state where one peak is still dominant.

MR. CATTON: Is there any reason you didn't overlay this on the data?

MR. SHAUG: I didn't have the data up on a -- in a form that would allow me to graph it.

MR. CATTON: The scales are little bit different and it's kind of hard to compare.

MR. SHAUG: I think what we're looking at is the basic shape of the curves and approximate magnitudes. There are a lot of things going on in the plant that we just can't get in the simulation to pick up all the little details.

MR. SCHROCK: The distortions seem to change cycle-to-cycle on that last one.

MR. SHAUG: In this one?

MR. SCHROCK: Yes. Somewhat more symmetric than others or is that --

MR. SHAUG: It may be the graphics interval. It's hard to predict. Again, some of the channels picked it up and some of them didn't.

MR. LEE: What is the difference between the one that you just took off and the one a few transparencies before which shows the result of cancellation between two LPRMs?

MR. SHAUG: Was that the first chart that I showed?

MR. LEE: Yes. That's the first one.

[Slide.]

MR. SHAUG: That's a TRAC prediction of total core power. So that would be perfect cancellation, including every bundle of power in the calculation.

[Slide.]

MR. SHAUG: This one is a simulation of what the APRM channels have available. So they do not have every bundle of power available and they do not have every axial level available, only four discreet levels and a finite number -- a distribution of LPRM strings in the core.

MR. LEE: So APRM does not represent a co-average, is that what you're saying?

MR. SHAUG: Under these conditions, that's true. We saw under a core-wide, they gave you a very good representation of the core, but under a regional oscillation you get cancellations which, depending on the axis and where the LPRM strings that the APRM channel is taking for its signal, the results will vary. So even in the plant data, the APRMs, you might see some at two percent, some at four percent, depending on which LPRMs they were selected from.

MR. LEE: So apart from the infrastructure, had I not been informed of the infrastructure, I would have assumed that this has the same period, like a two-second period of oscillation, while the average, core average behavior would show one-second period.

MR. SHAUG: That's what we see in the data and I think that was what Glen indicated could be used to distinguish the various oscillation modes, whether it was a core-wide where we saw the APRM in the two-second period, or a regional oscillation where we began to see this distortion in the APRM signal.

[Slide.]

MR. SHAUG: As far as channel groupings, we have run a couple of sensitivities to it. The north-south 20 channel being our reference case and normalizing everything to the amplitude of that calculation.

When we went to 18, we kept the nodalization around the dominant bundles the same. We just removed some of the details in the areas where we expected smaller oscillations toward the center of the core. And we got essentially no effect from doing that.

Again, as long as we kept the same grouping around the dominant bundle, we got a good prediction. When we dropped down to ten, we did much the same thing, maintaining the grouping. I've got it upside down, but since it's regional half-core, we're okay.

We kept the same nodalization around the dominant bundles, but we removed a lot of the intermediate regions to give us our ten bundle grouping case. We did get some drop in the peak channel oscillation, but not a great deal. We were still at 90 percent of the 20 channel case.

Now, we got a more significant change when we grouped to a different axis of symmetry. If we did not pick the actual oscillation mode, we dropped down to 70 percent. So, again, you conclude from this that as long as you've got the dominant channel and a reasonable number of channel groups, you're going to pick up a pretty good simulation.

If you can't pick the dominant mode or the mode of the oscillation, you're going to be -- it could be considerably low.

MR. CATTON: That means you're going to have to really exercise your code. You're going to have to try essentially N equal zero, N equal one, and N equal two probably.

MR. SHAUG: Or we're going to have to have something that tells us which mode we're going to get.

MR. CATTON: But how are you going to know that beforehand? Do you know why this particular --

MR. SHAUG: I think tomorrow you'll see a presentation where we'll present some -- a study and some analysis to determine which mode we are going to see.

MR. CATTON: What happens if you were to pick N equals two, which would be put them together? What mode would you see?

MR. SHAUG: We'd have to -- based on what we have now we'd have to do that and run the simulation and see what we get. Again, for this qualification, we, again, wanted to give ourselves the best shot at predicting the data given the test oscillation mode.

[Slide.]

MR. SHAUG: This next slide gives you a summary of the data comparisons for the tests that we've completed to this point, at least initially. We see for Test 4, it's the one that we saw the plots for, agree well with the data as far as LPRM and APRM. Good agreement as far as frequency.

If you look at the 4A case, this is the case that Glen indicated was starting at condition 4 and then reducing the core flow from that case, the test data we see is 66 percent peak-to-peak, LPRM oscillation. Our TRAC calculation so far, we see a 35 percent. So not terribly good under those conditions.

We see the same kind of comparison as far as APRM, we're about half. Frequency is not too bad. If we go to reduced feedwater cases, the five series. These we compare quite well with. We're right on as far as LPRM data. We're a little bit low as far as APRM, but here, again, I think we're down to a point where it's the cancellation effect that's causing us to be a little bit low. Again, reasonable agreement as far as frequency.

[Slide.]

MR. SHAUG: Given that these calculations are preliminary, the same as before for the corewide. This time we can predict regional oscillations observed in the test conditions with no external perturbation. We did not have to force those channels out of phase to calculate the oscillation. That was done on its own.

Limit cycle oscillations were predicting at all test conditions. We agree quite well as far as contour of oscillation compared to test data. We're picking up the axial characteristics of a density wave by virtue of a phase lag up to channel quite well.

Five seconds in, the LPRM and the APRM oscillation amplitude and frequency are in good agreement with data. I think we have to look at the four AKs a little harder and see if there's something in the test that we're just not considering in our calculation. From our sensitivities, it's important to identify the dominant channel and the axis of oscillation, the mode.

Having done that, it's much less sensitive. I will draw a channel view to simulate the channels away from the dominant channels. I think we've come to the end.

MR. CATTON: Now, the --

MR. SHIRALKAR: Just to summarize the TRACG capabilities, I think we've gone through the models and talked about the thermohydraulics, kinetics and the capabilities we have in TRAC.

I don't think that the two-fluid thermohydraulics is as critical there, as just being able to predict -- for this purpose, to predict the void fractions for boiling and void prorogation accurately. In the theory of neutron kinetics, it does not seem like a big issue sometimes, but I believe it's a crucial thing, because the main difficulty we have in these predictions is good kinetics data.

The fact that we can directly use the kinetics data from our design codes has been of tremendous value to us in terms of having it accessible and useful, plus qualified because we use the same code for monitoring our core, and we are periodically are checking on power distribution, the values and so on.

I think that's a very key factor, the quality of the nuclear data that you have available. We've gone through some of the qualification. We talked about void factions and subcool voids, the kinetics and the stability-specific studies. I believe that the kinetics, thermohydraulics models we have n TRAC are quite adequate for predicting the stability phenomena that we need to predict.

I was also asked to talk about limitation and further plans. I'd like to talk about the TRAC limitations. Clearly one of the limitations is that we do have one dimensional bundle representation. I don't believe that's a significant limitation, but clearly the flow within the bundle is multidimensional.

You're going to have variations in void propagation. The last is a cross section of the bundle. You're going to have some damping introduced as a result of that, but we think that one dimensional model does a reasonably good job of characterizing the bundle.

We've talked about numerical damping and clearly there's going to be some questions, some residual numerical damping and nodalization sensitivities. We think we have it under control. We have got both analytical confirmation as well as comparison with data. I would think that this not a remaining major issue.

We are using quasi-static phenomenological relations as are all of the codes. I think that this is not a major limitation in this case, because the transient is fairly mild from a thermohydraulics point of view. We're talking about a two second prorogation time for the transient. That's two time -- the period is two times the transit time through the channel.

It's a relatively slow transit compared to the other ones you have looked at, like LOCAs and so on, in terms of thermohydraulics. I do not believe it's a major limitation. On the other hand, where I do believe that we have to be very careful is in the application of these codes. This is not just TRAC, but any code, I believe, that we need to look at.

The process is extremely complex, the stability phenomena, and that's not necessarily because of thermohydraulics or kinetics, but just because of a combination of a huge amount of things. The parameters that affect the phenomena range from the gap inductance of the fuel, which affects the stability to loop characteristics in terms of returning circulation flow to bundle groupings to power distribution and so on.

So, in my view, the real care that we ought to take in these codes is to apply them extremely carefully to be sure that we have really good data to use, and do very careful sensitivity studies. We've seen that the results are sensitive to things like core flow, like chaning the flow from 26 to 27 percent, can reduce the oscillation from 60 percent, peak-to-peak to 20 percent peak-to-peak, so these are large sensitivities. Power distribution is important.

Gap conductance is important. Bundle grouping is important, as you have seen, so I think that those parameters are much more important than any phsyical modeling limitations within TRAC. We need to be very careful to make sure that we have the right parameters and the right uncertainties in using these parameters.

MR. CATTON: You also have to pick the right plant form?

MR. SHIRALKAR: The right what?

MR. CATTON: The right instability horizontal structures.

MR. SHIRALKAR: Yes, yes, in fact, as I talk about future plans, let me say that what we'd like to do is -- we've looked at two plants, from which we have got quite a lot of data, Leipstadt and LaSalle. We picked these plants for a variety of reasons that Glen Watford described, the two modes of oscillation, the variety of data within the Leipstadt testing and so on.

We'd like to perhaps go on and look at some other platn data which exists, given time and given the resources or longer time schedule. We need to quantify sensitivities. That, to me, is extremely important. We need to make sensitivity studies, such as the ones that you brought up to bundle groupings, to gap conductance, to subcooling and so on -- core flow. We need to quantify those.

Finally, the issue of bundle grouping is a thorny one. We have -- either we have to take a brute force approach and look at a large number of groupings, if you don't know what the mode is going to be, or we need to have a way of predicting the mode.

We are going to look tomorrow -- I think Dick Stirn has said that a couple of times -- tomorrow, we are going to look at some ideas we had, some analysis we had to do that, and basically it's an extension of Jose March-Leuba's ideas about higher modes and how they can be predicted and which modes are most likely to happen and across which diagonals. We'll be talking about that tomorrow, but that's an area clearly in which we need to do some more work.

MR. CATTON: Thank you. I'd like to say that I think that the GE presentation, I think, has demonstrated that there's a lot mroe in the heart of TRAC than some of us thought.

Gary, are you going to give the presentation? Gary, you realize that we are an hour and a half behind, and any way that you can think to expedite it a little bit, we would appreicate it.

MR. WILSON: I am going to take about five minutes to finish this topic, which is a disc ssion and a presentation on the basic and stability related capabilities of TRAC BF1. TRAC BF1 is one of the codes, one of the BWR codes in the NRC table of codes and is the one that resides at INEL.

In many respects, our presntation is going to follow closely that of the GE presentation that you've just seen. I want to ive a short introduction and talk briefly about code use. The intent of my introduction is to place the following two presentations which have the meaning of what we want to say into the proper context. I will be followed by Dr. Rouhani who will discuss the basic and stablity-related code features and Dr. Rouhani then will be followed by Dr. Weaver who will talk about the basic and numerical dampening assessment that we performed.

Now, with respect to code usage, in the NRC's stability-related program, Harold Scott is going to go into some depth about that tomorrow. So I'm only going to present enough information to provide for the following presentations.

Now, I currently envison the role of TRAC BF1 and the stability analysis is to help evaluate the effectiveness of the Atlas emergency operating procedures to prevent or mitigate limit cycle oscillations. The sutyd objectives, we will encompass i in that role -- is to detemrine instability initiation and oscillation amplitude, and then to look at the suppression cool loading as a result of the limit cycle.

The studies that are planned are a water level contorl, feedwater flow control, feedwater temperature, pressure effects and perhaps boran injection effects. The interfaces with the other codes and the NRC stable and the program are indicated here. We envision a cooler which is a frquency to main code, EPA which is a time-to-main code, will be used for mapping the necessary analysis space and for selected code-to-code benchmarks.

TRACEF1 will be used for in-phase studies, in-phase instability in the core, and code-to-code benchmarks and then RAMONA with its multidimensional behavior, both in a thermal hydraulics and the kinetics will be used for multi-D oscillation modes, out-of-phase modes.

Now, the strategy to accomplish the stability research objectives in the context of TRACKBF1 are indicated here and they consist of the following elements: First, the TRACKBF1 validation which is in four areas: critical validation of the models and then FRIGG assessments, particularly in the area of thermal hydraulic oscillations and frequency repsonses.

Those first two items will be covered by Dr. Rouhani shortly. We will then -- the validation effort includes previous stability related assessments and a convergence study in terms of spacial and temporal effects. Dr. Weaver will cover those last two items. The red line here indicates that thes is where we are. This is the status of where we are in the program, and that's going to be the subject of the follwing two presentations.

Just for completeness, I'll go ahead with the other items listed here. We see there will be the four EPA and TRAC BF1 benchmarks in the validation effort, and then TRAC BF1 LaSalle event benchmarks calculations, which are very similar to what you've seen GE just present.

We then turn to the real work in the program and tha'ts the application of TRAC BF1. Elements in that application are; we'll look at Atlas EOP, operator actions. We'll use the LaSalle model. We expect the analysis base to be provided by core and EPA for sensitivity calculations and we believe that there will be RAMONA and TRAC BF1 comparisons in this effort.

[Slide.]

MR. WILSON: Now, with that preface, I will turn the floor over to Dr. Rouhani who will cover the first two items shown here.

MR. ROUHANI: My name is Rouhani from INEL.

As Gary said, I will be presenting -- and how we tend to assist the core. For BF-1 solutions critical to this outline, as you may see, it is almost a rewind of what we have from General Electric, but to speed it up, because most of it is repetition.

MR. CATTON: That would be appreciated.

MR. ROUHANI: The thing that I should perhaps do, in order to study this development, I can skip over it, because it would be mostly what we heard this morning. A somewhat different person has done a good job in using that. We try to use the same data. Finally, I intend to present to this evidence of TRACG.

[Slide.]

MR. ROUHANI: The development of this code was started in '79. It was used for BWR capabilities. It has been very effective, and actually, it was continued during this time period. Although eventually finished in '84, it continued for 2 years. All of this was under the sponsorship of US NRC, a number of different versions of these codes and of these particular models.

[Slide.]

MR. ROUHANI: There are many features of the code that we have. In most cases, they are similar, like six-equation basis to fluid flow, one-dimensional, three-dimensional components. We have a possibility of moving the multiple fuel rods, and we have not condensable gas as a component.

There is a difference between us and GE regarding the kinetics. Unfortunately, INEL is limited to one dimension, and there are other limitations.

We can skip over the other part, which is the hardware description. There is not anything new in it.

[Slide.]

MR. ROUHANI: There are also other features of the code from General Electric, like the level tracking level of the seat conduction model is only one-dimensional, except when it comes to the propagation of the level which we use.

MR. MICHELSON: Maybe you said it, but will you refresh my memory on the time period during which TRAC BF1 was developed versus when TRACG was developed.

MR. ROUHANI: We started this whole operation in '79. Until '84, we had those technical collaborations.

MR. MICHELSON: When did TRACG start?

MR. ROUHANI: At the same time.

MR. MICHELSON: So, you have been doing this in parallel, in other words. Almost exactly in parallel. Is that correct?

MR. WILSON: It's more than parallel. From '79 to '84, it was in parallel together, a collaborative effort under the sponsorship of the NRC.

MR. MICHELSON: And then after '84, you broke this apart.

MR. WILSON: The program formally came to a close in '84. For another 2 years, there was some informal exchanges of information, but formalities closed in 1984.

MR. MICHELSON: Is it too much to ask why we developed both TRAC BF1 and TRACG?

MR. WILSON: We developed TRAC BF1. In 1984, it became TRACG for GE.

MR. MICHELSON: And they further embellished it.

MR. WILSON: They further embellished it.

MR. MICHELSON: So it's not really a duplicative effort.

MR. WILSON: 1979 to '84 was not a duplicate effort.

MR. ROUHANI: It was developed for the same purpose. In the NRC, it would be an audit code. For GE, it was to use as a design code, with some specific correlations used for licensing. So, we actually stopped duplication.

MR. MICHELSON: The NRC only has access to BF1. We don't have access to G.

MR. CATTON: Did BF1 essentially become frozen in 1984?

MR. ROUHANI: In '85-'86, actually, because NRC's funding did not allow us to work on it.

MR. CATTON: I didn't ask that.

MR. WILSON: In 1986, it became frozen. There was some developmental assessment that went on in that period.

MR. SHOTKIN: Beginning in 1985, we froze -- essentially froze all of our --

MR. CATTON: Okay. That's what I thought.

Now, the bottom line on your previous slide says that you have the core limit violating numerics.

MR. ROUHANI: Right.

MR. CATTON: That's says two-step. That's says that you are going to calculate a decay ratio that's a bit too low.

MR. ROUHANI: There is a difference, too, of the core limit, and as a result of recent focus on using the code for oscillation calculations, we find that one has to resolve the question of sensitivity to nodalization. That would report our presentations by Dr. Weaver.

MR. CATTON: Okay.

MR. ROUHANI: Finally, similar to what Jens said this morning, we have come to the conclusion that on the explicit method used with core limit or core number 1 is expected to give the best results. Probably Dr. Weaver will elaborate on that more.

[Slide.]

MR. ROUHANI: This is, again, a repetition of what Yens presented this morning. This are certain phenomena. So far, I was trying to say the general capabilities of the core, but there are certain phenomena which are of importance to the stability or oscillation calculation.

Most of important of them is density-weight propagation prediction. That depends on weight propagation or predicting the void as a function of time and space and, also, effect of single-phase and two-phase friction in the bundle, as well as localized frictions at the beginning or end of the bundle, that both experimentally and theoretically are the ones likely to affect the results very considerably.

Also, calculation of reactor power and its dependence on the hydraulic variable, such as void and temperature in the fuel are very important.

The part of this which has to be taken into account is the conduction in the free-load attenuation of the heat being generated in the fuel and transported to the fluid.

I want to say that these are the features which exist in the core.

[Slide.]

MR. ROUHANI: Again, the core has features for calculating sub-cool void. It is very similar to what is in GE's code at the point of departure or initiation of sub-cooled boiling is according to a modified version of Saha-Zaber correlation. The interfacial shear package is the same as was described this morning. The same is true with the interfacial heat transfer.

[Slide.]

MR. ROUHANI: For other aspects of heat transfer from the fuel to the fluid, we have a whole package of different conditions of heat transfer that are explained here for single-phase, different forms. If it is for circulation, we use Dittus-Boelter. For natural situation, McAdam is used, and if there is a laminar flow, another correlation is used for that purpose.

Nuclear boiling, including sub-cooled region is calculated according to correlation and critical heat flux is similar to the one that GE was measuring this morning, based on past history of boiling local quality, and there is a zone of transition boiling and then fuel boiling between these two. There is an interpolation procedure for calculating what heat flux.

There is a radiation heat transfer model that includes wall-to-wall heat transfer and wall-to-fluid heat transfer above a certain cutoff point in void fraction that is user-specified. It has a condensation calculation model and, also, dealing with any kind of power, we use a specified power distribution. We can specify how much of the power is deposited directly into the fluid, because that's important in reflecting the effect on oscillations.

[Slide.]

MR. ROUHANI: These are the features used for calculating friction. I simply mention that each one of these have been assessed against separate effect tests and, also, integral tests. Part of these will be shown by Dr. Weaver later.

We use a two-phase multiplier, according to Martinelli-Nelson model, although these are different for straight pipes. A model is sed for localized two-phased multiplier, based on the density ratio of the two-phase density.

[Slide.]

MR. ROUHANI: Now, we have a point of difference regarding neutronics. We have only a one-dimensional neutron connection, and that is where this code differs from GE's version. That is based on a two-group, one-dimensional in axial direction. That is called a nodal representation. It gives us reflections from the bottom of the bundle and the top. It has effect of the reflection in the radial direction.

There are two routines in the code for calculation of steady-state neutronic distribution and the transient version of it that is using the integration method, and operation of the inputs for this is a lengthy process that is requiring assistance from another code.

In order to calculate the effect of transient variation of void and other hydraulic parameters under neutronics, you have a set of equations which are giving these effects as a polynomial in void fuel temperature or moderator temperate. That was according to recommendations from Brook Haven, and it was planned to use the code at Brook Haven to produce the coefficients which are needed to make an input set for running it. Those coefficients can be generated for different positions of control rods or different nodalizations.

Unfortunately, this is not an easy or safe manner. I must say that this is one of the limitations that we have in the code, and it has been used, probably, two or three times so far.

Now we get to assessment of the hydraulics of the code with the use of FRIGG data. The objectives of the assessment are described here. Firstly, to be able to predict a steady state drive that will meet in the free loop that was distribution of the void along the channel or distribution of mass as a function of power in a steady state version and also as a continuation of that to try to see if we can reproduce the response in frequency or the effect of the loop in responding to a change in modulation in power, and its effect on inlet mass velocity or an exit void in a channel. The studies, which began with studying the effect of power on the mass distribution, resulted in finding out that there was a sensitivity to nodalization, and that initiated a separate activity that is going to be addressed today by Dr Weaver.

[Slide.]

MR. ROUHANI: Today we saw a schematic of the FRIGG loop. I will show it again in a different version, just to show the size of this. This was experiments done in Sweden, and this height is a full scale height of about 4.3 meters.

The distance from here to here is about 10 meters.

The loop was run essentially in two modes, either through this one as forced circulation case or by closing valves here and opening this valve in a natural circulation system.

One of the series of measurements that they did, that we intend to reproduce in a TRAC calculation, was firstly variations of flow, using these as independent variables, getting steady state data and on flow, and using these as independent variables, and getting steady state data on flow, and then studying dryout and oscillations. The transient tests were pertubations on one of these variables and trying to find the transfer function on the other one.

Here I could show you a sample of one such case.

[Slide.]

MR. ROUHANI: This is intended to use as one case for assessing TRAC BWR, the TRAC that GE chose to use in a different way. On this axis you have the gain or the ratio of the relative variations in inlet mass velocity, or the relative variations in the power as a function of frequency.

This one is generated from experiments, the measurements, in which they perturbed the power according to a certain pattern, and obtained a response on other variables, and this is showing a phase shift in this response. We intend to reproduce this with TRAC, and the procedure for doing it.

[Slide.]

MR. ROUHANI: The procedure is to make a TRAC run first, and then the response usually is if you have a calculation with steady power, and then perturb that power, the result in mass velocity or void is a number of oscillations that we can make an approximation to, and try to fix these coefficients or these constants, that is initial amplitude, the multiplier for the exponent, and a frequency and a phaseout.

After finding this, then we can subject this to full air transfer and also do a full air transfer on the signal which was used to generate this perturbation, this signal, and make a comparison of the two as a function of frequency, which will result in a curve of this kind.

Unfortunately I don't have these data, but six weeks ago I made this presentation that we intended to do, but for reasons that NRC knows, w were not able to continue the work for a while. It would have been better to show you the results today as to how to do it.

[Slide.]

MR. ROUHANI: This is an example of how an approximation is generated. There are two curves here, actually one of them is the result of perturbation made with TRAC calculations, on the inlet velocity, and the other one is an approximation obtained according to this equation. I wanted to show that this is a practical way, and it has been used before, for making transient studies by codes of this kind.

Another usage of this approximation is to get the ratio of these signals here as a way of predicting where the limit of stability is, by making separate calculations of this kind for two different powers.

We make two different runs of that kind, steady state with a certain power, perturb it, and then get one of those approximations for the result on exit void fraction or inlet mass velocity, and then change the power to a different level, make a similar run, and then plot the two values of lambda that we obtain from these two runs.

That will give us extrapolation as to what point in terms of power this lambda will go to zero. That is the limit of stability.

Now both that transfer function calculation and this kind of calculation provides us with two sets of calculations that can be directly compared with FRIGG data. In that manner, we can prove the accuracy and usefulness of the code or see where it deviates.

By that, I finish this presentation and just give you a summary of what the intent was.

[Slide.]

MR. ROUHANI: I intended to show you that the TRAC BF1 code possesses the models needed to predict BWR instability behavior. I wish I had some of the results which could really prove it, rather than just saying it. Our assessment right now is ongoing, and is expected to show a good compatibility, at least regarding thermal hydraulics data.

I would like to add a couple of words regarding limitations of the code. Just like Dr. Shaug said a while ago, we have two major limitations on the usage of this code:

Firstly, it's neutronic, it's one-dimensional, and that in itself demands a good deal of time and budget investment to get appropriate inputs for it, and as a whole, the code is time-consuming on the computer, and you must realize that before assigning its application. But since this is the only two correlations that I think exist, as Gary explained, there is a range in which this code is the only one that can be used for these applications.

Finally, before I leave, I just would show you this slide to show you the relation between our code and GE's.

[Slide.]

MR. ROUHANI: Just as a statistic, there are 24 different capabilities which are common between these codes, and many of them were developed by us, GE used them, and vice versa and several of them were developed as a joint effort. Altogether, it has been very fruitful, very useful collaboration.

That ends my presentation. I would like to answer any questions, if there are any.

MR. CATTON: I see none. I guess the question is, do we take a break now, or hear the next speaker. Why don't we take a 10-minute break and start back at 4:00 o'clock.

[Recess.]

MR. CATTON: Would Mr. Weaver please begin?

MR. WEAVER: My name is Walter Weaver. I am from the Idaho National Engineering Laboratory. I will be talking about the assessment that has been done on the TRAC BWR code, the INEL version, or the NRC version of the TRAC BWR code.

[Slide.]

MR. WEAVER: A short introduction, to cover the assessment that has been done on TRAC BWR. Again, we are going to get some of the limitations of TRAC BWR for the application stability analysis, and then finish with a short summary.

MR. WARD: Now, Walt, is TRAC BWR something different from TRAC BF-1?

MR. WEAVER: TRAC BWR is the name of the program. There are different code versions. TRAC-BD1 was the first version, B for BWR.

MR. WARD: Okay.

MR. WEAVER: D for detail and 1 for the first.

MR. WARD: All right. I got it.

MR. WEAVER: Then there is BD1/MOD1.

MR. WARD: I got it.

MR. WEAVER: BF --

MR. WARD: I surrender. I got it.

[Laughter.]

MR. WEAVER: Generic name for the NRC is TRAC BWR in its several versions. I realize that this is a little confusing.

The code has independent assessment, and we have done some assessment of models like the jet pump, and the separator drier. We have assessed the process models like countercurrent flow limiting, multitransfer, including the subgroup model, interfacial friction, treatment flow range and heat transfer, et cetera.

We have also done assessments using integral test data, large-break LOCA and small-break LOCA tests in integral facilities, some reflood facility tests, some startup tests in reactors, and we have done some ATWS simulations, both simulations in full-scale plants and simulations of ATWS tests in integral facilities.

MR. TIEN: But you are going to cover also stability phenemona?

MR. WEAVER: That's right. This is the assessment that was done when the code was frozen in '85.

[Slide.]

MR. WEAVER: The next two slides are the list of all of the different assessments that are available either in the code manuals or in independent assessment reports. I will tell you, they are there for your information.

I really want to get to the stability-related assessments.

[Slide.]

MR. WEAVER: And just like GE, we think that assessing the void profile models, the void propagation models and the pressure drop models, because the density wave is an interplay between void propagation, and its effect on the two-phase pressure drop through the assembly is important. I've listed some here.

Again, these were done as the code was developed, not necessarily done for the purposes of qualifying the code for stability.

The adiabatic pipe tests, there are some GE levels throughout the Southern Zone this morning from GE. Some heated tube and test section tests, Christenson, Marchature, Bennett. Some of the THTF boiloff tests.

In the area of two-phase pressure drop, we assessed the, or we used the FRIGG natural circulation flow tests to qualify BF-1 four or five years ago. I am not going to show you those tests, because we have redone those recently. I will show you the recent ones, but I am going to show you some of the old ones of these void propagation tests. These are steady-state tests.

MR. TIEN: In the assessment, did you, are you going to discuss anything about the American alerts?

MR. WEAVER: Later. It's coming.

MR. TIEN: It's coming. Okay.

MR. WEAVER: I want to emphasize that the interfacial friction model, which is one of the models of control with the void profiles, is exactly the same as in the GE code, and the models developed mainly at GE have been modified by collaboration between the two of us. This is one of the tests. The major data is in the circles, in the dark circles. The TRAC-calculated void profile is a function of the inlet qualities of the test section. This is an adiabatic test, where they made a two-phase mixture through a heated test section, ran it through a long pipe with quick-acting valves, closed the valves, measured the amount of vapor mass in the test section. Part of this is a function of the inlet quality of the flow that they found in the test section.

[Slide.

MR. WEAVER: Another one of the tests, similar to the one Glenn showed this morning, is the GE levels flow. This is one of the one-foot diameter tests at 40 seconds into the test. This was the same kind of a test where the test section was pressurized, the break was open, the levels swelled up. And then you've got flashing below the two-phase mixture level And you see that the void model does a very good job of predicting the void profile.

There are lots of other predictions like this in the code manuals and in the assessment documents.

[Slide.]

MR. WEAVER: All of those assessments were done three or four years ago. But since the LaSalle transient, in the interest of BWR stability, we've gone back and done some more what we've called stability-specific assessments.

And one of those is the FRIGG series of natural circulation tests, and the FRIGG stability tests. And I am going to show you first the FRIGG natural circulation tests. You see from the diagram where it says FRIGG facility a couple of different times.

The tests were done by increasing the, in natural circulation, by increasing the power, to go to a natural circulation mass flow rate, then taking it up a step in power so that as you first started increasing the density ration between the core and the downtimer, you've got an increase in the natural circulation flow. Eventually, it got to a point where the two-phase losses at the outlet overcame, and the flow rate started to decrease, as the power rate increased.

[Slide.]

MR. WEAVER: What is shown here is the FRIGG data in squares, and two separate TRAC calculations, one with an outlet loss factor at the outlet of the bundle, a decay of 4 and one a decay of 5. And those two numbers were used because it is very difficult to model the particular geometry of the separator that the FRIGG facility used. It was a piece of pipe with a lot of little holes in it. And it is very difficult to model that particular set of losses in TRAC, or for any code, because there were no measurements to the facility as to what the loss coefficient was. Also, the loss coefficient to that separator was a function of the water level outside.

So with a small variation in the outlet loss code, as we see, we get very good agreements with the data.

The inlet losses were all taken right from the test reports. We haven't done any tuning other than changing the outlet loss coefficient, which is the least well-known loss in the whole system. What this does is gives us confidence both that we can calculate the void profile correctly, and also the two-phase pressure losses, which are the two components in predicting density waves correctly.

[Slide.]

MR. WEAVER: As part of this, we also looked at the void profiles at some of these different test points. This is a comparison of the TRAC-calculated void profile and the major data at 2.8 megawatts, and at 4.6 megawatts.

These calculations were done by Rich Henson and reported in the BWR at the BWRs Instabilities Symposium held in Idaho in August.

[Slide.]

MR. WEAVER: Then, we started looking at whether the code could calculate decay ratio correctly. And what we did was we would run the code to a steady state, increase the power, the code would oscillate for a while. If the power was not high enough to cause instability, we would get a decaying amplitude of oscillation eventually reaching a new steady state. We would then increase the power again to see if we had gotten to the point of instability.

[Slide.]

MR. WEAVER: And this is the kind of transient response that you get out of TRAC. This is the stable point where the power was increased to 6-1/2 megawatts, which is 6500 kilowatts. So by looking at the magnitude of the successive peaks, you can get a decay ratio, a damping ratio, whatever you want to call it, as a function of time, or not as a function of time, but it just says the decay ratio is a function of the power level.

I've chosen these two particular ones because 6500 kilowatts is stable, which is stable on the bundle; 7500 kilowatts is unstable in the code and it is also unstable in the facility.

And this is a manifestation of both the drawing of the amplitude and also the calculation of the limit cycle, and what it looks like.

So for the decay ratio, you look at how the successive peaks decay. For growth, at least the initial parts of the growth, this is a ure exponential growth, you can get the gain ratio.

When you get close to the limit cycle, of course, you get higher, you get nonlinear, nonlinear phenomena that limit the magnitude, and then you go into the limit cycle.

We started investigating this, and we started changing the nodalization. The reason I show these time traces is to show you the results for different nodalizations in the bundle. We started out with 18 nodes in the FRIGG assembly. Then we started changing the nodalization. We thought it would be sensitive to the location of the boiling boundary. So we replaced the bottom three nodes. The first one is equally spaced, 18 equally spaced, and that was up the bundle.

We started increasing the number of nodes at the very bottom. We replaced the bottom three with five, with ten, and with 15, and looked at the effect of the nodalization on the decay ratio. It looks like it is going to f: .d over and get constant.

Then the guy who did the work decided well, I'll increase the number of nodes at the top of the bundle. We started out with three in the bottom and 15 in the top. We did double the number in the top. So we had 30 in the top. And the decay ratio jumped. It wasn't linear, or wasn't on a nice, smooth curve.

This is for the stable case where you perturb the power, increase it 5500 kilowatts to 6500 kilowatts. If the decay ratio is less than zero, it means it goes to steady state.

This is for the unstable case where it goes off and goes to the limit cycle.

So that motivated out work in looking at numerical banding, just like GE had done, Anderson had done.

[Slide.]

MR. WEAVER: I did basically the same kind of thing that he did. I started out with the underlying partial differential equations in TRAC looked like this. This is what we call a semi-implicit numeric where the flux terms of math and energy are at the beginning of time step, is what has been called explicit.

The represented traveling wave I have represented a little different way. An amplitude, and this is the spatial part. If you stick that back in here and do a whole bunch of algebra you can get the amplitude ratio between successive time steps in this function, where C is the top number, K is the wave number and X is the length of the node.

[Slide.]

MR. WEAVER: Now, if you look at the most unstable wave, which is the wave whose wavelength is twice the length of the test section, you want the wave to be 180 degrees out of phase from inlet to outlet, so the wavelength is twice the length of the test section.

That means that the wave number is 5 over N where N is the number of nodes over the test section line.

If you use the damping for a single time step, which is given on the previous slide, how many time steps does it take for the wave to go from the inlet on the test section to the outlet?

Well, it is the number of cells divided by the count number, time steps required for the wave to propagate from the inlet of the test section to the outlet.

So if you take the damping curve time step times the number of time steps, now this is a power, this is the decay ratio for the amplitude of the wave from the inlet of the test section to the outlet. This is a nice, big function. This is a power. This function in the brackets is a power.

You can have the fixed number of nodes and you can vary the Courant number. What that does is varies the time-step. Or you can do it another way. You can fix the Courant number and vary the number of nodes. What that does is it varies the time-step and the spacing at the same time, and in such a way as to keep the Courant number constant. I'll show you what that does in a numerical damping or the explicit numerics.

[Slide.]

MR. WEAVER: As Jens showed, if your Courant number is one, your numerical damping is one. That means that there is no damping at all. It has the same magnitude at the end of the test section as it has to begin with. If you plot that decay ratio function the first way, keeping the number of nodes constant and varying the Courant number, you get this family. This is the 12 node, 24 nodes, 36 nodes, 48 nodes.

What you notice is that these curves, as the number of nodes increases, the curves get flatter and flatter. So in an infinite number of nodes, this curve would be just flat right along the one line.

[Slide.]

MR. WEAVER: Another way to plot this would be to hold the Courant number fixed and vary the number of cells, and this is what you get. This says that depending on the Courant number, again the Courant number being closer to one, flatter slope. It's also almost a straight line, a very straight line.

So this motivated us to start investigating the convergence properties of the code by rather than fixing the nodalization and decreasing the time-step, what we're doing is fixing the Courant number and increasing the number of nodes and decreasing the time-step at the same time.

Now, it's true, in a practical sense, that it's impossible with equally spaced nodes and the real problem to keep the Courant number the same in each and every node. It is physically impossible. So what you would like to do when you're going to the numerical characteristics of your model, is to allow the least amount of damping.

[Slide.]

MR. WEAVER: If you go back to this slide, one of your cells might be up here. Another one of your cells might be at a very low Courant number. It is not good to have one cell giving a lot of damping where some of the others give no damping. What you'd like to do, you'd like to run along this curve.

So for a given cell, if it runs back and forth along this as the conditions of that cell changes, the dampening contribution, the numerical dampening contribution of that cell is small; it's close to one, not effecting the results.

[Slide.]

MR. WEAVER: So what we're doing is trying to motivate what we're doing with the code. What we've done is run the 6500 kilowatt simulation with equally spaced nodes. We kept the Courant number constant at .5. We've run different nodalizations and different time-steps as well.

This is with 12 nodes, 18 nodes, 36 nodes. You might be able to see that it looks like there's a little funny thing hanging down here. That's another point at 36 nodes at a Courant number of .2. Now, on TRAC, both in TRACB and in TRACG, what you put into the code as the user input is the maximum Courant number.

So when I say this is done at a Courant number of .5, that means you put in the Courant number of .5. The most limiting cells will have a Courant number of .5. All the other cells will have Courant numbers below that.

We're attempting to see whether this is linear. We fitted it to a straight line so you can see what the slope of it is. We've tried to get another point in here and we've been unsuccessful. We seem to have a bug in the code with large numbers of nodes.

The FRIGG facility has a uniform power shape so that the heat flux in a node should be independent of the number of nodes. And when we jump 36 to 54 nodes, for some reason the heat flux in the co-calculates exactly half of what it should be. I mean, exactly .50000, which is very strange to me. So we're in the process of looking at a coding problem of some kind.

[Slide.]

MR. WEAVER: We've done the same kind of thing for the limit cycle. The magnitude is a function of nodalization. What I've shown here is just a line for the three test cases that are shown. You probably can't tell, but it turns out that the change in the limit cycle magnitude is much smaller when you change the nodalization than the change in the decay ratio.

[Slide.]

MR. WEAVER: This work is ongoing. I would like to point out some problems with this. As I pointed out before, each cell has a different problem and a Courant number for a given cell changes as the flow oscillates up and down. Also, the decay is what I call a group phenomenon. It's a constant of the equation of motion.

So it depends on the dampening of all cells and since the Courant numbers are not the same in all cells and the Courant number for each cell is changing in time, it depends on the dampening of all the cells. The decay ratio is the way that the person who did the work, they did the calculation of the decay ratio by hand, so there is some scatter in the data because it was done by hand.

But the real problem is that for each successive calculation, the costs go up by a factor of four, because you normally double the number of nodes; that doubles your cost. If you want to keep the Courant number constant, you have to have the time-step. That's another factor. So the cost, it doesn't take very many powers or factors of four before you're talking about real money here to run these calculations.

[Slide.]

MR. WEAVER: As Zia pointed out, one of the limitations -- that concludes my discussion of the assessment of the thermal hydraulic models for stability applications.

MR. TIEN: All your numerical damping studies are based on the first order.

MR. WEAVER: Yes. It's first order, space and time.

MR. TIEN: How do you know that is exactly the results, the results for going to a higher order numeric or some other --

MR. WEAVER: I compared the results to the theoretical examination of the damping of a first order method. That's the one we have in the code. Ideally, you'd put higher order methods in the code so that you wouldn't --

MR. TIEN: You need some benchmark comparison.

MR. WEAVER: Yes, I agree. The purpose of putting it in the final analysis is the comparison of data.

MR. TIEN: Sure. But again when we compare the data, there are so many other things --

MR. WEAVER: That's right. That's why we like to know what the numerics, the first order numerics is doing to us in terms of the number of cells we use and the Courant numbers.

For reactor calculations, we do have the limitation of 1D kinetics. 1D kinetics has put in TRAC the NRC version of TRAC to do ATWS studies where the transient is in issue by normally closing of a main steam isolation valve. The pressure collapses the void. Most of the variations in the axial direction close.

It was chosen at that time to put in one-dimensional neutron kinetics. So that limitation would restrict the applicability of the NRC version of TRAC to the so-called in-phase oscillation.

The other real limitation is not so much a limitation of TRAC, it's the data. We're interested in the onset of instability, but we're also interested in the magnitude of the limit cycle and its effect on the average power of a reactor. If you're sitting after an ATWS and you've lowered the water level on a downcomer to raise the void fraction in the core, the power goes down as the void fraction goes up.

You might get in a situation where you lessen the driving head and it might start to oscillate. I think everybody agrees now that if you oscillate, the average power will slowly increase. The rate of increase is a function of the magnitude of the limit cycle. So we need to be able to qualify the codes for the calculated magnitude of the limit cycle.

We'd like to be able to do it with separate effects data. There is no separate effects data. The magnitude of limit cycle, sustained limit cycles in electric-heated facilities, for example. Most facilities are so afraid of oscillation that as soon as they see one, they shut it off. So there's a real lack of data in this area to qualify the basic hydraulic models for limit cycles.

You can do it in a couple, and that is what GE has done and what the LaSalle data. If your comparisons are good, you're real happy. If they're not so good, then you have the problem of is it kinetics, is the hydraulics, what is it. So you'd really like good, plain separate effects data in electrically-heated bundles. You just don't have that.

MR. CATTON: Why don't you get it?

MR. WEAVER: My wallet is real thin. We've recommended it. We've made our desires known. In summary, TRAC BWR has been established against a wide range of studies and transient test data. The stability-related assessment has shown that there are no fundamental limitations of TRAC BWR for stability analysis. Stability specific assessment is ongoing, also as Zia said. We've developed a methodology to -- I won't say remove, but to give us a handle on what the numerical dampening is doing to the answers that we're getting out of the codes so that we understand what the numerics are doing to the answer, whether we're solving the underlying partial differential equation correctly.

And we're doing confirmatory investigations to make sure that our projections are correct. That concludes my presentation. If there are any questions, I'd be happy to answer them.

MR. SCHROCK: Gary told us that you're going to have RAMONA coupled to track BF1 in order to do the regional oscillation problem. Did I understand that correct?

MR. WEAVER: No. RAMONA will calculate three-dimensional regional oscillations. That's part of the three-dimensional kinetics model. As part of RAMONA, they can take those three-dimensional cross-sections and create one-dimensional cross-sections for TRAC.

So we can one-dimensional in RAMONA. RAMONA also has a one-dimensional option. We can do one-dimensional comparisons with -- calculations with RAMONA and compare them with one-dimensional calculations in TRAC and compare the results, because they are quite different sets of constitutive relations, interfacial friction models, etcetera.

MR. WILSON: Let me answer the question. I think I probably misled you. The benchmark between RAMONA and TRAC-BF1 would be for those tests where both codes are requested, because TRAC-BF1's one-dimensional behavior, that limits you to an in-phase type oscillation. I did not mean to imply that the benchmark between the two codes would be a in a regime where TRAC-BF1 will not operate.

MR. SCHROCK: But you can already do the in-phase oscillation without RAMONA. So why do you need RAMONA?

MR. WILSON: Well, the outlet phase oscillations perhaps is symmetric about a diameter. That is potentially a very realistic behavior and that's where RAMONA would have its strengths.

MR. WULFF: The RAMONA code is intended to do full-core three dimensional. Whether or not that is an outer phase half-core against full-core oscillations. But there could be rotating half-core oscillations where the axis is not in one plane and rotates. That is the mission of RAMONA.

MR. CATTON: Could RAMONA be fit into the TRAC code, much like they fit COBRA into TRAC?

MR. WULFF: RAMONA has its own thermal hydraulics, one-dimensional everywhere outside the core. Whether that can be done, that would be, I think, a major undertaking.

MR. CATTON: Probably not worth it, unless you fix TRAC. Just to comment on your -- I was going to hold my peace, but this method that you've developed to remove the effects of numerical damping seems to go contrary to what any good numericist would do.

What you look for is a clean approach. If the time-step or differencing that you're using is hurting you, you don't clog it up with something that you can never sort out. You go back and you fix it. If you can't fix it, you trash it.

I just don't understand what you're trying to do. You're never going to know where you're at. You get velocities in the core that you're going -- you might even have a nodal point when you have that wave travelling up and down.

So the Courant number can go from zero to whatever. There is no way you're ever going to sort that out with the approach you're taking.

I think you would be better off not to make all those rounds and just fix the problem, if it's a problem. Either that, or don't use the TRAC code on stability problems. And so coupling it with RAMONA or coupling it with anything would be a mistake, in my view.

MR. WULFF: There is no coupling intended. There is a transfer of numerics -- of kinetics information from RAMONA to TRAC, but they will be run as separate codes.

MR. CATTON: But if you run them as separate codes, how can you get the coupling between the thermal hydraulics and the kinetics? You can't, unless you use some kind of iterative procedure.

MR. WULFF: No, the neutron kinetic parameters are collapsed in order to be input data for the neutron kinetics in TRAC.

MR. CATTON: Oh. Okay.

MR. WULFF: They pass a 3-D to 1-D.

MR. CATTON: Three-D?

MR. WULFF: Three-D will be collapsed to 1-D and then used in the 1-D TRAC code.

MR. MICHELSON: But it's also a multi-group.

MR. WULFF: Two group.

MR. MICHELSON: Two group. Only two group, huh?

MR. WULFF: Two groups.

MR. MICHELSON: I remembered more than that. Well, okay.

MR. SCHROCK: Even so, it's two group, and you're still collapsing, then, to one dimension with two-group kinetics. I don't know -- it just strikes me that the cost factor we keep hearing in doing this computation, it seems to me that it sounds like the GE scheme is working well with one group, and, I don't know, they didn't comment much about the great cost of doing that calculation that way. But with the Government codes, we seem to end up with a box where we can't do much without spending an awful lot of money on the computation.

MR. WULFF: RAMONA does not have a full two-group. It has what is known as a one-and-a-half group in that --

MR. SCHROCK: But it still costs more money to do the cross section evaluation.

MR. WULFF: Yes.

MR. WARD: Let me ask you a question. Is there a potential for axial instabilities, and, if so, will RAMONA be able to deal with that or identify whether there is some sort of axial mode?

MR. WULFF: The axial modes we have in kinetics in 1-D -- you mean the propagation of voids in axial directions are calculated with TRAC with RAMONA and in the plant analyzer.

MR. CATTON: You're referring to the neutronics, aren't you?

MR. WARD: Yes.

MR. CATTON: You know, all we've looked at, or all you've shown us, are instabilities in the horizontal platform. It's either full-core or it's about a diagonal. What about a double cell that's in the vertical direction, which is an axial instability?

MR. WULFF: There are 24 horizontal segments in RAMONA, and each one can be out-of-phase with the kinetic --

MR. CATTON: So RAMONA could address that question, Dave.

MR. WEAVER: Even the one-dimensional model in TRAC can do that. The power in the bottom is calculated on the whole plane basis, and that's a separate calculation from the one at the top. That's what we mean by in-phase oscillations. We mean that it's not in-phase from top to bottom; it's in-phase over each plane along the axial height of the reactor.

MR. WARD: Okay. I guess I'm thinking of combinations of --

MR. CATTON: You may have it about a diagonal, but also in the vertical direction, you have structure.

MR. WARD: Yeah.

MR. WEAVER: If you have that kind of thing, you get full three-dimensional simulator with it. That's what RAMONA is.

MR. WARD: Let me ask another question. Ivan, you don't like the approach Weaver has taken here, but as I see it, he's trying to develop some strategy for dealing with the problem that the code numerics may be obscuring whatever the reality is, and you don't like that, you tell him to fix the code, but is there some way -- let me ask Weaver -- is there some way to fix the code? I mean, what can be done?

MR. WEAVER: You can change the numerics.

MR. CATTON: You can change the numerics back to explicit, which are easier to deal with than the implicit.

MR. WEAVER: It is explicit, Ivan.

MR. CATTON: It is explicit?

MR. WEAVER: That is what GE is calling explicit. Semi-implicit and what GE calls explicit are the same things.

MR. CATTON: Then I didn't understand what GE said. What do you mean by explicit?

MR. ANDERSEN: The solution -- we have two options in the solution. We use either an explicit formulation or an implicit formulation.

MR. CATTON: I understood that, but he says that your explicit method is not explicit. So if you're explicit method is not explicit, what is it?

MR. ANDERSEN: It is explicit. What usually is referred to as semi-implicit integrating techniques has always been an explicit formulation of the continuity in any equation. The momentum equation is formulated such that you can exceed the sonic cool rod limit, and that's the origin of the name "semi-implicit," and I believe that you still have that formulation in your equation.

MR. WEAVER: Yes. If you look at this, Ivan, the time level on the flux terms on the property being fluxed is beginning of time step. That makes it explicit.

MR. CATTON: That's right.

MR. WEAVER: In TRAC, the velocity is new time. So we're using mixed. The velocity is new time; the property being fluxed is old time. For years, we've called that semi-implicit, but GE has chosen to call it explicit. So the GE explicit numerics and what I call semi-implicit are exactly the same.

MR. CATTON: Then why do you have the problem with adapting, and GE doesn't?

MR. WEAVER: They do. They have the same problem.

MR. CATTON: They do.

MR. WEAVER: They just didn't talk about it.

MR. CATTON: They just didn't talk about it, huh?

MR. ANDERSEN: They try to run the prong number of one, just like we do.

MR. CATTON: But he showed examples of amplitude ratios that were close to one with 18 nodes.

MR. WULFF: The problem is we have to distinguish numeric damping and decay ratio. These are not the same. They are being confused here. The decay ratio is an outcome of the characteristic equation of the whole system. That is confusing.

MR. CATTON: Yes.

MR. WULFF: The numerical damping is an outcome of truncation error, manmade numerics.

MR. CATTON: That's right. I understand that.

MR. WEAVER: The code answer that you get is the physical damping to which you add some more because of the numerics.

MR. CATTON: Right.

MR. WEAVER: You get the wrong answer. The code is trying to give you the right physics answer, but it can't because the numerical method adds an error on top of that. I'm trying to devise a way of dealing with the error caused by the numerical method. I haven't said anything about whether we're solving getting the damping ratio. That's why you have to compare your code to data, because that's the physical real damping ratio.

MR. CATTON: But normally what you want to do is first be sure you got the numerics under control, and then look at your --

MR. WEAVER: That's what we're trying to do here, to understand what errors do the numerics impose.

MR. CATTON: Somehow, I'm getting a headache.

[Laughter.]

MR. LELLOUCHE: May I perhaps make it more complex? I usually do.

[Laughter.]

MR. CATTON: Identify yourself, Jerry.

MR. LELLOUCHE: Gerald Lellouche, S. Levy, Incorporated. The proof theorems in numerics for these kind of equations say that if you want to find out how the numerical approximation to the PDEs converges to the solution of the PDE. What you do is you increase the number of spacial nodes and reduce the time step to maintain a constant ratio of time step to space node. And you keep doing that until you get to an answer which no longer changes significantly, and then you accept that as being the solution within whatever area you've said no longer changes significantly.

If you pick a Cournat number of one, and you happen to have a velocity which is constant, then you can get certain kinds of very clean answers, just as you have here. The velocity is a function of position and time because of temperature and void fraction and things like that. You can never get those kinds of answers. The results here are for constant velocity. That's what the clean answer is for here..

But in the real case, they are not constant, so the kinds of evidence we have can only be -- that approaches a real solution can only be gotten by doing the classical thing: increase the number of nodes, reduce the time step to maintain a constant ratio of delta T to delta X, and see how the answer converges. That's all you get. That's the way you're doing it, and that's why he was showing it.

He's saying that as you do that, the decay ratio appears to be moving linearly with the reciprocal of the number of nodes, or with the reduction in time step, and seems to -- if you assume it's linear, then it goes to some value at delta T from zero, which implies the number of nodes goes to infinity. That's all he was showing there. The same result would be obtained with TRAC. Exactly the same result would be obtained with TRAC.

MR. TIEN: I think it's getting more confused.

[Laughter.]

MR. TIEN: I really think not only the change in the time or spacial coordinate, but you really have to go to a higher order of numerics, and coupled with the changing of that, that will give you a good indication.

The damping product -- we got confused -- really, you have to kinds. One is physical damping, and the other numerical damping. We are talking about numerical damping. I am convinced that you have to go, if you want to do a real, good, solid work, you have to go to a higher order of numerics with some spacial or time step change. Otherwise, you never get a clearcut indication.

MR. CATTON: That's what GE showed with the SOC method, which was the center differencing, and the explicit time stepping.

MR. WEAVER: That propagates information upstream at a higher velocity than the fluid velocity. Second-order centered is not a panacea.

MR. TIEN: Oh, yes. Not second order. I never said second order. I said higher order.

[Laughter.]

MR. TIEN: You can get some indication --

MR. WEAVER: What you get by going to higher order numerics is you get -- if you want a five-percent answer, wiht second-order numerics, you might be able to get five percent with ten nodes, where with a first-order method, you might have tc have 25 nodes.

MR. TIEN: That's why I say you have to combine both.

MR. WEAVER: The order method affects the cost of the calculation, but in principle, first-order numerics will converge, but at a much higher monetary cost.

MR. CATTON: Well, if you're looking for stability, you do need order. There's no question.

MR. WEAVER: I don't agree with that.

MR. LELLOUCHE: There are other problems tnat arise where you can't really reduce your number of spacial nodes too small because it's going to start affecting your neutronics. And that's a problem that hasn't been raised, and that is that the information that transfers from the hydraulics to the neutronics is the average void fraction and the average temperature in the spacial node of the thermal hydraulics.

And it doesn't matter how many neutronics nodes you have in that thermal hydraulic node; each one of them gets the same information. And that information, if you pick too large a thermal hydraulics space node, will start to screw up the shape of the neutronics space result. So you can't get too large a spacial node in the thermal hydraulics because you get messed up on the kinetics.

And six inches is about the smallest you can get -- I'm sorry -- the largest you can get because that's the stop point for the control rods, and if you start getting larger than that, you start having control rods half inserted in a node, and that starts to screw things up, also. So there's a coupling which hasn't really been discussed here between kinetics and thermal hydraulics.

MR. CATTON: Okay.

[Laughter.]

MR. CATTON: I believe you, Jerry, but I'm still getting a headache.

[Laughter.]

MR. CATTON: I think we better proceed. The next speaker is Gary. You're going to give a quick summary?

MR. WILSON: I presume you want to go ahead and take about 15 minutes and overview the BWR or the stability symposium?

MR. CATTON: Yes. Yes.

MR. WILSON: I believe most or at least part of the consultants on the subcommittee have received the handout that was given out at the time of the stability symposium, and that has all of the results of the symposium in some excruciating detail. My purpose here is to spend about 15 minutes and just summarize what went on at the symposium.

[Slide.]

MR. WILSON: You can see here that there are several of us who have collaborated on this. The three topics that I will address in the presentation will be to summarize the symposium objectives, symposium structure and then to talk about what we believe to be the significant results of the symposium. The first two items will be rather brief in nature and we'll try to focus and spend most of our time on the significant results.

[Slide.]

MR. WILSON: The symposium was conducted in conjunction with the TRAC BF1 workshop which was conducted in August at INEL. The objective of the symposium was to provide an international forum for perspectives of the various organizations involved in reactor safety, presentation of recent studies relating to stability and then to provide an open discussion of common problems, questions, approaches and things of that nature.

The symposium was hosted by EG&G with tacit support from NRC and DOE-ID. There were approximately 60 participants from 20 organizations in 6 different countries, so it was international in flavor. There were four keynote speakers who offered perspectives from regulation, from the vendors, the utilities and the utility supported research.

There were 12 presentations covering research and experimentation and general analytical studies, and then in more specific code simulations, primarily BWR type simulations. There was a wide plenary session with an open discussion on common problems.

[Slide.]

MR. WILSON: The information that was presented at the symposium and, I think, the remarks that were made during the plenary session, tended to confirm existing opinions in the areas that are listed here. What I am really saying is, for those of us working in the field, I don't believe there was really any surprising results, but I think it was a chance for a large body of international flavor to come together and try to crystalize some of the important things.

There was a general consensus that, yes, there is a sensitivity of the time domain codes to nodalization and time step and you have seen some of that discussed here. In fact, I think part of the things that went on in the symposium motivated some of the work that you're seeing ongoing. There is a consensus that there is a sensitivity of the time domain code simulations to tracking of the boiling boundary and I'll talk about that --

I'll talk about each of these a little bit more subsequently. There's an average power level dependency on oscillation amplitude. There's potential interactions between local action and corewide hydraulic oscillations, and then we'll talk a little bit about prototypical data for assessment of code calculations, particularly oscillation amplitude.

[Slide.]

MR. WILSON: Nearly all of the code application studies that were presented, demonstrated the dependency on initiation of stability and the oscillation amplitude on nodalization and time step. You've seen results prior to this, both from Henson's studies. Mr. Weaver just talked about those and it showed the nodalization and time step dependencies and then, of course, Mr. Andersen had presented some of his earlier work on the explicit and implicit numerical simulations.

He has covered that well and I'm not going to say any more about that. Again, I just note that the studies provided motivation for additional work that was just reported by Dr. Weaver.

[Slide.]

MR. WILSON: Here is something that has not been discussed here. There was a study by Galor and Jensen and it indicated that in the time domain code, there is a time domain code sensitivity to boiling boundary tracking that appears to be independent of the numeric integration scheme and the time step.

The calculated decay ratios significantly influenced by the boiling boundary and large inlet nodes. The study results indicated a need for fine nodalization in the absence of a specific model to track boiling boundary location in large nodes. What I'm telling you is that their findings and their study -- and it's reported in the symposium handout that you have -- says that there's an additional sensitivity to boiling boundary location and large nodes that is independent of the numerical integration scheme and the nodalization.

I would refer you to their paper for those results. Dr. March-Leuba also presented then information from the frequency domain code in the core. I believe you're going to see more of that later on tomorrow when Jose gives a presentation. These four bullets capture our perception fo the important messages that Jose's work brought to the symposium; that a limit cycle does bound the power isolations.

Typically, there's an average power increase of one and a half to two percent of peak power oscillations when you go into an oscillate core mode or into a limit cycle mode. The limit cycles can become unstable and bifurcate and ultimately lead to aperiodic or chaotic regimes for peak powers of 500 percent of steady state.

Then, the limit cycle stability and bifurcation has not yet been as extensively analyzed with the time domain codes as it has with LAPUR, the frequency domain code, and there appears to be interactions between the channel and the core rod oscillations that are of particular interest, particularly with respect to the mode of the oscillations, whether it's in or out-of-phase.

Lastly, this message has already been spoken of several times here. I think there was a general consensus that their prototypical database for BWR stability for code assessment has certain limitations. The database is considered reasonable for assessment for the onset instability in single channels, however, the database for limit cycle amplitude assessment is, at best, not readily available to the general industry and general community at large and in my view, it is likely insufficient.

I believe Jerry Lellouche has a remark or two that he would like to make that covers and aspect that I have not covered, and I would like to turn the floor over to Jerry to speak for two or three minutes on that aspect of the symposium.

MR. TIEN: I have just one question. Do we have any database available for the limit cycle instability or bifurcation? You mentioned that the database is not readily available. Does that mean not available or somewhere available, but not readily?

MR. WILSON: All right, let me give you my perception of why I made that very statement. I think you've seen some data recorded today by General Electric for Leipstadt and from LaSalle. It is my view that because of GE's unique position as a vendor, they are able to acquire that kind of data readily. I'm not sure the community at large has the same opportunity to obtain this kind of data readily.

The other aspect of that is, and Walt Weaver touched on this, it would be nice to have some additional separate effects type data to work with. That is, to my knowledge, just not available. Now, perhaps I ought to make a remark to get this in the right context. I'm a code assessor.

That's my role in the business. I have to admit that code assessors tend to like to have lots and lots of data to do code assessment. Perhaps there's a little bias on my part; that maybe the data availability is a little better than I believe it to be, but I'd like to have more data to fulfill a code assessment role. Jerry?

MR. LELLOUCHE: My name is Gerald Lellouche. I'm from S. Levy, Incorporated. As far as the data is concerned, there is only one set of public data available on separate effects and that was the same Pierre thesis done at Argon in

1968-72, in which he applied oscillating wall heat and measured the void fraction, both in the saturated regime and the subcooled regime.

There are peak-to-peak amplitude void fraction pieces of information available, but that's the only public data that I know about that actually exists. The Swedes have great heaps and piles of such data, but for the BWR type assemblies, none of that has been made public. It's all proprietary.

There are only two things that I'd like to talk about and both relate to the connection between thermal hydraulics and the kinetic. The first is the question of using different time steps in thermal hydraulics relative to different time steps in kinetics.

If you hold the thermal hydraulics constant and then run two, three or whatever -- how many neutronic time steps, what you have is a linear neutronics problem, and neutronics moves on a constant exponential period during that period of time because the feedback doesn't change. If the power is going up because you've had effectively a positive reactivity input from the thermal hydraulics, then the neutronics moved on that period.

But if it's going up very fast, as it does in some of these transients, then the thermal hydraulics should change rather rapidly because both of Doppler and because of direct energy deposition into the liquid. So you can get an overshoot in the power from the neutronics because you're doing too many time steps in neutronics compared to thermal hydraulics.

Similarly, on the dropping side, you have exactly the opposite effect and the power drops too rapidly compared to what it should. As far as I know from all the studies that we've done, you really need to do one time step, the same time step for thermal hydraulics and neutronics, one part of each each time.

The second thing that I'd like to talk about is relative to tracking the boiling boundary. It's clear that you need to track the boiling boundary in the thermal hydraulics in order to be able to get to the right kind of thermal hydraulic response. When you covert thermal hydraulic information into neutronic information for the thermal hydraulic volume where you have the boiling boundary. you have part of that volume without voids and part of them with voids.

The voids are such a strong feedback phenomenon. The way you average that to provide the cross section information becomes very significant. The only way I can explain it is that in moving control rods, there is a classical reactor problem known as the cusping problem in which a control rod is moving through a neutronic volume, partially inserted into the volume.

You find that you can get five, ten, percent errors in local power, because of the way you average that control rod, partially inserted, over the entire neutronic volume. The presence of void fractions starting in a volume, is essentially the same as the cusping problem, except that you now have sort of a triangle of voids rather that a square of control rods.

That needs to be handled correctly in order to get the correct kind of neutronic response.

I said only two things. There is third thing. The third thing has to do with ALVEDO, rather than reflectors. We have just completed some calculations at comparing ARODA, which is a 3-D code -- 3-D neutronics code, which contains reflectors, with SIMULATE, which is a 3-D neutronics code which uses ALVEDO.

When we first banchmark the code using the same boundary conditions, what's called a vacuum boundary condition, and found that they produced very, very similar answers over the entire space, and then we did the same with the calculation again, with an ALVEDO from the SIMULATE and a reflector for errata, and we found that there were 5 percent power differences locally near the top of the core where there was strong voidage in the exit reflector.

So, I feel that that is something that needs to be considered, also. So, that's really all I wanted to talk about.

MR. CATTON: Thank you, Gerry.

The next speaker is Wolfgang Wulff, from Brookhaven. I think I'm right this time.

MR. WULFF: I am Wolfgang Wulff from Brookhaven National Laboratory, and I have been asked to discuss codes of two computational methods at Brookhaven National Laboratory. I found out this morning from the agenda that I have less time than I thought and, also, there are certain delays already.

[Slide.]

MR. WULFF: I will first turn to the discussion of RAMONA. We were asked to describe the RAMONA code, and I will do this very briefly. It is a rather large code, and we have already talked about its major characteristic, as it distinguishes itself from other codes. Then I will touch on RAMONA assessment, and after that, I will clearly state what our objectives are with RAMONA, and I will show some results from RAMONA today. Our RAMONA analyses are not completed at this time. Then I will summarize its limitations and explain what is on our agenda as defined by NRC to carry on RAMONA into fiscal 1990.

The RAMONA code, as it was at the end of 1981, is documented. It was in an inactive status for a long time. There have been changes made, particularly recently, and their documentation is only now being drafted.

The major characteristics are that it is a systems code for BWR, that has three-dimensional neutrons in it. It is a one and a half group core mesh diffusion model, which means that only one group is really calculated with the time-dependent. The second one is quasi-steady. It has six delayed neutron groups, calculates decay heat from ANS standards and has all the reactivity feedbacks for a moderator doppler and so on, and it uses rectangular coordinates, as we had discussed earlier.

The thermal hydraulics is now very much as in the plant analyzers. The mechanical disequalibria are calculated with the grid flux model. The non-equilibrium features come in for the non-equilibrium vapor generation rate. It has parallel channel flow for the hydraulics, as it has for the neutron kinetics. Everywhere else, there is one-dimensional flow.

The thermal hydraulics at the core equation reflux model, and I will say that the choice is something that was made here, but it came to Brookhaven from SCANDPOWER with four equations. It uses a loop momentum instead of -- uses a mixture of volumetric flux divergence equation, which allows us to replace the numerical integration of the mixture-mass balance to a quadrature in space, a mixture of energy and mass balance, vapor mass balance, are the only ones that need to be integrated as partial differential equations, the same way that has been discussed earlier.

The vapor is at saturation. That is an imposed condition and, also, a limitation on RAMONA, where, on the other hand, the liquid is either subcooled, saturated, or superheated, depending on what the mixture energy required or what the mixture energy calculates.

It has, then, all the U.S. components of a BWR check pumps recirculation, pump motor systems, and the generator dynamics. It has all the safety and relief valves. It accounts for the acoustics of which we have spoken earlier, which is important when you have sudden valve closures, and the feedwater conditions are imposed on RAMONA. That means we do not have condenser and pump models.

MR. CATTON: Where do you get into trouble with reverse flow?

MR. WULFF: Not anywhere. There was, in the earlier version of RAMONA, no reverse flow differencing. It was only in forward flow. I have a slide on which I discuss the changes that we have made, and at that time, I will point that out.

We used inclusive integration. This is the way the code came. I think people may have made the same choices as we made later on in the analyzer development. Only the neutron equations are integrated implicitly, because their time constants are much shorter. Their response times are much shorter than in the normal hydraulics.

All the other equations are explicitly integrated for the delayed neutron, for the N plus 1 for N channels plus the recirculation loop momentum balances. That is only used for global mass and energy balanced to calculate the system pressure, as an ordinary differential equation, then for rate of mass and energy, where we use volume averaging in order to calculate ordinary differential equations in each computational cell, and the same for the mass balance and momentum balance, that gives us the acoustic effect, and then, of course, for rotating machinery and control systems, where we have a large number of ordinary differential equations, that is integrated explicitly.

MR. CATTON: Is this a true explicit or this strange one that TRAC uses?

MR. WULFF: These are true, in essence, textbook integrations for ordinary differential equations.

MR. CATTON: So, it's true explicit.

MR. WULFF: Yes.

Also, there is no known linearization involved. Maybe that's on the next slide, but where we do have computational errors -- well, in addition to this integration method, we have quadratures in space, from which we use either trapezoidal rule or Simpson's rule, depending on whether we have mean values that we need to add up over a channel or we have discrete values at the boundary. That comes into action for the divergence equation and for the momentum balance, particularly for the gravity terms.

MR. CATTON: Are these different from the inertial terms? Do you use the differencing?

MR. WULFF: No, they are integrated. We integrate analytically over a section of a channel and have then the DDZ of GM-squared over RO, and we have the difference at exit and entrance of the channel, and that we carry out for every straight segment around the loop, and we then solve the -- for each step in the equation for delta-T and add that up and the close integration of DP is equal to DDZ is equal to zero.

MR. CATTON: I understand.

MR. WULFF: From that we can then get all the elements around the loop. Where have to link between such cells, we have to use the form losses where there are sudden changes in cross-section. So, that leaves, really, summations of the gravity terms where this curvature is needed. The others are simply summations, one for each computational segment.

Now, as I said, there is no linearization. One reason is that we use the product of two variables as our state variable. For instance, RO alpha is the state variable, and we use, then, the sum of caloric equations to separate ROV later on in analogy. We have, therefore, no linearization of our equation.

Our computational errors come from these sources, but the most important one is, as we discussed before, the numerical diffusion. This is a time domain code, and there is an error in neutron kinetics, as well as the hydraulics. Less important errors are from numerical quadratures, where we use Simpson's rule that is a higher-order, a fourth-order accurate, and trapezoidal rules, where we have mean values, also.

We have here computation error from the methods that we used. There is always an error which is known from standard textbooks. Numerical diffusion is the most important one, and we use the same method that was used before. We try to reduce it. We cannot eliminate it by using the largest possible number less than 1. You cannot use larger than 1, because it becomes unstable. In fact, you cannot use equal to 1, because you will then amplify truncation errors.

MR. CATTON: Do you use the Euler method? That damps quite heavily.

MR. WULFF: The Euler method is used in some differential equations.

As you have seen before, in Jens' presentation, when you use the first order Euler with domicile differencing, for the special case of core number equal to 1, the method becomes identical to the method of characteristics, and in that case, you reduce the diffusion to its minimum. In other cases, this is the only first-order method, and you have diffusion with it.

We have now a PWR development, used second-order upwind weighted differencing scheme that has no problems, as was mentioned before by GE, but it eliminates all truncation terms up to the fifth derivative. That is, we have second-order damping and second-order dispersion, but not the first-order diffusion and first-order dispersion.

That method should really be used here, but we did this in a new development. For RAMONA, we really inherited this, and we used it, then, also in the plant analyzer.

By the way, existing methods -- higher-order methods exist in the literature for as far back as 20 years.

What we did in recent times we replaced what we inherited in the SLIP model and replaced it by the drift flux model, and then we introduced the capabilities for flow reversal, so that we have upwind differencing, the same way as we had in positive flow, now also in negative flow.

And then, of course, in the branch where we have a large number of channels at the lower plenum with with an arbitrary distribution of upflow and downflow, we have to arrive at a method for branching. That is basically modeled as if we had an interface with no storage and we have distributed the void distribution from the plenum over all the channels. Those are the two conditions.

[Slide.]

MR. WULFF: We have had the RAMONA code for quite some time. There are a number of assessments that are reported in this NUREG CR report, start on page 315. We have used steady state channel to compare the axial void distribution, and then from Peach Bottom we had safety relief valve tests, and from Browns Ferry, we had recirculation pump trip tests, which has relevance to LaSalle.

And from Peach Bottom, we also had the turbine trip test, that is, I think, the highest frequency contents. The test with the highest frequency content and that we have a very sharp pressurized, in fact, reflections of pressures from the steam line that then collapsed bubbles, and lead to very sharp hollow spikes in the core.

We use this to determine the number of cells that were needed in RAMONA, and the others that are recirculation pump tests. Some of these tests are relevant to instability, but now that we resumed an acquaintance with RAMONA, we use the FRIGG test, both in uniform and nonuniform axial power. All have nonuniform radial power distributions.

[Slide.]

MR. WULFF: We used the test -- maybe I don't need to go through this, because Zia has shown this before -- we used the power oscillation test. I will show you instead the results and Zia has explained how this is done. There is a pseudo-random binary sequence imposed on the power, on the Q triple prime.

[Slide.]

MR. WULFF: That then produces a pattern of heat flux that links this to the hydraulics, that is here from the thermal capacity of the electrically heated thin tube that produces this pattern.

[Slide.]

MR. WULFF: From that, we get flow oscillations, a short section, from 138 to 152 seconds, as is shown here. Then we obtain the gain and the phase shift, as shown in this diagram.

[Slide.]

MR. WULFF: The X axis represents the frequency, the Y axis the gain. Here are the test points, test results, with our interpretation of the uncertainty or the measurement error, and this is the RAMONA calculation, so undoubtedly there is some numerical damping, or this difference is caused by the fact that there is nonuniform power distribution, and there is an internal relaxation which as we model a channel by three concentric segments may not catch because we do not have cross flow. We really have three parallel channels, and that is a shortcoming in the model.

As a result, we have to attribute the difference to a combination of numerical damping and to uncertainty about the nonuniformity in the power within the channel.

[Slide.]

MR. WULFF: This is on the phase shift. This is the phase shift. The maximum is here, and half -- I'm sorry, here it is, and we see mass data point here as we go to higher frequencies.

We really have focused our spectral analysis in the range of half hertz. We don't claim that this is the actual difference. We would have to focus and do the sampling in this area in order to determine whether that is at least high frequency, the true error.

MR. CATTON: You may have some filtering in your numerics, too?

MR. WULFF: Yes, I think that is the most likely explanation. We did the same thing for the high subcooling case.

[Slide.]

MR. WULFF: We obtained this answer for the gain on frequency. Here is a maximum, and here is the experimental points.

[Slide.]

MR. WULFF: For this case, we have the frequency shift. Sorry. That is the in the same train, then we have more shift at a higher frequency. But I think in general the kind of accuracy that can be achieved from these kinds of tests cannot be expected to be much higher than this for experimental reasons.

We have in the power an uncertainty of 1 percent, and that 1 percent is a large fraction of the amplitude with which the power was varied during the test. And I think that we may have to determine where the causes are for the discrepancy.

[Slide.]

MR. WULFF: We were also concerned about the differences obtained from different cell size. You see here the same frequency response carried out once with 12 nodes, and going to the larger gains with 24 nodes, which gives you almost the same, but slightly lower gains.

There is really not much difference between the 12 and 24 nodes, and these calculations.

[Slide.]

MR. WULFF: We also asked ourselves what do we get if we cannot maintain the largest Courant number, the number just below, and here the result is the top one showing the frequency with a Courant number around .92, and then half of that value reduces the gain from something like 2.1 to .98.

[Slide.]

MR. WULFF: We asked ourselves what is the consequence of going to thermal inertia and it is almost nothing because it's a very thin heated channel both under otherwise safe conditions but in one case it would be cross-correlated to the power versus flow, in one case and in the other the heat flux versus the power.

[Slide.]

MR. WULFF: This is what we have done with RAMONA, with assessment recently, specifically for the LaSalle instability anomaly. The purpose is to identify the causes of the mechanism, the conditions for out of phase region wide, power oscillations. That is the mission of RAMONA. That is the three dimensional calculations. We'd like to simulate the entire core and determine what pattern can develop and how does the pattern possibly change in time.

We not expect, in fact know from some reactor transients where allegedly the axis of symmetry or the plane of symmetry or spiral of symmetry is located.

The second one is to determine the inherent amplitude limits we have calculated.

I will give you some results later on region-wide calculations.

The third point is we want to identify control rod patterns which are prone to produce local oscillations or region-wide oscillations.

Finally, the NRC needs to have a general tool -- these are our stated objectives.

I will summarize the results.

[Slide.]

MR. WULFF: We have done preliminary calculations where we found region wide oscillations. These calculations are preliminary because they were done with an existing input depth, not with specific LaSalle conditions. In fact it was

1

IMAGE EVALUATION TEST TARGET (MT-3)

150mm

6"

1

IMAGE EVALUATION TEST TARGET (MT-3)

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IMAGE EVALUATION TEST TARGET (MT-3)

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IMAGE EVALUATION TEST TARGET (MT-3)

150mm

6″

done with Browns Ferry conditions, which think was -- Browns Ferry was stable and did not lose the instability so we had to use atypical boundary conditions, as is indicated below, and that means introduce sub-cooling or feedwater temperatures below the ones that were shown at LaSalle's on board computer results.

Because of oscillation there's a small number of unstable channels with high power and low flow that came out of these preliminary calculations. We calculated 300 percent of rated power amplitudes or before we reached flow reversal. We are now in the process of continuing this calculation, now that we have flow reversal accommodated.

The temperature oscillations for the fuel however with these 300 percent, 200 degrees C. I don't have the required temperature. We have to sort out the data to get that.

The period however was very much as that in LaSalle.

So in principle, RAMONA can calculate the oscillations from a thermohydraulics point of view. I think that the damping is within bounds much smaller than the uncertainties we have from other sources, particularly from new transkinetic parameters.

[Slide.]

MR. WULFF: Here is a summary of the limitations as we see them. It is first a computational resolution. We inherited the code with the limitation of 200 neutron kinetics channels. A reactor has on the order of 800. It means that in order to calculate the full core, three dimensional transient, we need to bundle the rods in two-by-two sets. We also have a maximum of 24 in axial segments. This can of course be changed. The code can always be changed but the way that multidimensional arrays were all strung up in one array for easy swapping in and out, it is a rather time-consuming job to change, to expand the dimensions in the code.

So we think that we should not expand to 800, not only because it is difficult to change the code but because it becomes prohibitively expensive once you have 800 cells to run this code and then to actually use it.

I think we should much more try to get good enough approximations with the two-by-two segments.

Here is the second limitation. It is an expensive code if we have the 200 channels here and for a neutron kinetics on the order of 35 for thermohydraulics calculations and these 24 axial segments, then it will run 120 times slower than real time and we need at least four minutes to calculate, so you will see that for every calculation we will really use a week of calendar time. The reason is that you can put this in the morning first thing into the machine and it is in the queue. You can watch it processing but it will not be done until the next morning at four o'clock or in the wee hours or something like that.

The other thing is that we'd need detailed kinetics parameters. We always have two years COSMO information to really get the neutron kinetics and that requires on the order of three months for every reactor to be prepared.

There is no superheated vapor simulated so if the oscillations become strong enough, and that would require to reach near critical problems, then we would reach a limit with RAMONA. Then we have no tracking of the boiling boundary which we find is less important for LaSalle but may be important for comparing with experiments with the non-boiling length is a reasonable factor of the total. For LaSalle with its sharp bottom peaking, the boiling boundary is within the first two centimeters are of the channel.

We have only one dimensional models in the plena that means that we cannot calculate a partial failure of the jet pumps or we cannot calculate the effects of flow exchange between channels in the plena other than through this one dimensional approximation.

[Slide.]

MR. WULFF: Finally --

MR. LEE: Wolfgang, why do you say you do not have boiling boundary tracking capability in RAMONA?

MR. WULFF: There is no LaGrangian boiling boundary tracking in RAMONA. It is not modelled. I don't think it is a major undertaking but we actually we haven't implemented it. That is the major reason. It may be that is the end of the answer.

MR. LEE: But I mean do you have so many finite measures to represent axial or density dispersion?

MR. WULFF: We know where the boiling boundary is within the uncertainty of that 15 centimeters.

MR. LEE: I understand.

MR. WULFF: Then you have seen with large sub-cooling in our FRIGG tests, the boiling boundaries should be not close to the boundary, should be a significant fraction of the entire channel length but the comparison with the data is about as good as that is with no sub-cooling where we have boiling directly from the beginning.

I would like to answer any questions on RAMONA that you might have before I go to the plant analyzer.

MR. CATTON: I don't see any questions so maybe you can proceed with it.

MR. WULFF: All right.

[Slide.]

MR. WULFF: Until now, we have really talked about computer calculations. Now we go to what is called computer simulation. That is engineering plant analyzer. I will follow the same pattern to explain what we have done for the assessment, and then I will tell you what the engineering plant analyzer's objectives are in NRC's grand scheme of BWR stability analysis.

Then I will give you some results, only a small fraction. We have done more than 60 different transients and documented them. I will tell you the ones that I can -- then I will summarize the EPA limitations and whatever future activities are planned for the engineering plant analyzer.

The engineering plant analyzer was developed less than three years ago by four people with documentation as of June, 1984 as this NUREG CR. As major characteristics, it is a simulation facility, not a calculation facility. It has three attributes.

It uses the computer that is designed for simulation, not much else, but for simulation. It uses a systems software -- along with that comes the systems software that provides the simulation environment. With a general purpose computer, normal you get a compiler and you get a file handling system, but a much larger package is here provided to give you online interactive operations.

That's part of it, and then they give you not only the standard Fortran, but also simulation language. In addition to that, we have used six modeling principles. The first one is model selection and that tells you that you should use the least complicated model that accommodates the experimental information.

The bottom line of that is that the two-fluid model is not the best choice to simulate BWR transients, primarily because of the difficulties of closing it. Instead, you should use the drift flux model. If you derive all your interfacial shear from the drift flux, you might as well use drift flux directly.

Then the second one is a priority identification. I think we have an obligation to eliminate unimportant features. We were forced into that because we had to scale and we had to scale in such a way that through the scaling parameters, we could see which terms in the equation are important under all circumstances. So we eliminated the unimportant ones.

The third principle is that we integrate analytically wherever possible. You saw some examples that are also used in RAMONA, in that we use the flux divergence equation and we integrate around the loop for the momentum plenum, but this is done in many different ways.

The fourth principle is to eliminate all iteration. We have many systems of coupled, non-linear equations which we've solved beforehand and then calculate the results in terms of the variables that we calculate. With that we reduce all iterative procedures to linear interpolations during the calculations.

We go even further and all our combinations of thermal physical properties and so on, in effect, the coefficient matrix element we pretabulate. The fourth and fifth principle reduce the number of algebraic and logical operations by orders of magnitude. That is, in hypothesis that we precalculate all the decisions on flow regime and heat transfer and as a result, we reduce the number of operations.

Finally, we selected between implicit and explicit integration on the basis of the frame time -- that is, the time it takes the computer to go from one time over to the next, and a ratio of that to the permissible time step that is dictated by accuracy. The bottom line of that is that explicit integration should be used.

We have done this from the outset. We think that anything that 10 Hertz or lower should really be integrated with explicit methods; certainly higher than 10 hertz, the large breakoff. So, it would be much more efficiently integrated.

In essence, we have optimized, as a whole, all of these three things, machine architecture, modeling and ending in numerical methods.

[Slide.]

MR. WULFF: Now, I don't want to describe the hardware in detail. The EPA engineering plant analyzer has this applied dynamic international system. I had said enough about that. There is the HIPA code and it stands for High Speed Inter-Plant Analyzer code.

MR. CATTON: What do you anticipate the time is for this?

MR. WULFF: I think 20 minutes.

MR. CATTON: Twenty minutes?

MR. WULFF: Yes. We may skip a few things because we have covered it.

MR. CATTON: Okay, twenty minutes.

MR. WULFF: We use point kinetics which is very much with the standard attributes. We use integral methods for conduction in fuel, and in thermohydraulics, we use the drift flux model and as in RAMONA, the non-equilibrium according to scan power.

The same feature is used : momentum bound mixture of volumetric flux divergence equations in which your energy bound and -- is integrated as part of the differential equation as in RAMONA. Again, vapor is limited to saturation and the liquid is free.

MR. STERN: The three channels that you represent are the core average, the hot channel and the bypass?

MR. WULFF: And the bypass, right, but it can be reassigned. What you said is the way it is implemented at this time. To simulate the nuclear steam supply system, the balance of the plant, the controls using the GE transport functions and converting them into ordinary differential equations, all the safety systems. That is, we don't need to impose boundary conditions on the system.

Then on the safety systems, scram trips and so on, I don't need to go through this. We have the containment with the dry and wet walls, in which we have the nitrogen and water vapor atmosphere with condensation. All of that is simulated.

Then most of the failures in components and systems we introduce online interactively from the keyboard.

[Slide.]

MR. WULFF: The solution method is, again, implicit integration for the neutron ordinary differential equation, after all, fine kinetics, one ordinary differential equation. There is explicit integration for the rest. I won't go through this, but I want to point out that it's built-in standard textbook method, Adams-Bashford, and some First Order Euler and so on.

Again, quadratures in space, trapezoidal and Simpson's rule are used for that. There is no linearization in space, no linearization of expressions. The computation errors are again from numerical diffusion in two equations that we mentioned before, and then from quadrature in space, truncation errors from ODE and covariance terms. That means we are saying that the function average of a volume is equal to that function in terms of its average argument. So we have here an approximation.

We have made estimates analytically for these errors for all them and the numerical diffusion is overwhelming the others. We control it, as we did before, by running with the maximum possible courant number.

The key is, however, that whatever we find out about the numerics in RAMONA or in HIPA, applies to the other code, too, as far as thermohydraulics is concerned.

[Slide.]

MR. WULFF: We have done some modifications as part of this instability analysis. We needed the average power once we get into large observations. We didn't really want to make estimates, so we introduced an integrator with a circular buffer to give us the average of the last minute -- the one minute sampling rate.

We introduced multistepping for kinetics, and Gerry has talked about that. We are not using the advanced or delayed values. Instead, we interpolate during each of the substeps from the values that we have from the last calculation. So we have considerable reduction by, on the one hand, integrating the flow with a time step controlled by courant number to minimize diffusion, and on the other hand, neutron kinetics to compute with the minimal truncation error.

We had to introduce not only the interpolation for the total reactivity and multistepping for the kinetic, but also, since we are coming very close to prompt or exceeding prompt critical conditions, we had to capture the Doppler feedback and had to calculate the thermal conduction on the level of the neutron kinetics with time response.

The flow coupling to the fuel is still the gap conductant and has similar response with the Q-triple prime from the kinetic influences. The result of these changes that we made recently is that with more careful calculations, the power peaks that used to be 28 or so times normal power, were reduced by 25 percent, but at the same time, because of a broadening of the spikes -- and I will explain that -- we had an increase in 53 percent of the mean power for the case that I will explain later where the scram failure is imposed and the control system is allowed to maintain inventory, which means a higher flow of feedwater into the core.

As far as the developmental assessment is concerned, we have this reference report, which consists of a number of comparisons we do with this model. Before we implemented on the AD10 on the special computer, we compared it with two transcripts that we have from GE and we compared it with all of the MSI calculations in RAMONA.

More related to the LaSalle interactions are the recirculated pump test. We did this before LaSalle, and then we used the LaSalle event up to the scram to see how well the plant analyzer simulates the stability.

[Slide.]

MR. WULFF: These are the kinds of errors we obtained to the pump test, core flow for power, extreme flow and pressure, and collapsed liquid level in terms of the initial value and of the total span that was calculated between 10 and 1 percent variation.

[Slide.]

MR. WULFF: When we compare with LaSalle, we have this bottom line that if we calculate with our best-estimate parameters, as we obtain them from GE for all of the neutronics data for particularly the thermohydraulics that are important for instability and they are input impedance and output impedance imposed to the feedwater measurement from LaSalle and you have some problems with that, and I think it was discussed in part this morning, that we do get some oscillations, but very small ones. So, they don't lead to scram.

In the second case, if we use the same best-estimate calculation and allow the feedwater control system to do this assignment -- that is, maintain inventory, maintain constant level, then we get oscillations and scram.

Alternatively, if we use the feedwater flow as imposed from STARTREC and introduce one-half for the void reactivity for the last coefficient at the exit, then we get oscillation to the scram.

These are reactions.

Now, we have some problems. We don't really know what the decode regulator did. There is an outstanding question.

[Slide.]

MR. WULFF: I have a number of comparisons.

MR. WARD: Wait a minute, Wolfgang. What do you mean by using uncertainties at the one-half signal level for those things? Are they all in the same direction?

MR. WULFF: There are basically three of the ones that I mentioned here, and they are in the directions of destabilizing. Some are positive and some are negative.

MR. WARD: Okay. But this was one set of uncertainties added all in the destabilizing direction.

MR. WULFF: Yes.

MR. WARD: Okay.

MR. WULFF: Or we could use the 30-percent uncertainty of void reactivity and that will lead to scram. That is, of course, going to one signal in void reactivity.

[Slide.]

These are the kind of comparisons which we have. As mentioned earlier, we have every limit on the data point. What we don't know is whether there are any oscillations in between, and what you see here is the core flow.

I think I will skip the others until I come to the ones that we have discussed and that should be with the power.

[Slide.]

MR. WULFF: You notice that the calculated power is somewhat higher, particularly in this region here, and the reason for that is that the peak load is somewhat higher. The reason is really not the temperature that is met, as you see, in this slide.

[Slide.]

MR. WULFF: The feedwater temperature that is caused by failure of the feedwater pre-heater, but it is because of the flow rate.

[Slide.]

MR. WULFF: That is shown on this slide. The regulator for feedwater adjusts to the new level within less than a minute, whereas the measurement, we don't know whether there was some oscillation, but the measurement seemed to come from -- and I think it was shown this morning that there is a smooth, continuous reduction in feedwater flow. This is the mass flow rate.

We then maintain this on a slight increase. This is where the trip occurs, and the measurements are here.

Now, we have done several variations. Since we don't know what and how the regulation or the valve failed, we closed this flow, and then we get the three answers that I gave, that in a case where we impose this, the first thing is that the main steam isolation valve did not trip at LaSalle, either because its initial level and the downcomer was above the level that we think it had or its trip-set point is lower than we think it ought to be.

If we then suppress -- yes?

MR. LEE: In GE's simulation of the LaSalle event, they accounted for the 30-second period of oscillation in feedwater flow and temperature.

MR. WULFF: Yes.

MR. LEE: Did you consider that, also, in your --

MR. WULFF: We have some oscillations here. We would have to really zoom on a larger scale. You see, the oscillations, but we don't see the high-frequency oscillations that we saw this morning, because the oscillations we see in the level inside the core but not in the feedwater.

MR. LEE: I was curious if this 30-second feedwater oscillation could destabilize your system.

MR. WULFF: It is the reactivity absorption that destabilizes. I don't think it is the flow, the long-period oscillation that sets up the thermohydraulic power and flow oscillation.

Now the first problem is that if we place the steam flow over it, then you see that the plant analyzer maintaining inventory matches the two, except for some changes in void fraction and in level, but in the plant, the steam flow is always below, meaning that there should be a gain in inventory, and when we look at the level as obtained from STARTREC, we find that it drops. But we have some problems that need to be resolved with a mismatch between steam flow, peak water flow, and level.

[Slide.]

MR. WULFF: This is the typical LaSalle calculation for power that we obtained, and it scrams here at about seven and a half minutes. The top flow is the power spans here at 118 percent, and the bottom is the flow, and if you presume that you see all of the detailed oscillations, the void reactivity is shown in the bottom draft here, and you see that we are far from critical.

[Slide.]

MR. WULFF: Also I should say that minimum critical power ratio was not coming close to the limit, and another indication of how the plant analyzer reproduces the TRAC conditions is shown here. Here it was a circle, and then I will explain later, but we reproduced 100 percent broad line, and the 80 percent broad line and the natural circulation which is from the plenum.

Now what we you see here is our stability boundary. We reduced the flow and then withdrew control rods until we achieved oscillations. You will see here the sequence of points, and at this point if we keep the feedwater flow constant to this point, which is now somewhat higher, if we allow the inventory to be maintained.

With this, I think I will stop and go to the objectives.

[Slide.]

MR. WULFF: We have answered most of these questions in the first cut, certainly. What are the causes of large amplitude oscillations and thermal hydraulic enhanced by kinetic feedback. What are the inherent limits, if any, on the amplitude of power and fuel temperature oscillations? And we have answered that, too.

Can corewide power flow oscillations occur during any type of ATWS, scram failure, and the answer is yes. What are the amplitudes of fuel pellet and cladding temperatures during such an ATWS, and we have answers for that also.

Can safety limits of minimum critical power ratio, MCPR, equal 1.05 be violated? And the answer is not if scram occurs; but, yes, if scram fails. How do the time rates of suppression pool temperature and containment atmosphere rise? The answer is almost twice as fast as we have oscillations, and have to dump steam.

That is, if you have MSIV open and turbine trip, some flow goes into the bypass, but the rest has to go into this suppression pool. Then you will have twice the rate of suppression pool temperature increase that you would have if there were no oscillations.

MR. CATTON: Is there some of the rest of this that you could skip?

MR. WULFF: I think you can read our EPA results to date.

MR. CATTON: I would like to hear about the EPA limitations.

MR. WULFF: All right.

MR. WARD: Wait. Could I ask, are these EPA results presented somewhere else? Are they in this -- were they presented at the stability symposium, for example?

MR. WULFF: No. We have drafted a report, if you ask about where are the 60 or so transient documents, we have a report. Most of these are in the Chapter 4 of that report that has been drafted, and the first draft was given to the NRC.

The report ought to -- well, it is scheduled to come out in the first three months of next year.

MR. CATTON: Are these --

MR. WULFF: But I have viewgraphs on the results. And depending upon the time that we want to spend to discuss them here. I had some results on the power oscillations, the difference that we have with GE calculations is that our plant analyzer produces power peaks up to 20 times normal power when scram fails, and I think maybe we should spend --

MR. WARD: Ivan, I don't know if this is the appropriate time, but we have been hearing all day about the tools. Some time we need to start hearing about --

MR. CATTON: Hearing about results?

MR. WARD: -- hearing about what's important, you know.

MR. CATTON: Okay.

MR. WULFF: I think we have certainly more results than we can present here in one afternoon. The problem is we were asked to describe the tools, and to show its numerics or discuss its numerics, discuss its limitations, and so on.

If you would like me to discuss results, I am certainly happy, but here is one of the results --

MR. WARD: We are getting all prepared for the future meeting. Are we going to have it before then?

MR. CATTON: Well, I would hope so.

MR. WULFF: My proposal is that we present to you the report. If you feel after that that we should have a meeting, I would be happy to present it and answer any questions about it.

[Slide.]

MR. WULFF: But this is the kind of power oscillations that you get.

There are two questions here:

The first, how well do we calculate reactivity, which is shown here? Total reactivity? And its mean is about minus $7. But you notice that we are getting to prompt critical. In fact, some of these peaks that we see here above this number one line show $1.12 criticality.

The question is, do we for a given reactivity calculate the power right, and two, do we calculate the reactivity right?

[Slide.]

MR. WULFF: And these questions we answered in two ways.

I am skipping the rest of the results.

You actually have three transients. One is where we allow the feedwater regulator to maintain inventory, and that produces the largest oscillations that we have, tremendous influence from the feedwater flow rate, along with the drop in temperature.

In fact, one of our key conclusions is that three things have to happen for LaSalle:

One is reactivity insertion from the feedwater; two is flow reduction from the trip of the recirculation pumps, and three is sharp power peaking near the bottom of the core. If you remove any of these three, our plant analyzer doesn't show instability.

[Slide.]

MR. WULFF: This is the shape of the power. You realize even though it is very high in peaks, the energy content of the spike is rather limited. What we have shown on this diagram is a calculation of the solid line with AD 100, a 64-bit machine that we have calibrated with the exact solution to produce for sinesoidal reactivity variations in time the solution within 1 percent or better.

The open circles are with the AD-10 as they are calculated in the simulation you saw earlier, which is now simply a zoom over a few seconds where before you saw tens of minutes on the diagram.

This is the shape of the power we calculated. Our question is, if we calculate for given reactivity -- incidentally, for this we have had reactivity that is calculated with 7 millisecond time intervals from EPA into the AD-10, and then got the power solution.

The question is, do we calculate the reactivity right, and I don't think we have calculational errors. We have uncertainties in the reactivity coefficients. That is the major question in these peaks here.

Some -- I think, KWU had power peaks at 25. Most people claim it ought to be around 7. We have uncertainty, and we agreed to that.

Let's skip the following viewgraphs and go to the limitations.

[Slide.]

MR. WULFF: So, you see, what we are skipping is really results. By the way, this is maybe not in your viewgraph. This is the calculation you get if you change the reactivity by 30 percent, and impose the same feedwater flow as it was imposed, or as it was measured from the start of the output, and you get very early here an unreal oscillation. And then you get these peaks here. One of them could have tripped the scram system. But the mean power is very much the same as GE calculations show.

MR. STIRN: Is that where the 30 percent uncertainty comes from?

MR. WULFF: There is a BNL report, I think that Dave Diamond was the author of that, and he specified for void reactivity 50 percent. I can only say that much.

MR. STIRN: 50 percent? You cannot do it within 50 percent?

MR. WULFF: I am not saying that. I think it may be a 2 sigma boundary.

MR. STIRN: I think like 100, I would guess.

MR. WULFF: I am not a neutron kinetic specialist. I think -- there is some uncertainty. I don't think it is that high, and I don't think this result has significance except to show the sensitivity.

I don't think that the initial oscillations are real, because they don't reflect LaSalle. Now the limitations are here, that we have point kinetics, that the axial shape form has to be known. In our case, we had the LaSalle data, and we actually imposed a transient distortion of the initial power shape, because we knew it.

Before we knew it, we used the initial steady state, but we got the scram conditions and the oscillation, the same as we had later.

The radial power shape we cannot simulate, we have to use the peaking factor for radial peaking.

The other problem is that we only have one dimensional core flow, so the plant analyzer is limited to in-phase corewide oscillations.

We have no model for superheat, superheated vapor, no tracking for the boiling boundary.

I mentioned earlier that we calculated the location of the boiling boundary in LaSalle during all of the oscillations prior to scram, and their motion was between 1 and 7 centimeters from the entrance of the core, and the fuel conduction model is actually limited to thermally thick fuel cells. And that depends on the time rates of change, of the time it takes the thermal boundary layer to penetrate through the cylinder, and we may be pushing the limit here also.

The integral method is described in the report.

I think the most worrisome uncertainties that is a part of this is we have to deal with void reactivity uncertainty. I'm not really supporting this 50 percent, but there is uncertainty here, and what I would like to point out is that for every percent in uncertainty of void reactivity, we get 15 percent in uncertainty of the peak power. We made a sensitivity study on that when the reactivity increases, and 12 percent when it decreases.

So it is an amplification of either 15 or 12, depending upon whether it's up or down, in the vicinity of our current calculations.

This may not be linear, but certainly that is the sensitivity. The loss coefficient for two-phase flow, and particularly the exit, there is an uncertainty of 30 percent, and the fuel clad gap conductance, if it is the same as in PWR, it is largely to be this order of magnitude, which is 45. And we have made or March-Leuba has made comparison with the frequency domain code. After all, it is a time domain code. It has 20 percent differences in the decay ratio.

We also expect to have about 20 percent, as I showed, for RAMONA with the same thermal hydraulics, and 20 percent uncertainty in the gain.

So this is, in summary, limitations for instability calculations of the plant analyzer. Unless you have some questions --

[Slide.]

MR. WULFF: -- we have done most of the things we were assigned to do with the plant analyzer, and I think, as I said, there are 60 different transients documented, and we may have carried out three times as many, which is a different type of calculation, where you answer "what if" questions, which is four times faster than real time. And you don't have to wait a day to get one minute of calculations as in RAMONA.

So one really has to experience this to believe the power of this kind of simulation. We are supposed to provide support for the BWR stability analysis. This was referred to by Gary Wilson.

There may also be additional transients requested by NRR, and we have to complete our computation on error analysis. That is nearly completed, and then our document that we have drafted, we must revise in light of the most recent calculations.

I think that will complete what I had to say.

Is it more than 20 minutes?

[Laughter.]

MR. CATTON: It was 35 minutes. It's only a factor of two.

MR. WULFF: Any questions that you would like me to answer?

MR. CATTON: I don't see any.

What I would like to do is postpone what Dave has to tell us until tomorrow.

Thank you. We will meet here tomorrow morning at 8:30.

[Whereupon, at 6:20 p.m., the subcommittee was recessed, to reconvene at 8:30 a.m., Thursday, November 9.

1989.]

REPORTER'S CERTIFICATE

This is to certify that the attached proceedings before the United States Nuclear Regulatory Commission

in the matter of:

NAME OF PROCEEDING: ACRS Thermal Hydraulic Phenomena Subcommittee

DOCKET NUMBER:

PLACE OF PROCEEDING: San Francisco, CA

were held as herein appears, and that this is the original transcript thereof for the file of the United States Nuclear Regulatory Commission taken by me and thereafter reduced to typewriting by me or under the direction of the court reporting company, and that the transcript is a true and accurate record of the foregoing proceedings.

Dean A. Robinson

Dean A. Robinson

Official Reporter
Ann Riley & Associates, Ltd.

GE PRESENTATION AGENDA

(NON-PROPRIETARY SESSION)

o	INTRODUCTION - TRACG Historical Development - Application	BS Shiralkar
o	TRACG MODELS SIGNIFICANT TO STABILITY - IF Shear } Different from TRAC-P - Subcooled Boiling } Different from TRAC-P - Numerical Methods } Different from TRAC-BD1 - Kinetics } Different from TRAC-BD1	JGM Andersen/ JC Shaug
o	SUMMARY OF PREVIOUS TRACG QUALIFICATION	JGM Andersen
o	QUALIFICATION FOR BWR STABILITY ANALYSIS	
	- Numerical Damping	JGM Andersen
	- Thermal Hydraulic Stability	JC Shaug
	- Transient MCPR	JC Shaug
o	PLANT DATA DESCRIPTION - Leibstadt - LaSalle 2	GA Watford
o	PLANT STABILITY QUALIFICATION - Leibstadt - LaSalle 2	JC Shaug
o	SUMMARY - Capabilities/Limitations/Future Plans	BS Shiralkar

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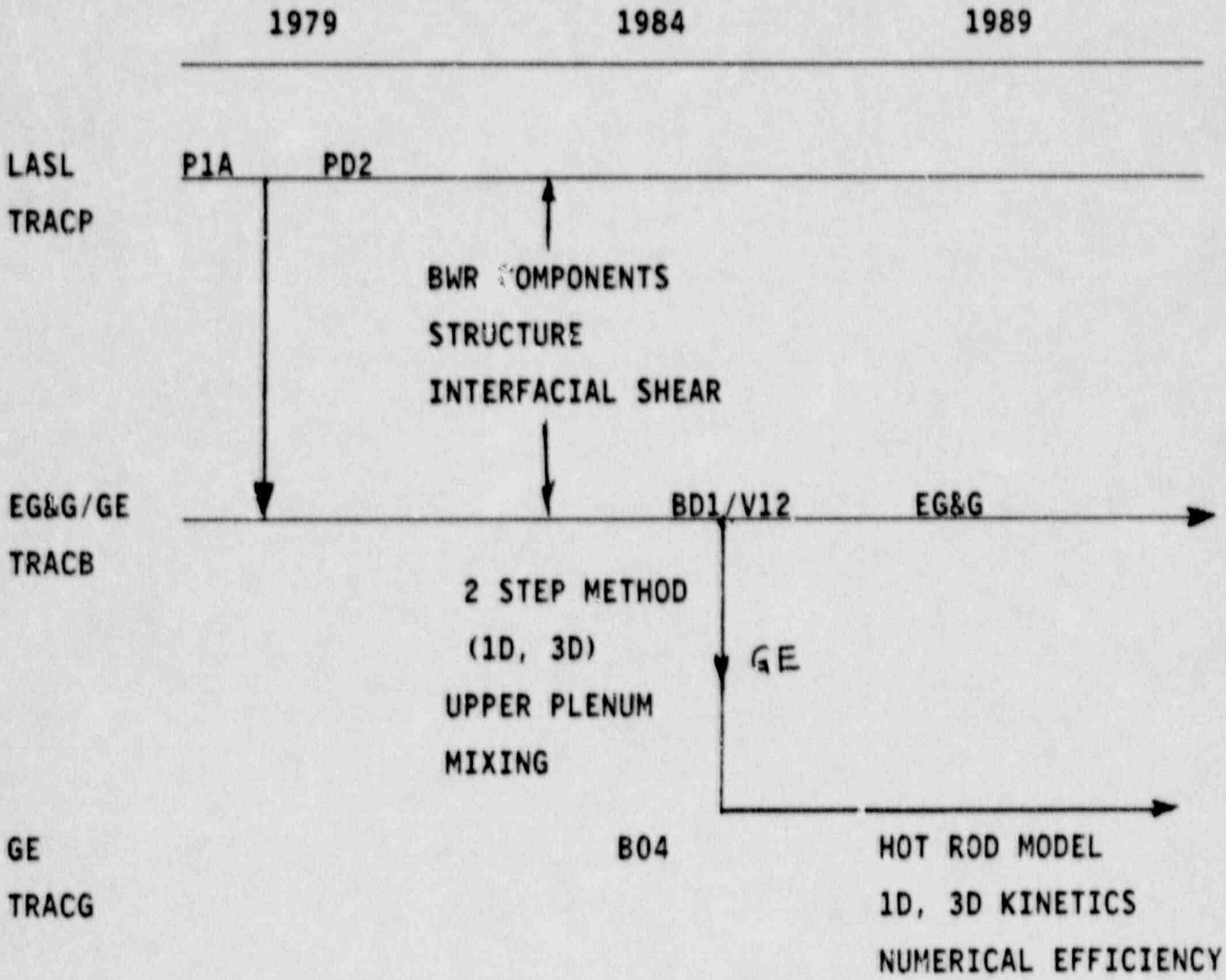
TRACG HISTORICAL DEVELOPMENT



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TRACG APPLICATIONS

- GENERAL BWR TRANSIENT ANALYSIS TOOL
  - Coupled Thermal Hydraulics And Kinetics
  - Control Systems
  - BOP Components

- APPLICATIONS
  - LOCA
  - Operational Transients
  - ATWS
  - Stability.

  Operating BWRs

  Advanced BWRs

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STABILITY ANALYSIS

OBJECTIVES/APPLICATIONS

- o DEVELOPMENT OF INCREASED UNDERSTANDING OF PHENOMENA PARTICULARLY FOR OUT-OF-PHASE REGIONAL OSCILLATIONS

- o DEMONSTRATION OF COMPLIANCE WITH GDC-12

    - Prediction Of Onset Of Oscillations
    - Allowable Amplitude Of Oscillations
    - Attenuation Of Localized Oscillations (Detectability)

- o QUANTIFICATION OF EFFECTS OF DESIGN CHANGES

    - Fuel Designs
    - Operating Strategies.

CHOICE OF TRACG FOR STABILITY ANALYSIS

- o FEATURES RELEVANT TO STABILITY ANALYSIS
  - INTERFACIAL SHEAR MODEL EXTENSIVELY QUALIFIED FOR BWR VOID FRACTION PREDICTIONS
  - 3D NEUTRON KINETICS MODEL CONSISTENT WITH GE DESIGN CODES
  - VERSATILE NUMERICAL SCHEME FOR EXPLICIT OR IMPLICIT INTEGRATION
  - MODULAR STRUCTURE CAN REPRESENT COMPONETS, FACILITIES, PLANTS
  - POSSIBILITY OF EXPLORING MULTI-DIMENSIONAL EFFECTS
- o EXTENSIVE QUALIFICATION
  - PREVIOUS QUALIFICATION OF THERMAL HYDRAULICS/KINETIC MODELS
  - STABILITY SPECIFIC STUDIES

TRACG CHOSEN BY GE AS BEST AVAILABLE CODE

TRACG MODELS SIGNIFICANT TO STABILITY

BASIC MODELS:

- Interfacial Shear
- Subcooled Boiling

NUMERICAL METHOD

KINETICS

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TRACG HYDRAULIC MODEL

Gas mass:

$$\frac{\partial}{\partial t}(\alpha\rho_g) + \nabla \cdot (\alpha\rho_g \bar{V}_g) = \Gamma_g + M_g \qquad (2\text{-}2)$$

Mixture mass:

$$\frac{\partial}{\partial t}((1-\alpha)\rho_\ell + \alpha\rho_g) + \nabla \cdot ((1-\alpha)\rho_\ell \bar{V}_\ell + \alpha\rho_g \bar{V}_g) = M_m \qquad (2\text{-}3)$$

Air mass:

$$\frac{\partial}{\partial t}(\alpha\rho_a) + \nabla \cdot (\alpha\rho_a \bar{V}_g) = M_a \qquad (2\text{-}4)$$

Gas momentum:

$$\frac{\partial}{\partial t}\bar{V}_g + \bar{V}_g \cdot \nabla \bar{V}_g + \frac{k\rho_c}{\alpha\rho_g}\left[\frac{\partial}{\partial t}\bar{V}_R + \bar{V}_d \cdot \nabla \bar{V}_R\right] = -\frac{1}{\rho_g}\nabla P - \bar{g} - \frac{1}{\alpha\rho_g} f_{\ell g} - \frac{1}{\rho_g} F_w + B_g \qquad (2\text{-}5)$$

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Liquid momentum:

$$\frac{\partial}{\partial t}(\bar{V}_\ell) + \bar{V}_\ell \cdot \nabla \bar{V}_\ell - \frac{k\rho_c}{(1-\alpha)\rho_\ell}\left[\frac{\partial}{\partial t}\bar{V}_R + \bar{V}_d \cdot \nabla \bar{V}_R\right] =$$

(2-6)

$$-\frac{1}{\rho_\ell}\nabla P - \bar{g} + \frac{1}{(1-\alpha)\rho_\ell}\,\Gamma_{\ell g} - \frac{1}{\rho_\ell}F_w + B_\ell$$

where $\bar{V}_R = \bar{V}_g - \bar{V}_\ell$ (2-7)

Gas energy:

$$\frac{\partial}{\partial t}(\alpha\rho_g e_g) + \nabla \cdot (\alpha\rho_g e_g \bar{V}_g) + P\left[\frac{\partial\alpha}{\partial t} + \nabla \cdot (\alpha \bar{V}_g)\right] =$$

(2-8)

$$q_{ig} + \Gamma_g h_{gs} + q_{wg} + E_g \quad .$$

Mixture energy:

$$\frac{\partial}{\partial t}((1-\alpha)\rho_\ell e_\ell + \alpha\rho_g e_g) + \nabla \cdot ((1-\alpha)\rho_\ell e_\ell \bar{V}_\ell + \alpha\rho_g e_g \bar{V}_g)$$

(2-9)

$$+ P\nabla \cdot ((1-\alpha)\bar{V}_\ell + \alpha \bar{V}_g) = q_{w\ell} + q_{wg} + E_m$$

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MAJOR FLOW REGIMES

WALL CONDITION

VOID FRACTION	WETTED	DRY
1.0		SINGLE PHASE VAPOR
$\alpha_E < \alpha < 1$	DISPERSED ANNULAR	DROPLET
$0 < \alpha < \alpha_E$	BUBBLY/ CHURN	INVERTED ANNULAR
0	SINGLE PHASE LIQUID	

: INTERCEPT BETWEEN CHURN AND DISPERSED ANNULAR FLOW

WALL CONDITION: : BOILING TRANSITION

- CISE-GE BOILING LENGTH
- ZUBER POOL BOILING

ENTRAINMENT : ISHII

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BASIC ASSUMPTIONS OF MODEL

- FOR ADIABATIC AND STEADY-STATE CONDITIONS THE TWO-FLUID MODEL AND THE DRIFT FLUX MODEL ARE EQUIVALENT.

  THE DRIFT FLUX PARAMETERS CAN BE USED TO CHARACTERIZE THE RELATIVE VELOCITY AND FLOW DISTRIBUTION.

- THE CORRELATIONS FOR THE INTERFACIAL SHEAR AND DRAG, AS DERIVED FROM ADIABATIC STEADY STATE CONDITIONS, ARE APPLICABLE TO TRANSIENT CONDITIONS.

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<u>INTERFACIAL DRAG</u>

$$f_{lg} = \frac{1}{A}\int M_g \, dA = C|\bar{V}_R|\bar{V}_R$$

$$\bar{V}_R \neq \bar{V}_g - \bar{V}_l$$

- NO ACCELLERATION

$$f_{lg} = C|\bar{V}_R|\bar{V}_R = \langle\alpha\rangle\langle 1-\alpha\rangle \Delta\rho g$$

FROM DRIFT FLUX MODEL

$$\bar{V}_R = \frac{1 - \alpha C_0}{1 - \alpha} \bar{V}_g - C_0 \bar{V}_l$$

$$\bar{f}_{i_g} = \bar{C}_i \left| \frac{1 - \alpha C_0}{1 - \alpha} \bar{V}_g - \bar{V}_l C_0 \right| \left(\frac{1 - \alpha C_0}{1 - \alpha} \bar{V}_g - C_0 \bar{V}_l \right)$$

IT IS CONVENIENT TO INTRODUCE:

$$C_i = \frac{1}{8} \frac{C_0}{d_i} \rho_c$$

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FLOW REGIMES AND CONSTITUTIVE CORRELATIONS

BASED ON M. ISHII'S RECOMMENDATIONS

BUBBLY/CHURN FLOW

$$f_{i\ell} = \frac{1}{8}\frac{C_D}{d_i}\rho_\ell \bar{V}_R^2 = \alpha(1-\alpha)\Delta\rho g$$

INTERFACE AREA IS GIVEN BY CRITICAL WEBER NUMBER

$$\frac{1}{d_i} = 6\alpha\frac{\rho_\ell \bar{V}_R^2}{\sigma We_c}$$

$$\bar{V}_R = \frac{1.4}{1-\alpha}\left\{\frac{\Delta\rho g \sigma}{\rho_\ell^2}\right\}^{0.25} \quad \text{(ISHII)}$$

$$\frac{C_D}{We_c} = \frac{1}{3}(1-\alpha)^5$$

DISTRIBUTION PARAMETER

$$C_0 = C_\infty - (C_\infty - 1)\sqrt{\frac{\rho_g}{\rho_\ell}} \quad \text{(ISHII)}$$

$$C_\infty = 1.393 - 0.015\ln(Re) \quad \text{(NIKURADSE)}$$

FLO[illegible]IME	$\bar{V}_{gj}$	$\frac{1}{d_i}$	ρ_c	C_D	
BUBBLY/CHURN	$1.4\left[\frac{\Delta\rho g\sigma}{\rho_l^2}\right]^{\frac{1}{4}}$	$6\alpha\,\frac{\rho_l V_r^2}{\sigma We_c}$	ρ_l	$\frac{1}{3}(1-\alpha)^5 We_c$	$C_0+(C_0-1)\sqrt{\frac{\rho_g}{\rho_l}}$ $C_0=1.393-0.0015\ln(Re)$
ANNULAR	$\frac{(1-\alpha)^{\frac{3}{2}}}{\alpha+a}\sqrt{\frac{\Delta\rho g D_h}{0.015\rho_l}}$ $a=\sqrt{\frac{(1+75(1-\alpha))\rho_g}{\sqrt{\alpha}\,\rho_l}}$	$\frac{4}{D_h}\sqrt{\alpha}$	ρ_l	$0.03\sqrt{\alpha}(\alpha+a)^2$	$1+\frac{1-\alpha}{\alpha+a}$
DROPLET	$1.4(1-\alpha)\left[\frac{\Delta\rho g\sigma}{\rho_g^2}\right]^{\frac{1}{4}}$	$6(1-\alpha)\frac{\rho_g V_r^2}{\sigma We_c}$	ρ_g	$\frac{1}{3}\alpha We_c$	1
DROPLET HIGH VELOCITY	$\frac{3}{2}d_i(1-\alpha)^2\left[\frac{(\Delta\rho g)^2}{\mu_g\rho_g}\right]^{\frac{1}{3}}$	$6(1-\alpha)\frac{\rho_g j^2}{We_c}$	ρ_g	$\frac{32}{2}\alpha\,Re_d^{-\frac{1}{2}}$	1
CCFL	SAME AS ABOVE	SAME AS ABOVE	SAME AS ABOVE	SAME AS ABOVE	CALCULATED SO THAT CONSTANT VOID FRACTION LINES ARE TANGENT TO CCFL CORRELATION $\sqrt{K_g}+m\sqrt{-K_l}=\sqrt{C}$

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TRACG MODELS

SUBCOOLED BOILING

$$C_{0,s} = C_0 * \frac{h_l - h_{ld}}{h_f - h_{ld}}$$

Saha-Zuber Model

$$h_f - h_{ld} = \begin{cases} 0.0022 \dfrac{q_w D_h c_{pl}}{k_i} & Pe < 70000 \\ 154 \dfrac{q_w}{\rho_l V_l} & Pe > 70000 \end{cases}$$

11-04-1989

TRACG MODELS

SUBCOOLED BOILING

$$q_w = q_l + q_{evap}$$

Rouhani-Bowring Model

$$q_l = \begin{cases} q_w & h_l < h_{ld} \\ q_w * \left\{ \dfrac{h_f - h_l}{h_f - h_{ld}} + \left(1 - \dfrac{h_f - h_l}{h_f - h_{ld}}\right) \dfrac{e}{1-e} \right\} & \end{cases}$$

$$e = \frac{\rho_l \, (h_f - h_l)}{\rho_g \, h_{fg}}$$

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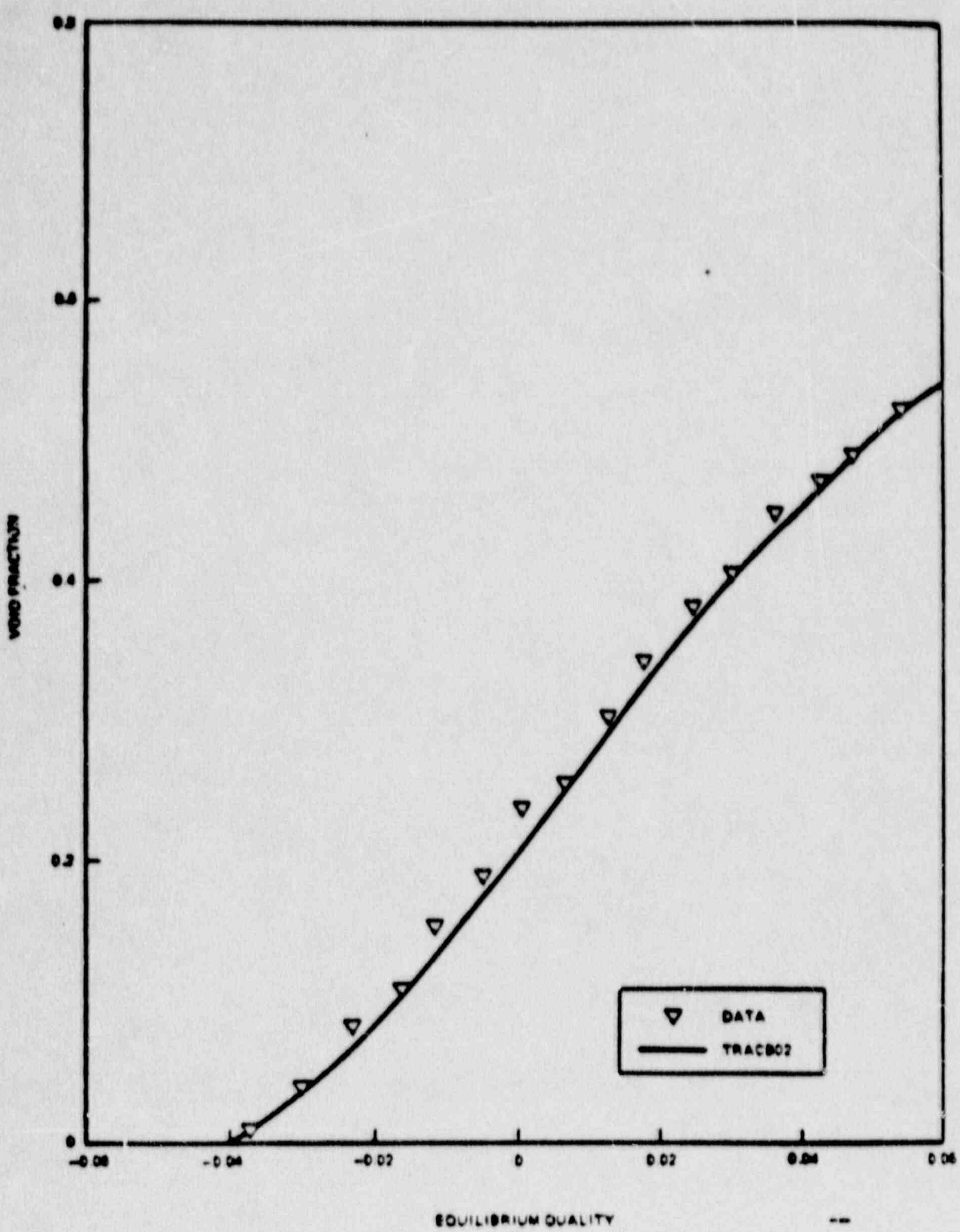

Figure 6.5.1. Comparison of Christensen Test With TRAC, P = 5.5 MPa

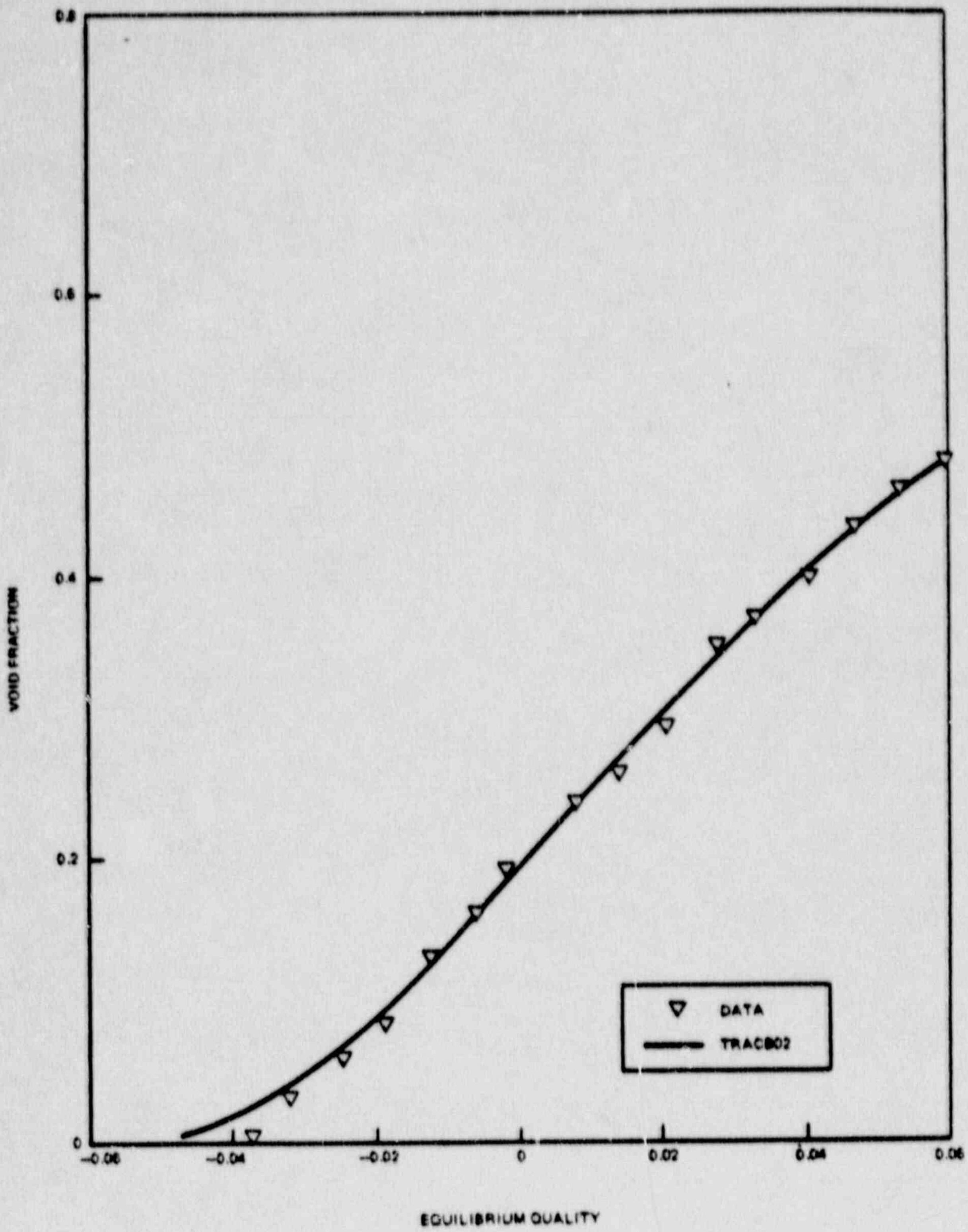

Figure 6.5.2. Comparison of Christensen Test With TRAC, P = 6.9 MPa

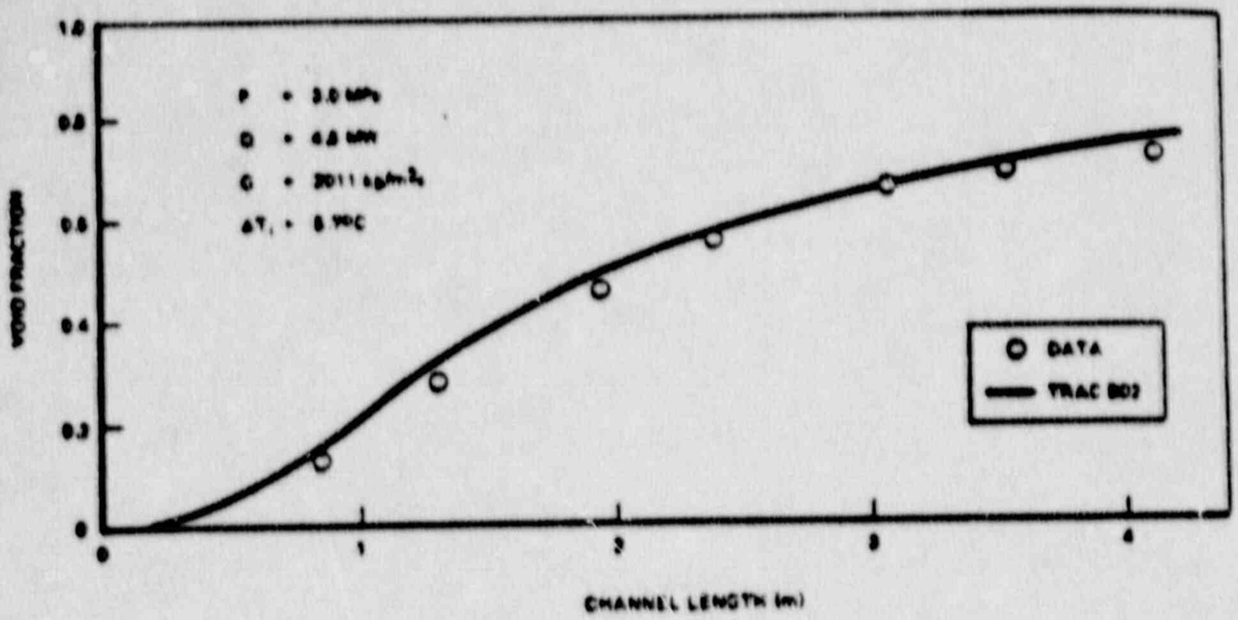

Figure 6.1.3. Comparison of FRIGG Results with TRAC (FRIGG 613123)

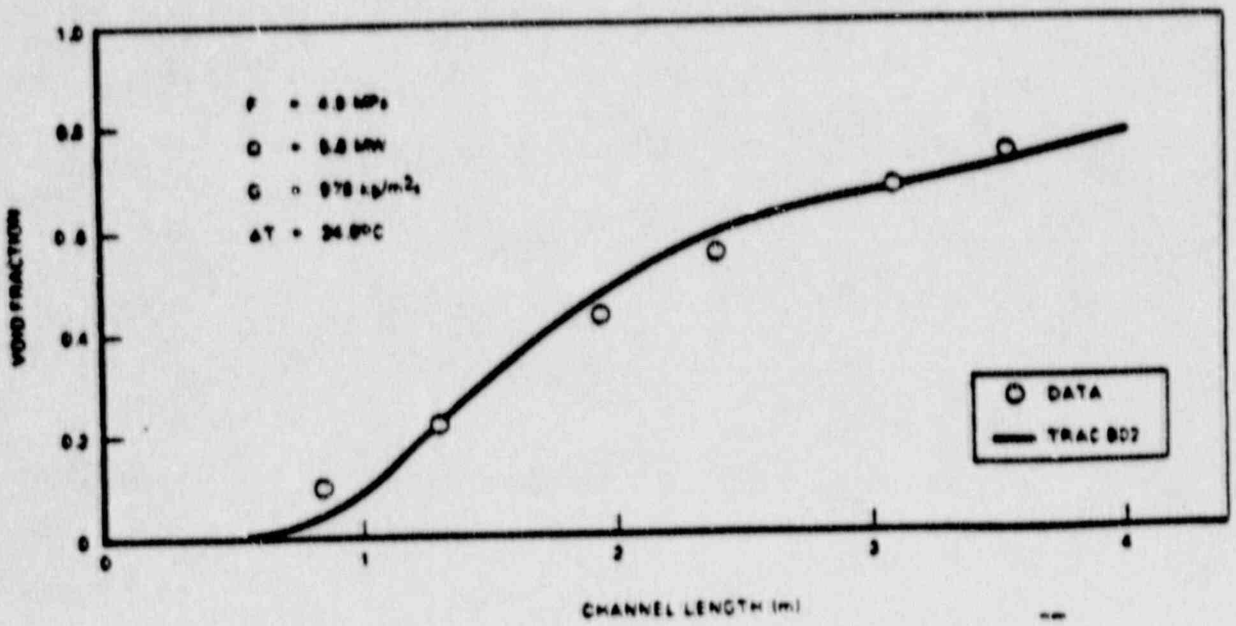

Figure 6.1.4. Comparison of FRIGG Results with TRAC (FRIGG 613014)

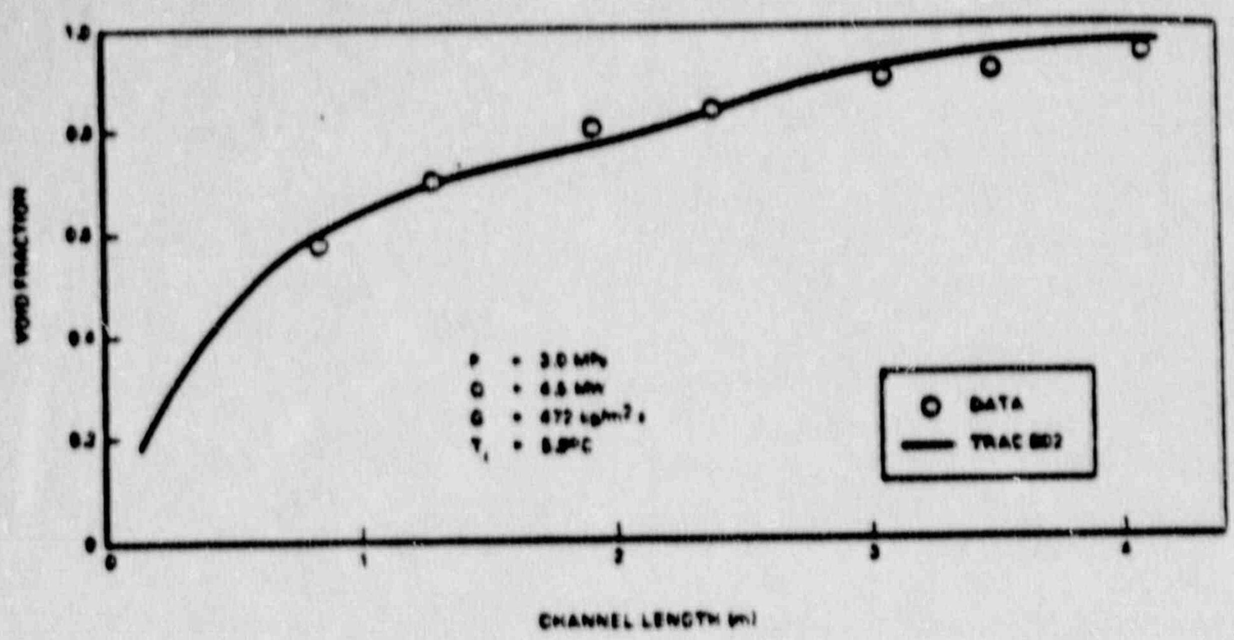

Figure 6.1.1. Comparison of FRIGG Results with TRAC (FRIGG 613124)

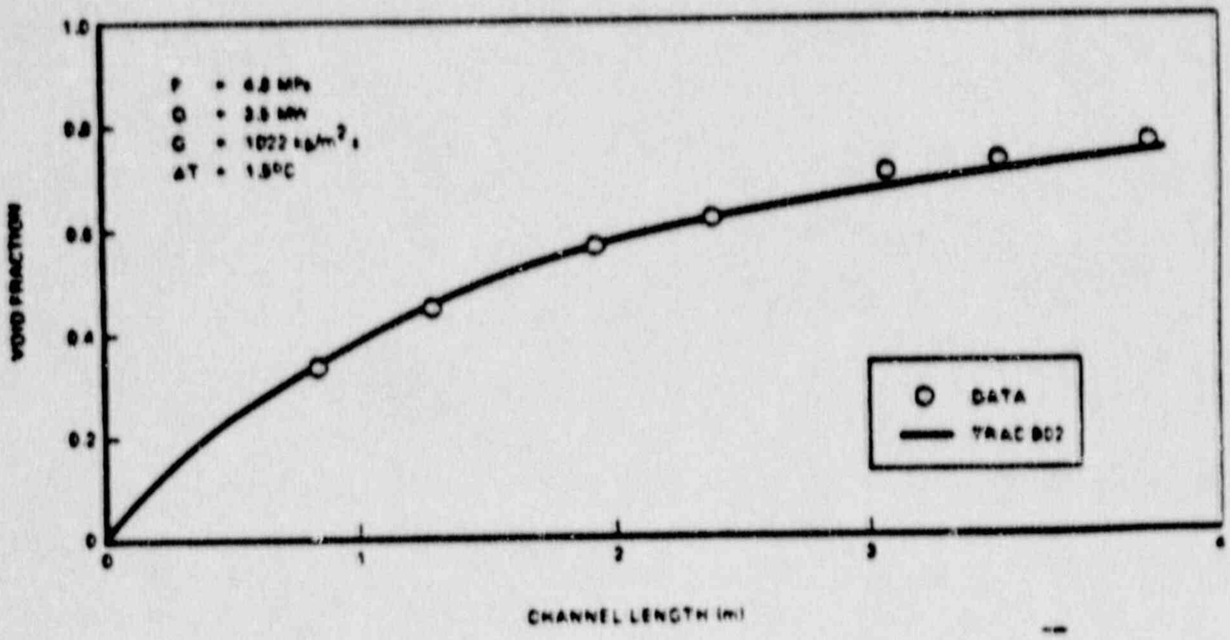

Figure 6.1.2. Comparison of FRIGG Results with TRAC (FRIGG 613005)

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TRACG MODELS SIGNIFICANT TO STABILITY

BASIC MODELS:

- Interfacial Shear
- Subcooled Boiling

VOID FRACTION IS ACCURATELY PREDICTED BY TRACG

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TRACG NUMERICS HISTORY

VERSION	NUMERICAL METHOD	ACCURARY	STABILITY	DATE
TRACB01	Semi Implicit Momentum Explicit Mass and Energy	1. Order	$\Delta t < \frac{\Delta X}{\lvert V \rvert}$	1981
TRACB03	2-Step Method for 1D Components	1. Order	$\Delta t < \frac{\Delta X}{\lvert V \rvert}$ (3D only)	1982
TRACB04	2-Step Method for 1D and 3D Components	1. Order	No Limit	1985
TRACG	Fully implicit of Mass and Energy for 1D and 3D Components	1. Order	No Limit	1987
TRACG	Experimental Second Order 1D Central Difference Method	2. Order	No Limit	1989

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TRACG NUMERICS

SOLUTION OF CONSERVATION EQUATIONS FOR

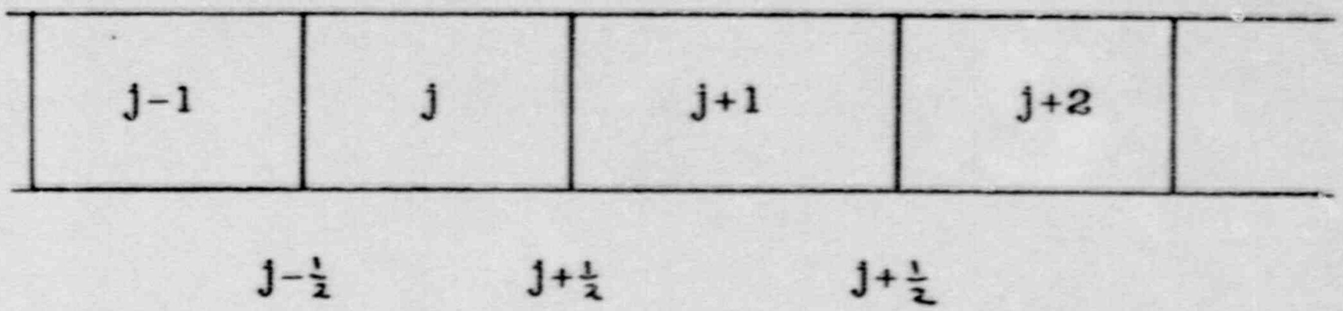
MASS, MOMENTUM AND ENERGY

MOMENTUM EQUATION: SEMI-IMPLICIT SOLUTION

MASS AND ENERGY EQUATIONS: EXPLICIT

IMPLICIT

TRACG NUMERICS. MOMENTUM EQUATION (VAPOR)



$$\frac{V^{n+1}_{j+\frac{1}{2}} - V^{n}_{j+\frac{1}{2}}}{\Delta t} = V^{n}_{j+\frac{1}{2}} \begin{cases} \dfrac{V^{n+1}_{j+\frac{1}{2}} - V^{n}_{j-\frac{1}{2}}}{\Delta X_{j}} & , \text{ for } V^{n}_{j+\frac{1}{2}} > 0 \\ \dfrac{V^{n}_{j+\frac{3}{2}} - V^{n+1}_{j+\frac{1}{2}}}{\Delta X_{j-1}} & \end{cases}$$

$$+ \frac{1}{\rho^{n}_{g,j+\frac{1}{2}}} \frac{P^{n+1}_{j} - P^{n+1}_{j+1}}{\Delta X_{j+\frac{1}{2}}} - g - \frac{1}{\alpha^{n}_{j+\frac{1}{2}} \rho^{n}_{g,j+\frac{1}{2}}} f^{n+1}_{lg,j+\frac{1}{2}} - \frac{1}{\rho^{n}_{g,j+\frac{1}{2}}} F^{n+1}_{wg,j+\frac{1}{2}} + VM$$

TRACG NUMERICS

MOMENTUM EQUATION

$$V^{n+1}_{j+\frac{1}{2}} - V^{n}_{j+\frac{1}{2}} = b^{n}_{j+\frac{1}{2}} + c^{n}_{j+\frac{1}{2}} \left(P^{n+1}_{j} - P^{n+1}_{j+1}\right)$$

ITERATIVE SOLUTION

$$V^{n+1,k+1}_{j+\frac{1}{2}} - V^{n}_{j+\frac{1}{2}} = b^{n}_{j+\frac{1}{2}} + c^{n}_{j+\frac{1}{2}} \left(P^{n+1,k+1}_{j} - P^{n+1,k+1}_{j+1}\right)$$

$$V^{n+1,k+1}_{j+\frac{1}{2}} = V^{n+1,k}_{j+\frac{1}{2}} + c^{n}_{j+\frac{1}{2}} \left(P^{n+1,k+1}_{j} - P^{n+1,k+1}_{j+1} - P^{n+1,k}_{j} + P^{n+1,k}_{j+1}\right)$$

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TRACG NUMERICS, EXPLICIT CONTINUITY EQUATION (VAPOR)

$$\mathrm{Vol}_j \left(\alpha_j^{n+1} \rho_{g,j}^{n+1} - \alpha_j^{n} \rho_{g,j}^{n} \right) = \Delta t A_{j-\frac{1}{2}} V_{g,j-\frac{1}{2}}^{n+1} \left\{ \begin{array}{ll} \alpha_{j-1}^{n} \rho_{g,j-1}^{n} & , V_{j-\frac{1}{2}}^{n+1} > 0 \\ \alpha_j^{n} \rho_{g,j}^{n} & \end{array} \right.$$

$$- \Delta t A_{j+\frac{1}{2}}^{n+1} V_{g,j+\frac{1}{2}}^{n+1} \left\{ \begin{array}{ll} \alpha_j^{n} \rho_{g,j}^{n} & , V_{j+\frac{1}{2}}^{n+1} > 0 \\ \alpha_{j+1}^{n} \rho_{g,j+1}^{n} & \end{array} \right. + \Delta t \mathrm{Vol}_j \Gamma_{g,j}^{n+1}$$

TRACG NUMERICS, EXPLICIT CONTINUITY EQUATION (VAPOR)

Jorra 11-08-89

LINEARIZATION AROUND ITERATION k

$$\alpha_j^{n+1} \rho_j^{n+1} \rightarrow \alpha_j^{n+1,k} \rho_j^{n+1,k} + \rho_j^{n+1,k} (\alpha_j^{n+1,k+1} - \alpha_j^{n+1,k})$$

$$+ \alpha^{n+1,k} \frac{\partial \rho_g}{\partial T_g} (T_{g,j}^{n+1,k+1} - T_{g,j}^{n+1,k})$$

$$+ \alpha^{n+1,k} \frac{\partial \rho_g}{\partial P_g} (P_j^{n+1,k+1} - P_j^{n+1,k})$$

$$V_{g,j+\frac{1}{2}}^{n+1,k+1} = V_{g,j+\frac{1}{2}}^{n+1,k} + c_{g,j+\frac{1}{2}}^{n} ((P_j^{n+1,k+1} - P_{j+1}^{n+1,k+1}) - (P_j^{n+1,k} - P_{j+1}^{n+1,k}))$$

TRACG NUMERICS, IMPLICIT CONTINUITY EQUATION (VAPOR)

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$$Vol_j\left(\alpha_j^{n+1}\rho_{g,j}^{n+1} - \alpha_j^{n}\rho_{g,j}^{n}\right) = \Delta t A_{j-\frac{1}{2}} V_{g,j\ \frac{1}{2}}^{n+1} \begin{cases} \alpha_{j-1}^{n+1}\rho_{g,j-1}^{n+1} & , V_{j-\frac{1}{2}}^{n+1} > 0 \\ \alpha_{j}^{n+1}\rho_{g,j}^{n+1} & \end{cases}$$

$$-\Delta t A_{j+\frac{1}{2}}^{n+1} V_{g,j+\frac{1}{2}}^{n+1} \begin{cases} \alpha_{j}^{n+1}\rho_{g,j}^{n+1} & , \text{for } V_{j+\frac{1}{2}}^{n+1} > 0 \\ \alpha_{j+1}^{n+1}\rho_{g,j+1}^{n+1} & \end{cases} + \Delta t Vol_j \Gamma_{g,j}^{n+1}$$

TRACG NUMERICS, IMPLICIT CONTINUITY EQUATION (VAPOR)

LINEARIZATION AROUND ITERATION k

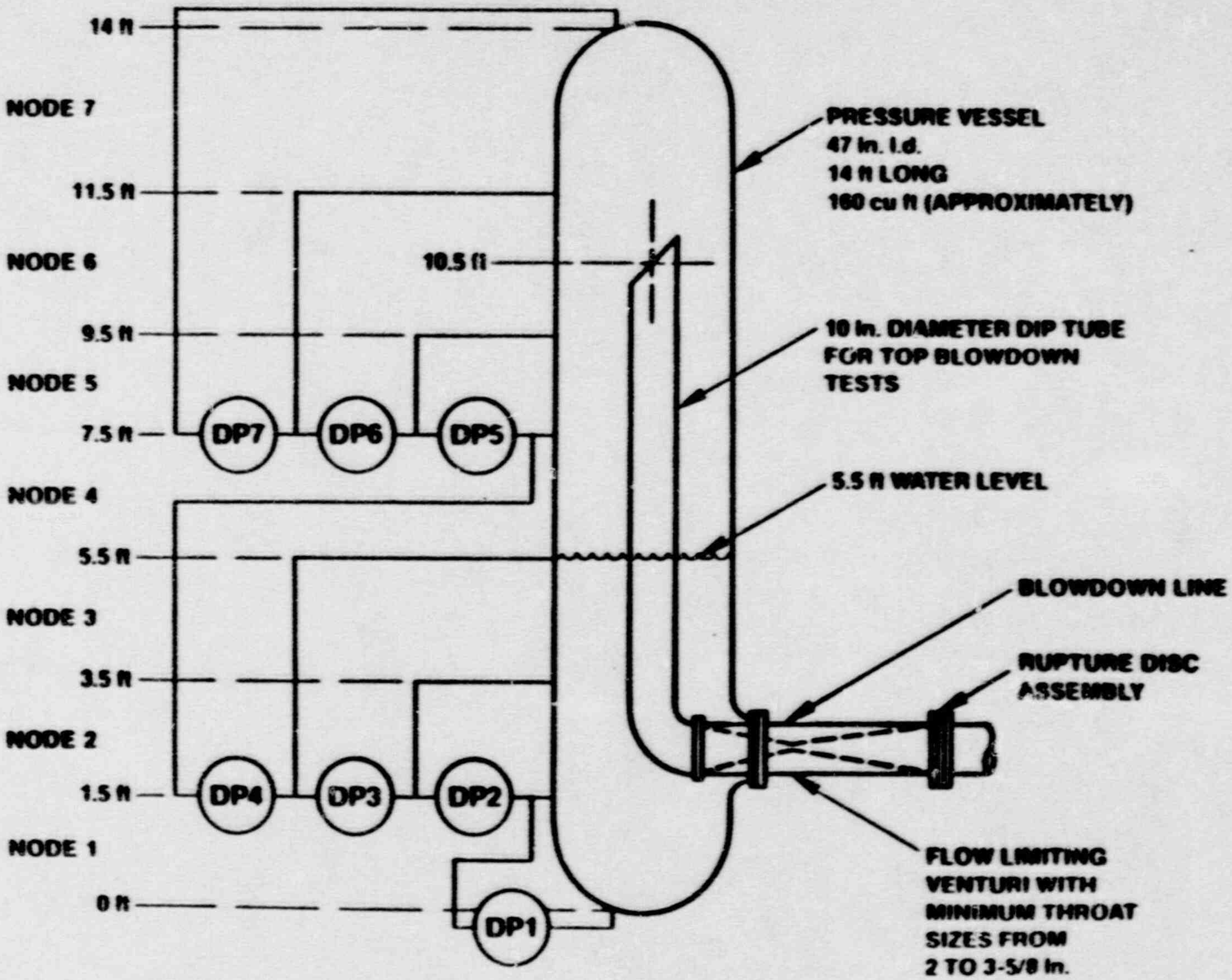
$$\alpha_j^{n+1} \rho_j^{n+1} \rightarrow \alpha_j^{n+1,k} \rho_j^{n+1,k} + \rho_j^{n+1,k} (\alpha_j^{n+1,k+1} - \alpha_j^{n+1,k})$$

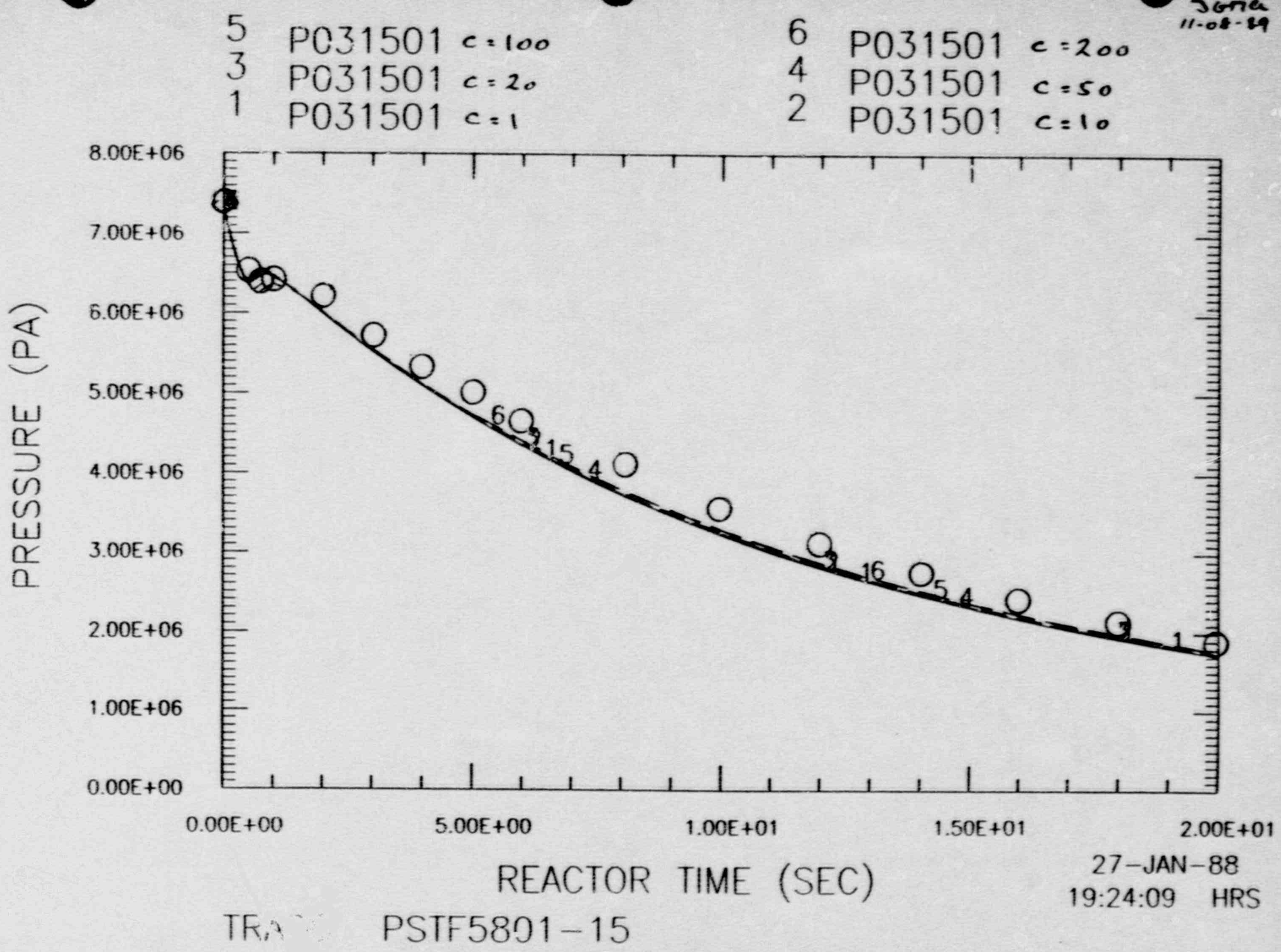
$$+ \alpha^{n+1,k} \frac{\partial \rho_g}{\partial T_g} (T_{g,j}^{n+1,k+1} - T_{g,j}^{n+1,k})$$

$$+ \alpha^{n+1,k} \frac{\partial \rho_g}{\partial P_g} (P_j^{n+1,k+1} - P_j^{n+1,k})$$

$$V_{g,j+\frac{1}{2}}^{n+1,k+1} = V_{g,j+\frac{1}{2}}^{n+1,k} + c_{g,j+\frac{1}{2}}^{n} ((P_j^{n+1,k+1} - P_{j+1}^{n+1,k+1}) - (P_j^{n+1,k} - P_{j+1}^{n+1,k}))$$

ASSESSMENT OF PREDICTOR-CORRECTOR METHOD
PSTF TEST 5801-15





5 P031501 c=100
3 P031501 c=20
1 P031501 c=1
6 P031501 c=200
4 P031501 c=50
2 P031501 c=10
PRESSURE (PA)
8.00E+06
7.00E+06
6.00E+06
5.00E+06
4.00E+06
3.00E+06
2.00E+06
1.00E+06
0.00E+00
0.00E+00
5.00E+00
1.00E+01
1.50E+01
2.00E+01
REACTOR TIME (SEC)
27-JAN-88
19:24:09 HRS
PSTF5801-15

3 651a
11-08-89

TRACG NUMERICS

COMPUTER TIME FOR THE PSTF TEST

COURANT NUMBER	TIME STEPS	CPU TIME	AVERAGE ITERATION COUNT.
1.C	13253	17380 sec	1.6
2.0	6696	9950	2.1
5.0	2683	4479	3.3
10.0	1347	2416	4.0
20.0	680	1367	4.0
50.0	281	612	4.9
100.0	149	360	4.9
200.0	84	232	5.8
500.0	53	213	9.5

TRACG NUMERICS

METHOD	ACCURARY	STABILITY
EXPLICIT	FIRST ORDER	$\Delta t < \frac{\Delta X}{\lvert V \rvert}$
IIMPLICIT	FIRST ORDER	NO LIMIT

KINETICS MODELS

MODELS AVAILABLE

- POINT KINETICS
  - TOTAL POWER LEVEL VARIES AS A FUNCTION OF TIME.
  - SPATIAL DISTRIBUTION OF POWER REMAINS CONSTANT.
  - THERMAL HYDRAULICS COLLAPSED TO PROVIDE CORE AVERAGED PARAMETERS FOR REACTIVITY FEEDBACK.

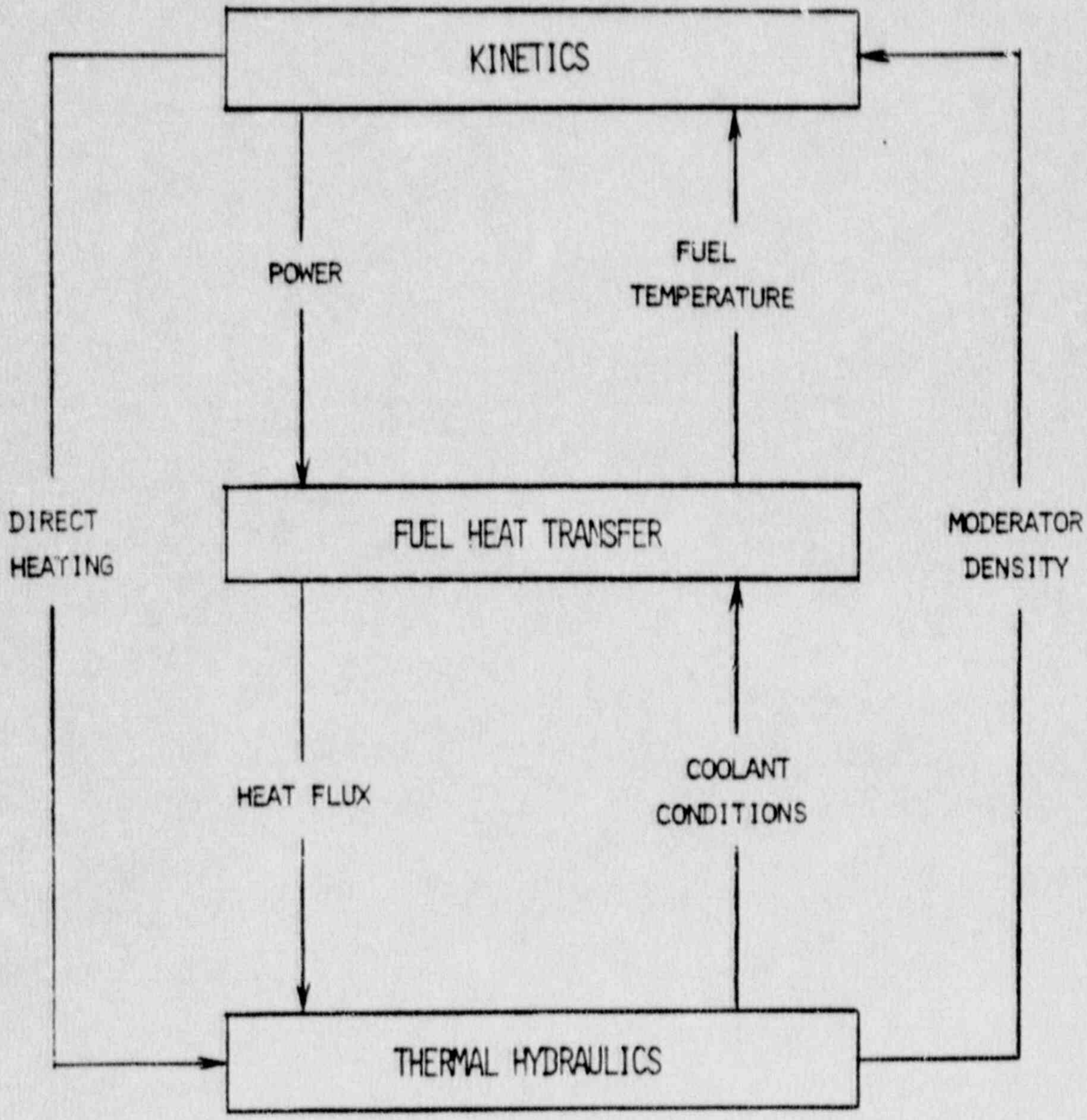
- 1D KINETICS
  - TOTAL POWER AND CORE AVERAGE AXIAL POWER DISTRIBUTION VARIES WITH TIME.
  - BUNDLE TO BUNDLE (RADIAL) POWER DISTRIBUTION REMAINS CONSTANT.
  - THERMAL HYDRAULICS COLLAPSED TO PROVIDE CORE AVERAGE AXIAL PARAMETERS FOR REACTIVITY FEEDBACK.

- 3D KINETICS
  - POWER LEVEL AND SPATIAL DISTRIBUTION (RADIAL AND AXIAL) VARIES AS A FUNCTION OF TIME.
  - HYDRAULIC CHANNELS PROVIDE CHARACTERISTIC RESPONSE FOR SPECIFIED GROUP OF KINETICS BUNDLES.

MODEL CONSISTENCY

- EACH MODEL FORMULATED CONSISTENT WITH THE GE 3D BWR CORE SIMULATOR.
- EACH MODEL OBTAINS ITS NUCLEAR DATA AND OPERATING CONDITIONS FROM THE BWR SIMULATOR.
- BWR SIMULATOR
  - BASIC TOOL FOR CORE DESIGN.
  - 3D COUPLED NUCLEAR/THERMAL HYDRAULICS FOR ANALYSIS OF BWR CORE.
  - 1 GROUP DIFFUSION EQUATION WITH COARSE MESH. (1 MESH PER BUNDLE)
  - CROSS SECTIONS AND K_∞ DERIVED FROM 3 GROUP CROSS SECTIONS FROM LATTICE PHYSICS CODE.

KINETICS / THERMAL HYDRAULICS INTERFACES



3D KINETICS MODEL

- CONSISTENT WITH BWR CORE SIMULATOR.
  - 3D FINITE DIFFERENCE MODEL
  - 1 NEUTRON ENERGY GROUP
  - 6 DELAYED NEUTRON PRECURSOR GROUPS
  - 1 MESH PER BUNDLE IN RADIAL DIRECTION
  - UP TO 25 MESHES PER BUNDLE IN AXIAL DIRECTION
- TIME DEPENDENT POSITIONING OF CONTROL RODS.
- FULL CORE THROUGH OCTANT SYMMETRY GEOMETRY OPTIONS

BASIC EQUATIONS

TIME DEPENDENT 3D DIFFUSION EQUATIONS

$$\nabla^2\phi_1 + B^2\phi_1 + \frac{1}{D_1}\sum_{n=1}^{N} \lambda n C n = \frac{1}{v_1 D_1} \frac{\partial \phi_1}{\partial t}$$

$$\frac{\beta n}{\lambda} k \Sigma_1 \phi_1 - \lambda n C n = \frac{\partial C n}{\partial t}$$

PARAMETERS ARE FUNCTIONS OF TIME DEPENDENT NODAL CROSS SECTIONS.

AMPLITUDE FUNCTION EQUATION

$$\frac{dA(t)}{dt} = \frac{\rho(t) - \beta(t)}{\Lambda(t)} A(t) + \sum_{\ell=1}^{N} \lambda\ell\, G_\ell(t)$$

$$\frac{dG_\ell(t)}{dt} = -\lambda\ell G_\ell(t) + \frac{\beta_\ell(t)}{\Lambda(t)} A(t)$$

PARAMETERS ARE FUNCTIONS OF TIME DEPENDENT NODAL CROSS SECTIONS

AND SHAPE FUNCTION.

SHAPE FUNCTION EQUATION

$$\frac{1}{v_1 D_1(r,t)} \left(\frac{\partial}{\partial t} S(r,t) + \frac{S(r,t)}{A(t)} \frac{dA(t)}{dt}\right)$$

$$= \nabla^2 S(r,t) + B^2(r,t) S(r,t) + \frac{1}{D_1(r,t)A(t)} \sum_{\ell=1}^{N} \lambda\ell\, C\ell(r,t)$$

PARAMETERS ARE FUNCTIONS OF TIME DEPENDENT NODAL CROSS SECTIONS

AND AMPLITUDE FUNCTION.

TRANSIENT SOLUTION

- TIME DEPENDENT CHANGE IN BASIC EQUATION TERMS CALCULATED AS A FUNCTION OF MODERATOR DENSITY AND CONTROL STATE.
- K_∞ ADDITIONALLY CALCULATED AS A FUNCTION OF FUEL TEMPERATURE.
- TRANSIENT SOLUTION UTILIZES FLUX FACTORIZATION METHOD.

$$\phi(r,t) = A(t) * S(r,t)$$

- AMPLITUDE FUNCTION $A(t)$ REPRESENTS THE MAGNITUDE OF THE NEUTRON FLUX OVER THE CORE.
- SHAPE FUNCTION $S(r,t)$ REPRESENTS THE SPATIAL DISTRIBUTION OF THE NEUTRON FLUX IN THE CORE.
- DIFFERENT TIME STEPS ARE ALLOWED IN THE SOLUTION OF THE FUNCTIONS TO PERMIT SMALLER TIME STEPS FOR THE FASTER CHANGING AMPLITUDE FUNCTION.

CALCULATIONAL SEQUENCE

- AMPLITUDE FUNCTION IS SOLVED USING QUADRATIC EXTRAPOLATION OF EQUATION PARAMETERS.
- SHAPE FUNCTION IS ESTIMATED USING LINEAR EXTRAPOLATION.
- SOLVE THERMAL HYDRAULIC EQUATIONS. IF SOLUTION REQUIRES SMALLER TIME STEP, BEGIN AGAIN.
- UPDATE NODAL CROSS SECTIONS AND AMPLITUDE PARAMETERS.
- SOLVE 3D PRECURSOR EQUATIONS.
- IF SHAPE FUNCTION IS TO BE SOLVED, THE PROCESS CONTINUES.

- SHAPE FUNCTION IS SOLVED USING LATEST AMPLITUDE FUNCTION AND CROSS SECTIONS.
- AMPLITUDE FUNCTION PARAMETERS ARE RECALCULATED AND AMPLITUDE FUNCTIONS CALCULATED WITH EXTRAPOLATED SHAPE FUNCTIONS ARE RECALCULATED.

QUALIFICATION

- CONSISTENCY WITH 3D BWR CORE SIMULATOR HAS BEEN CONFIRMED FOR STEADY STATE OPERATION AND SCRAM RESPONSE.
- ASSESSMENT OF TRANSIENT CAPABILITY HAS BEEN PERFORMED AGAINST TURBINE TRIP PLANT DATA.
- QUALIFICATION FOR STABILITY AND ROD DROP ANALYSIS IS IN PROGRESS.

SUMMARY OF PREVIOUS TRACG QUALIFICATION

JGM Andersen

TRACG DEVELOPMENT AND QUALIFICATION APPROACH

- o DEVELOP DETAILED MODELS FOR INDIVIDUAL PHENOMENA
  - Best Estimate First Principle Models
  - Validation on Basic Separate Effects Tests
- o DEVELOP DETAILED MODELS FOR BWR COMPONENTS
  - Best Estimate First Principle Models
  - Validation on Component Effects Tests
- o QUALIFICATION ON SYSTEM EFFECTS TESTS AND PLANT DATA
- o APPLY FOR BWR PREDICTIONS

JGMA
11-08-1989

TRACG DEVELOPMENT AND QUALIFICATION APPROACH

INDIVIDUAL PHENOMENA AND SEPARATE EFFECTS TESTS

- INTERFACIAL SHEAR - VOID FRACTION PREDICTION

- HEAT TRANSFER - VOID FRACTION AND TEMPERATURE PREDICTION

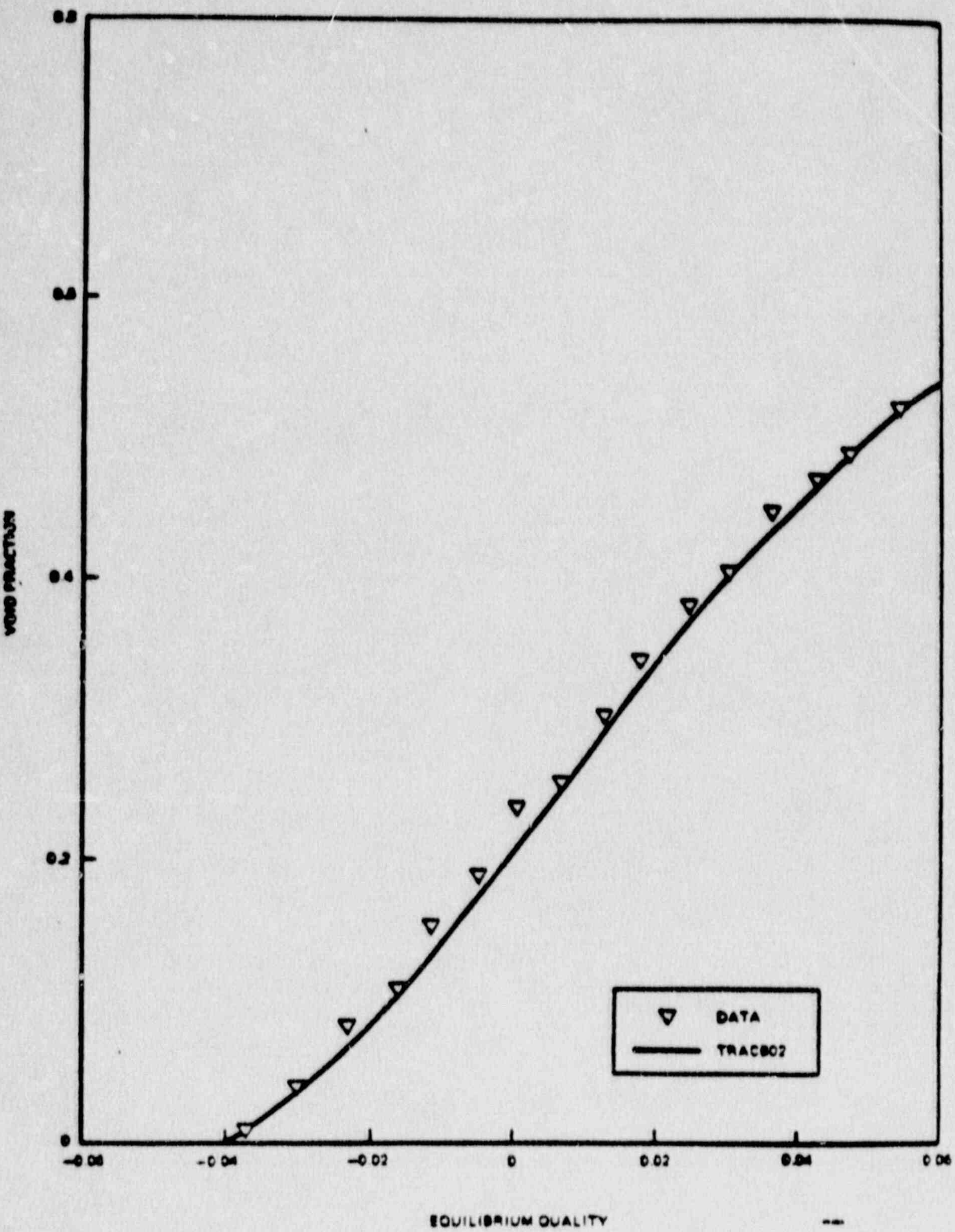


Figure 6.5.1. Comparison of Christensen Test With TRAC, P = 5.5 MPa

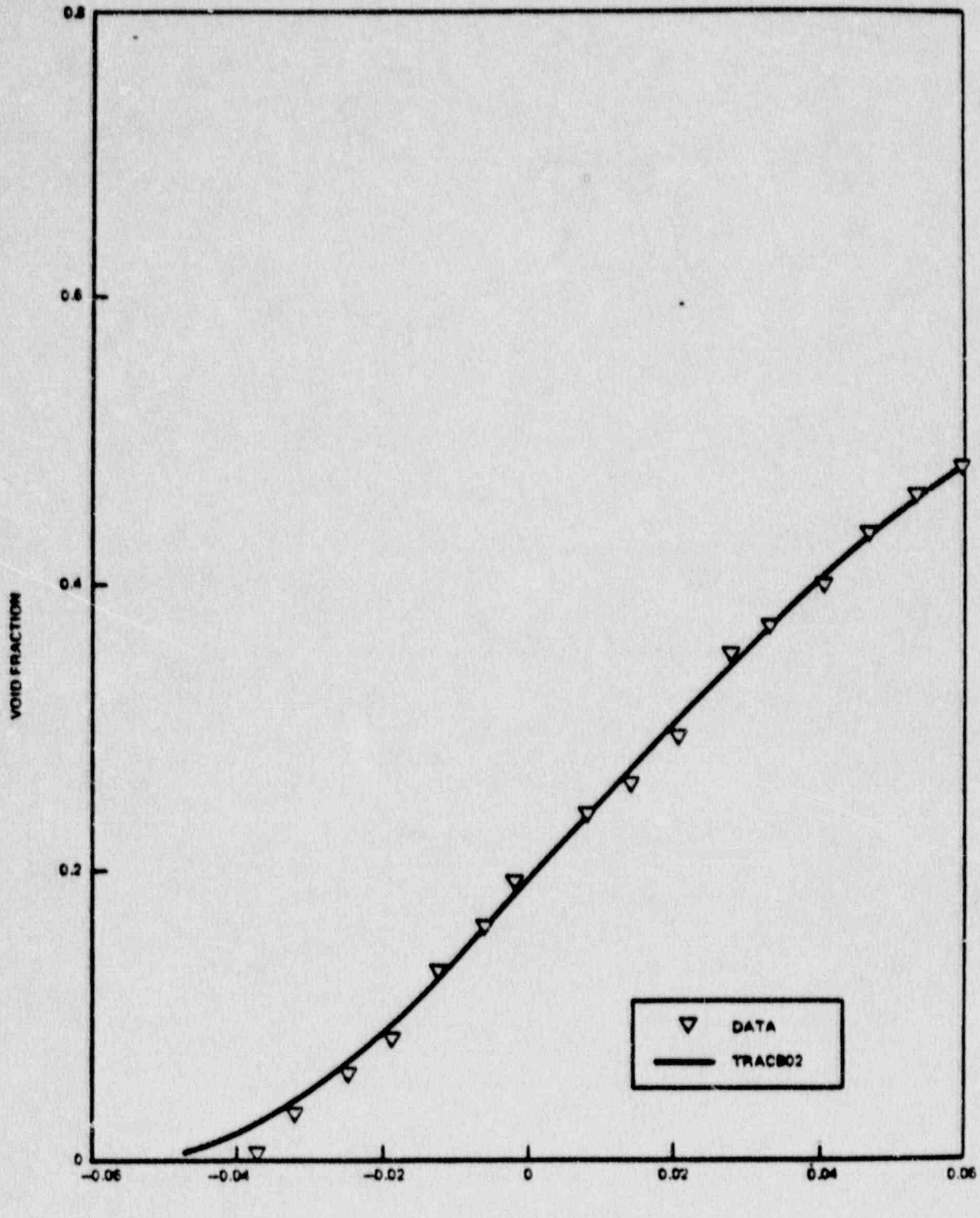


Figure 6.5.2. Comparison of Christensen Test With TRAC, P = 6.9 MPa

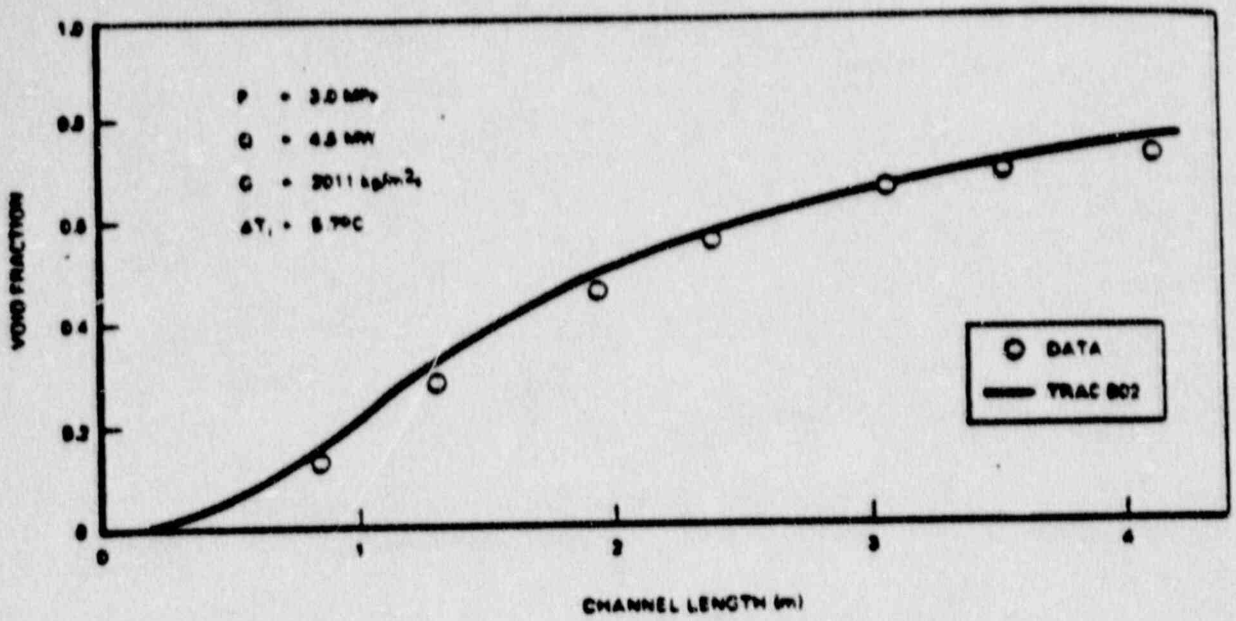


Figure 6.1.3. Comparison of FRIGG Results with TRAC (FRIGG 613123)

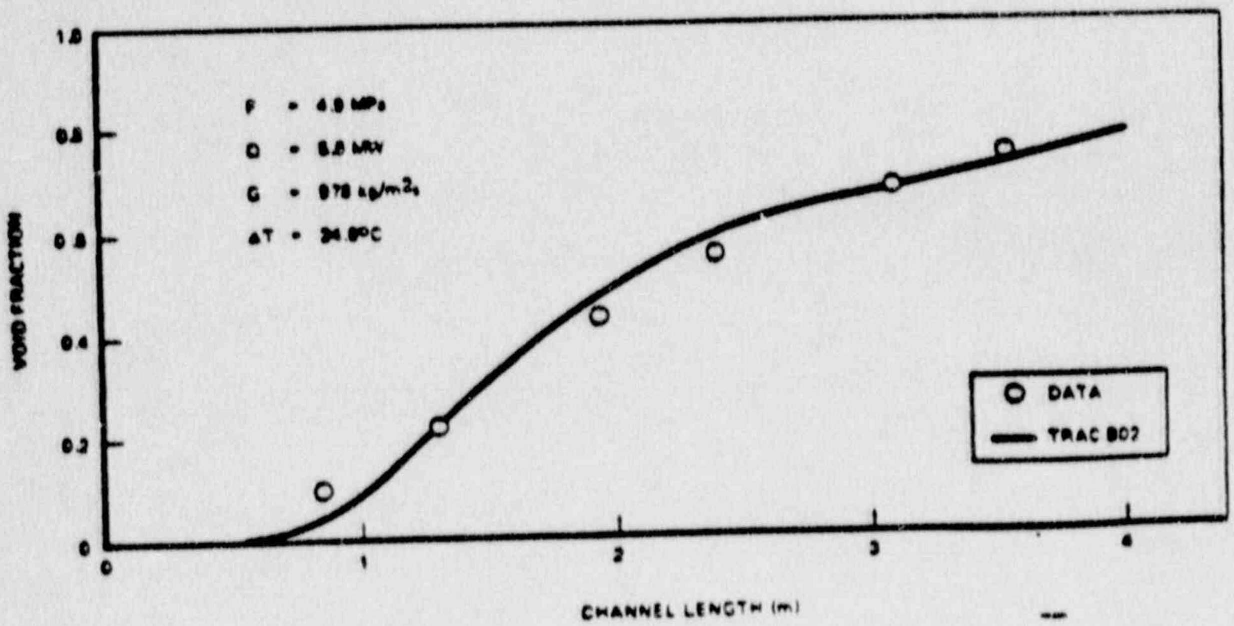


Figure 6.1.4. Comparison of FRIGG Results with TRAC (FRIGG 613014)

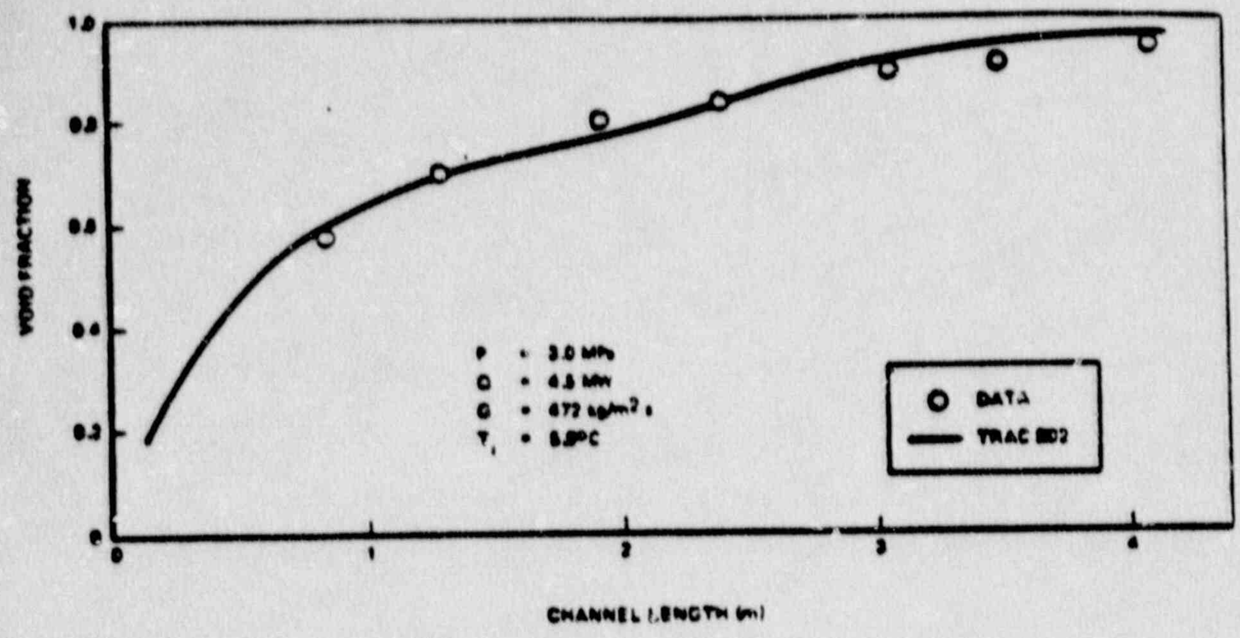


Figure 6.1.1. Comparison of FRIGG Results with TRAC (FRIGG 613124)

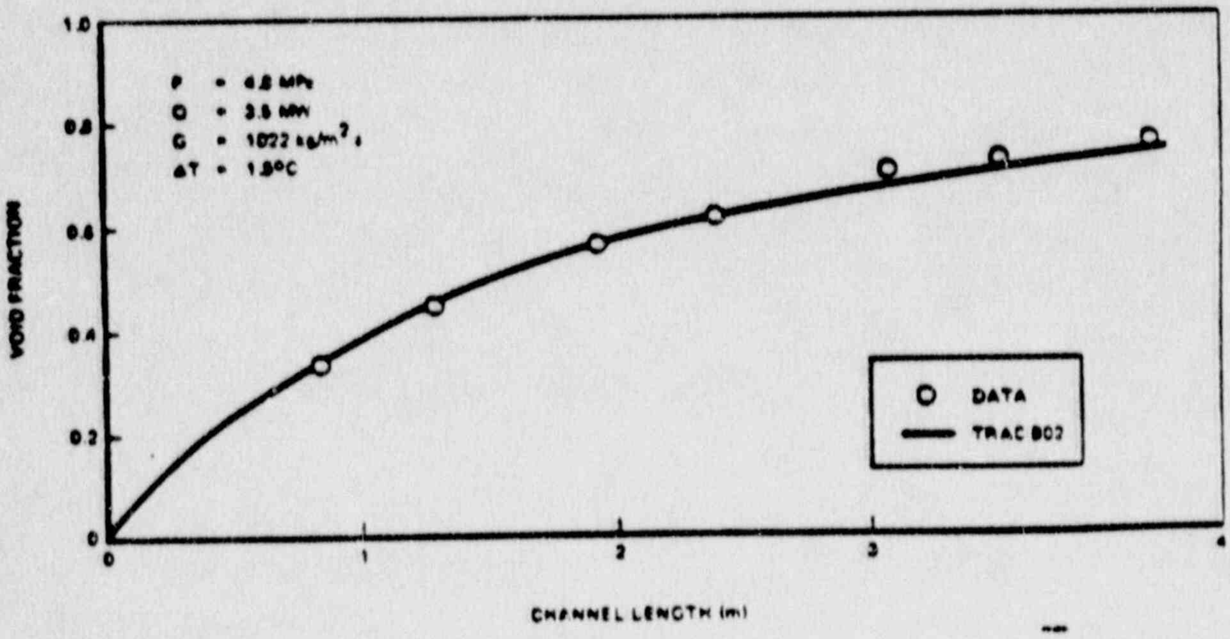


Figure 6.1.2. Comparison of FRIGG Results with TRAC (FRIGG 613005)

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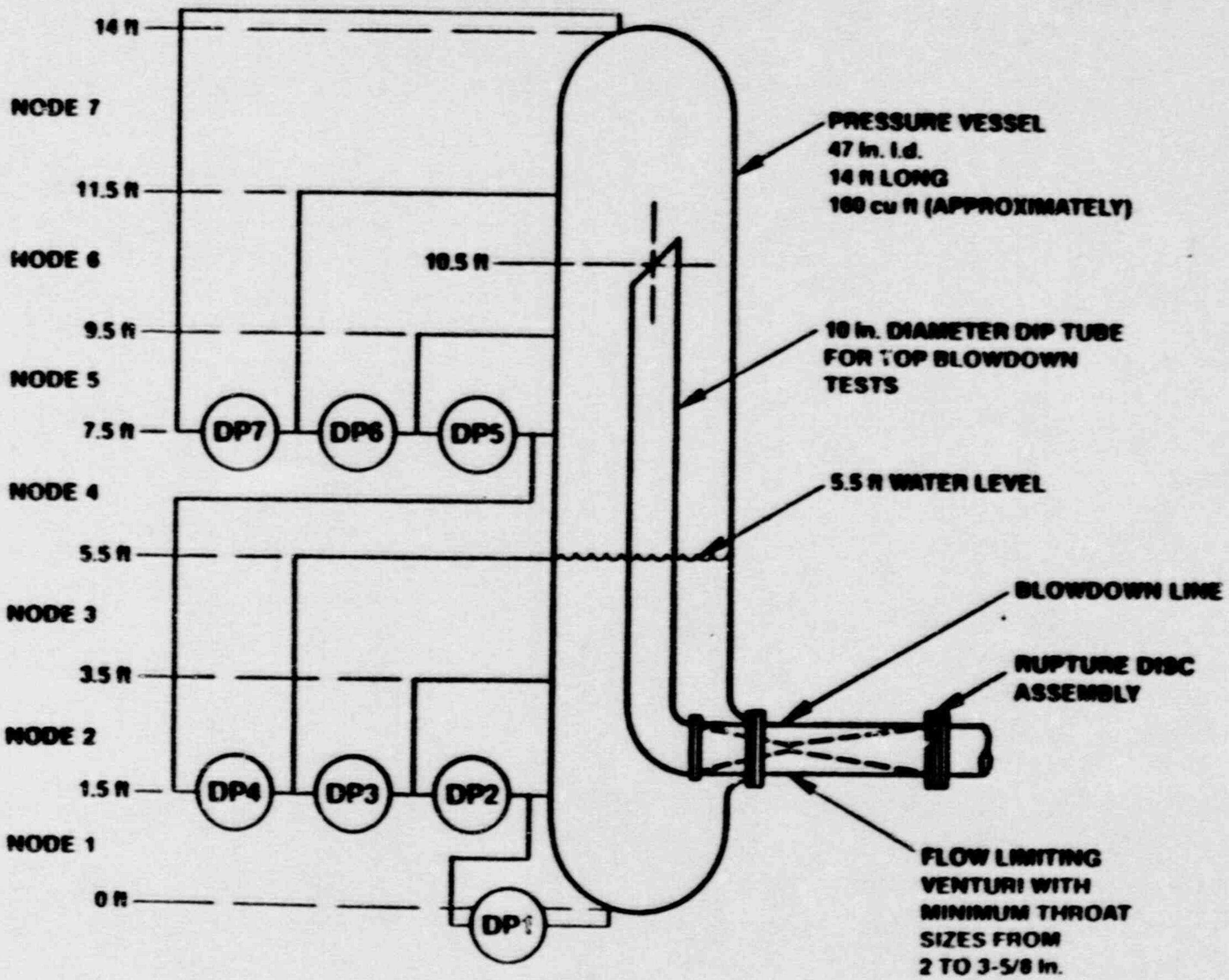
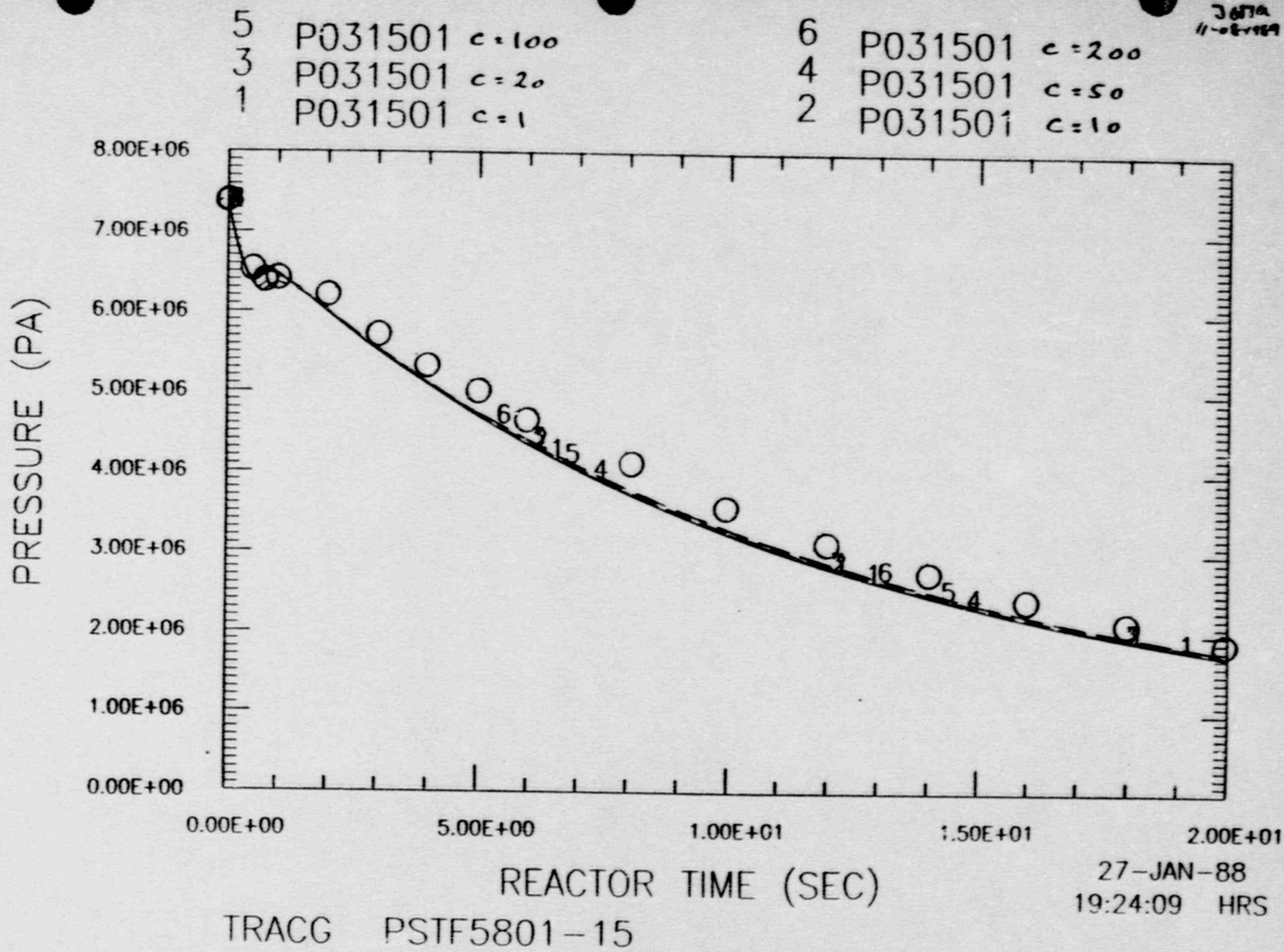
11-08-1989

ASSESSMENT OF PREDICTOR-CORRECTOR METHOD
PSTF TEST 5801-15





5 P031501 c=100
3 P031501 c=20
1 P031501 c=1
6 P031501 c=200
4 P031501 c=50
2 P031501 c=10
PRESSURE (PA)
8.00E+06
7.00E+06
6.00E+06
5.00E+06
4.00E+06
3.00E+06
2.00E+06
1.00E+06
0.00E+00
0.00E+00
5.00E+00
1.00E+01
1.50E+01
2.00E+01
REACTOR TIME (SEC)
27-JAN-88
19:24:09 HRS
TRACG PSTF5801-15

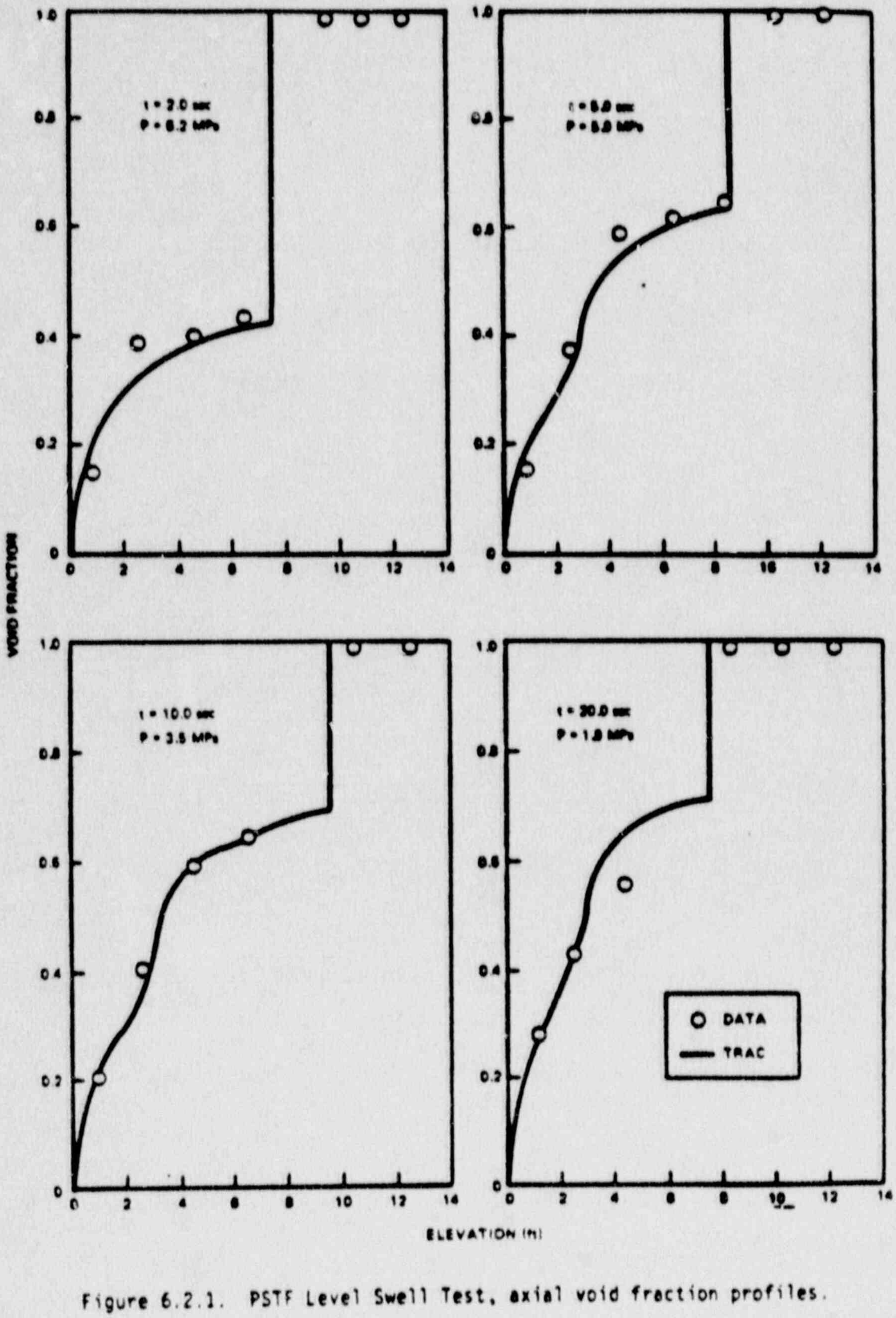


Figure 6.2.1. PSTF Level Swell Test, axial void fraction profiles.

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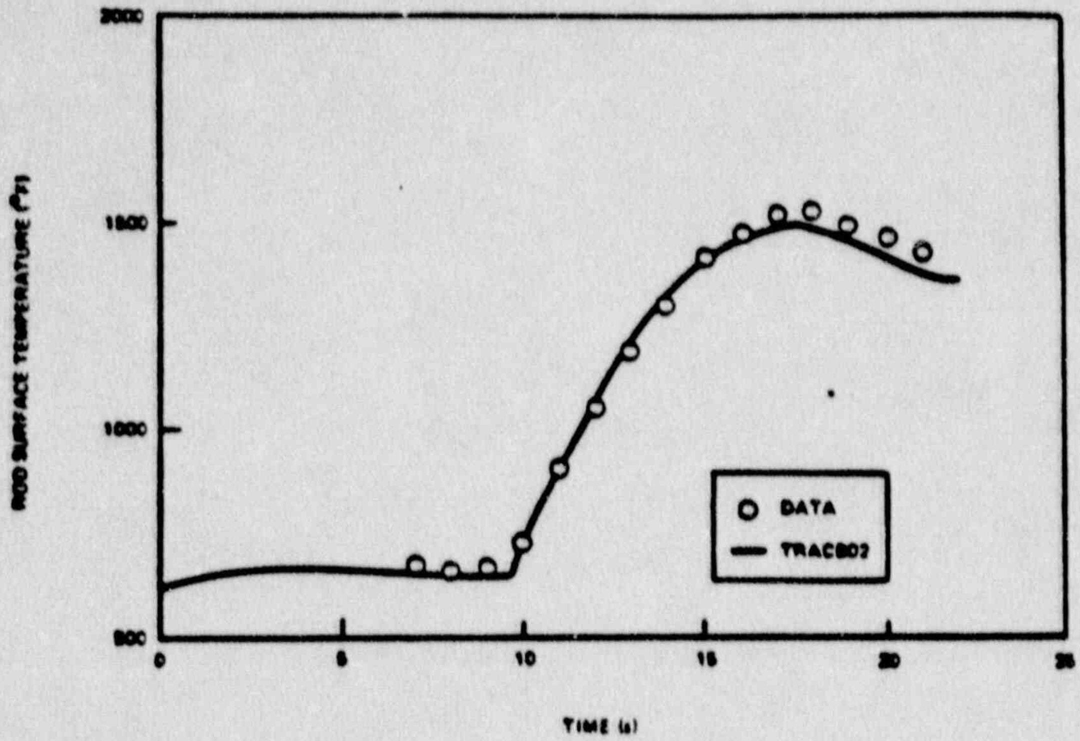


Figure 6.3.1. Comparison of THTF Test With TRAC, Elevation 3.6 m

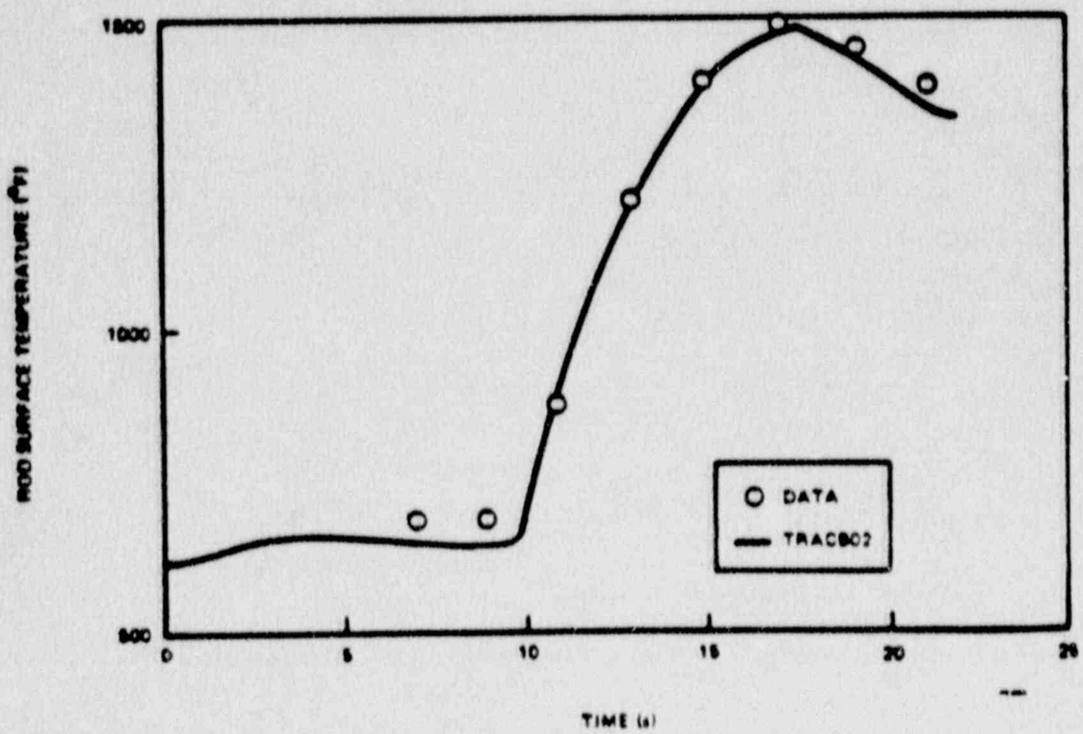


Figure 6.3.2. Comparison of THTF Test With TRAC, Elevation 3.0 m

TRACG DEVELOPMENT AND QUALIFICATION APPROACH

INDIVIDUAL PHENOMENA AND SEPARATE EFFECTS TESTS

- o INTERFACIAL SHEAR - VOID FRACTION PREDICTION
- o HEAT TRANSFER - VOID FRACTION AND TEMPERATURE PREDICTION
- o VOID FRACTION AND TEMPERATURES ARE WELL PREDICTED

JGMA
11-08-1989

TRACG DEVELOPMENT AND QUALIFICATION APPROACH

BWR COMPONENT MODELS AND TESTS

- o JET PUMP - M AND N RATIOS (PUMP CURVES)

- o STEAM SEPARATORS - PHASE SEPARATION AND PRESSURE DROP

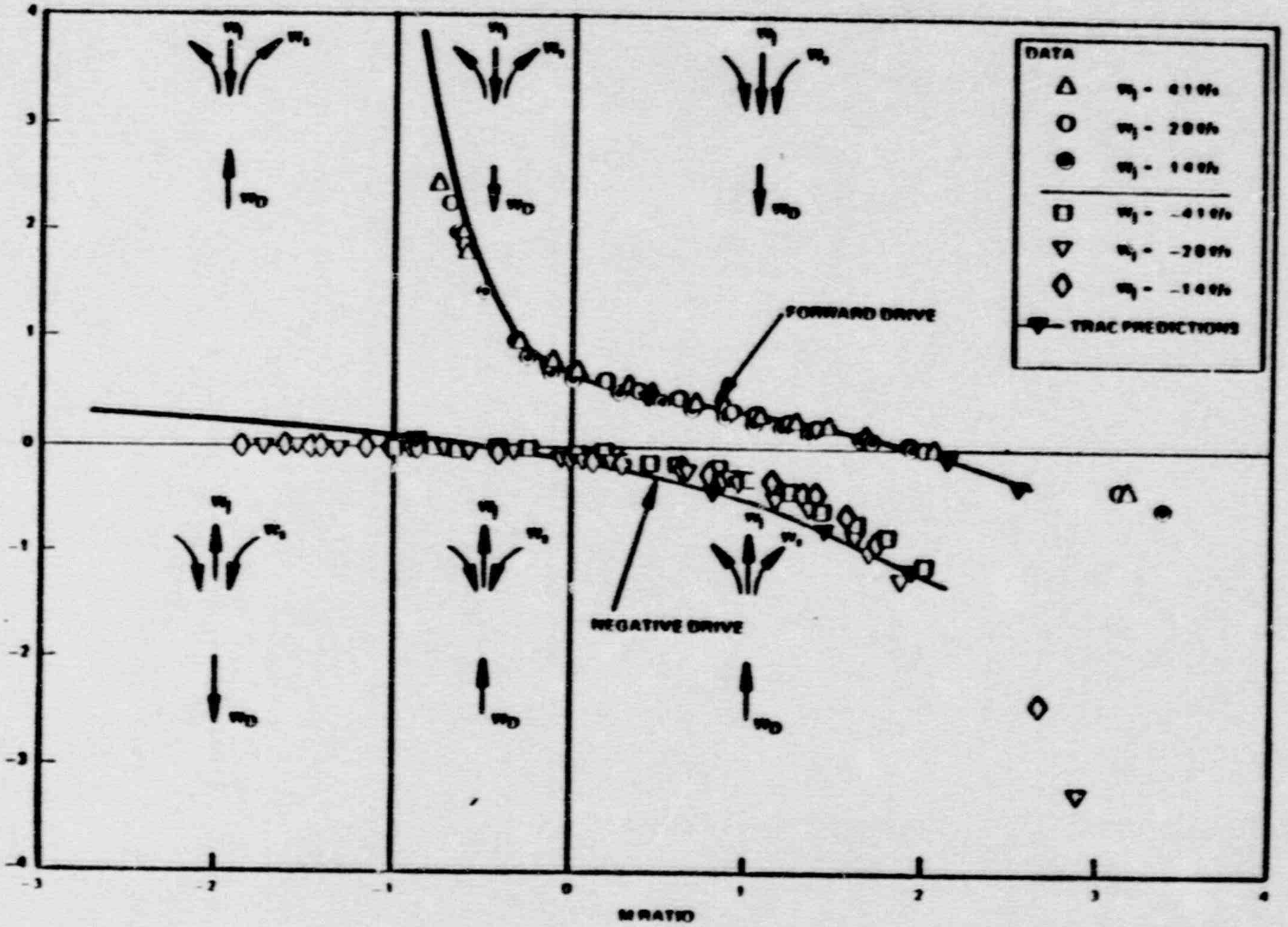


Figure 2-10. Comparisons of Predicted VS Measured MN Curves for INEL Tested Jet Pump

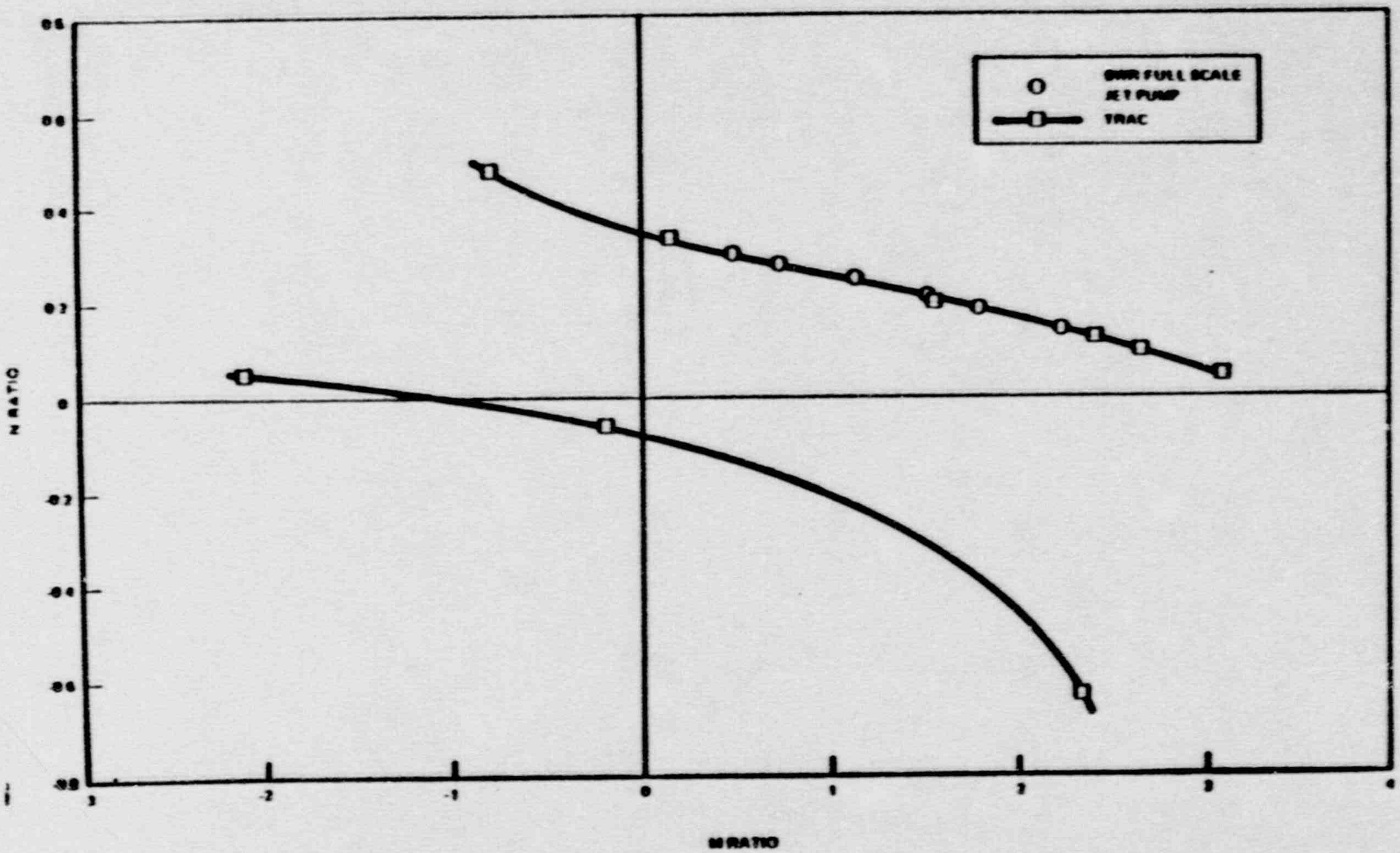
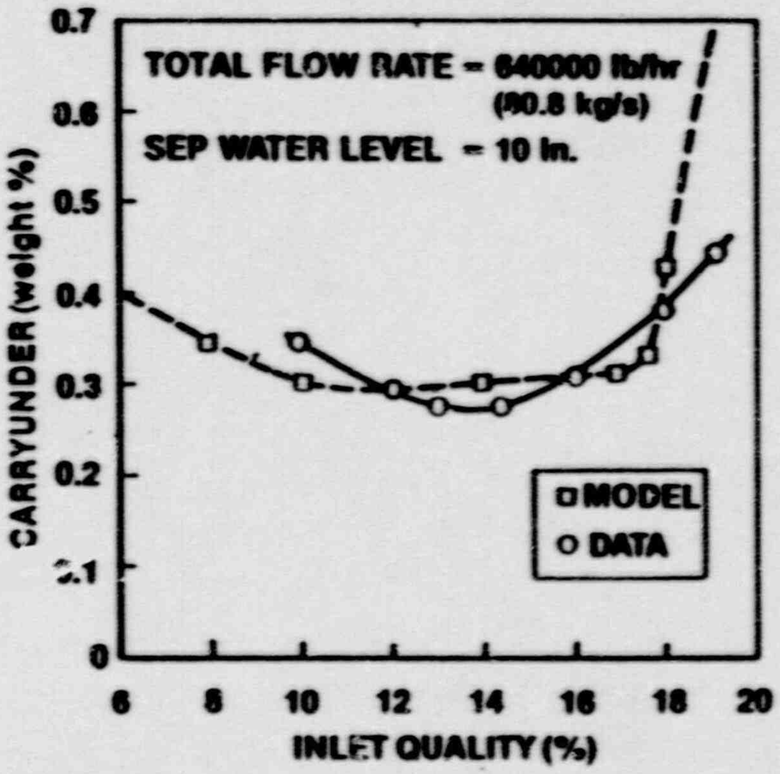


Figure 2-12. Comparisons of Predicted VS Measured MN Curves for Full Scale Jet Pump

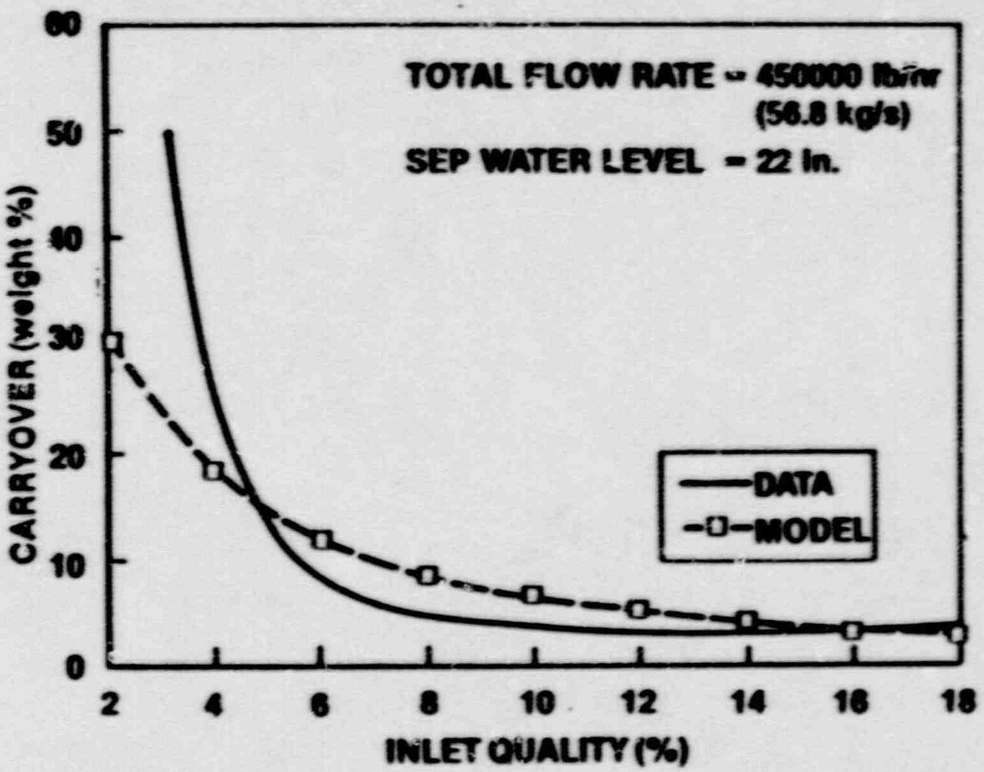
3 of 10
11-06-1989

COMPARISON OF TEST DATA AND MECHANISTIC MODEL PREDICTION ON CARRYUNDER FOR 3-STAGE SEPARATOR



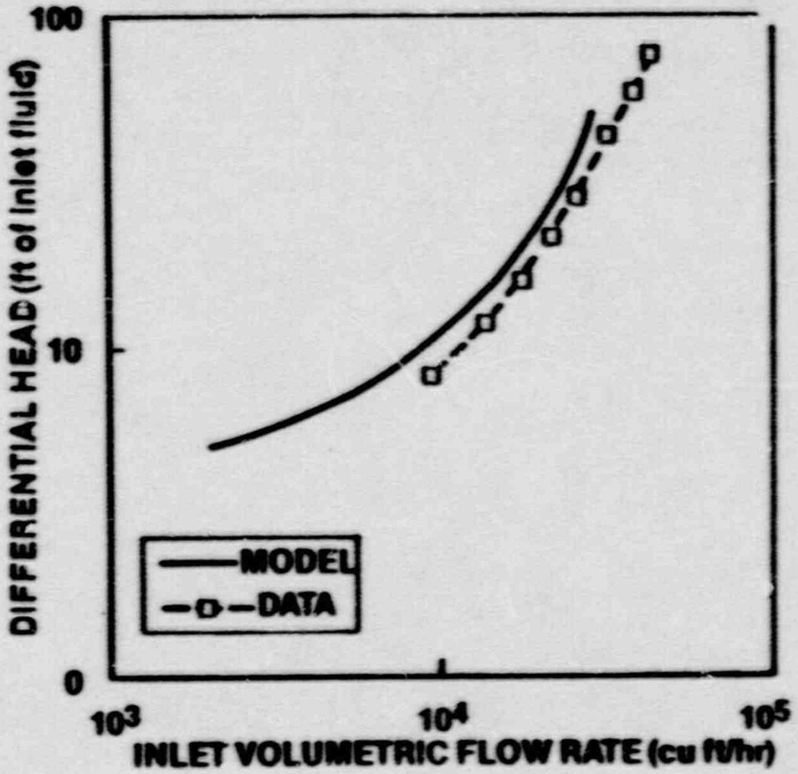
3 6571 A
11-05-1969

COMPARISON OF TEST DATA AND MECHANISTIC MODEL PREDICTION ON CARRYOVER FOR 3-STAGE SEPARATOR



J GMA
11-08-1989

COMPARISON OF TEST DATA AND MECHANISTIC MODEL PREDICTION ON SEPARATOR PRESSURE DROP FOR 2-STAGE SEPARATOR



TRACG DEVELOPMENT AND QUALIFICATION APPROACH

BWR COMPONENT MODELS AND TESTS

- o JET PUMP - M AND N RATIOS (PUMP CURVES)
- o STEAM SEPARATORS - PHASE SEPARATION AND PRESSURE DROP
- o BWR COMPONENT PERFORMANCE IS WELL PREDICTED.

TRACG DEVELOPMENT AND QUALIFICATION APPROACH

SYSTEM EFFECTS TESTS AND PLANT DATA

- TLTA AND FIST TESTS - INTEGRAL SYSTEM EFFECTS TESTS
  SCALED SIMULATION OF BWR

- PLANT DATA - START UP DATA, TURBINE TRIP AND STABILITY

FIST LARGE BREAK LOCA (6DBA1B)

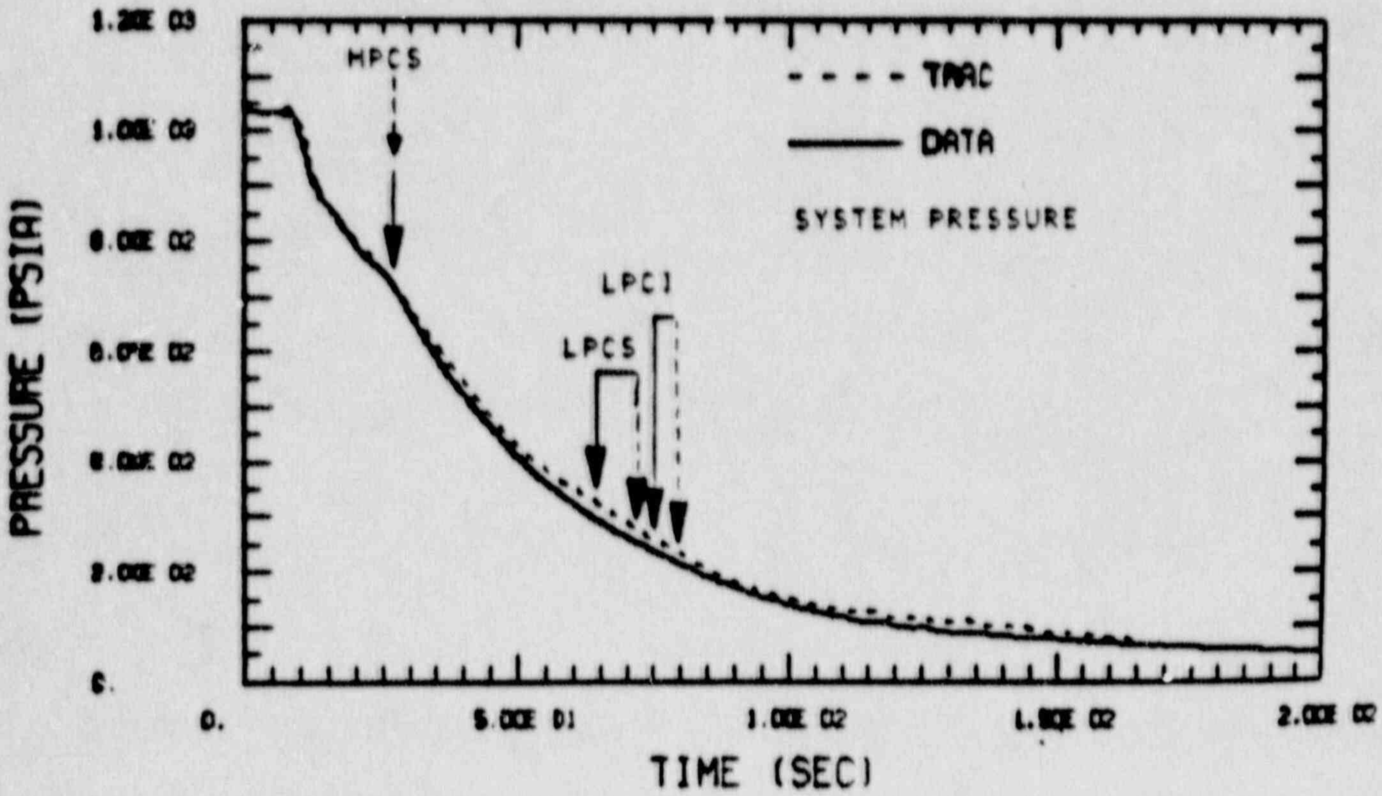
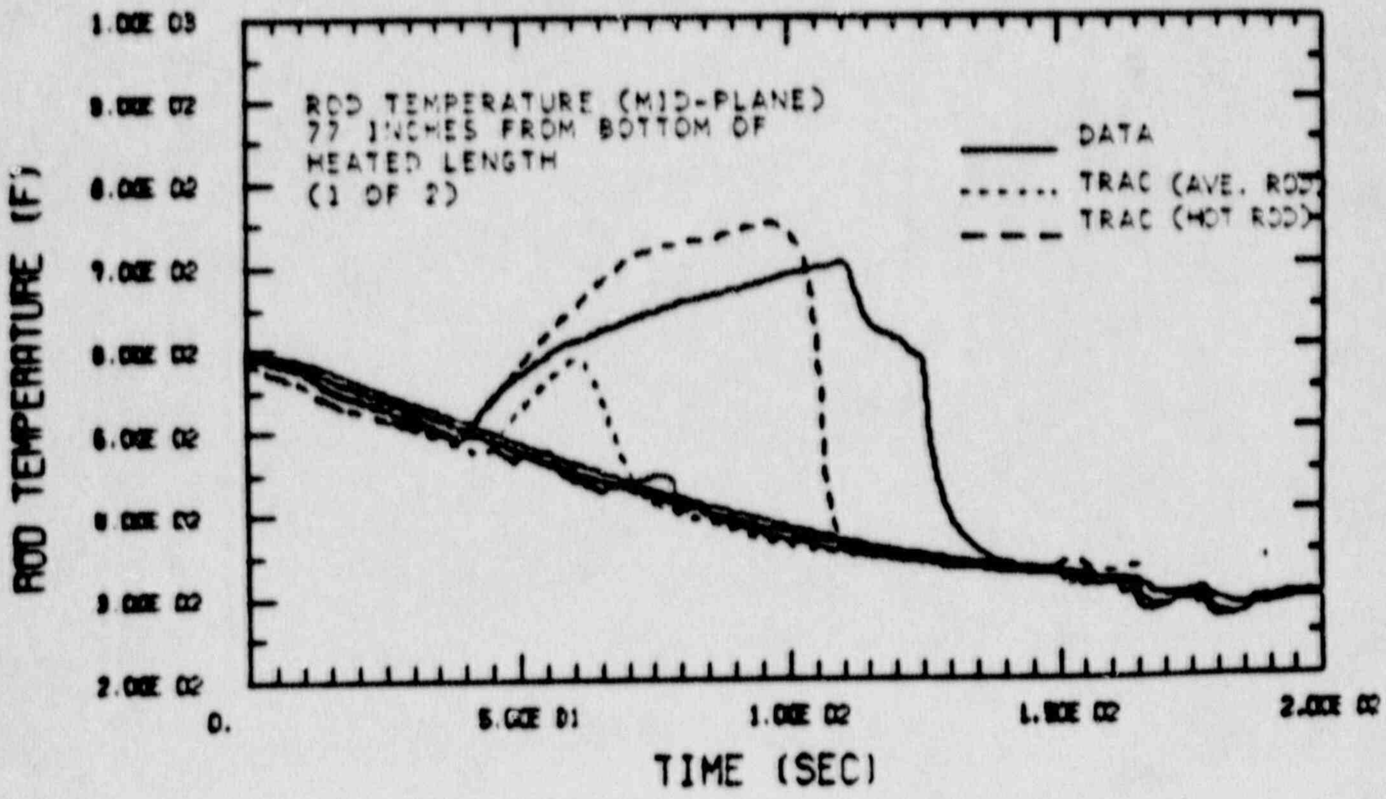


FIGURE L-1.



ROD TEMPERATURE (MID-PLANE)
77 INCHES FROM BOTTOM OF
HEATED LENGTH
(1 OF 2)
DATA
TRAC (AVE. ROD)
TRAC (HOT ROD)
ROD TEMPERATURE (F)
1.00E 03
9.00E 02
8.00E 02
7.00E 02
6.00E 02
5.00E 02
4.00E 02
3.00E 02
2.00E 02
0.
5.00E 01
1.00E 02
1.50E 02
2.00E 02
TIME (SEC)

FIGURE L-4(1)

J6MCa
11-06-1989

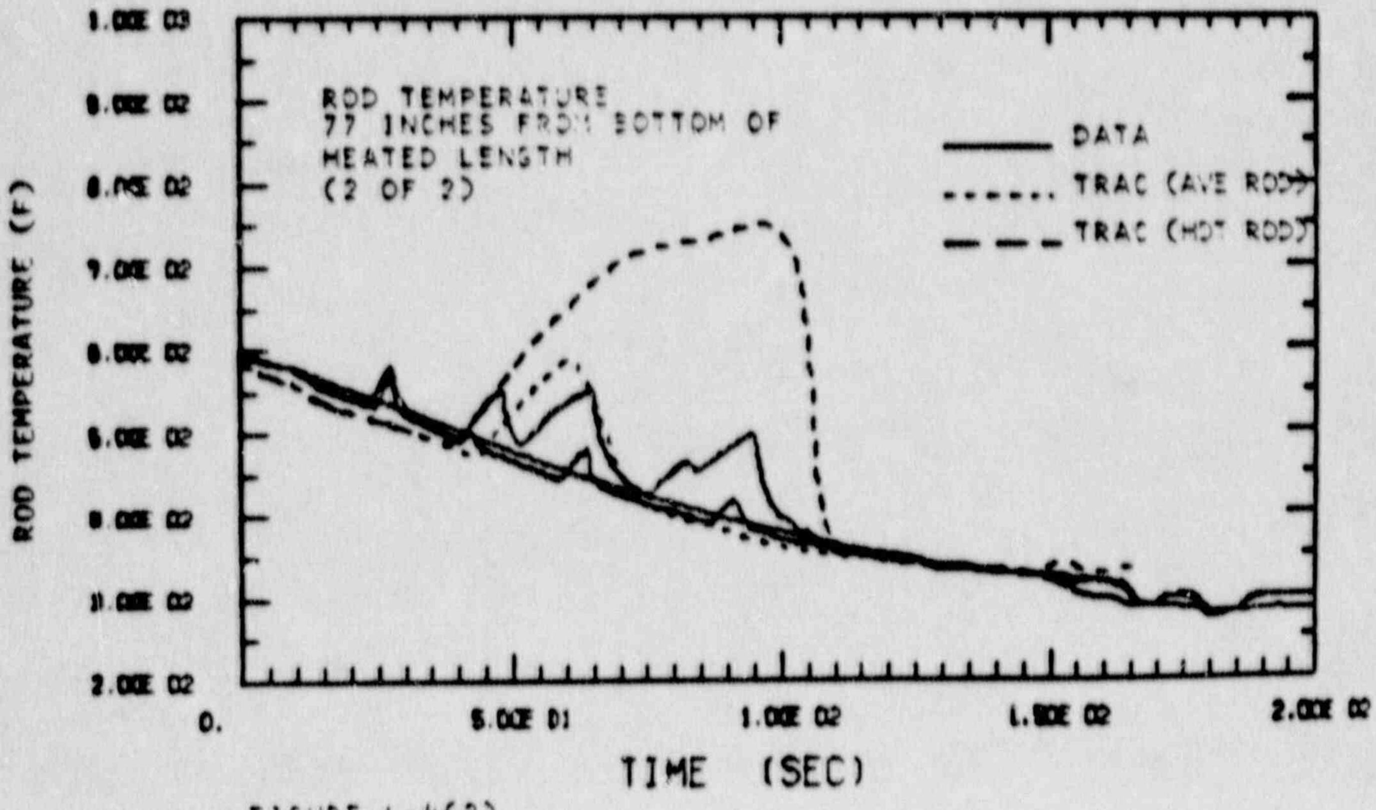


FIGURE L-4(2)

FIST SMALL BREAK LOCA (6SB2C)

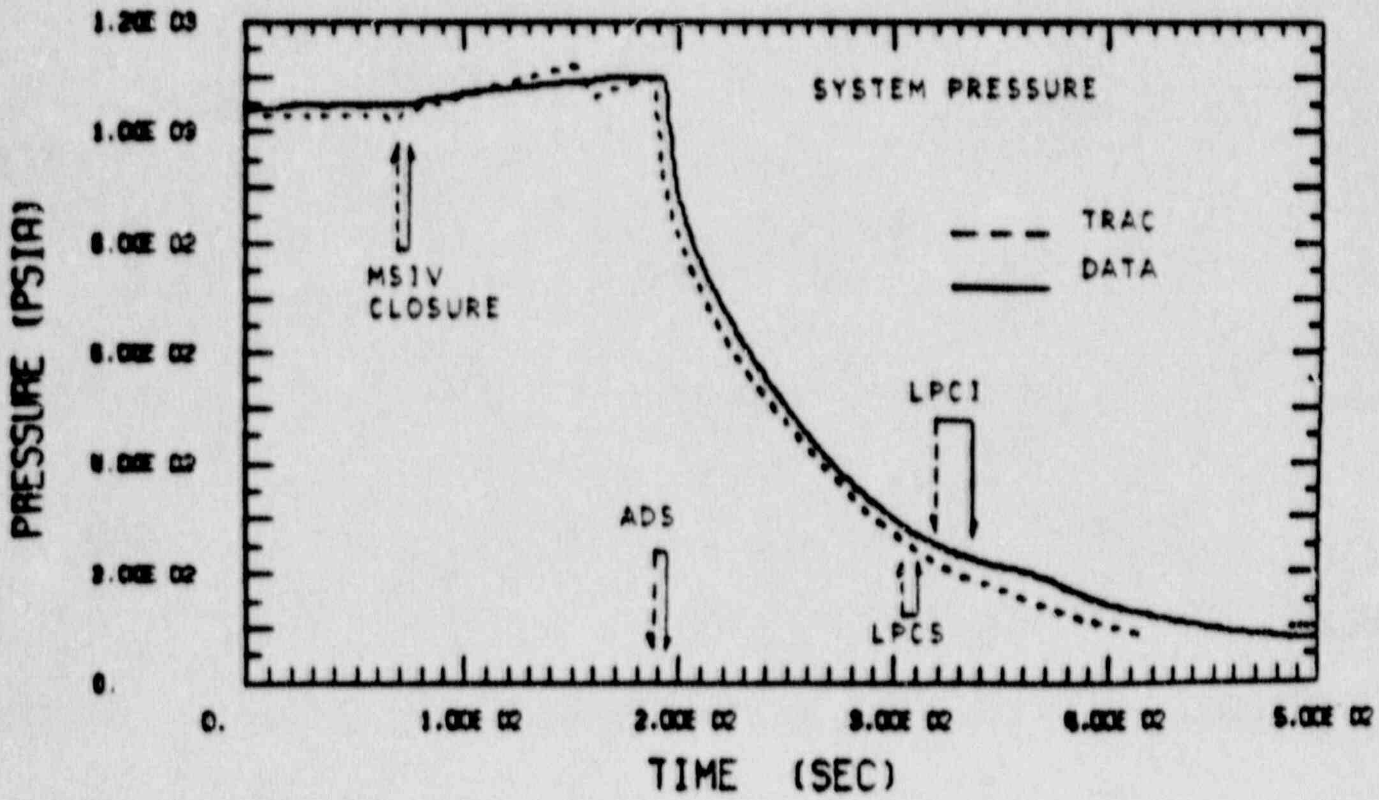


FIGURE 5-1.

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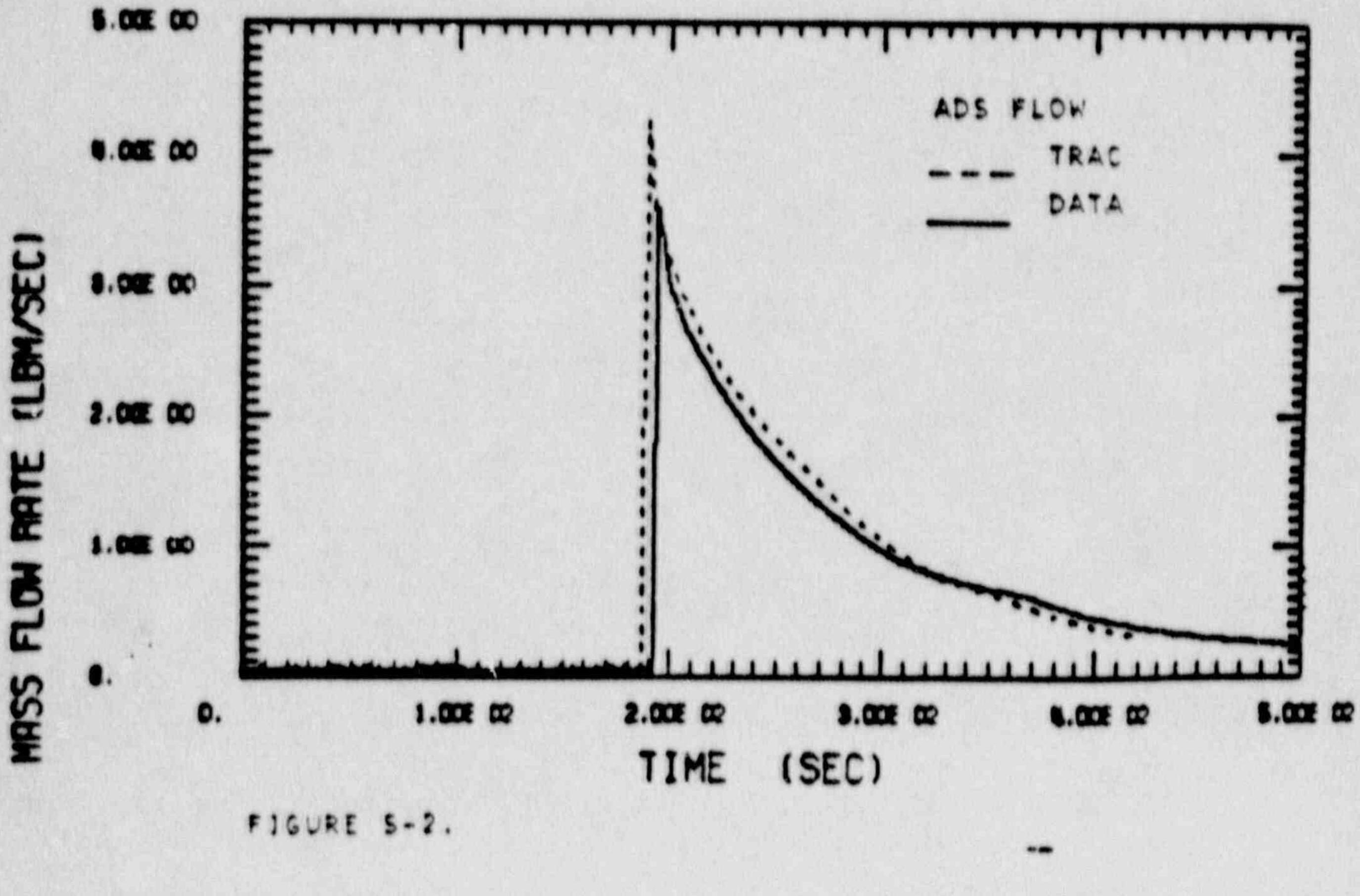

FIGURE 5-2.

Dana
11-08-1984

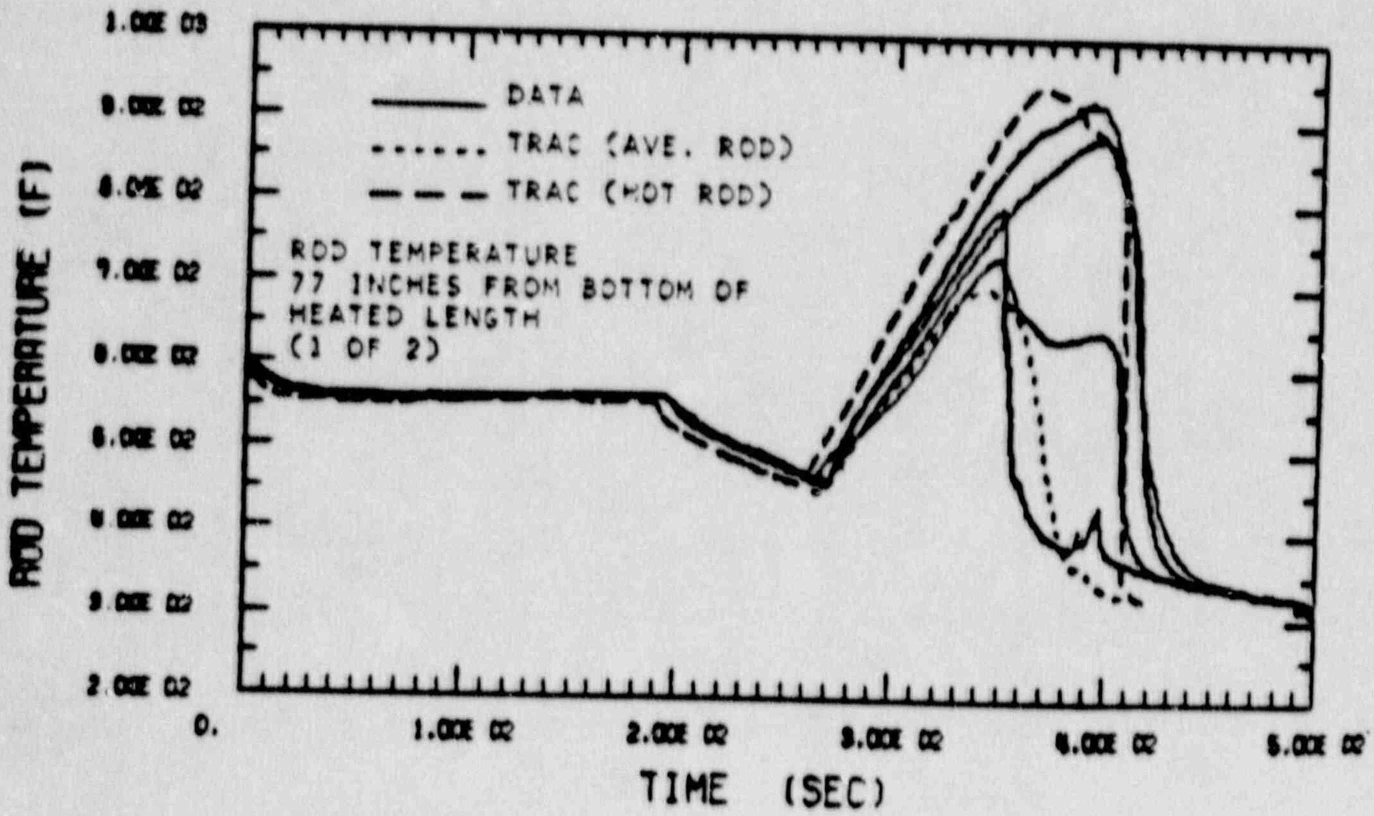

FIGURE 5-4(1)

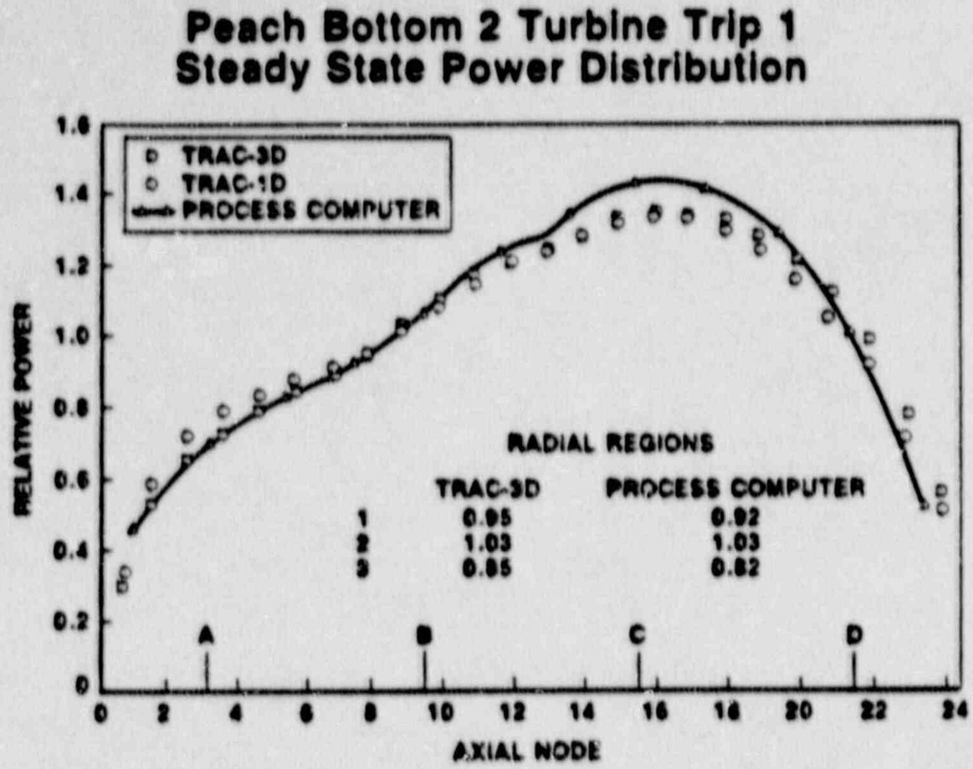

Figure 5. Peach Bottom TT1: Steady state power distribution.

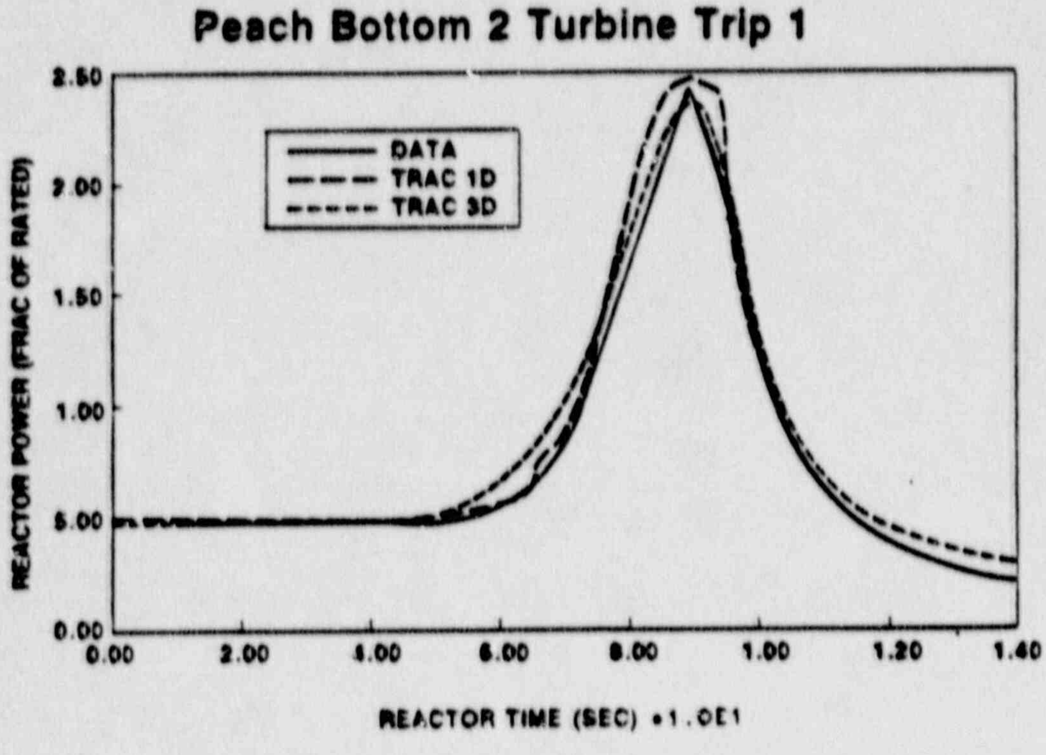

Figure 6. Peach Bottom TT1: transient power response.

TRACG DEVELOPMENT AND QUALIFICATION APPROACH

SYSTEM EFFECTS TESTS AND PLANT DATA

- TLTA AND FIST TESTS - INTEGRAL SYSTEM EFFECTS TESTS
  SCALED SIMULATION OF BWR

- PLANT DATA - START UP DATA, TURBINE TRIP AND STABILITY

- INTEGRAL SYSTEM PERFORMANCE WELL PREDICTED

JGMA
11-08-1989

TRACG DEVELOPMENT AND QUALIFICATION APPROACH

o INDIVIDUAL PHENOMENA ARE WELL PREDICTED

o BWR COMPONENT PERFORMANCE IS WELL PREDICTED

o SYSTEM EFFECTS TESTS AND PLANT PERFORMANCE WELL PREDICTED

```
**********************************************************
*                                                        *
*                                                        *
*     TRACG CAPTURES ALL MAJOR PHENOMENA IN THE BWR      *
*                                                        *
*                                                        *
**********************************************************
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QUALIFICATION FOR BWR STABILITY ANALYSIS

3011a
11-05-89

TRACG TIME DOMAIN ANALYSIS OF THERMAL HYDRAULIC STABILITY SENSITIVITY TO NUMERICAL METHOD AND COMPARISON TO DATA

J. G. M. Andersen

J. C. Shaug

A. L. Wirth

GENERAL ELECTRIC NUCLEAR ENERGY

3671A
11-08-89

TRACG TIME DOMAIN ANALYSIS OF THERMAL HYDRAULIC STABILITY

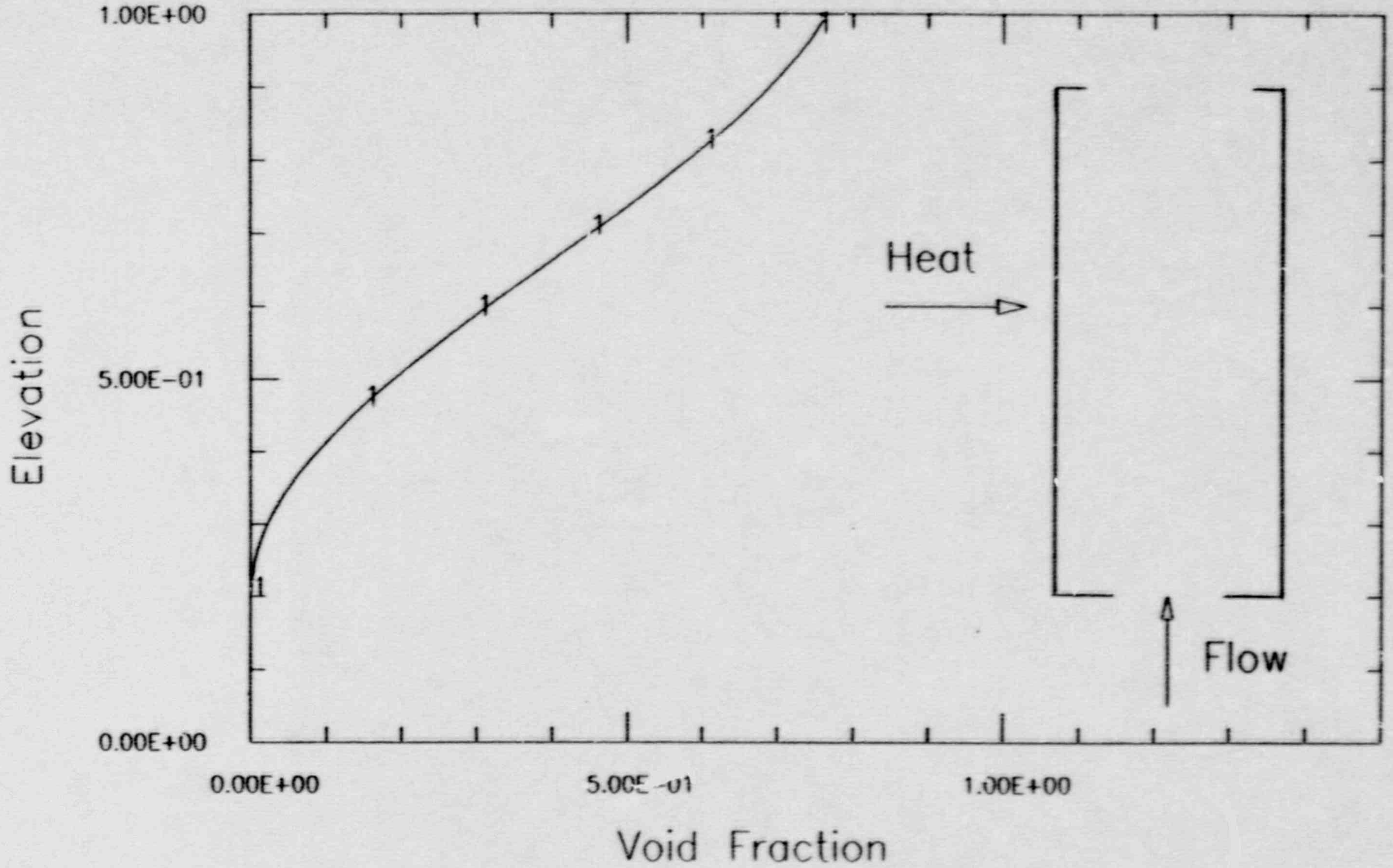
SENSITIVITY TO NUMERICAL METHOD AND COMPARISON TO DATA

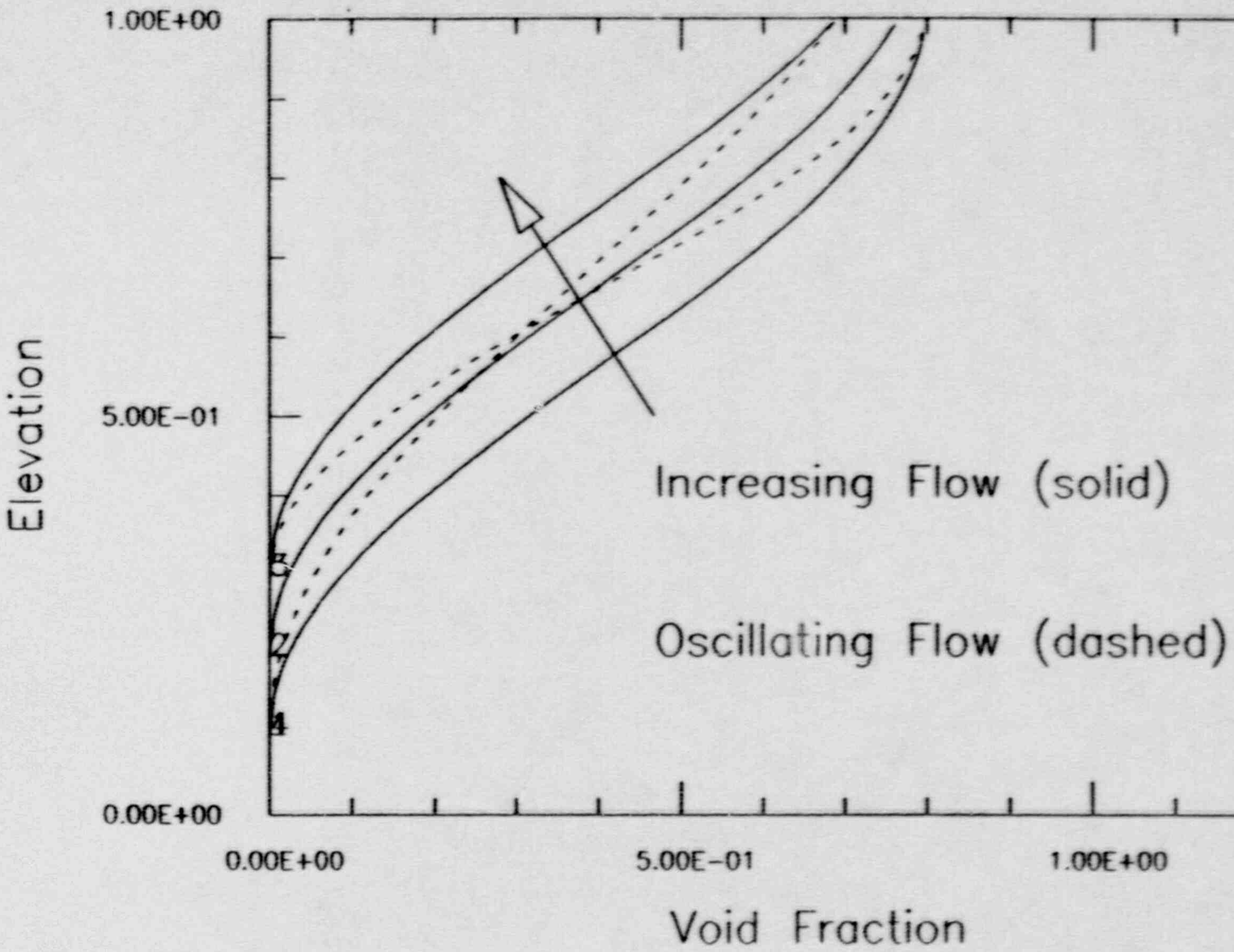
- THERMAL HYDRAULIC STABILITY
  - Frequency Domain / Time Domain

- NUMERICAL DISSIPATION
  - Damping for Various Numerical Methods
  - Comparison of TRACG with Exact Solution

- COMPARISON OF TRACG WITH FRIGG STABILITY DATA

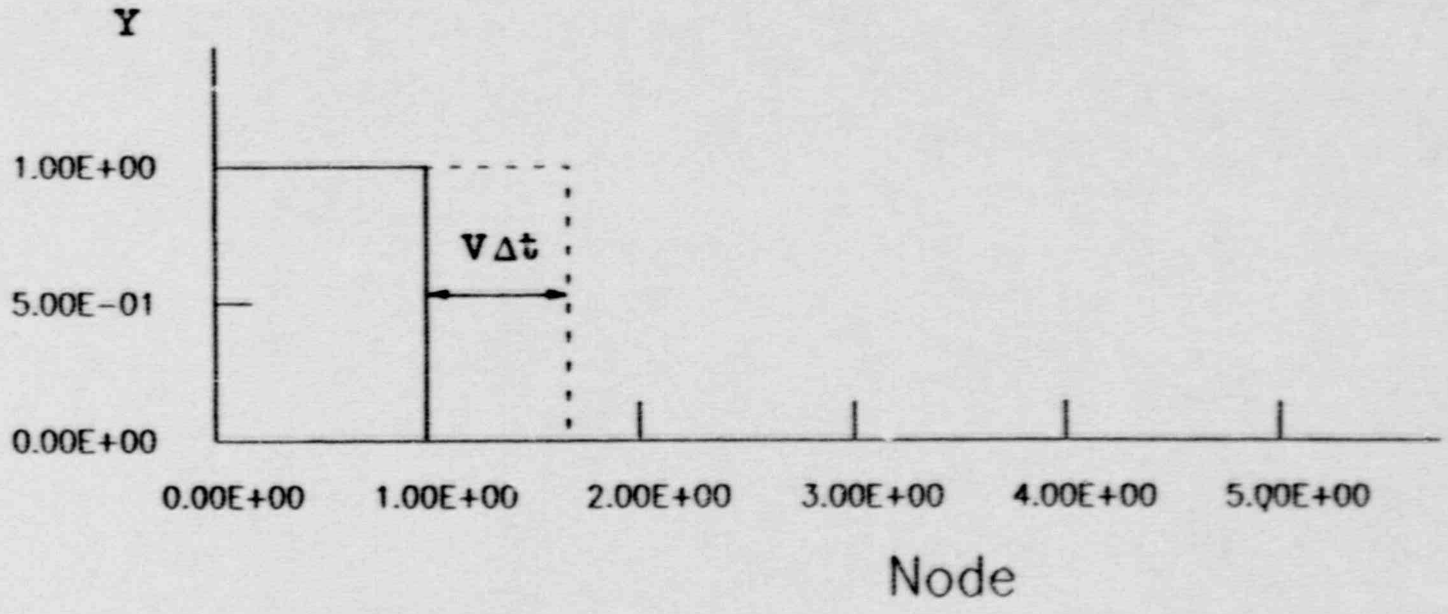
- CONCLUSION


1.00E+00
5.00E-01
0.00E+00
Elevation
0.00E+00
5.00E-01
1.00E+00
Void Fraction
Heat
Flow

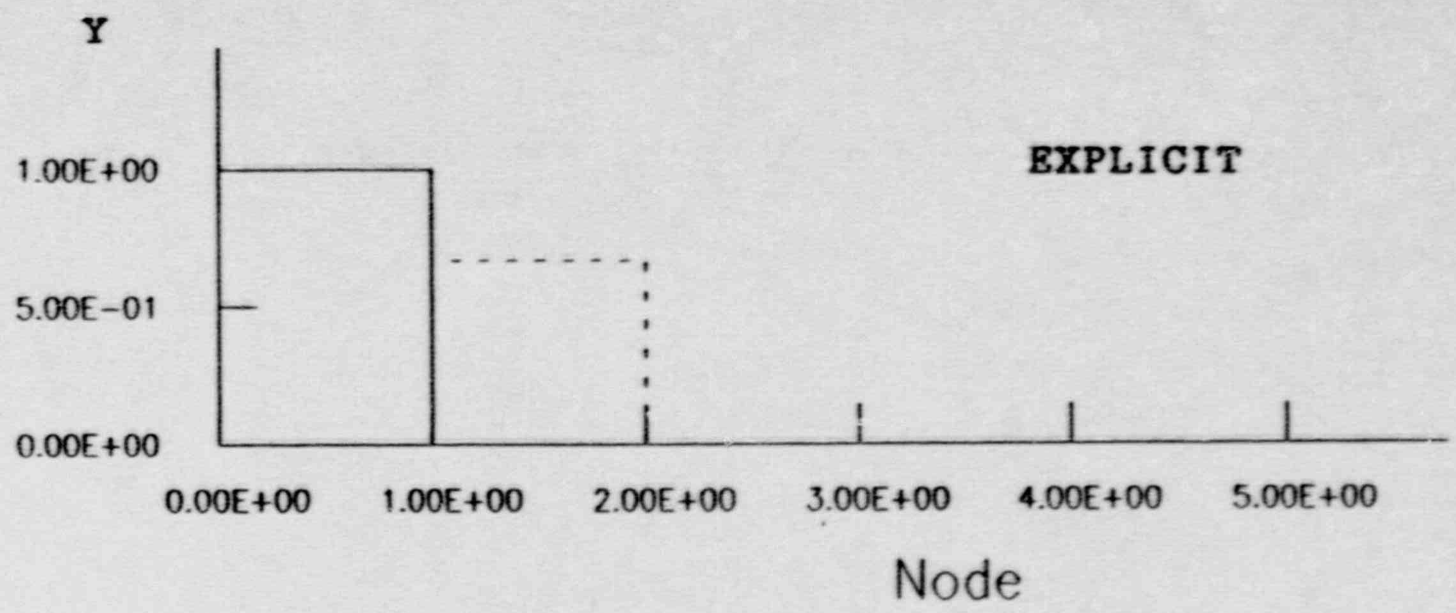


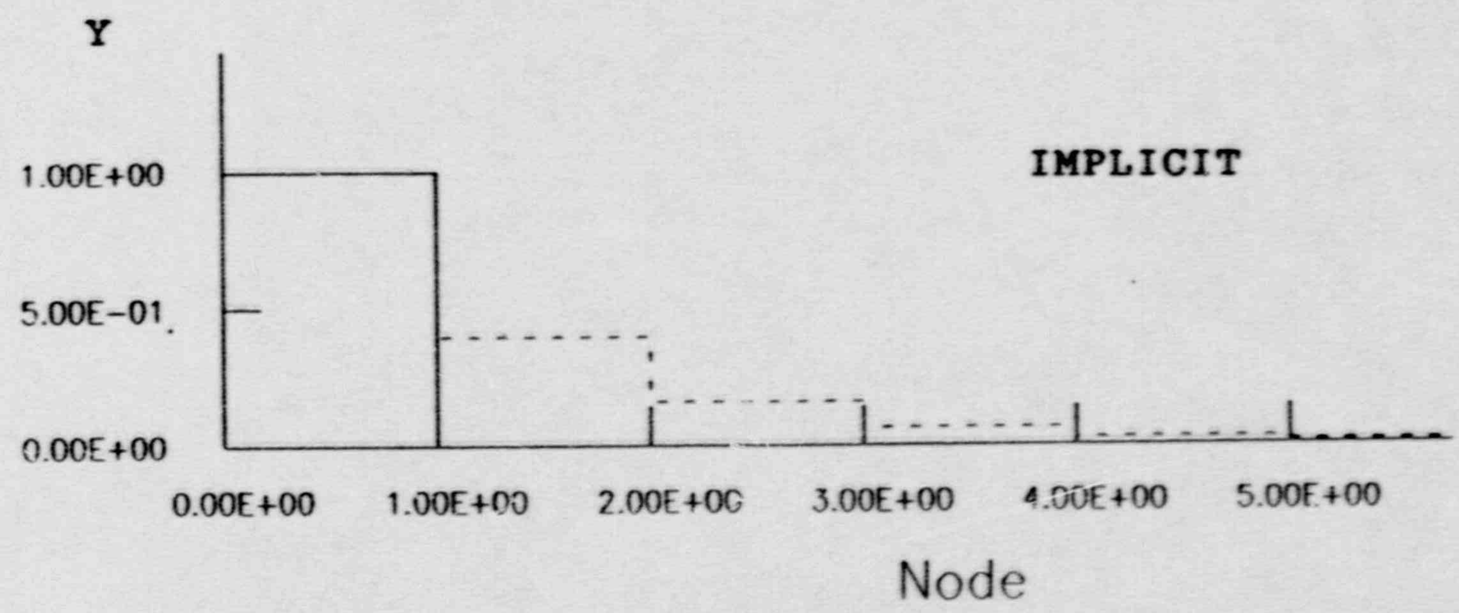
1.00E+00
5.00E-01
0.00E+00
Elevation
0.00E+00
5.00E-01
1.00E+00
Void Fraction
Increasing Flow (solid)
Oscillating Flow (dashed)
3
2
4

$$\frac{\partial Y}{\partial t} = -V\frac{\partial Y}{\partial x}$$



EXACT SOLUTION


Y
1.00E+00
5.00E-01
0.00E+00
EXPLICIT
0.00E+00
1.00E+00
2.00E+00
3.00E+00
4.00E+00
5.00E+00
Node


Y
1.00E+00
5.00E-01
0.00E+00
IMPLICIT
0.00E+00
1.00E+00
2.00E+00
3.00E+00
4.00E+00
5.00E+00
Node

3 G/7a
11-08-89

TRAVELING DAMPED WAVE - EXPLICIT NUMERICAL METHOD

$$Y(x,t) = Y_o e^{-i(\omega t - kx) - \lambda x}$$

$$Y_j^{n+1} - Y_j^n = -C(Y_j^n - Y_{j-1}^n) , \quad V > 0$$

$$\cot(k\Delta x) = \frac{C - 1 + \cos(\omega\Delta t)}{\sin(\omega\Delta t)}$$

$$e^{\lambda\Delta x} = \frac{\sin(\omega\Delta t)}{C \sin(k\Delta x)}$$

Jona
11-08-89

TRAVELING DAMPED WAVE - IMPLICIT NUMERICAL METHOD

$$Y(x,t) = Y_o e^{-i(\omega t - kx) - \lambda x}$$

$$Y_j^{n+1} - Y_j^{n} = -C(Y_j^{n+1} - Y_{j-1}^{n+1}), \quad V>0$$

$$\cot(k\Delta x) = \frac{C + 1 - \cos(\omega\Delta t)}{\sin(\omega\Delta t)}$$

$$e^{\lambda\Delta x} = \frac{\sin(\omega\Delta t)}{C \sin(k\Delta x)}$$

3619
11-08-84

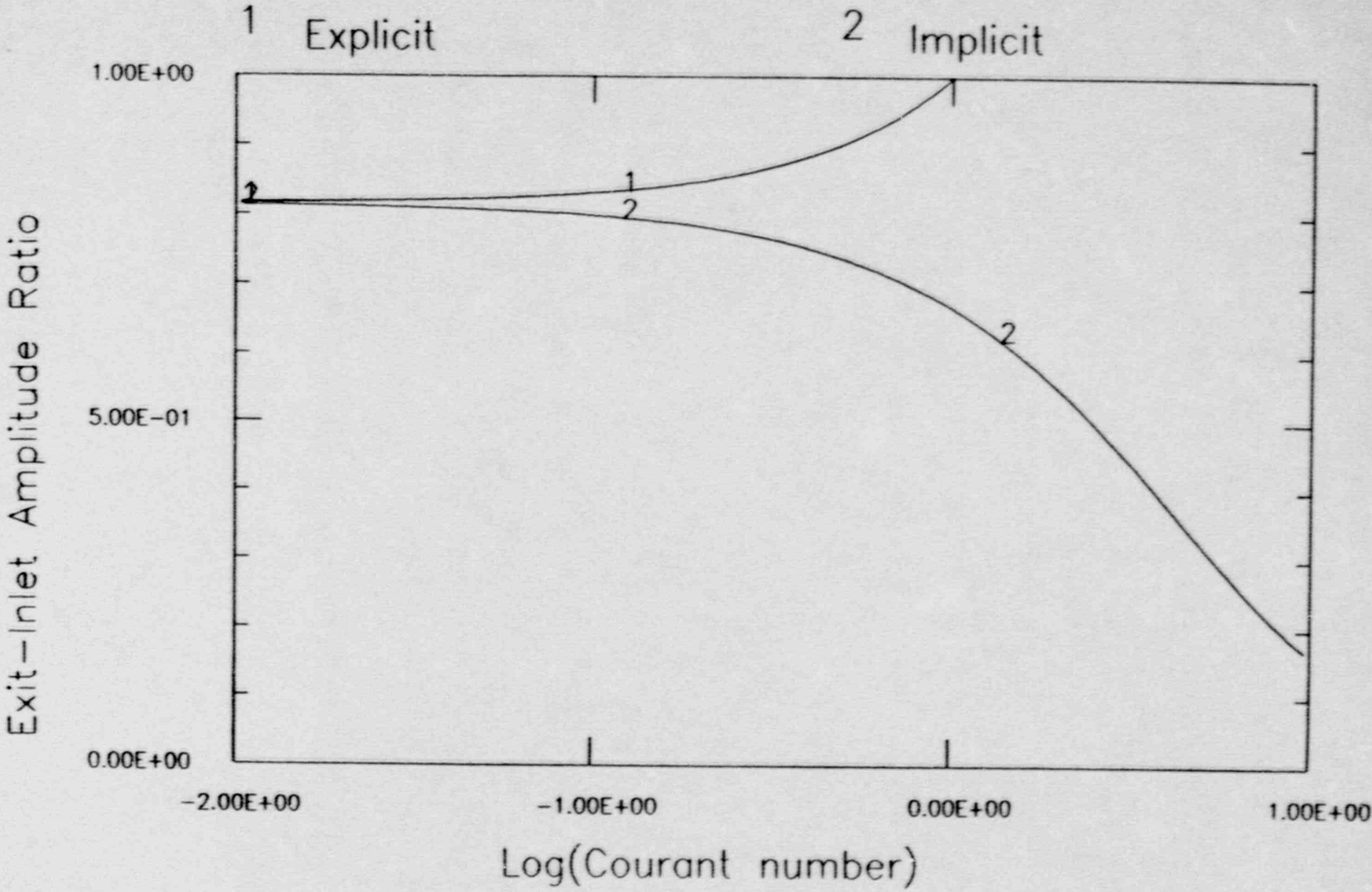
TRAVELING DAMPED WAVE - SECOND ORDER CENTRAL DIFFERENCING

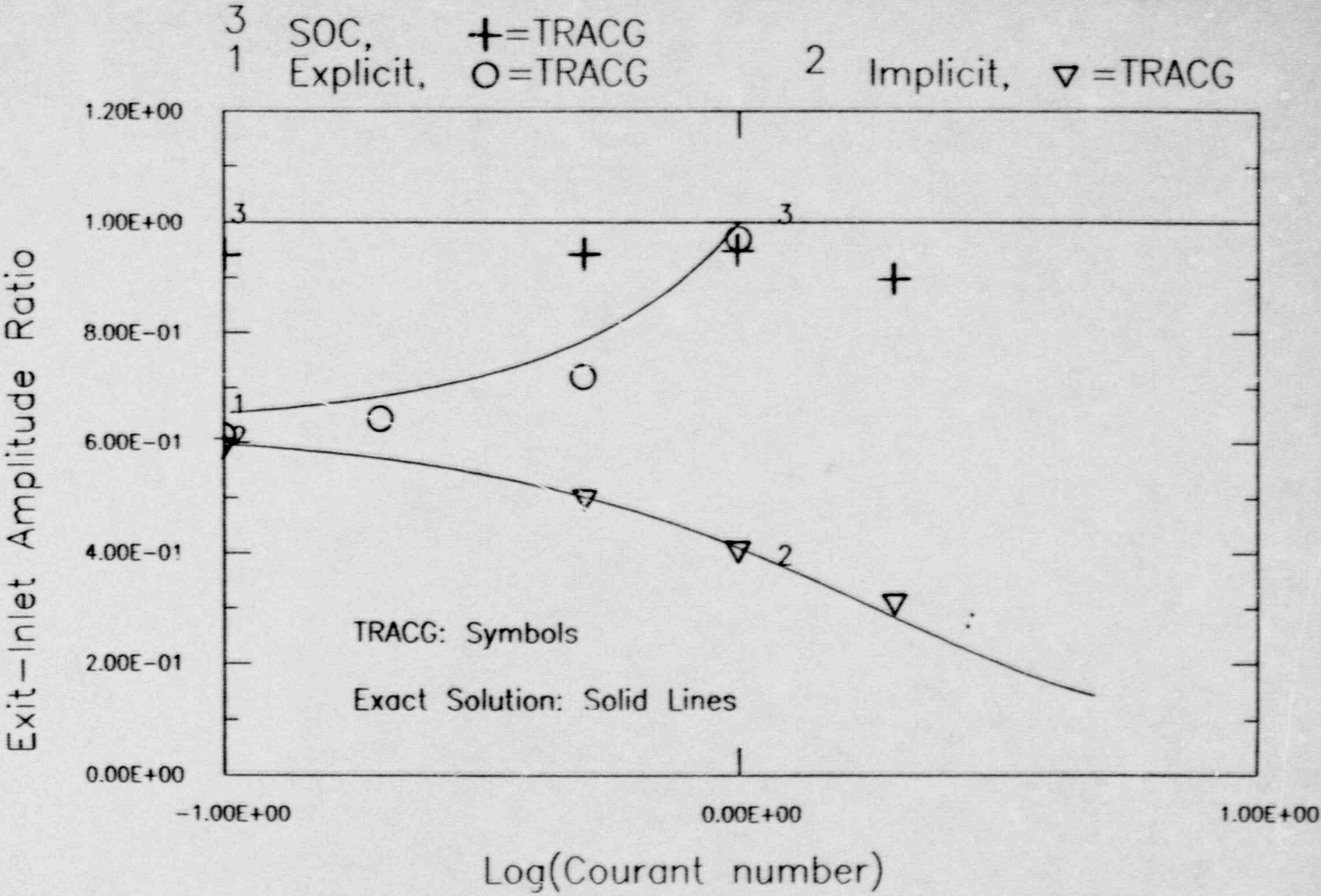
$$Y(x,t) = Y_o e^{-i(\omega t - kx) - \lambda x}$$

$$Y_j^{n+1} - Y_j^{n} = \frac{C}{2}\left(Y_j^{n+1} + Y_{j+1}^{n} - Y_{j-1}^{n+1} - Y_j^{n}\right), \quad V > 0$$

$$\cos(k\Delta x) - 1 + \sin(k\Delta x)\frac{\sin(\omega\Delta t)}{1-\cos(\omega\Delta t)} = \frac{2}{C}$$

$$\lambda = 0$$


1 Explicit
2 Implicit
1.00E+00
5.00E-01
0.00E+00
-2.00E+00
-1.00E+00
0.00E+00
1.00E+00
Exit-Inlet Amplitude Ratio
Log(Courant number)



3 SOC, +=TRACG
1 Explicit, O=TRACG
2 Implicit, ▽ =TRACG
Exit-Inlet Amplitude Ratio
1.20E+00
1.00E+00
8.00E-01
6.00E-01
4.00E-01
2.00E-01
0.00E+00
-1.00E+00
0.00E+00
1.00E+00
Log(Courant number)
TRACG: Symbols
Exact Solution: Solid Lines

TRACG COMPARISON WITH FRIGG DATA

NATURAL CIRCULATION

THERMAL HYDRAULIC INSTABILITY

DECAY RATIO FOR DIFFERENT NUMERICAL METHODS

FRIGG P = 5MPa , Q = 6.58MW (Onset of Instability)

METHOD	DECAY RATIO
First Order Implicit	0.61
First Order Explicit	0.97
Second Order Central	1.08

CONCLUSION

Jona
11-08-89

o FIRST ORDER IMPLICIT METHOD

- More Dissipation than Other Methods
- Substantial Damping for Large Time Step Sizes

o FIRST ORDER EXPLICIT METHOD

- Small Amount of Numerical Dissipation
- Good Agreement with Data:

  Onset Well Predicted

  Limit Cycle Oscillations Tend to be Overpredicted

o SECOND ORDER CENTRAL DIFFERENCING

- No Numerical Damping
- Insensitive to Time Step Size
- Conservative Compared to Data

307a
11-08-89

CONCLUSION

- NUMERICAL DISSIPATION MUST BE MINIMIZED WHEN APPLIYNG TIME DOMAIN CODES FOR STABILITY ANALYSIS.

- THE LACK OF MODEL FOR PHYSICAL DISSIPATION IS LIKELY TO CAUSE TRACG TO OVERPREDICT THE MAGNITUDE OF LIMIT CYCLE OSCILLATIONS.

```
***************************************************************
*                                                             *
* TRACG IS APPLICABLE FOR TIME DOMAIN STABILITY ANALYSIS.     *
*                                                             *
***************************************************************
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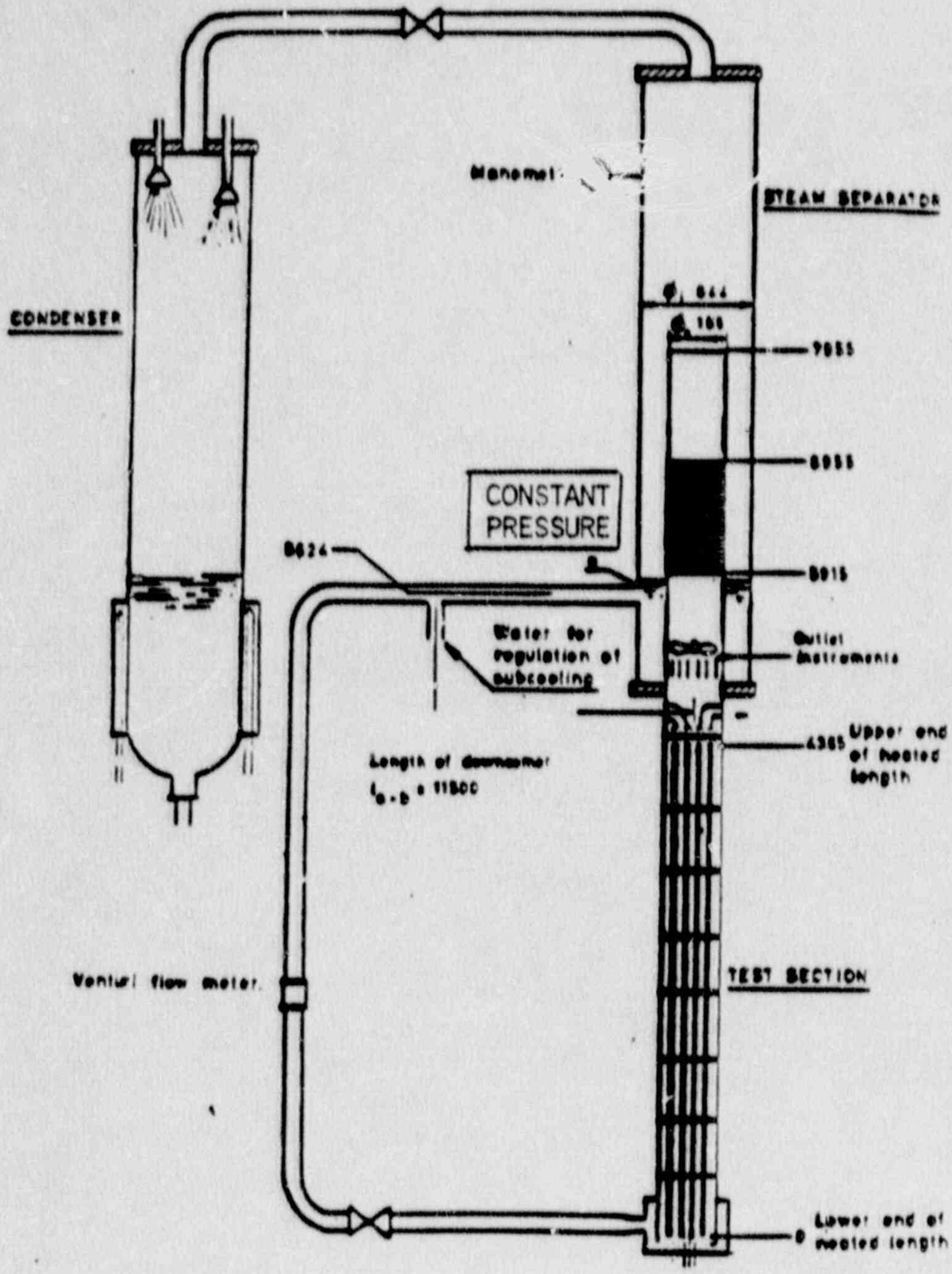
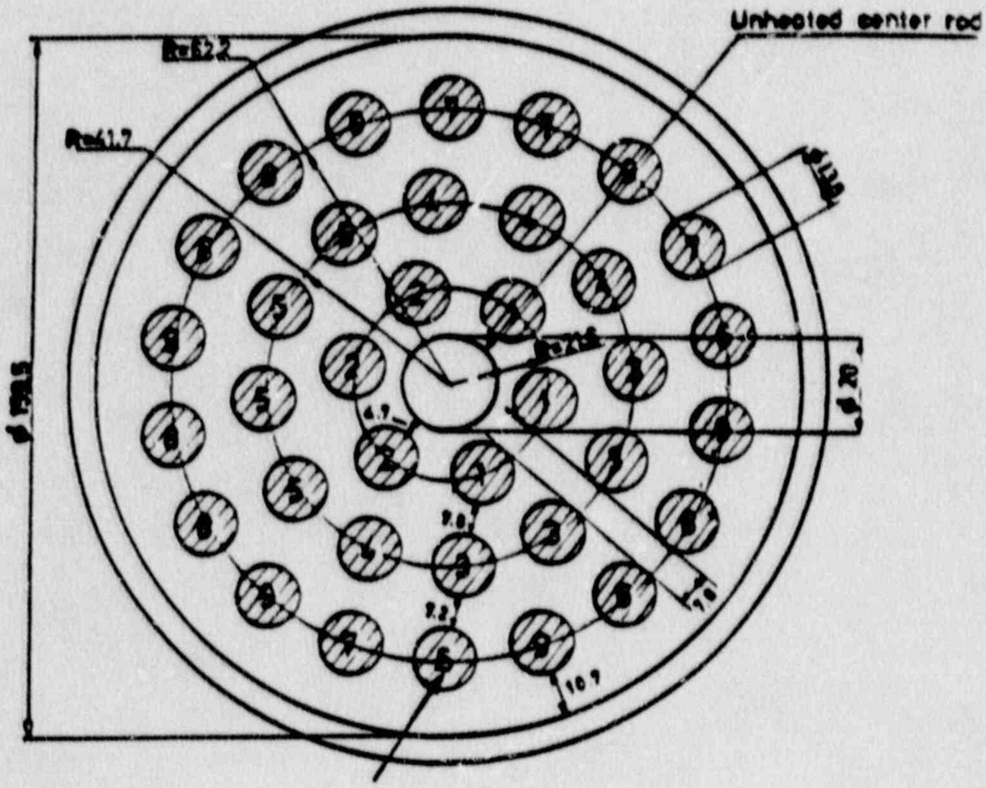
FRIGG NATURAL CIRCULATION
STABILITY TESTS



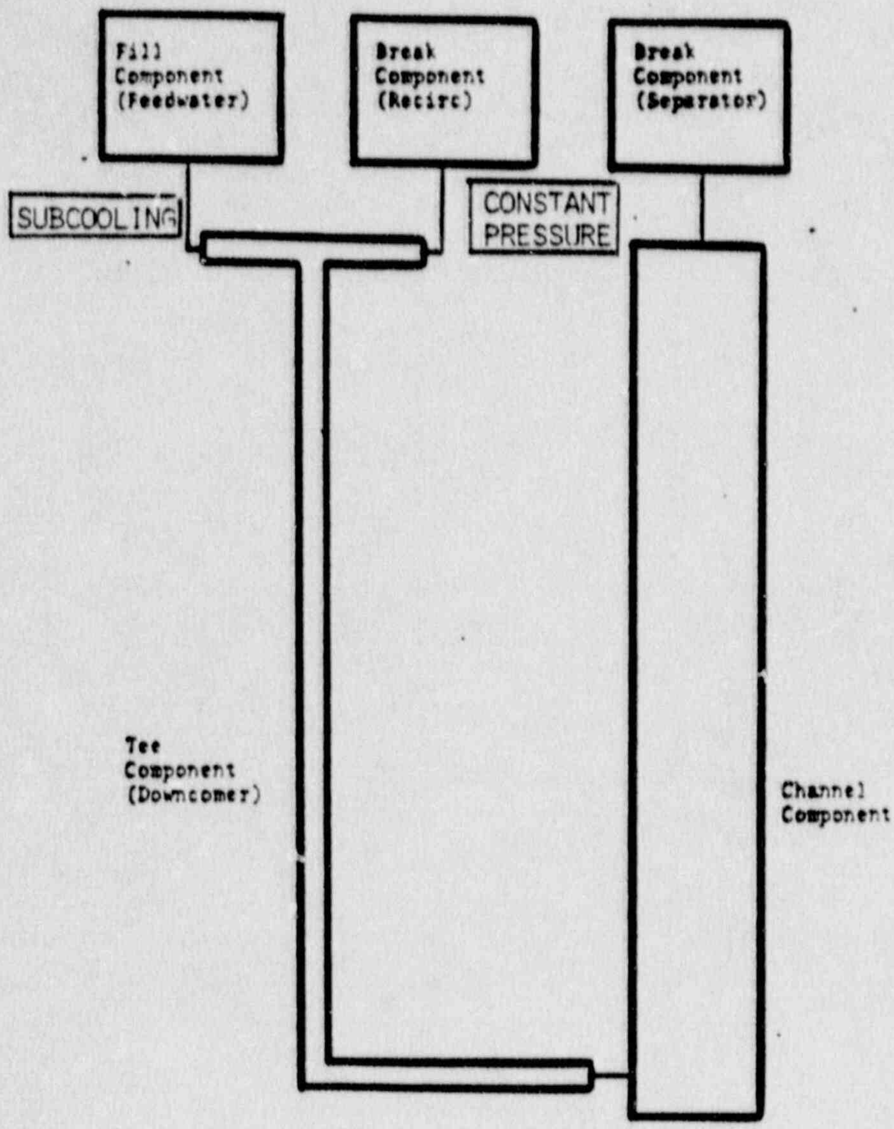
FRIGG TEST LOOP

4.4 M HEATED LENGTH

CENTER PEAKED POWER DISTRIBUTION



36 ROD BUNDLE CONFIGURATION



TRACG LOOP NODALIZATION

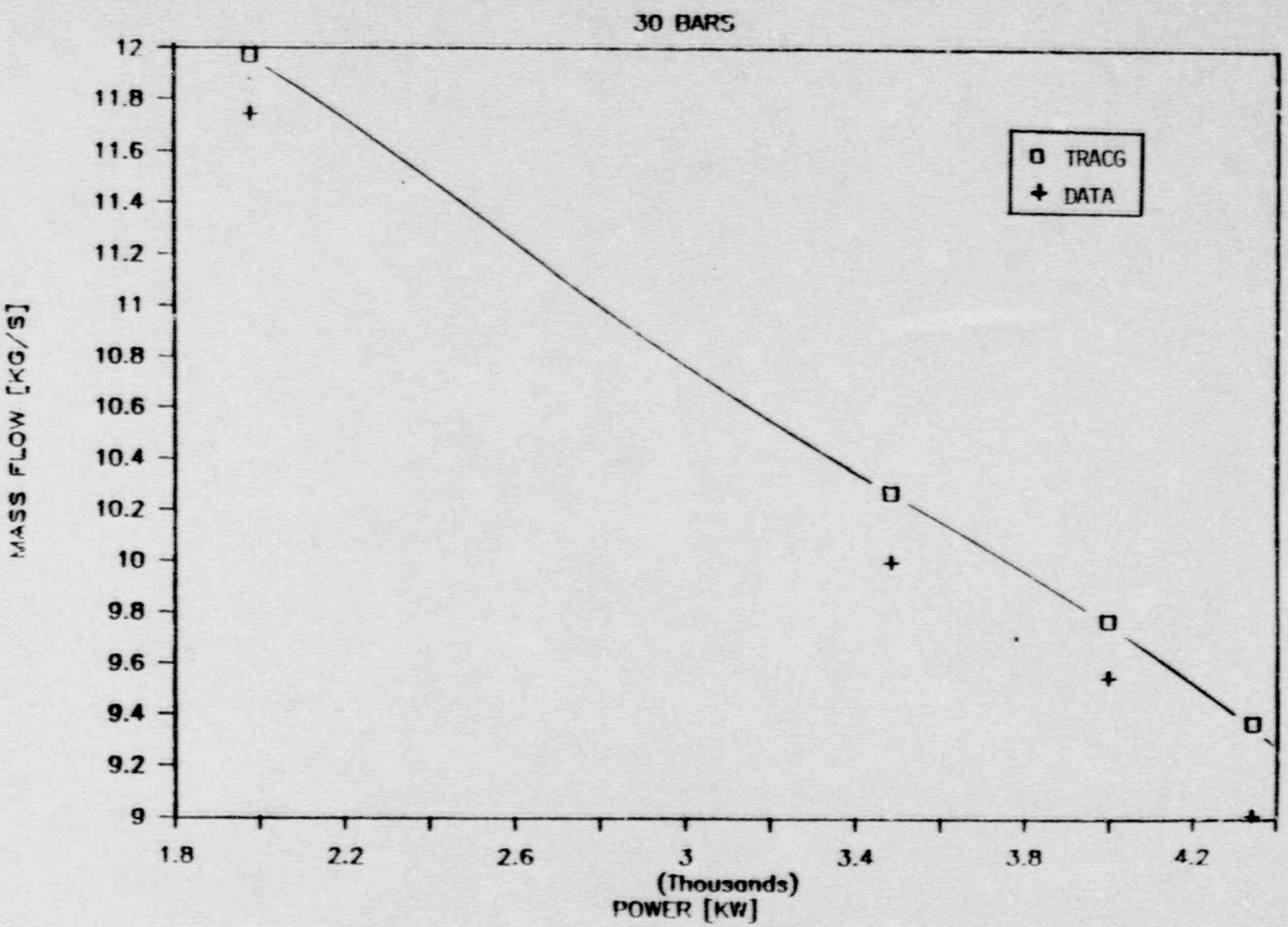
TEST PROCEDURE

- INITIALIZE LOOP TO DESIRED STEADY CONDITIONS AT POWER BELOW EXPECTED INSTABILITY ONSET POWER.
- INCREASE POWER AND HOLD CONSTANT UNTIL STEADY STATE IS OBSERVED. ADJUST FEEDWATER TO OBTAIN DESIRED SUBCOOLING.
- CONTINUE INCREASING POWER UNTIL INSTABILITY ONSET, INDICATED BY OSCILLATIONS IN DOWNCOMER FLOW, IS DETECTED.

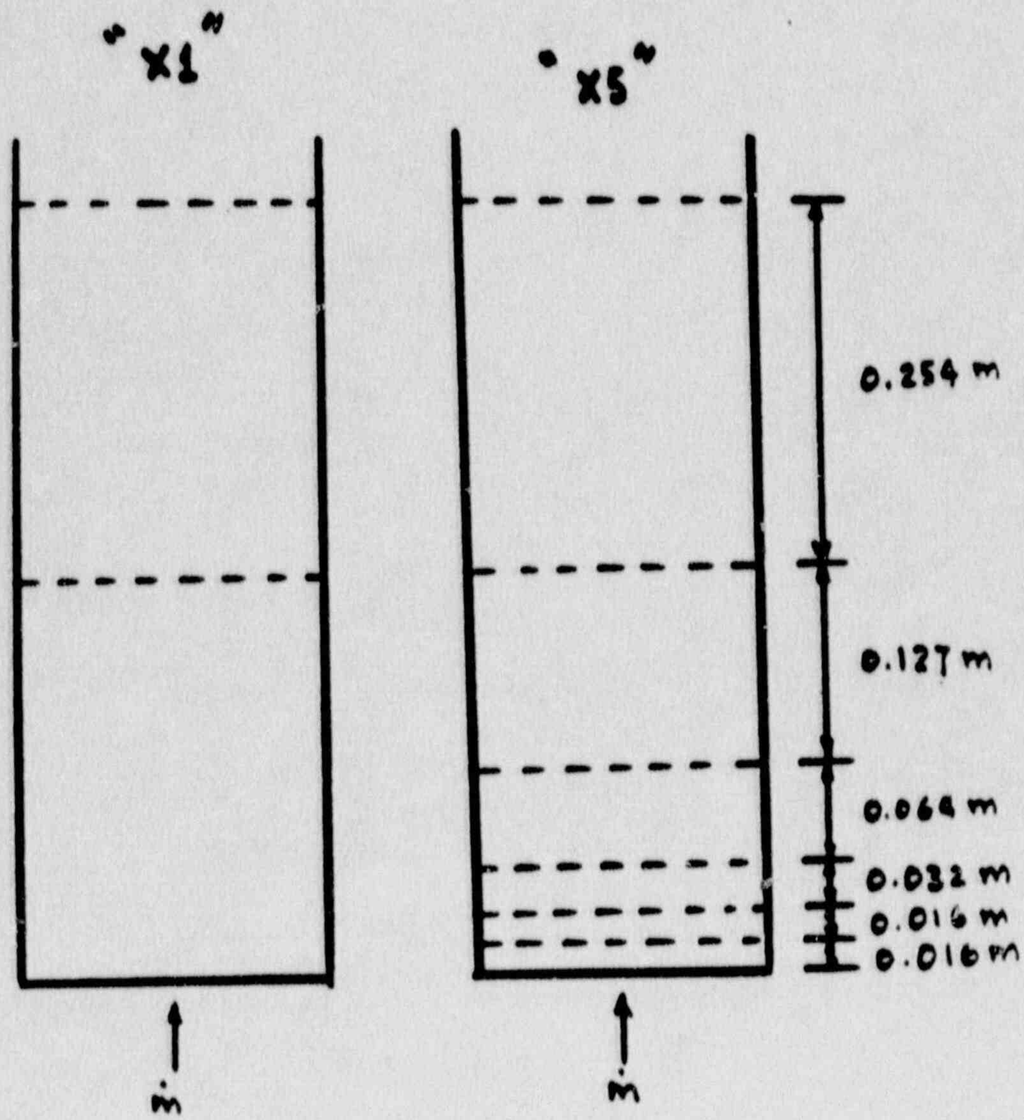
SIMULATION PROCEDURE

- INITIALIZE TO TEST CONDITIONS FOR PREDICTION OF NATURAL CIRCULATION FLOW.
- USING EXPLICIT NUMERICS, ADJUST POWER TO PREDICT INSTABILITY THRESHOLD.

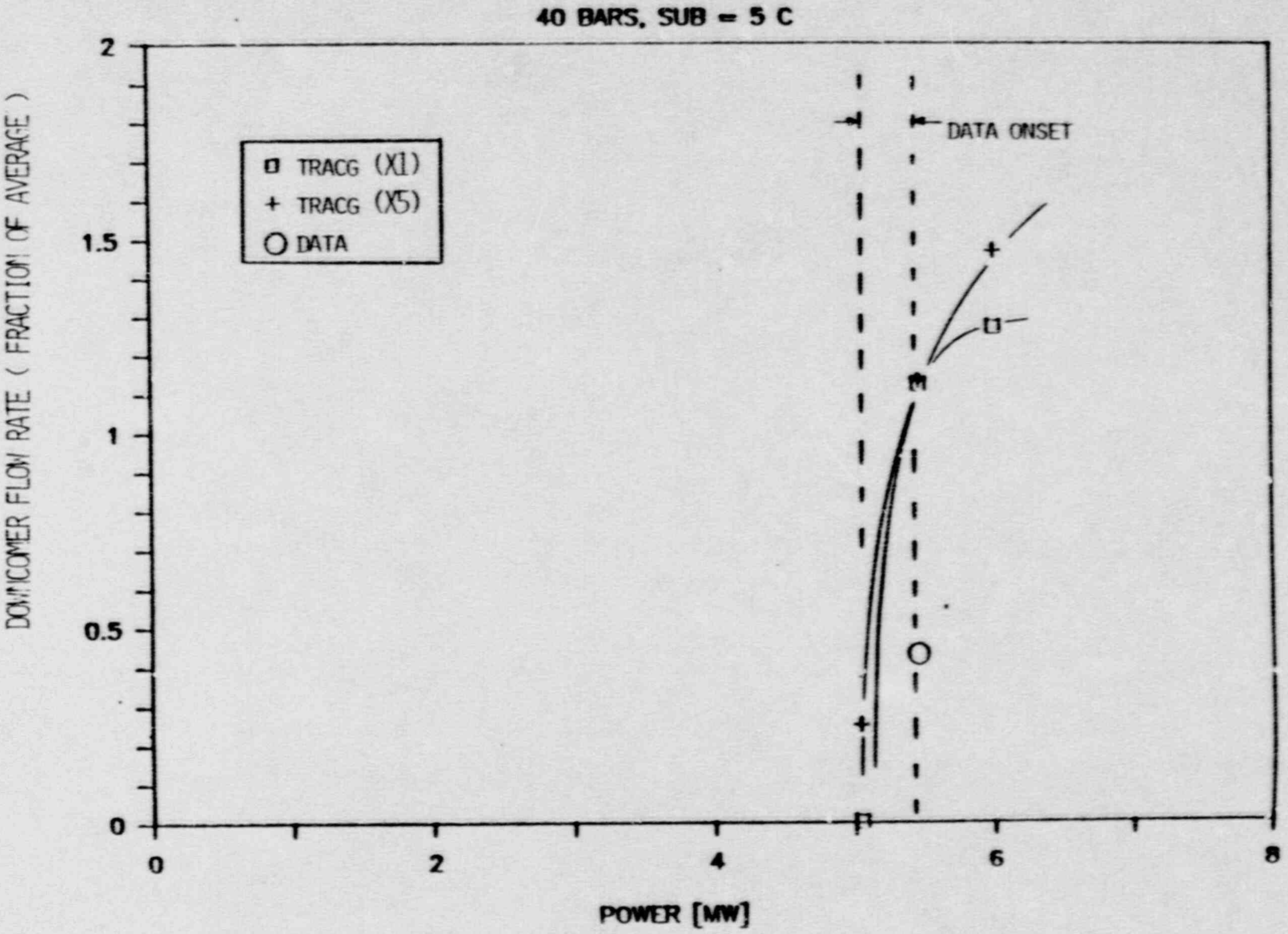
LOOP NATURAL CIRCULATION COMPARISON



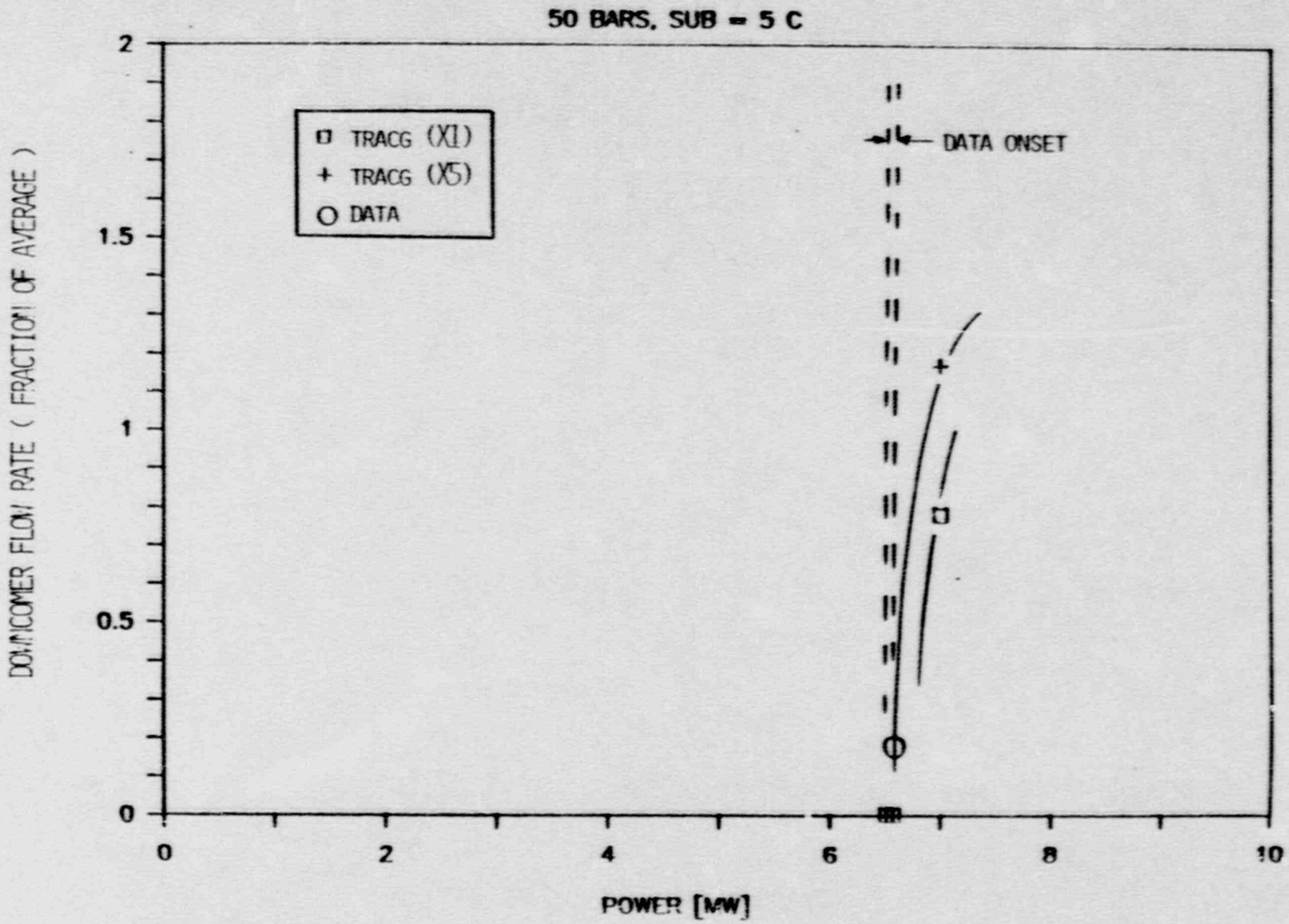
BOILING BOUNDARY NODALIZATION



INSTABILITY ONSET COMPARISON

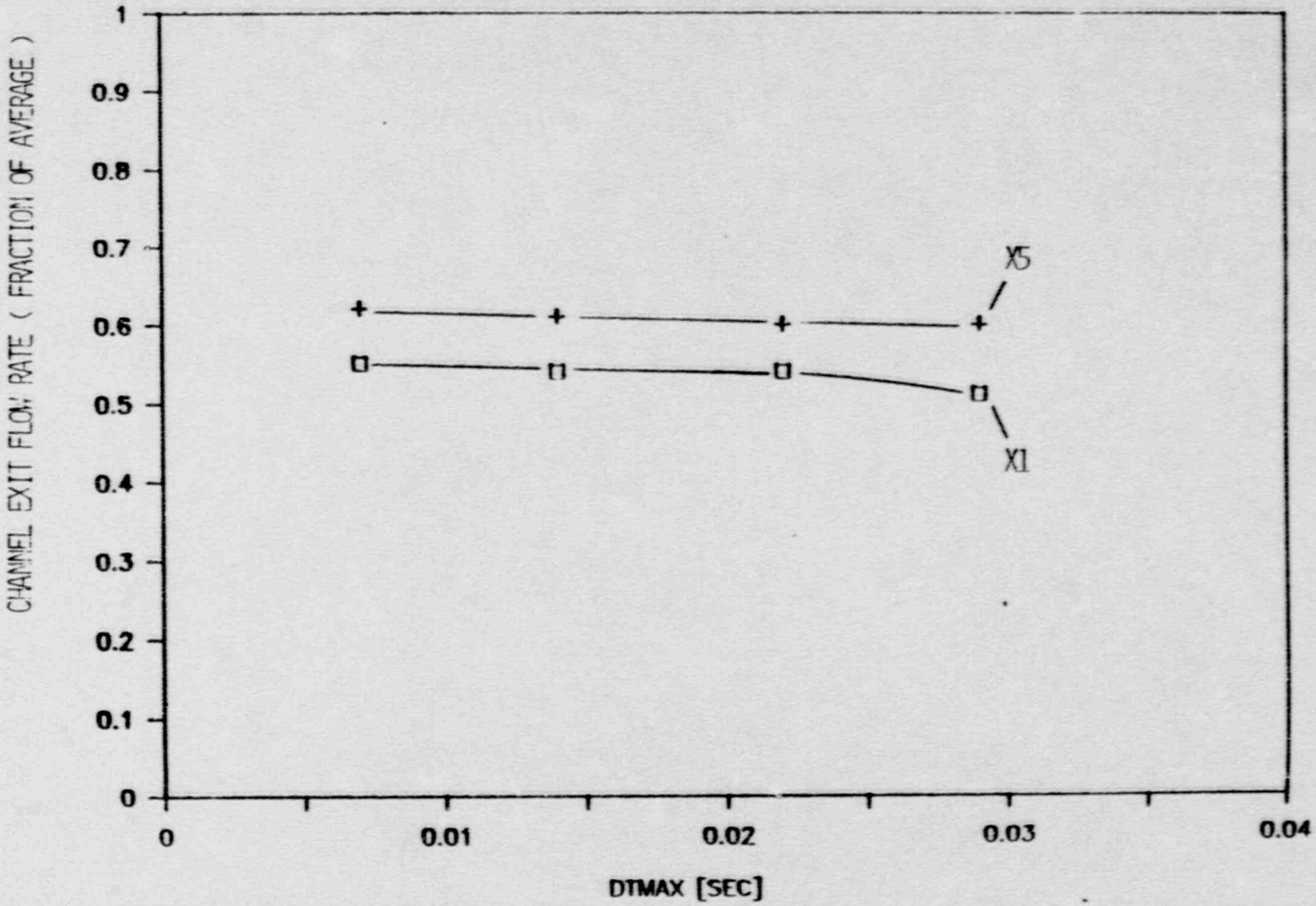


INSTABILITY ONSET COMPARISON

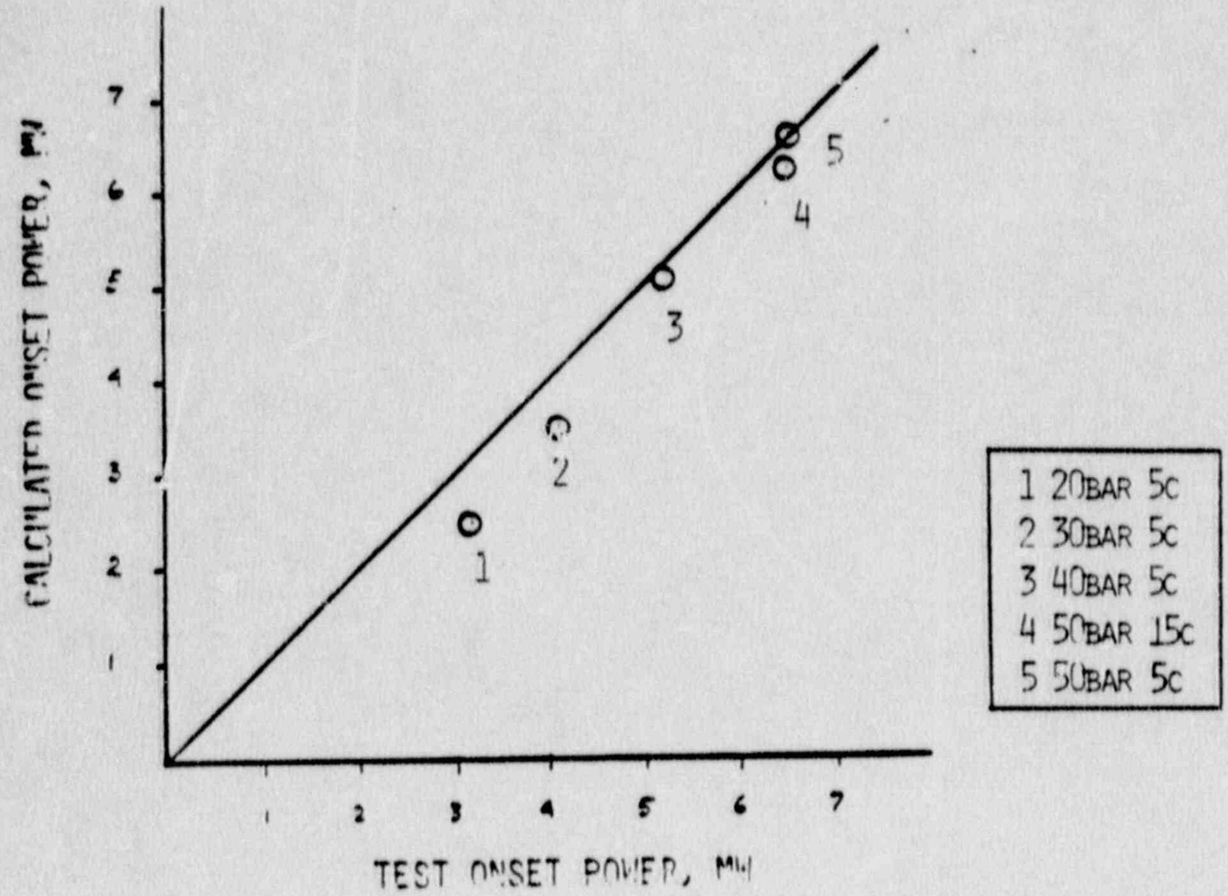


TIME STEP SENSITIVITY

30 BARS, Q = 4.34 MW, SUB = 5 C



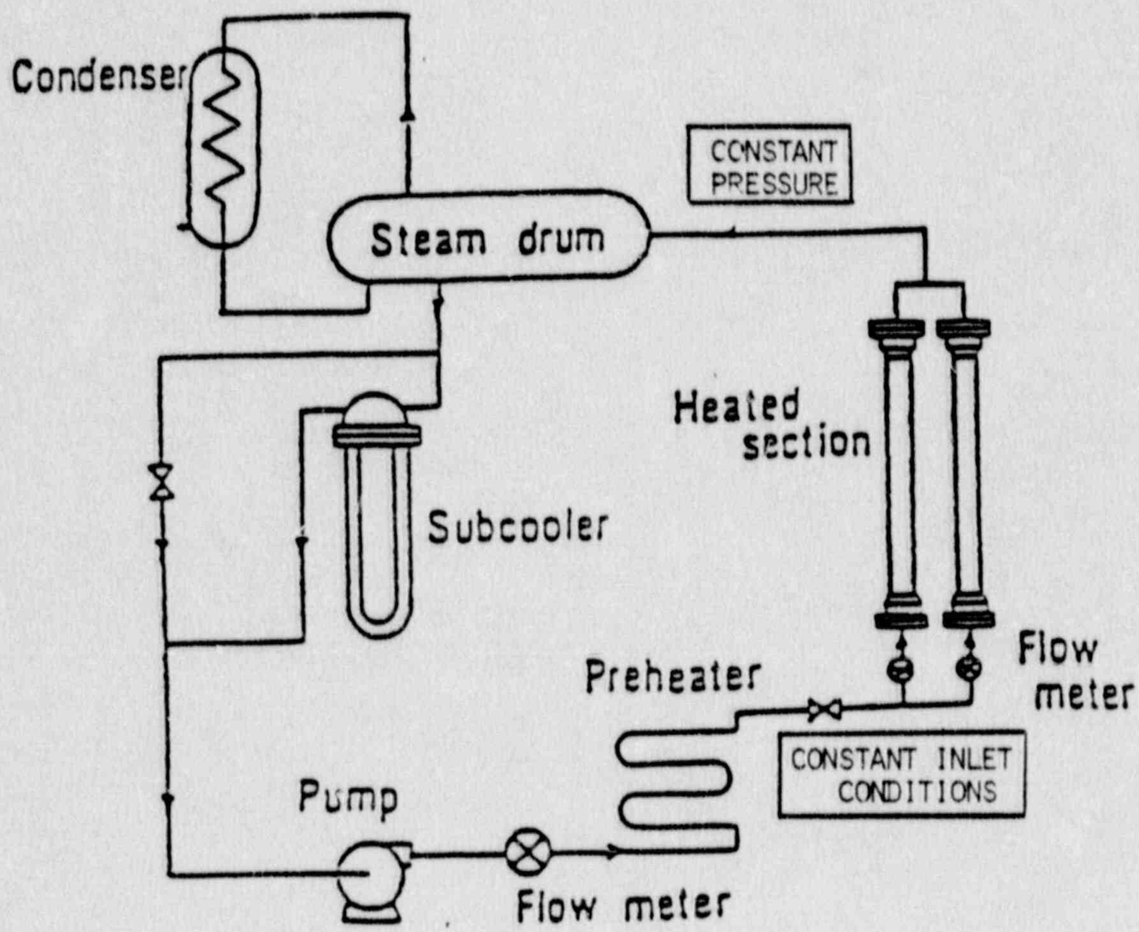
COMPARISON OF CALCULATED OSCILLATION INCEPTION POWER VERSUS TEST DATA



CONCLUSIONS

- TRACG CALCULATIONS OF NATURAL CIRCULATION FLOW VERSUS POWER AGREE WELL WITH DATA.
- ONSET OF INSTABILITY PREDICTED WELL AT PRESSURES ABOVE 30 BARS. AT LOWER PRESSURES CALCULATION IS CONSERVATIVE.
- OSCILLATION AMPLITUDE AND ONSET SLIGHTLY SENSITIVE TO NODALIZATION AT BOILING BOUNDARY.
- LITTLE SENSITIVITY TO TIMESTEP SIZE USING EXPLICIT NUMERICS.

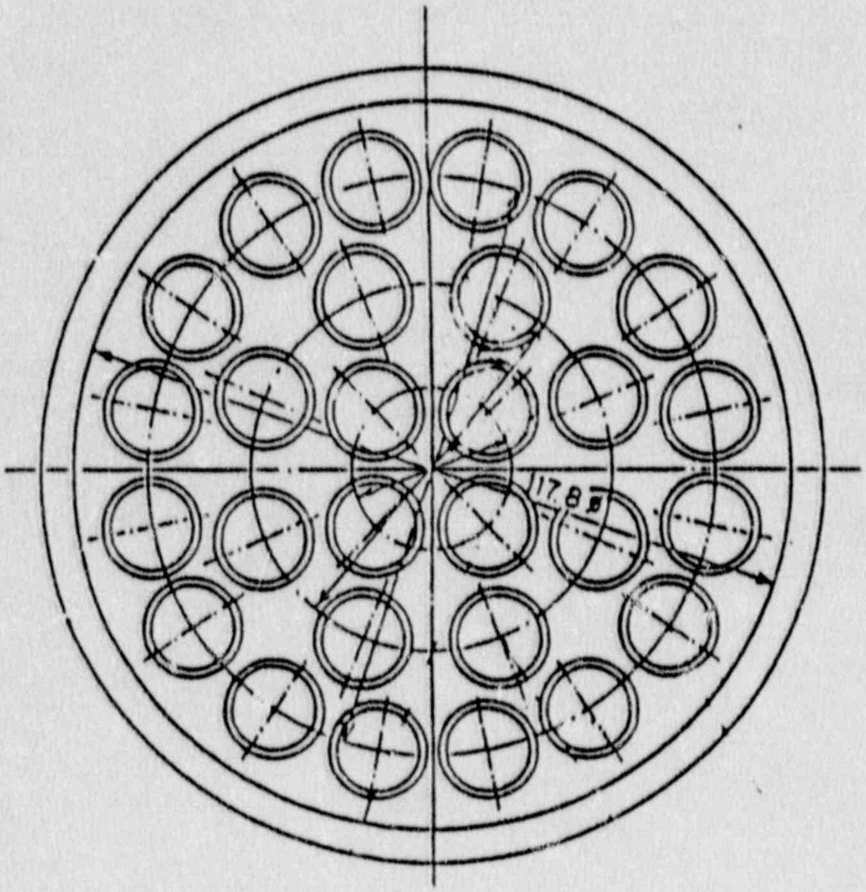
JAPANESE TWO BUNDLE TEST



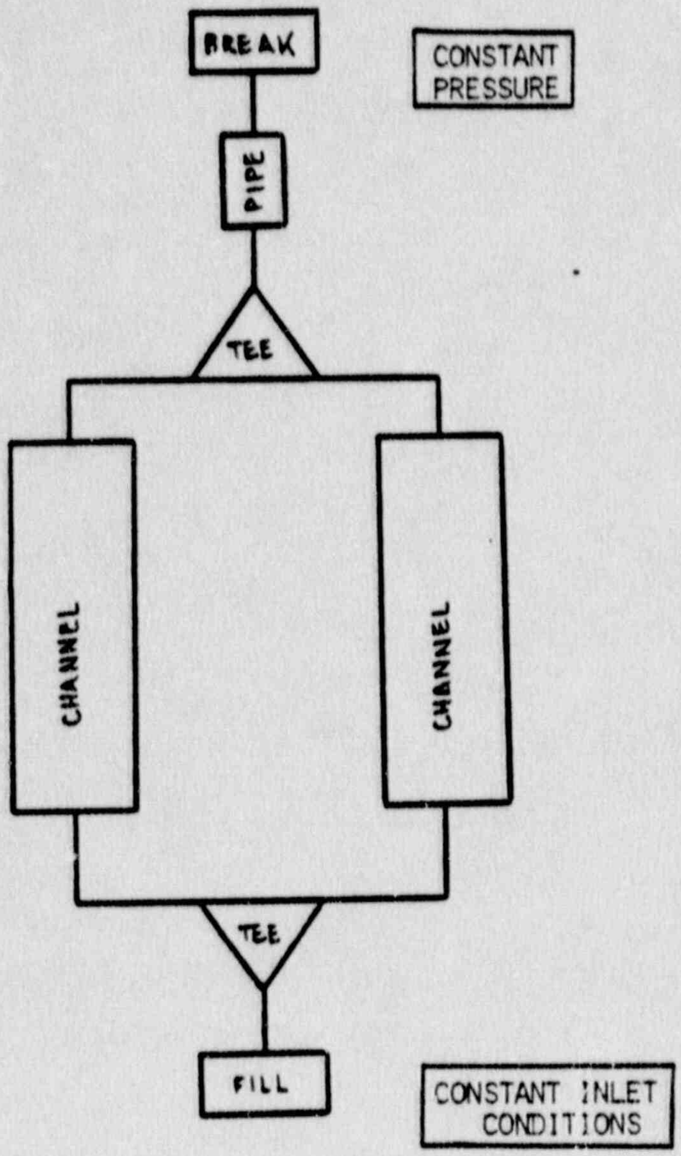
TEST LOOP CONFIGURATION

3.7 M HEATED LENGTH

INLET PEAKED POWER DISTRIBUTION



28 ROD BUNDLE CONFIGURATION



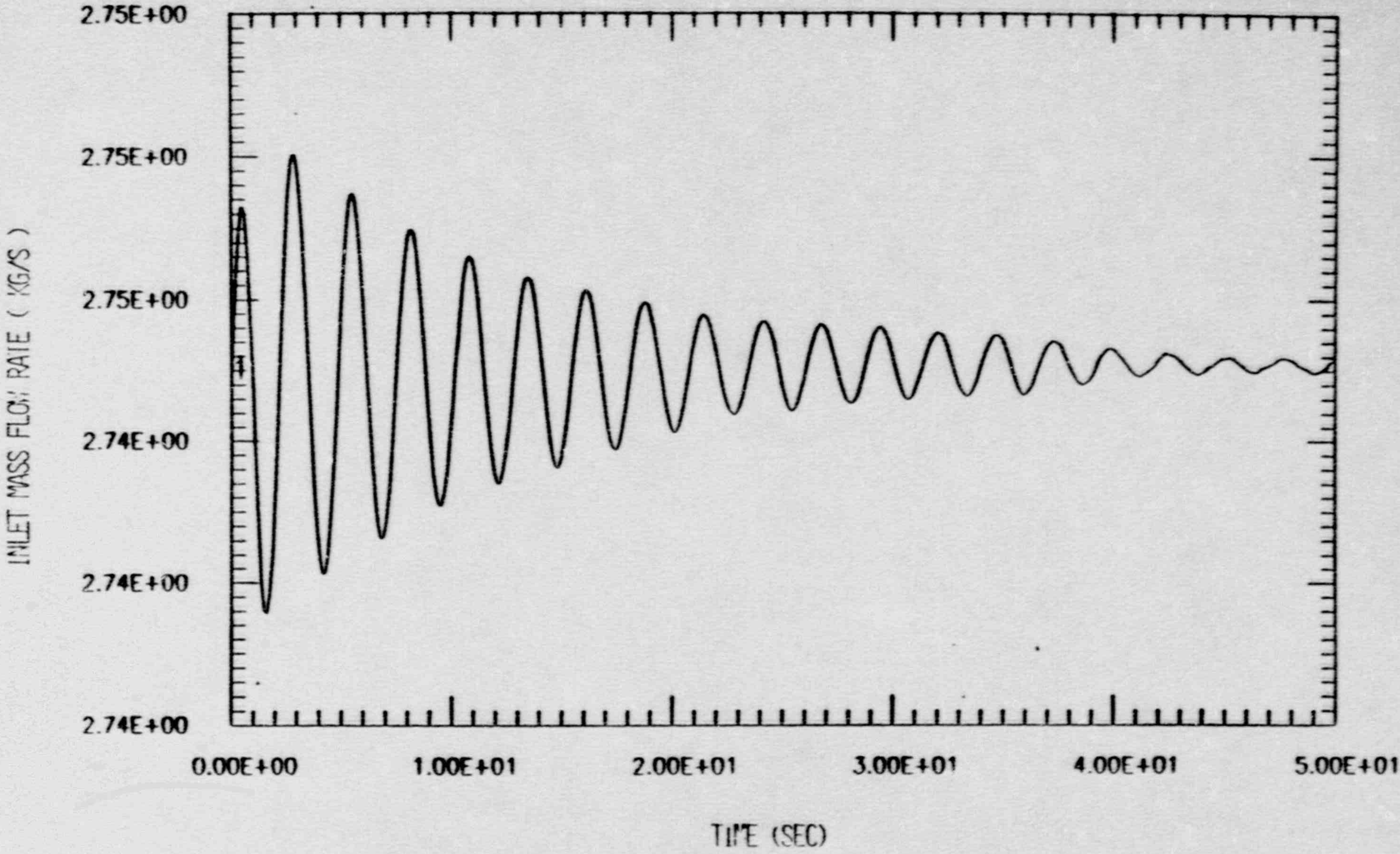
TRACG LOOP NODALIZATION

TEST PROCEDURE

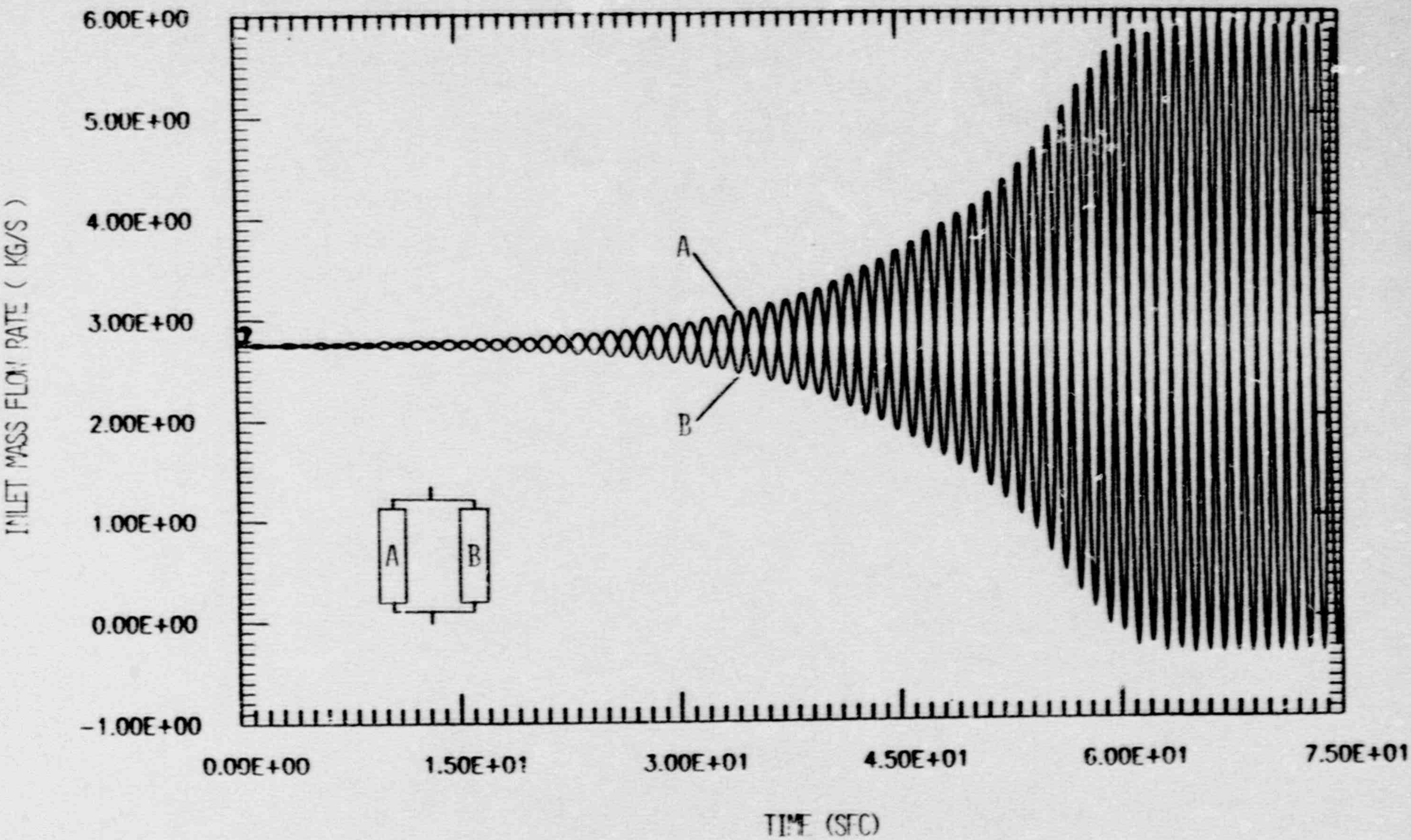
- INITIALIZE LOOP TO DESIRED STEADY CONDITIONS AT POWER BELOW EXPECTED INSTABILITY ONSET POWER.
- INCREASE POWER TO BOTH CHANNELS AND HOLD CONSTANT UNTIL STEADY STATE IS OBSERVED.
- CONTINUE INCREASING POWER UNTIL INSTABILITY ONSET, INDICATED BY OSCILLATIONS IN FLOW AT CHANNEL INLET, IS DETECTED.
- TESTS PERFORMED AT DIFFERENT SETTINGS OF INLET VALVES.

SIMULATION PROCEDURE

- INITIALIZE TO TEST CONDITIONS AT INSTABILITY ONSET POWER LEVEL.
- ADJUST POWER TO BOUND INSTABILITY THRESHOLD.



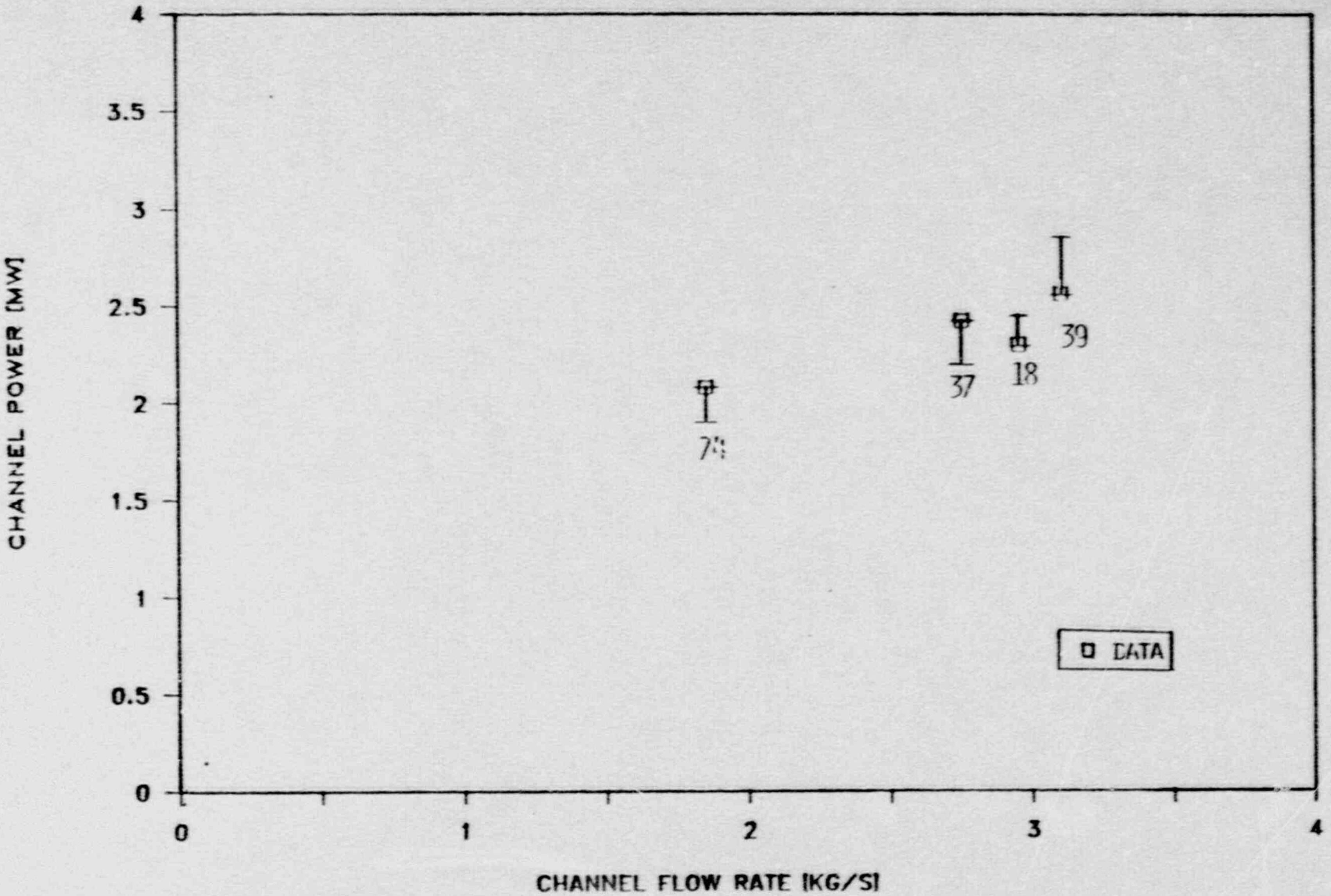
TRAG CALCULATION - EXAMPLE OF STABLE CONDITION

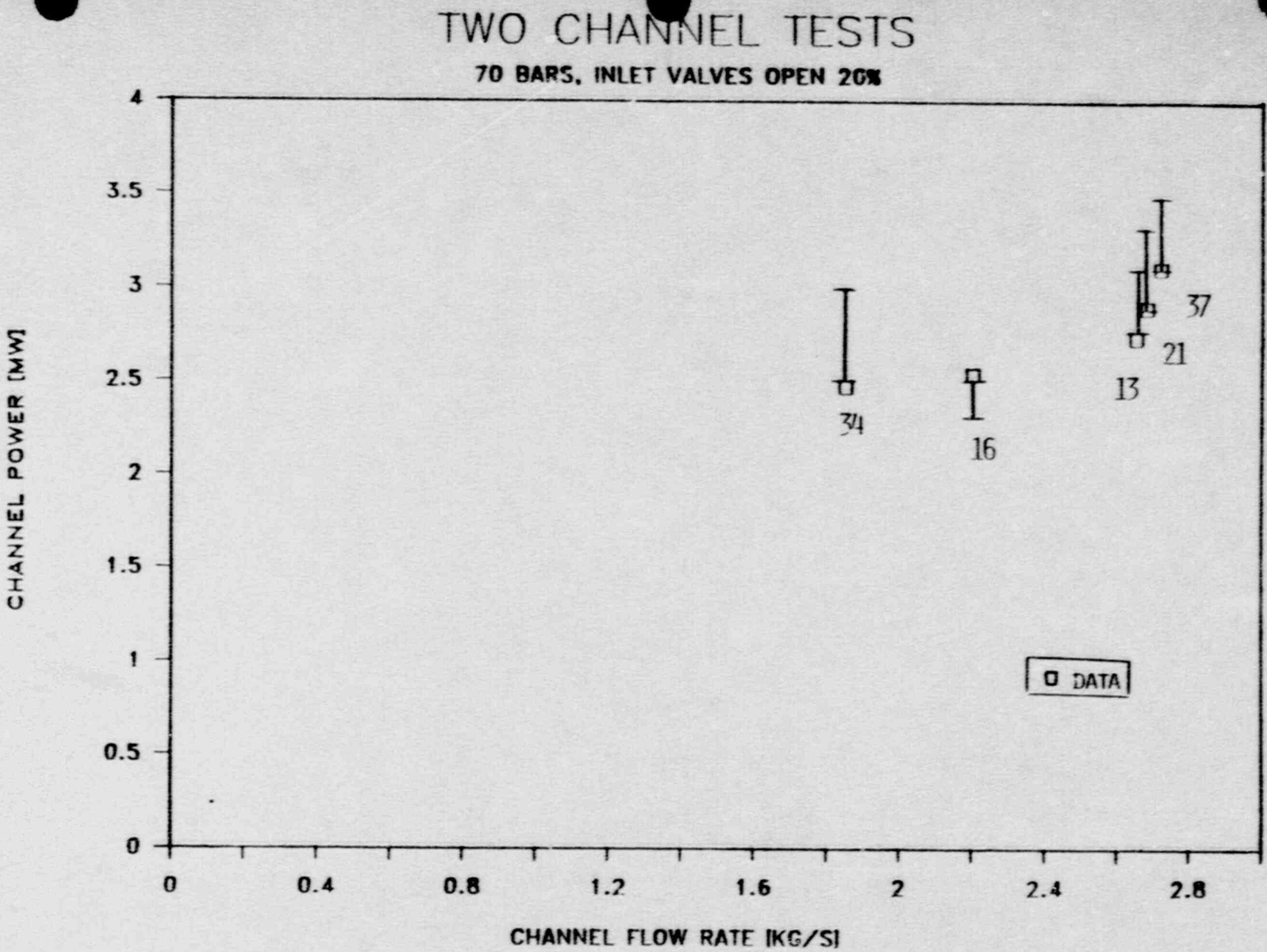


TRACG CALCULATION - EXAMPLE OF UNSTABLE CONDITION

TWO CHANNEL TESTS

70 BARS, INLET VALVES OPEN 100%





TWO CHANNEL TESTS
70 BARS, INLET VALVES OPEN 20%
CHANNEL POWER [MW]
CHANNEL FLOW RATE [KG/S]
0
0.5
1
1.5
2
2.5
3
3.5
4
0
0.4
0.8
1.2
1.6
2
2.4
2.8
34
16
13
21
37
DATA

CONCLUSIONS

- TRACG PREDICTIONS OF INSTABILITY THRESHOLD POWER ARE IN GOOD AGREEMENT WITH DATA.

TRACG HYDRAULIC STABILITY CALCULATIONS QUALIFIED AGAINST SINGLE CHANNEL AND PARALLEL CHANNEL DATA.

GEXL APPLICATION TO STABILITY ANALYSIS

TESTING SUMMARY

- IN 1979, GE CONDUCTED THERMAL HYDRAULIC STABILITY TESTS IN THE ATLAS HEAT TRANSFER LOOP TEST FACILITY.

- THE ATLAS STABILITY TESTS USED ELECTRICALLY HEATED FULL SIZE BWR BUNDLES WITH INLET PEAKED AXIAL POWER SHAPES AT CONDITIONS SIMULATING NATURAL CIRCULATION.

- TWO BASIC TYPES OF TESTS WERE PERFORMED.

  - LIMIT CYCLE CRITICAL POWER TESTS

    POWER INCREASED BEYOND INSTABILITY THRESHOLD UNTIL BOILING TRANSITION (BT) WAS OBTAINED. TEST POWER THEN HELD CONSTANT.

  - POWER OSCILLATION TESTS

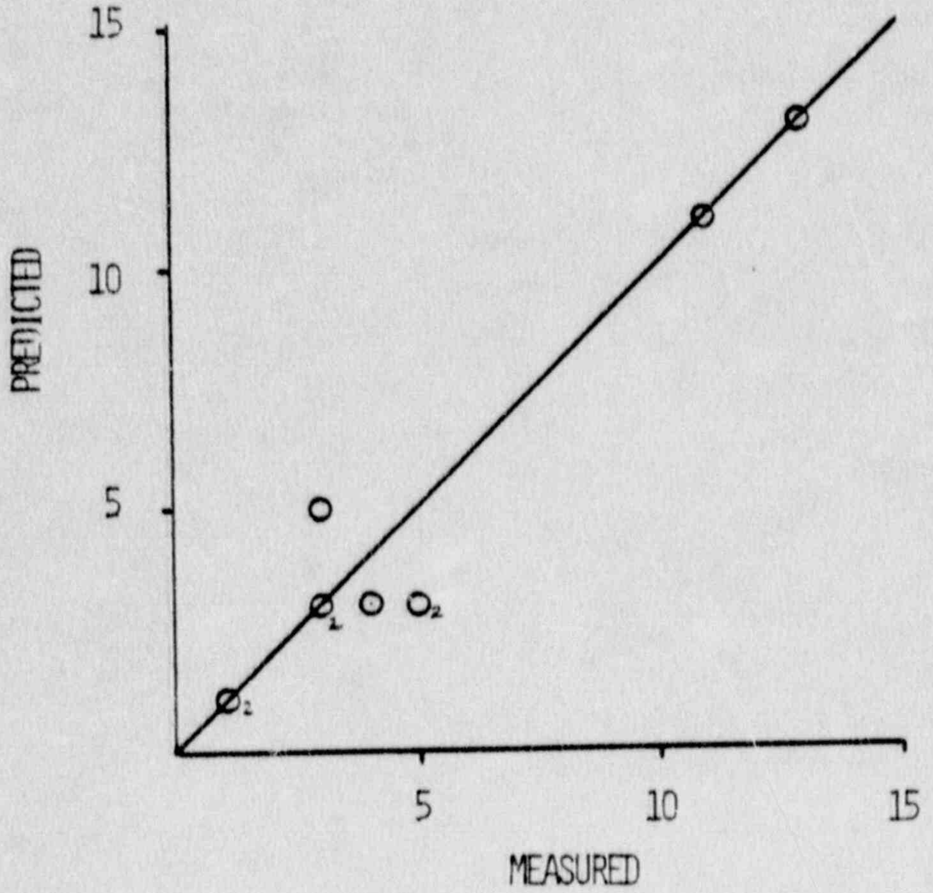
    AS ABOVE WITH 10 - 20% OSCILLATION IN TEST POWER

- BUNDLE POWER, PRESSURE, INLET SUBCOOLING, AND INLET FLOW RATE WERE RECORDED DURING THE TESTS. THE NUMBER OF BT CYCLES EXPERIENCED BY THE BUNDLE WAS ALSO DETERMINED.

ANALYSIS

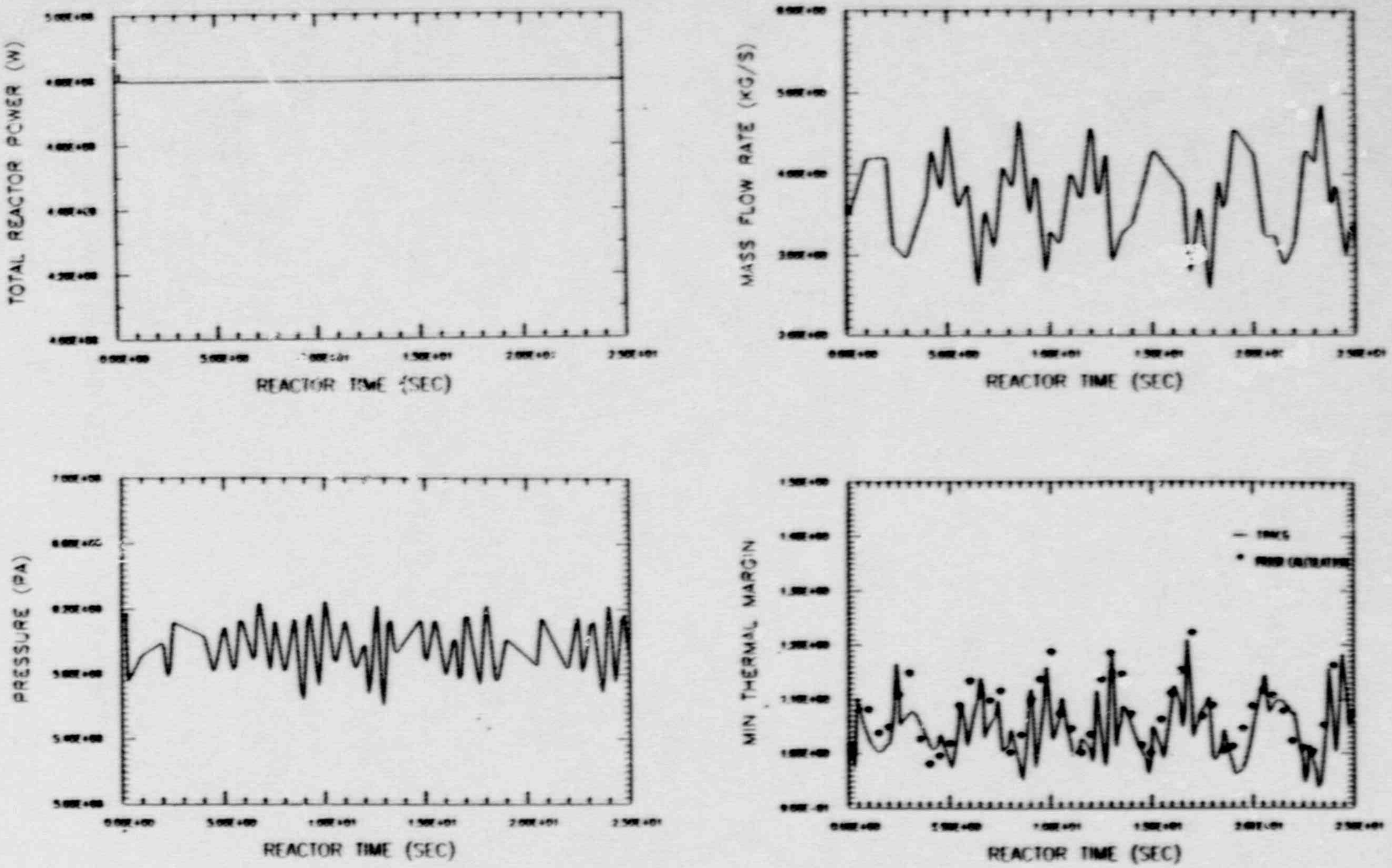
- THE GEXL CRITICAL QUALITY CORRELATION HAS BEEN USED TO ANALYZE THE BUNDLE CRITICAL POWER PERFORMANCE DURING OSCILLATIONS.
- THE ANALYSIS WAS PERFORMED USING THE GE SINGLE CHANNEL TRANSIENT HYDRAULICS DESIGN CODE.
- MEASURED BUNDLE BOUNDARY CONDITIONS WERE INPUT TO THE CODE AND TRANSIENT CRITICAL POWER WAS CALCULATED.
- BOILING TRANSITION WAS PREDICTED FOR ALL TEST CONDITIONS AS OBSERVED.

COMPARISON OF CALCULATED NUMBER OF BOILING TRANSITION CYCLES VERSUS TEST DATA

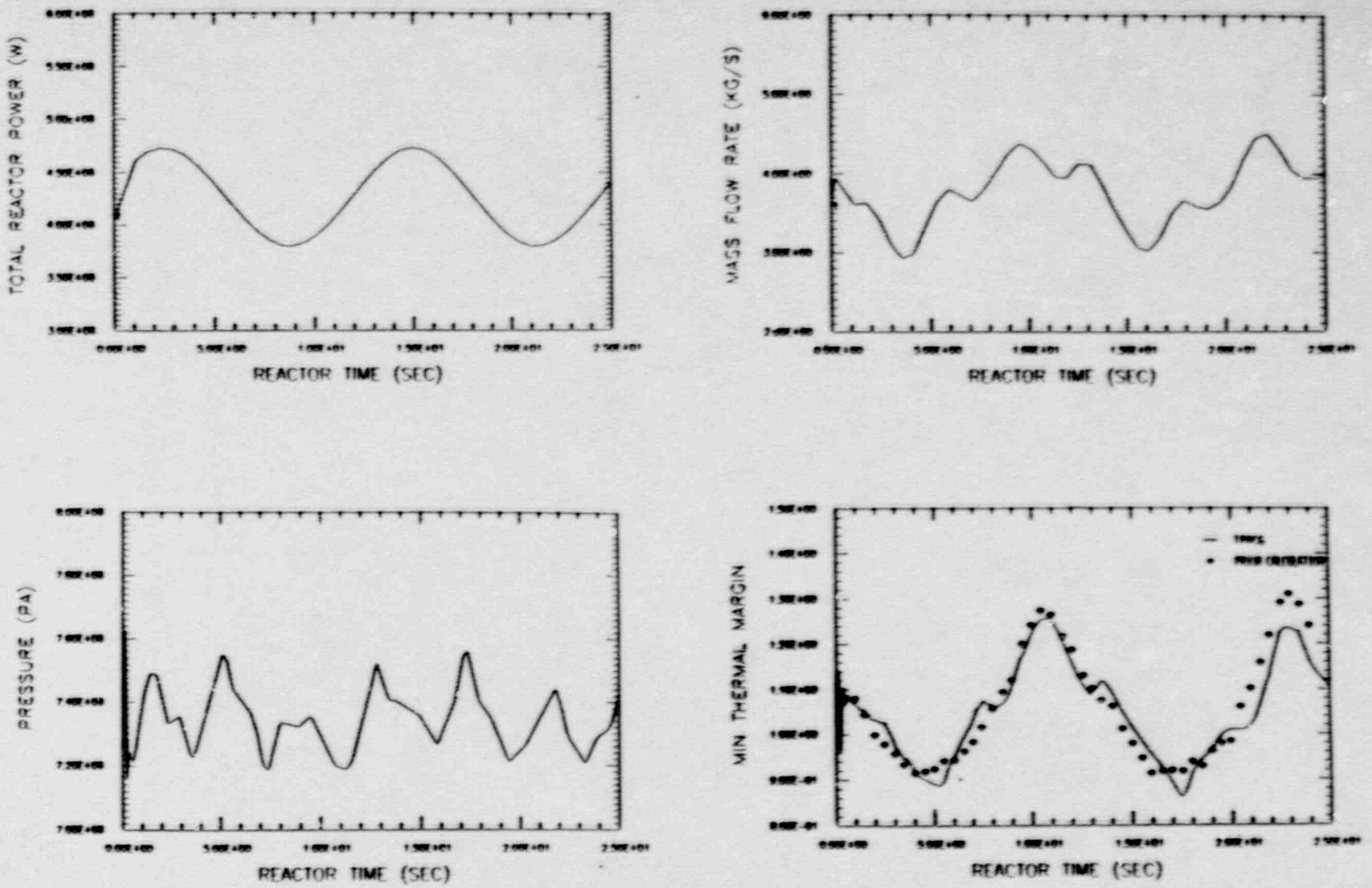


TRACG APPLICATION

- CONFIRM THAT THE GEXL CORRELATION AS IMPLEMENTED INTO TRACG IS CONSISTENT WITH PRIOR CALCULATIONS.
- TWC TEST CASES HAVE BEEN SIMULATED WITH TRACG.



LIMIT CYCLE CRITICAL POWER TEST



POWER OSCILLATION TEST

CONCLUSIONS

- THE GEXL CORRELATION IS APPLICABLE TO CONDITIONS DURING OSCILLATIONS.
- TRACG USING THE GEXL CORRELATION IS APPLICABLE FOR CALCULATING CRITICAL POWER DURING OSCILLATIONS.

STABILITY PLANT QUALIFICATION DATA

G. A. WATFORD
GE NUCLEAR ENERGY

OBJECTIVES

O PROVIDE BRIEF DESCRIPTION OF PLANT DATA USED IN CURRENT TRACG STABILITY QUALIFICATION

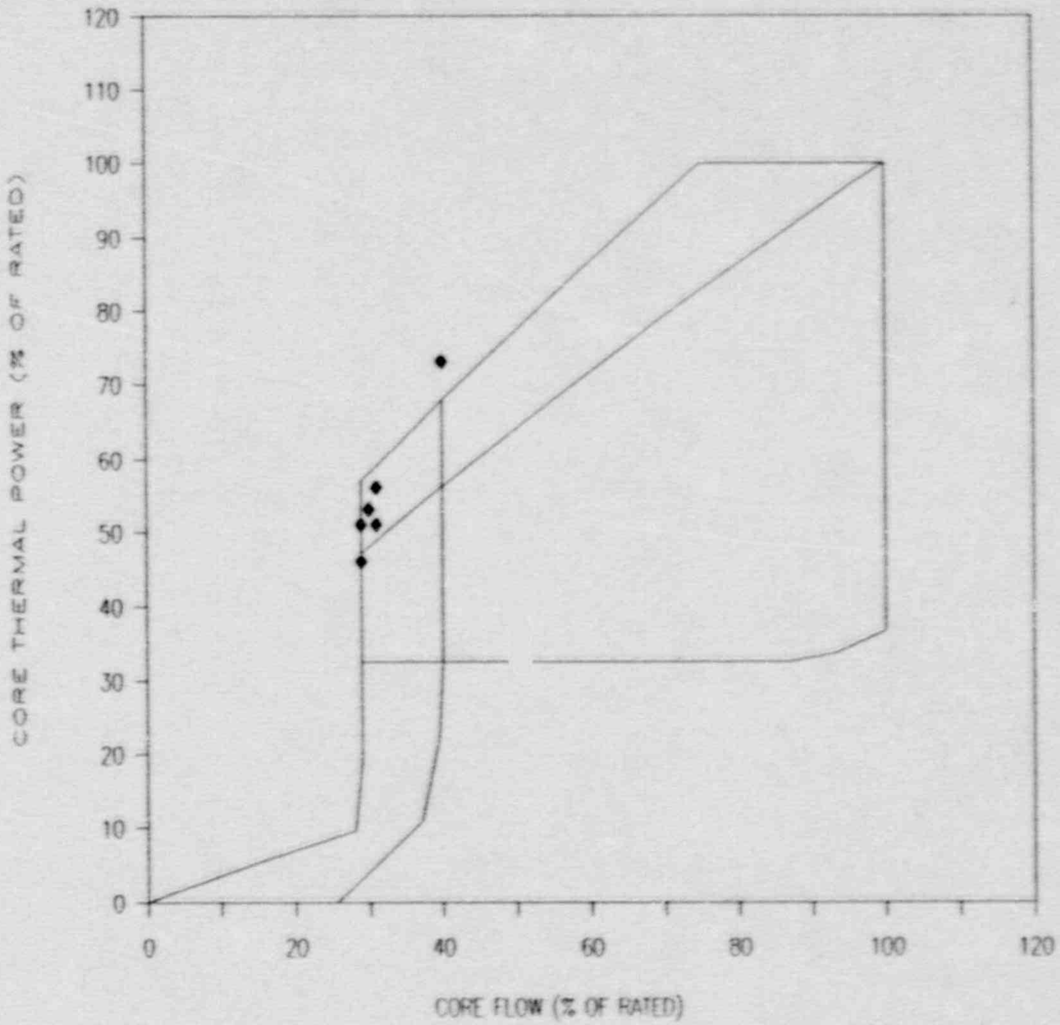
- LEIBSTADT TESTS
- LASALLE-2 EVENT

O PLANTS ANALYZED PROVIDE:

- CORE WIDE AND REGIONAL OSCILLATIONS
- OPERATION BEYOND INCEPTION OF OSCILLATIONS
- VARIETY OF POWER/FLOW CONDITIONS

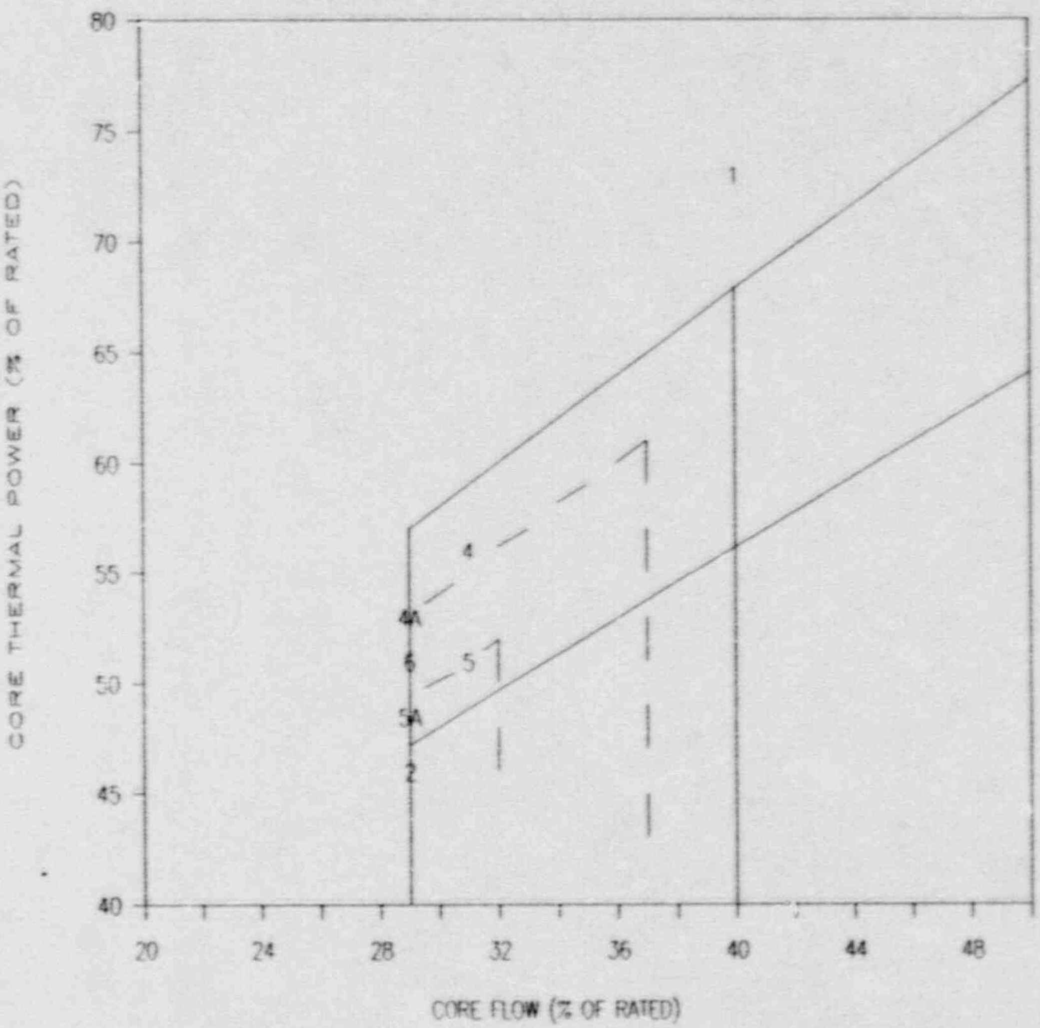
O UNDERSTANDING TEST/EVENT SEQUENCES VITAL TO CORRECT INTERPRETATION OF DATA

LEIBSTADT STABILITY TESTS



GAW-3
11/8/89

TESTING SEQUENCE



TC 1, 2 = STARTUP TESTS

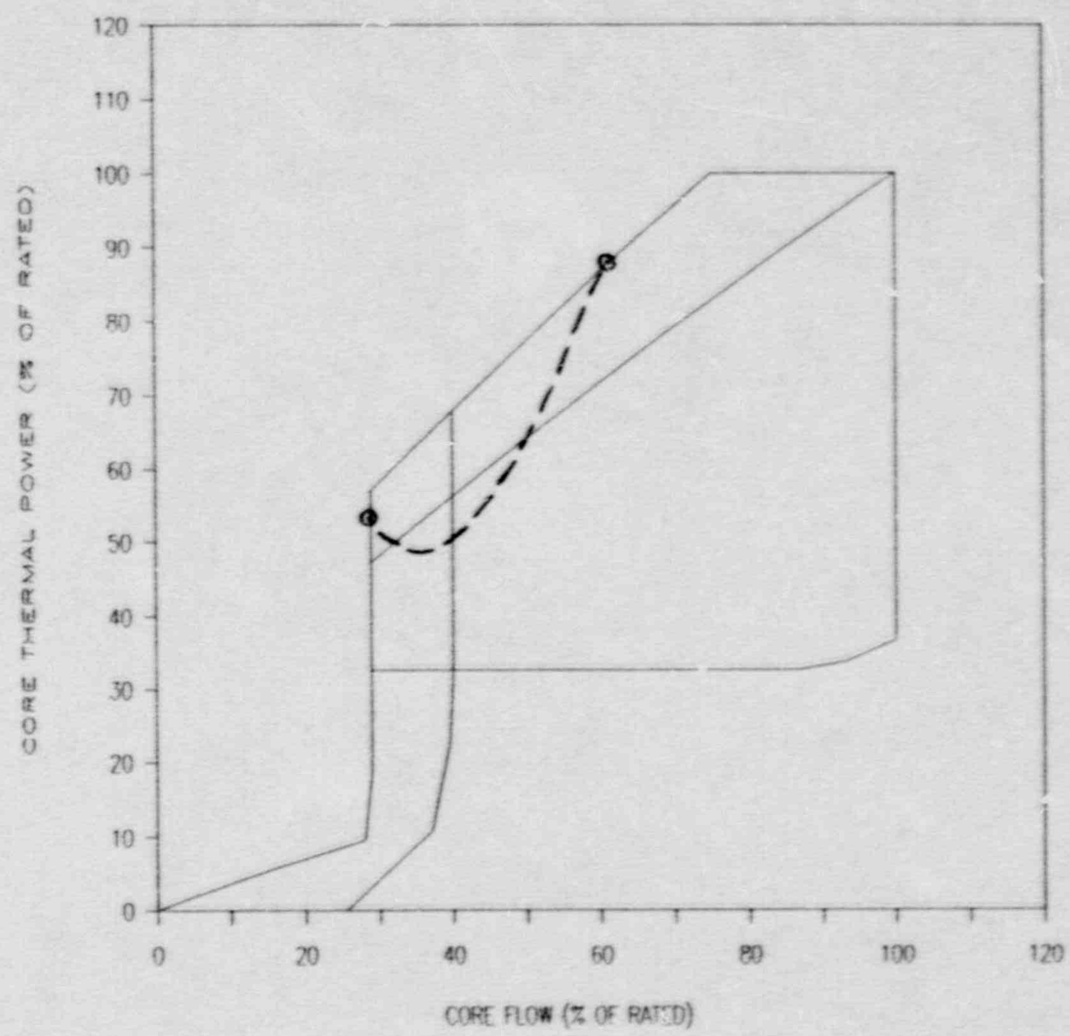
TC 4, 4A = SPECIAL TESTS NORMAL FWT

TC 5, 5A = SPECIAL TESTS REDUCED FWT

TC 6 = TWO PUMP TRIP TEST

GAW-4
11/8/89

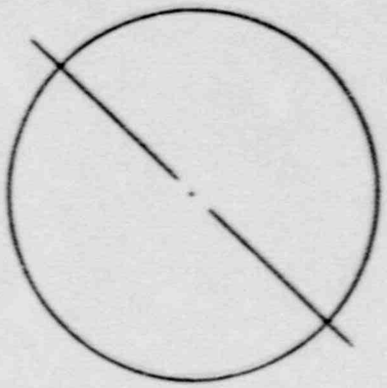
TWO RECIRCULATION PUMP TRIP



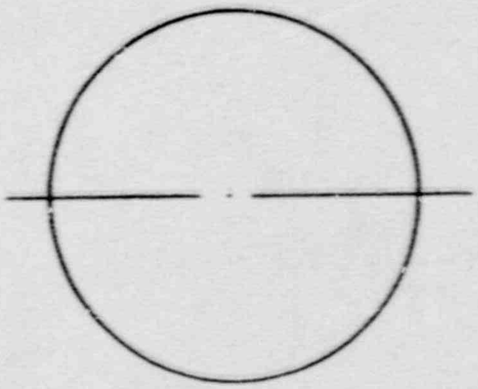
GAW-5
11/8/89

OBSERVED BEHAVIOR

OSCILLATION MODE



TEST CONDITION 1, 2



TEST CONDITION 4, 4A
5, 5A

GAW-6
11/8/89

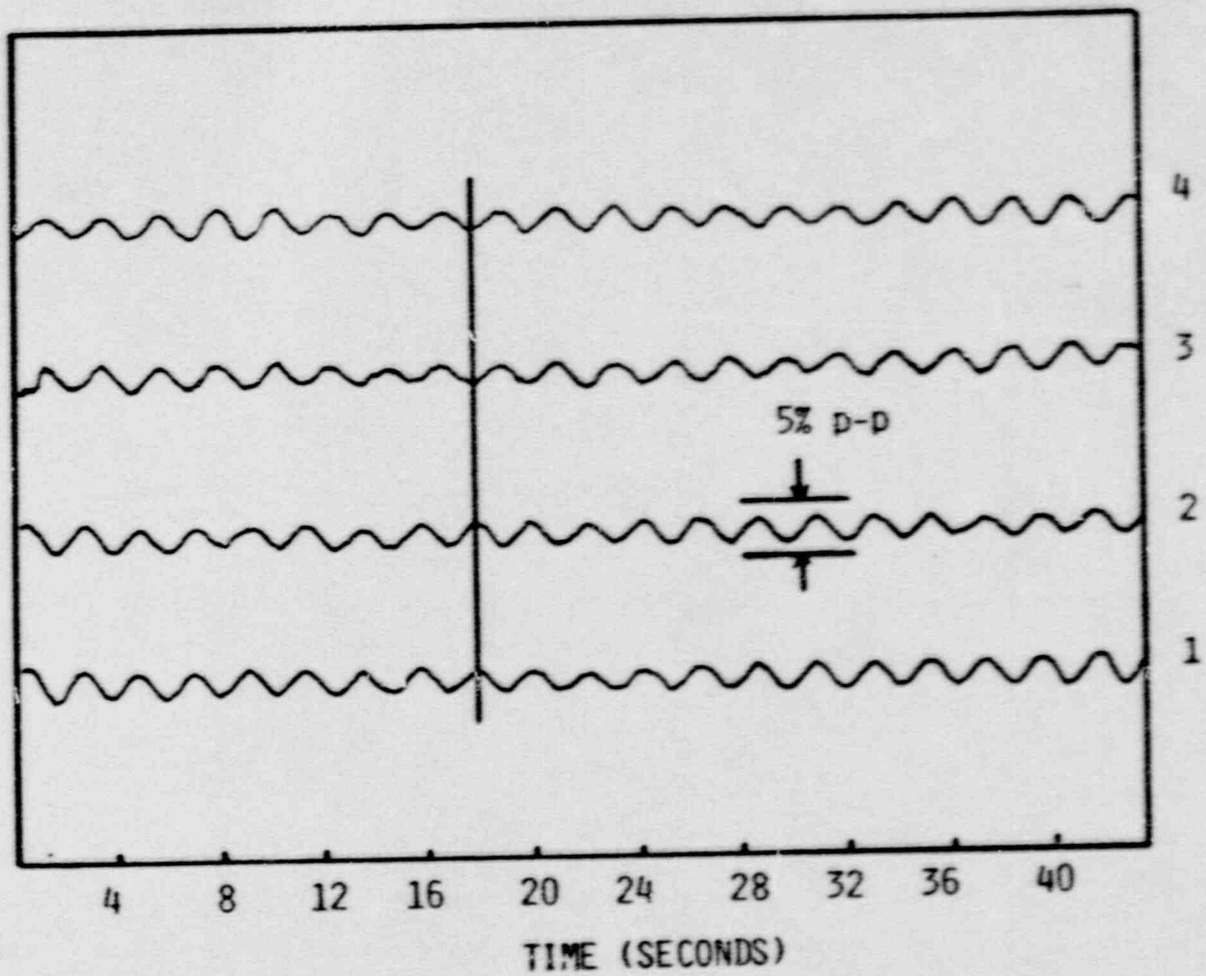
OBSERVED BEHAVIOR

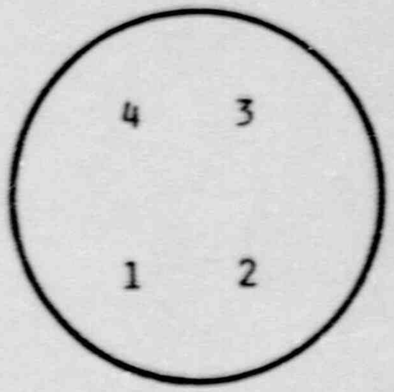
OSCILLATION FREQUENCY/MAGNITUDE

TEST	POWER/FLOW (%/%)	FREQUENCY (HZ)	PEAK-TO-PEAK (% OF AVERAGE) LPRM	PEAK-TO-PEAK (% OF AVERAGE) APRM
1	73/40	0.58	25	3
2	46/29	0.48	10-50	4-10
4	56/31	0.46	14	4
4A	53/30	0.45	66	8
5	51/31	0.46	12	4
5A	46/29	0.46	12	4
6	51/29	(STABLE)		

GAW-7
11/8/89

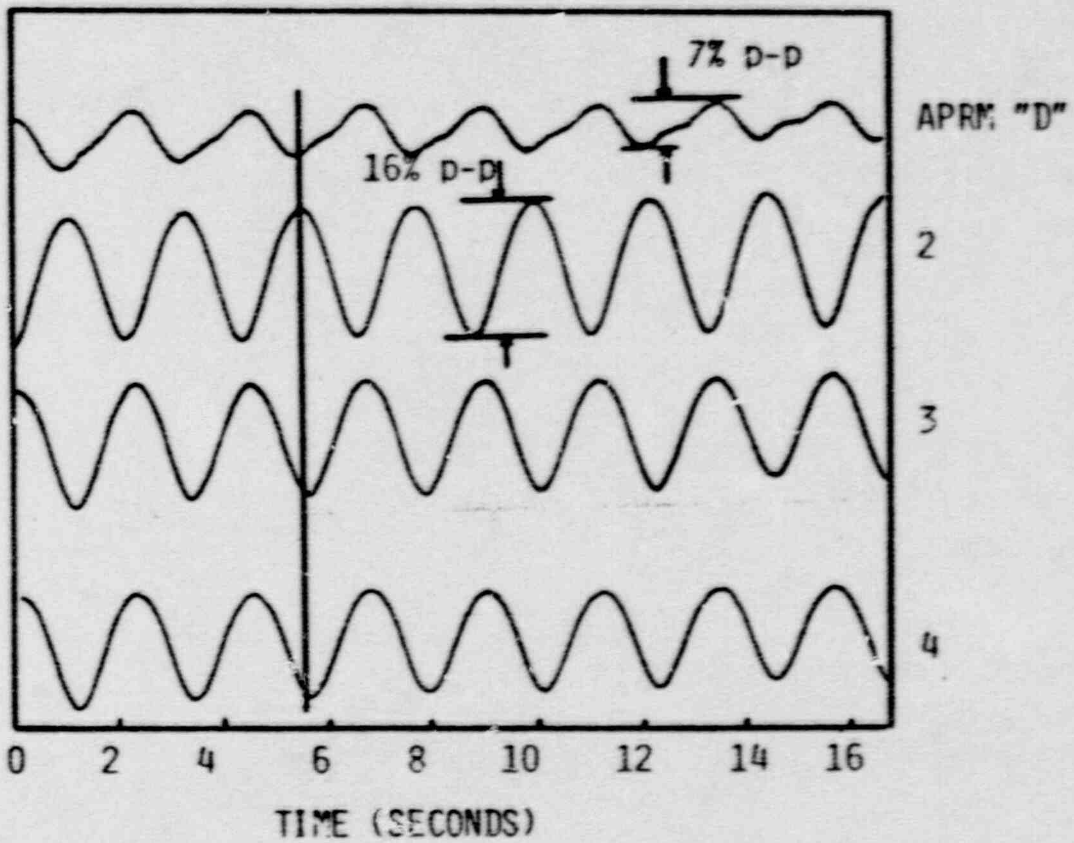
TEST CONDITION 4

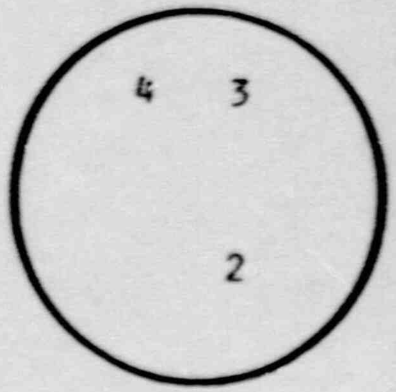




GAW-8
11/8/89

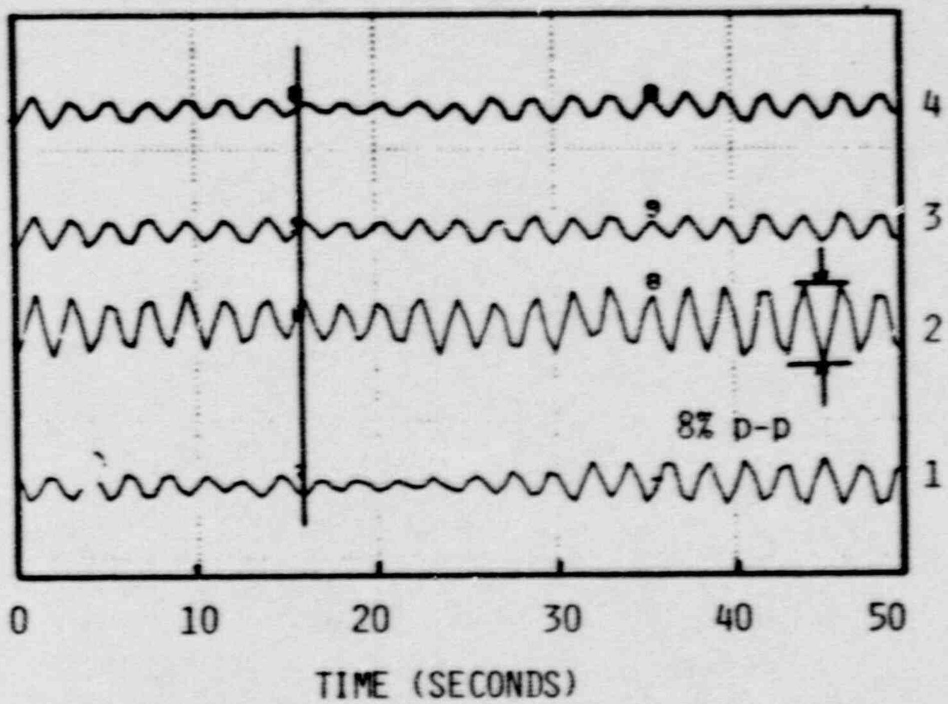
TEST CONDITION 4A

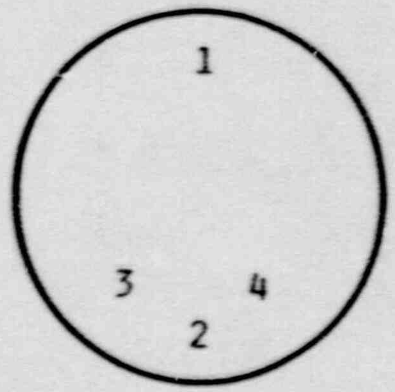




GAW-9
11/8/89

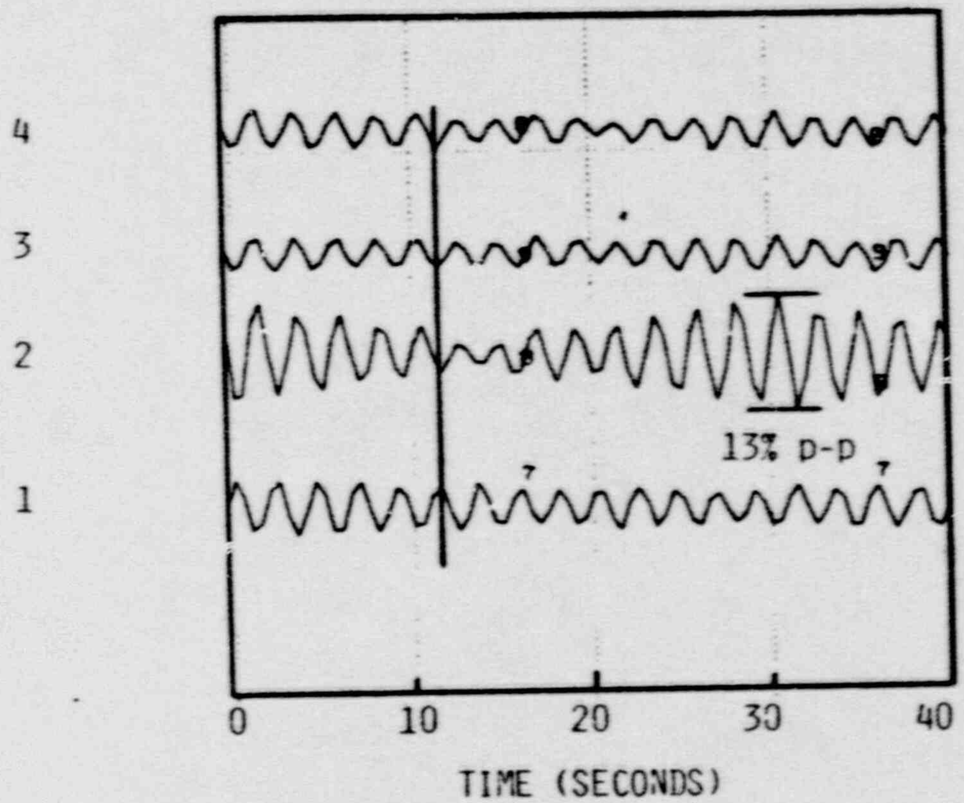
TEST CONDITION 5

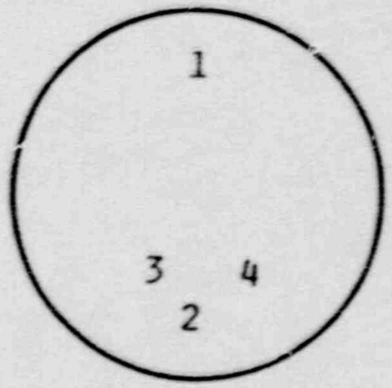




GAW-10
11/8/89

TEST CONDITION 5A





GAW-11
11/8/89

LASALLE-2 STABILITY EVENT

TIME	SEQUENCE OF EVENTS
0 MIN	PLANT INITIALLY AT 84% POWER/75% FLOW NEAR RATED ROD LINE
0	TECHNICIAN ERROR CAUSES TWO RECIRCULATION PUMP TRIP
1:00	CORE FLOW COASTDOWN TO 29% OF RATED
1:00	RESULTANT POWER COASTDOWN TO 41% OF RATED
1-5	FEEDWATER HEATER LEVEL ALARMS, FEEDWATER HEATER ISOLATIONS (OPERATOR ACTION TO MINIMIZE HEATER LOSS)
4-6	ATTEMPTS TO RESTART RECIRC PUMPS
6:50	REACTOR SCRAM

GAW-12
11/8/89

LASALLE-2 STABILITY EVENT

TIME	SEQUENCE OF EVENTS
4:48	FIRST LPRM DOWNSCALE ALARMS
5:07	RFP A LOW FLOW ALARM
5:15	FIRST LPRM UPSCALE ALARMS
5:22	LPRM UPSCALE ALARMS CLEAR LEVEL 7 HIGH WATER LEVEL
5:38	LPRM UPSCALE ALARMS RESUME
5:41	RFP A LOW FLOW ALARM
5:44	LPRM UPSCALE ALARMS CLEAR (TRANSIENT RECORDER STARTED)
5:47	LEVEL 7 HIGH WATER LEVEL
5:53	LPRM DOWNSCALE ALARMS CLEAR

GAW-13
11/8/89

LASALLE-2 STABILITY EVENT

TIME	SEQUENCE OF EVENTS
6:05	LPRM DOWNSCALE ALARMS RESUME
6:08	LEVEL 4 LOW WATER LEVEL
6:11	LPRM UPSCALE ALARMS RESUME
6:15	LPRM UPSCALE ALARMS CLEAR
6:27	LEVEL 7 HIGH WATER LEVEL
6:43	LPRM UPSCALE ALARMS RESUME (TRANSIENT RECORDING ENDED)
6:50	REACTOR SCRAM

GAW-14
11/8/89

FEEDWATER FLOW/TEMPERATURE RESPONSE

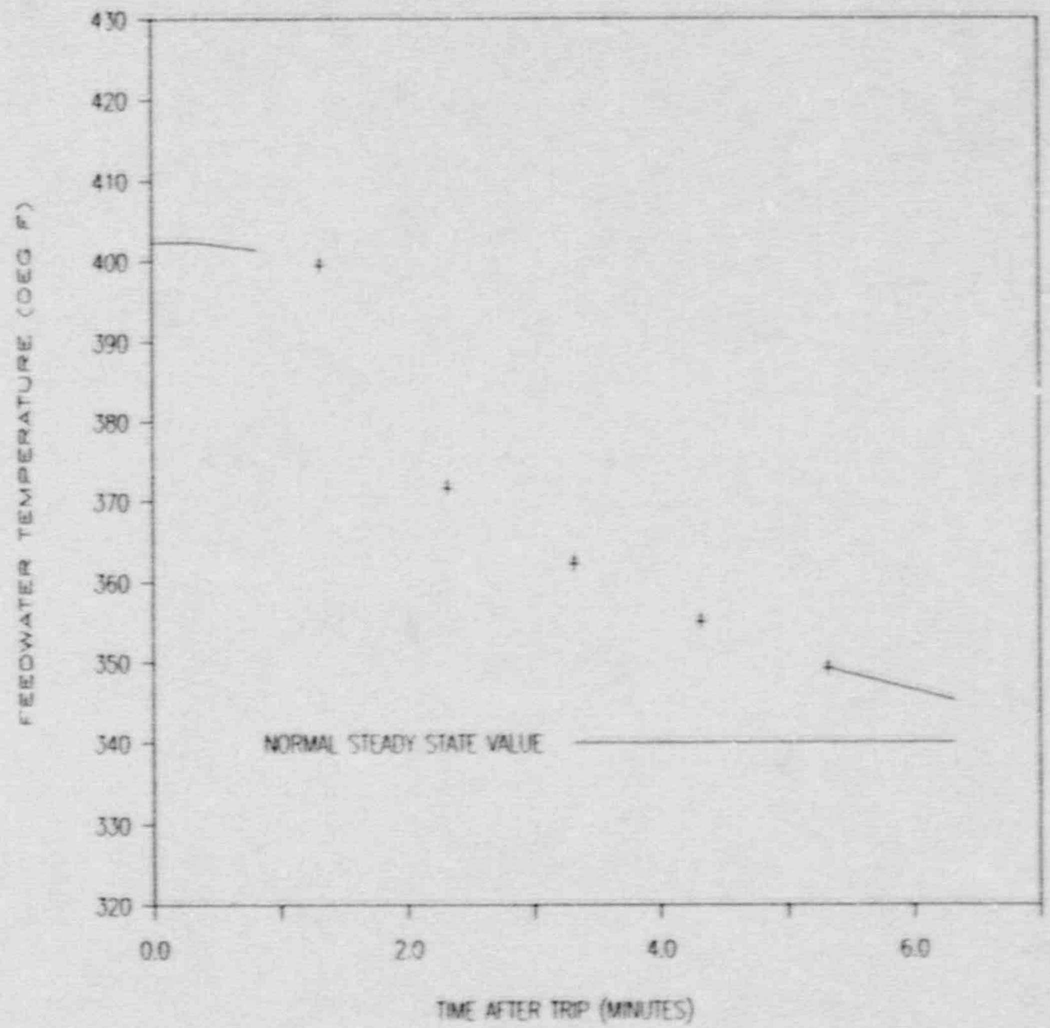
- O FEEDWATER FLOW AND TEMPERATURE MEASURED DURING EVENT
  - CONTINUOUSLY FOR FIRST AND LAST MINUTE OF TRANSIENT
  - 1 MINUTE AVERAGES DURING MIDDLE OF EVENT
- O FEEDWATER TEMPERATURE RESPONSE SIMILAR TO THAT EXPECTED DURING NORMAL TWO PUMP TRIP
  - REDUCED STEAM FLOW REDUCES FEEDWATER HEATING
  - SLOW REDUCTION IN FEEDWATER TEMPERATURE
    - o EXPECTED STEADY STATE FW TEMPERATURE

      340°F
    - o MEASURED FW TEMPERATURE AT SCRAM

      347°F

GAW-15
11/8/89

FEEDWATER TEMPERATURE RESPONSE



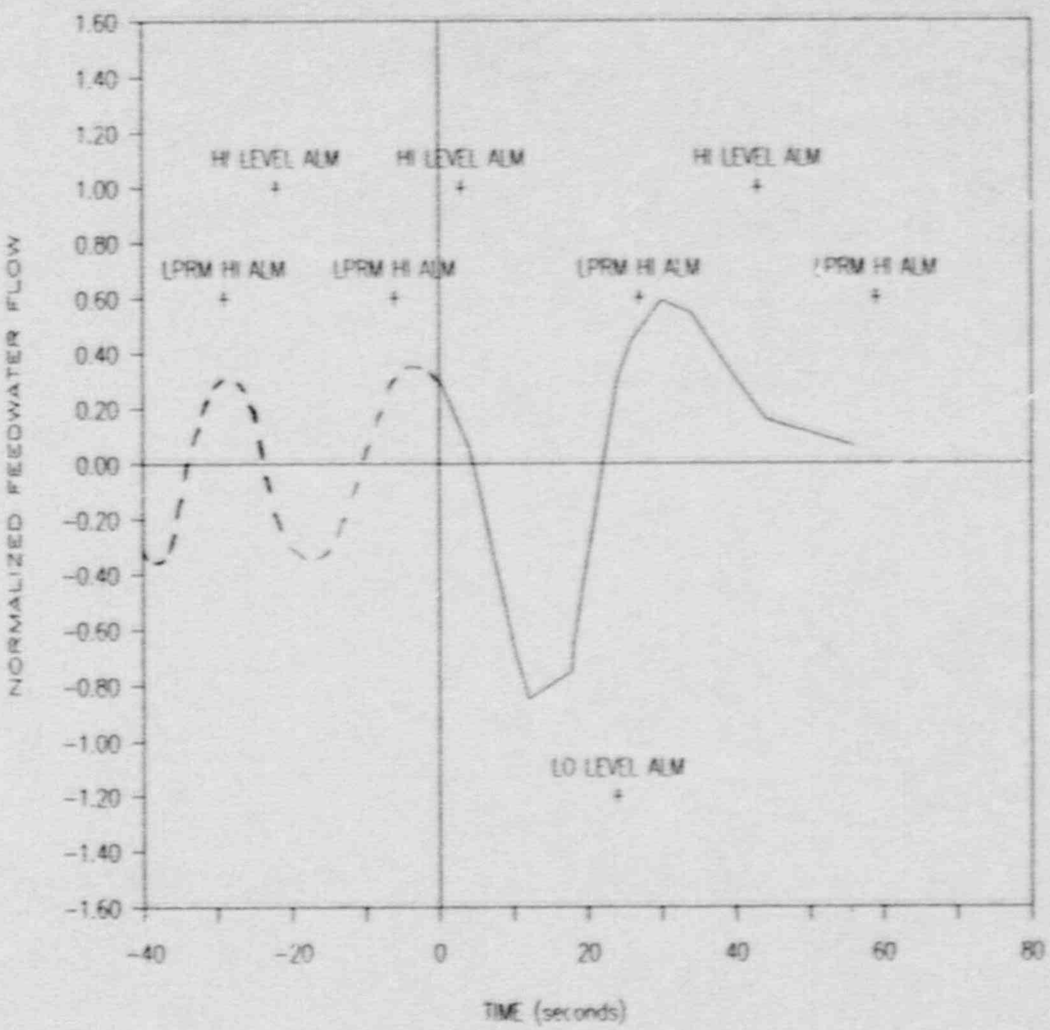
GAW-16
11/8/89

FEEDWATER FLOW RESPONSE

- O POST EVENT DISCOVERY OF STUCK FW ACTUATOR VALVE
  - FLOW CONTROL VALVE DID NOT PROPERLY RESPOND DURING EVENT
  - LARGE SWINGS IN FW FLOW OBSERVED DURING EVENT
- O MULTIPLE EFFECTS OF FW FLOW VARIATIONS
  - WATER LEVEL VARIATIONS
  - CORE FLOW VARIATIONS
  - CORE INLET TEMPERATURE VARIATIONS
- O TIMING OF VARIATIONS IMPORTANT TO STABILITY RESPONSE OF CORE
  - INTEGRATED EFFECT ON LEVEL/CORE FLOW
  - LAG IN CORE INLET TEMP RESPONSE

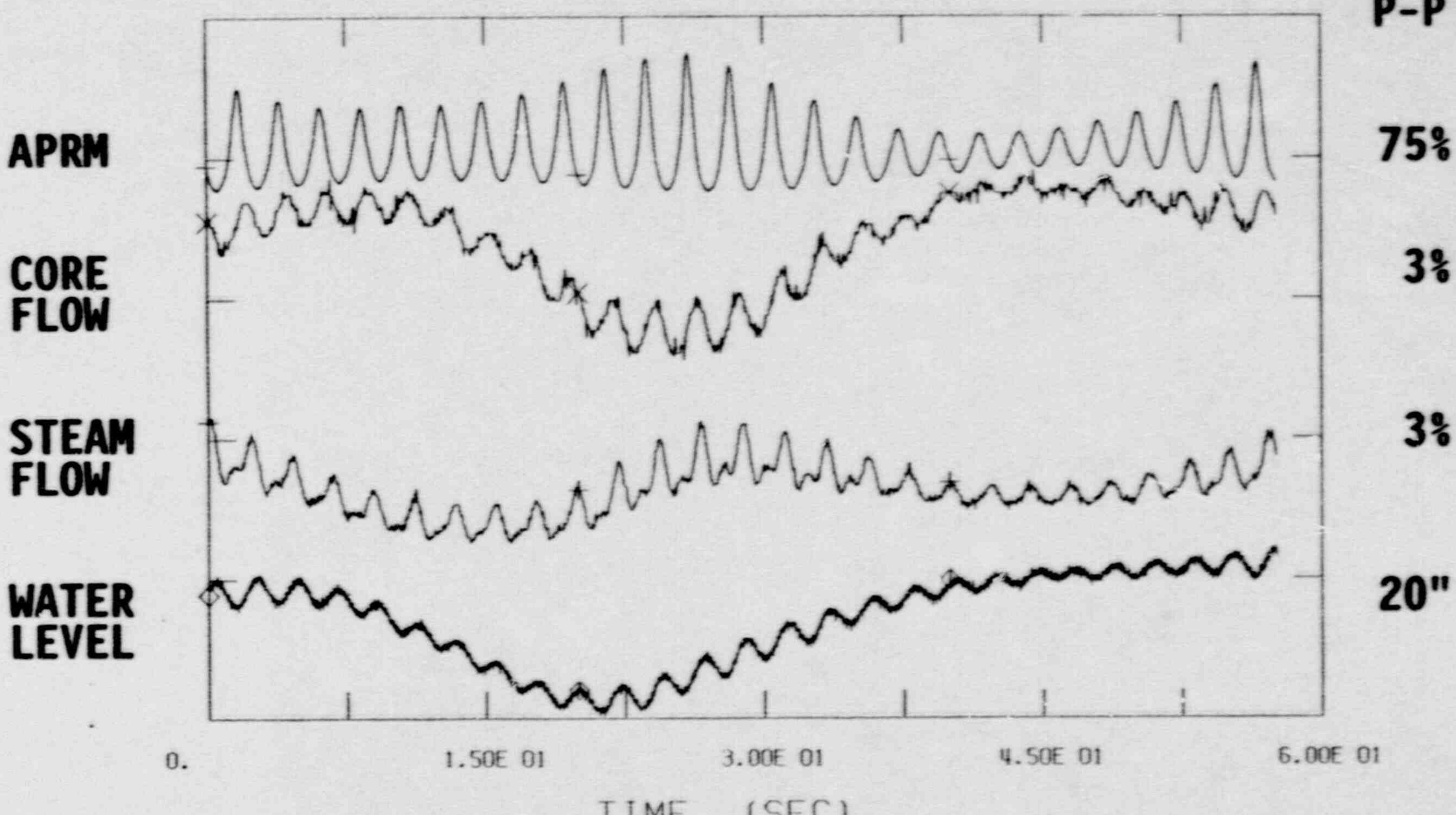
GAW-17
11/8/89

FEEDWATER FLOW RESPONSE



GAW-18
11/8/89

OSCILLATION CHARACTERISTICS



GAW-19
11/8/89

SUMMARY

- 0 GOOD SELECTION OF DATA AVAILABLE TO QUALIFY MODELS

- 0 UNDERSTANDING OF TESTS/EVENTS IMPORTANT IN CORRECT INTERPRETATION OF DATA

- 0 LEIBSTADT TESTS PROVIDES BROAD RANGE OF DATA FOR REGIONAL OSCILLATION MODES

- 0 LASALLE-2 EVENT PROVIDES BENCHMARK FOR INTEGRATED SYSTEM EFFECTS ON STABILITY

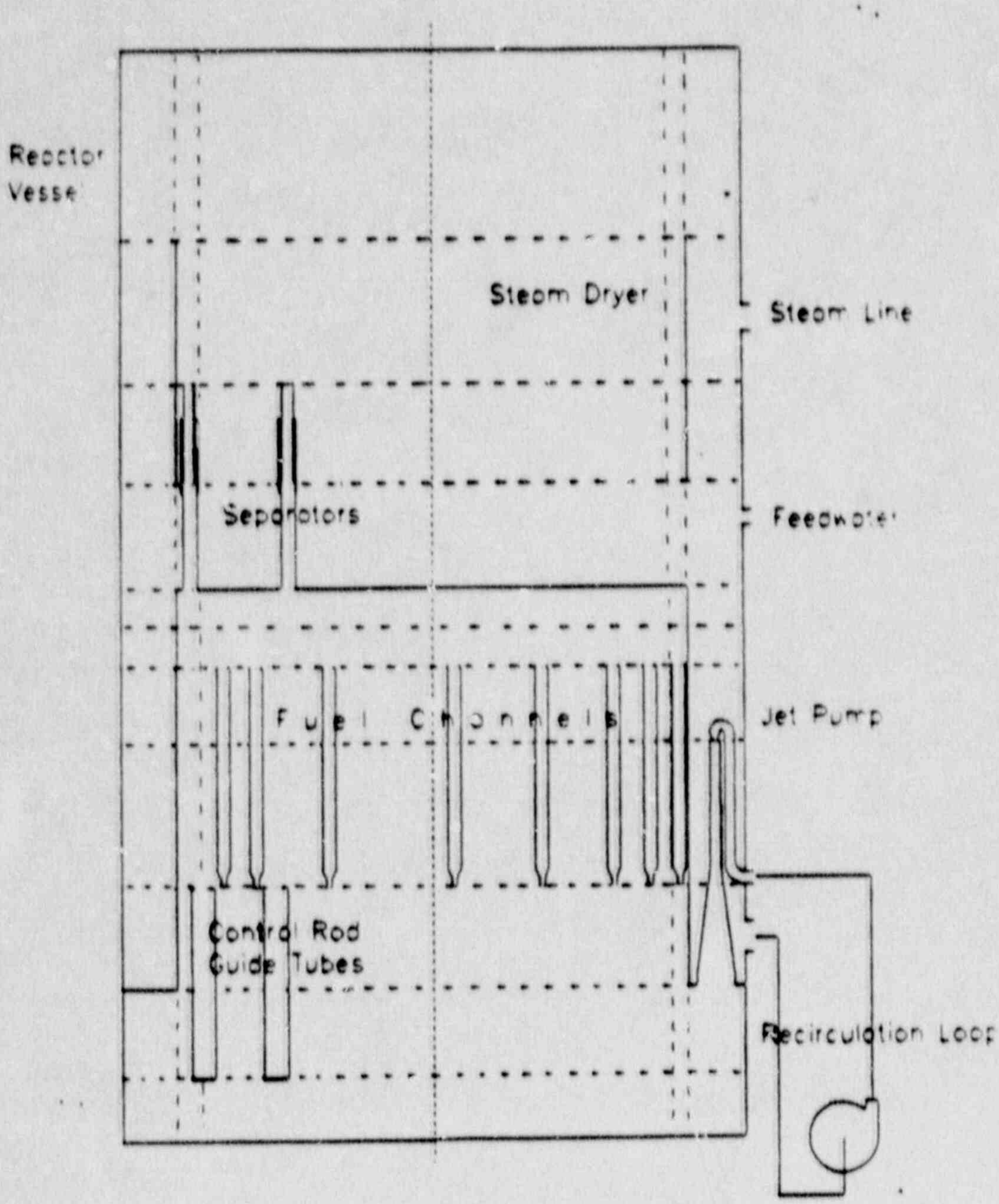
PLANT STABILITY QUALIFICAITON

JC Shaug

PRELIMINARY

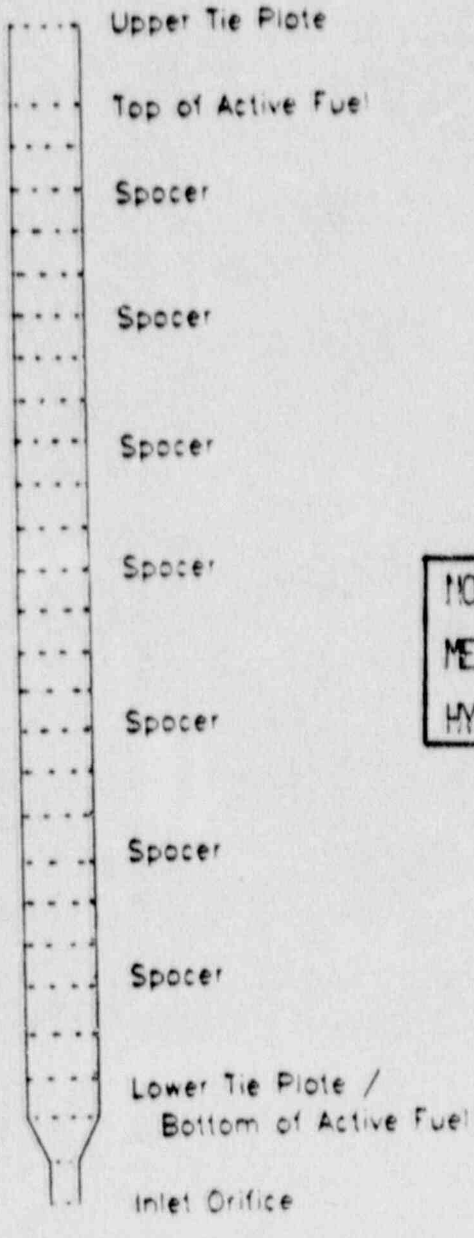
LASALLE STABILITY EVENT

PRELIMINARY



REACTOR VESSEL NODALIZATION

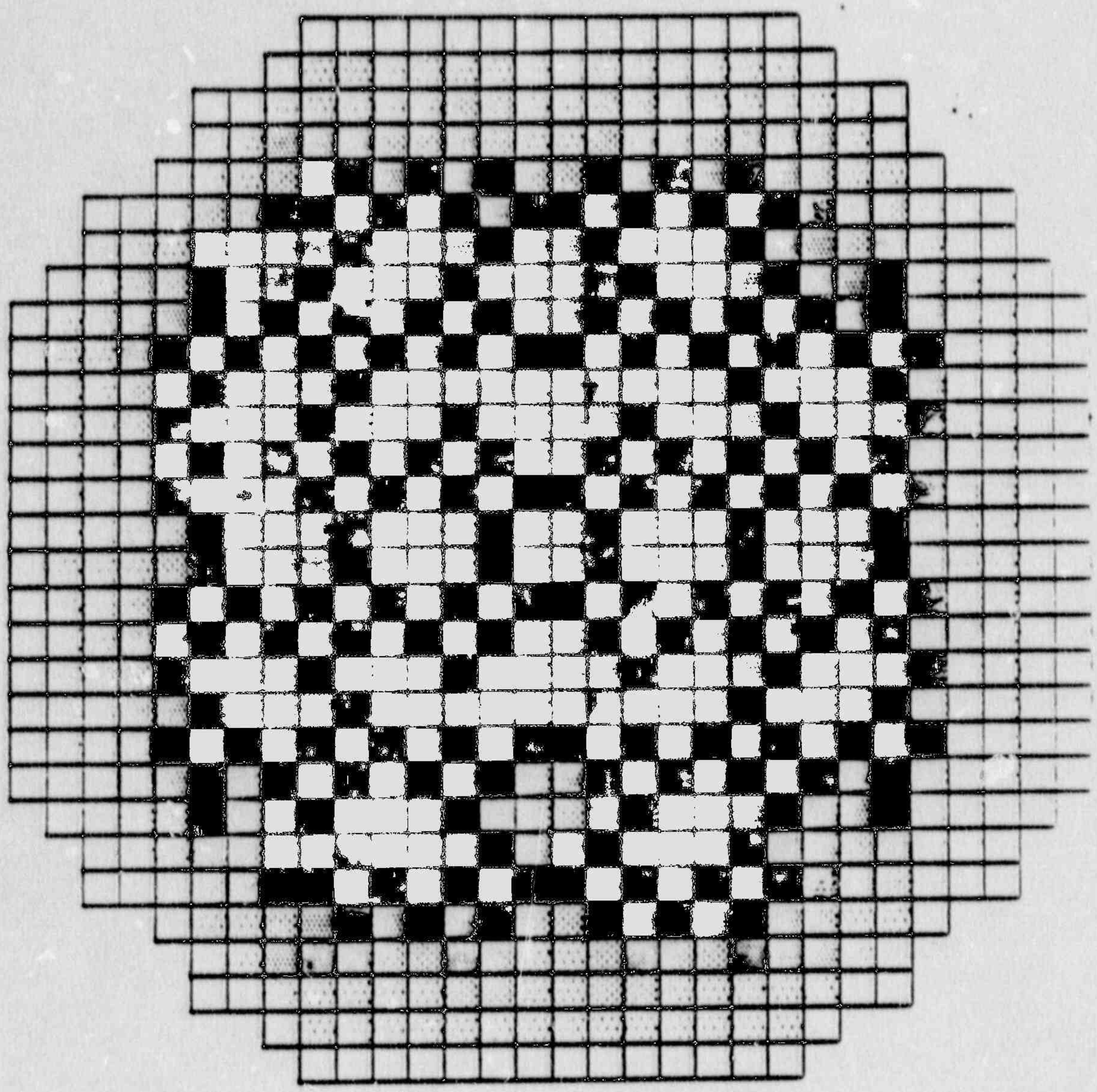
PRELIMINARY



NODALIZATION AND NUMERICAL METHOD CONSISTENT WITH HYDRAULIC STABILITY TESTING

FUEL CHANNEL MODEL

HYDRAULIC CHANNEL GROUPING



RPF=0.36, N=92	RPF=1.22, N=100
RPF=0.72, N=168	RPF=1.40, N=84
RPF=0.99, N=124	RPF=1.47, N=87
RPF=1.11, N=108	RPF=1.50, N=1

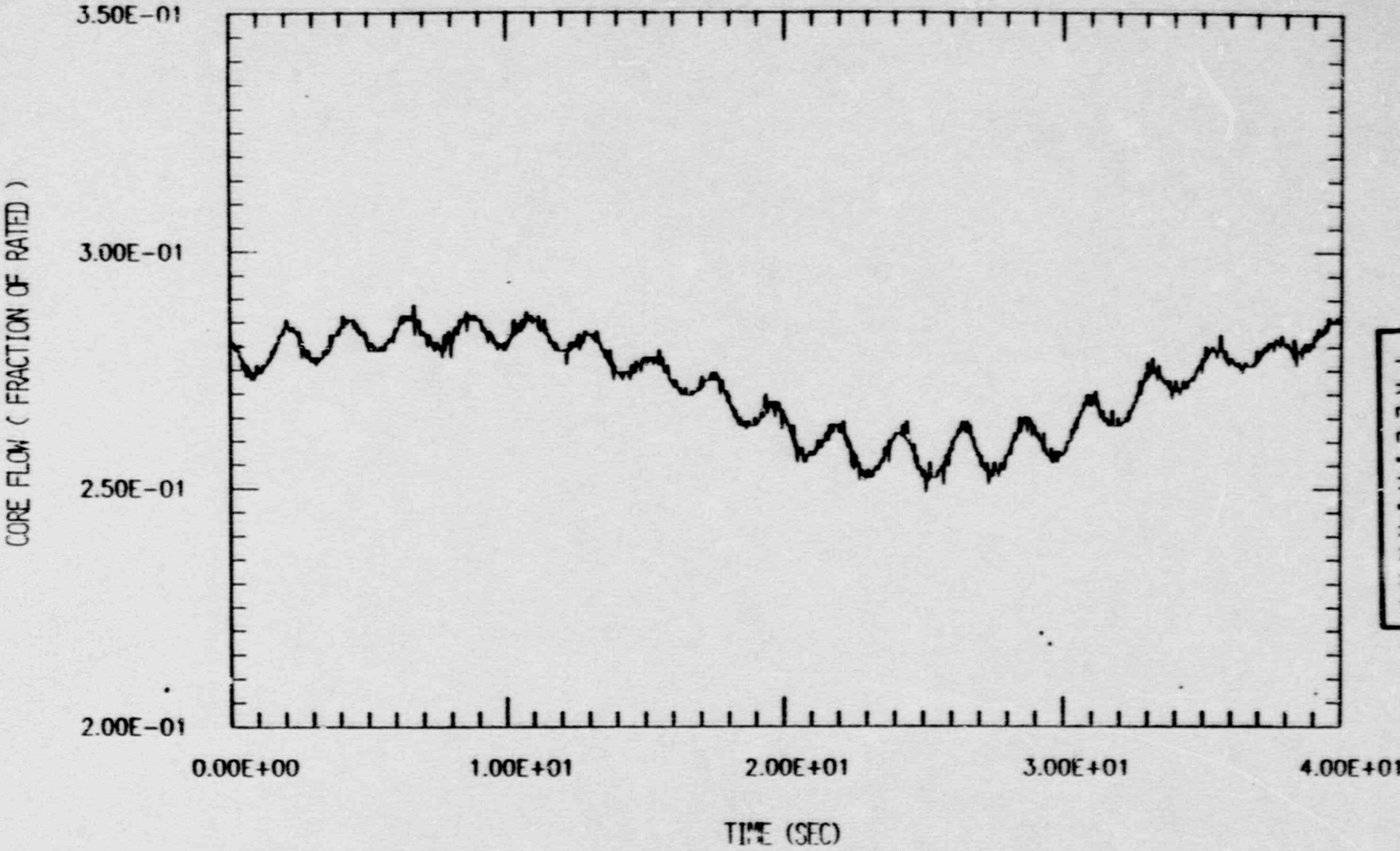
RPF = INITIAL RADIAL PEAKING FACTOR
N = NUMBER OF FUEL ASSEMBLIES

- 3D KINETICS FOR DISCRETE POWER CALCULATION FOR EACH BUNDLE.
- 8 CHANRACTERISTIC HYDRAULIC CHANNELS.
- KINETICS COUPLED TO CONTROL SYSTEM TO PROVIDE SIMULATED LPRM AND APRM CALCULATIONS.

PRELIMINARY

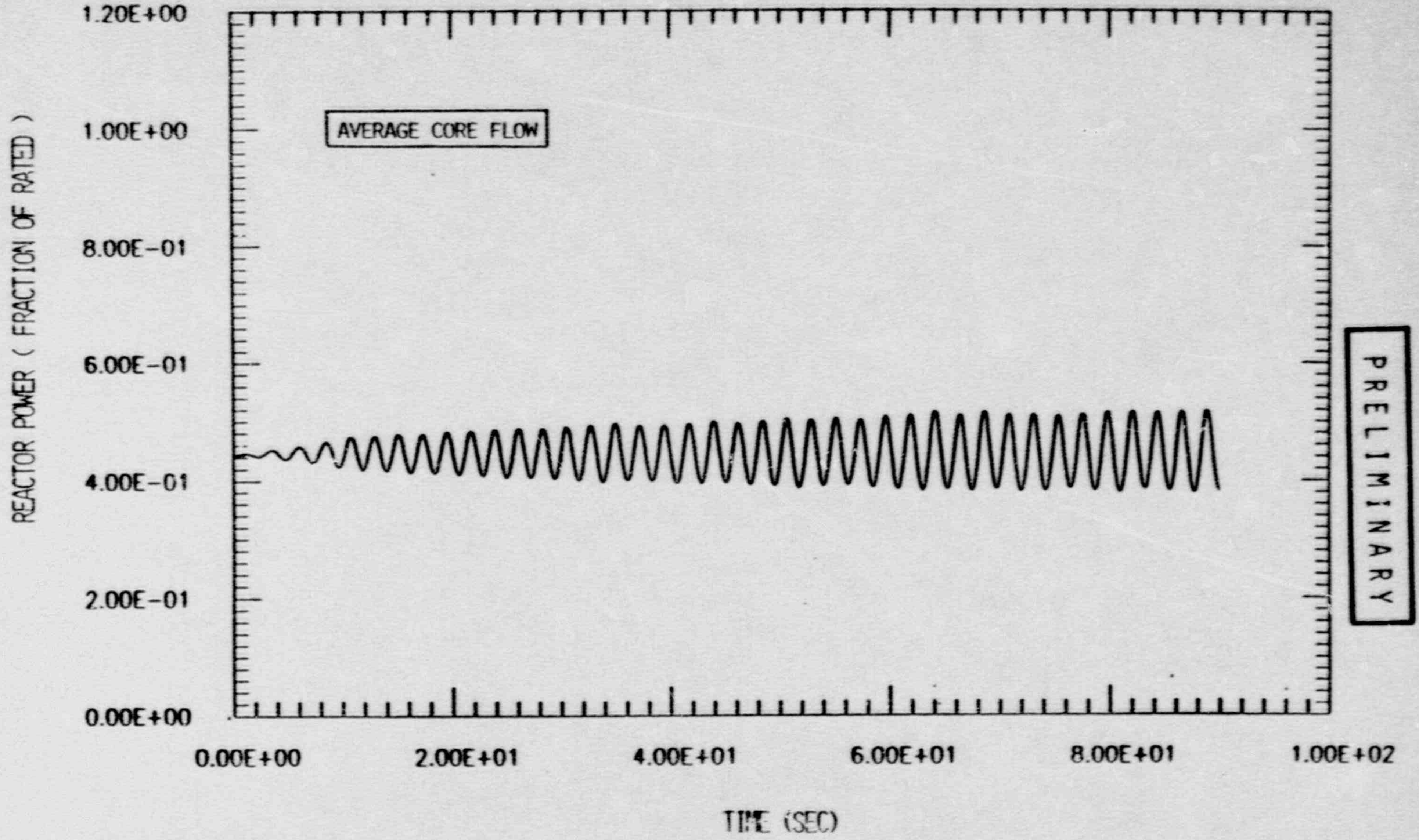
EVENT SIMULATION

- INITIALIZE TRACG TO PRE SCRAM CONDITIONS.
- UTILIZE 3D BWR CORE SIMULATOR WRAPUP TO PROVIDE NUCLEAR DATA AND POWER SHAPE.
- UTILIZE PLANT DATA TO CHARACTERIZE HYDRAULIC CONDITIONS.

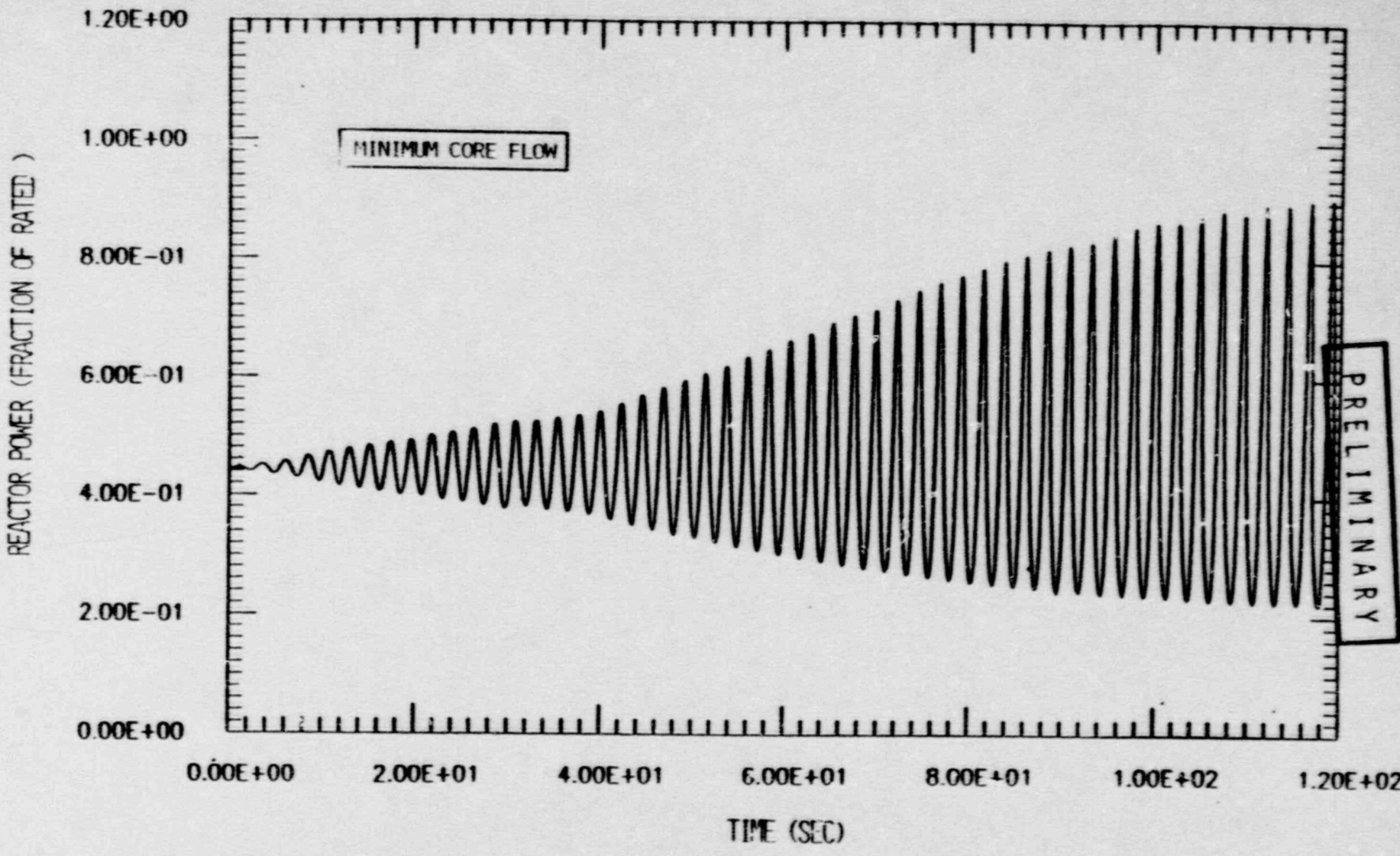


PRELIMINARY

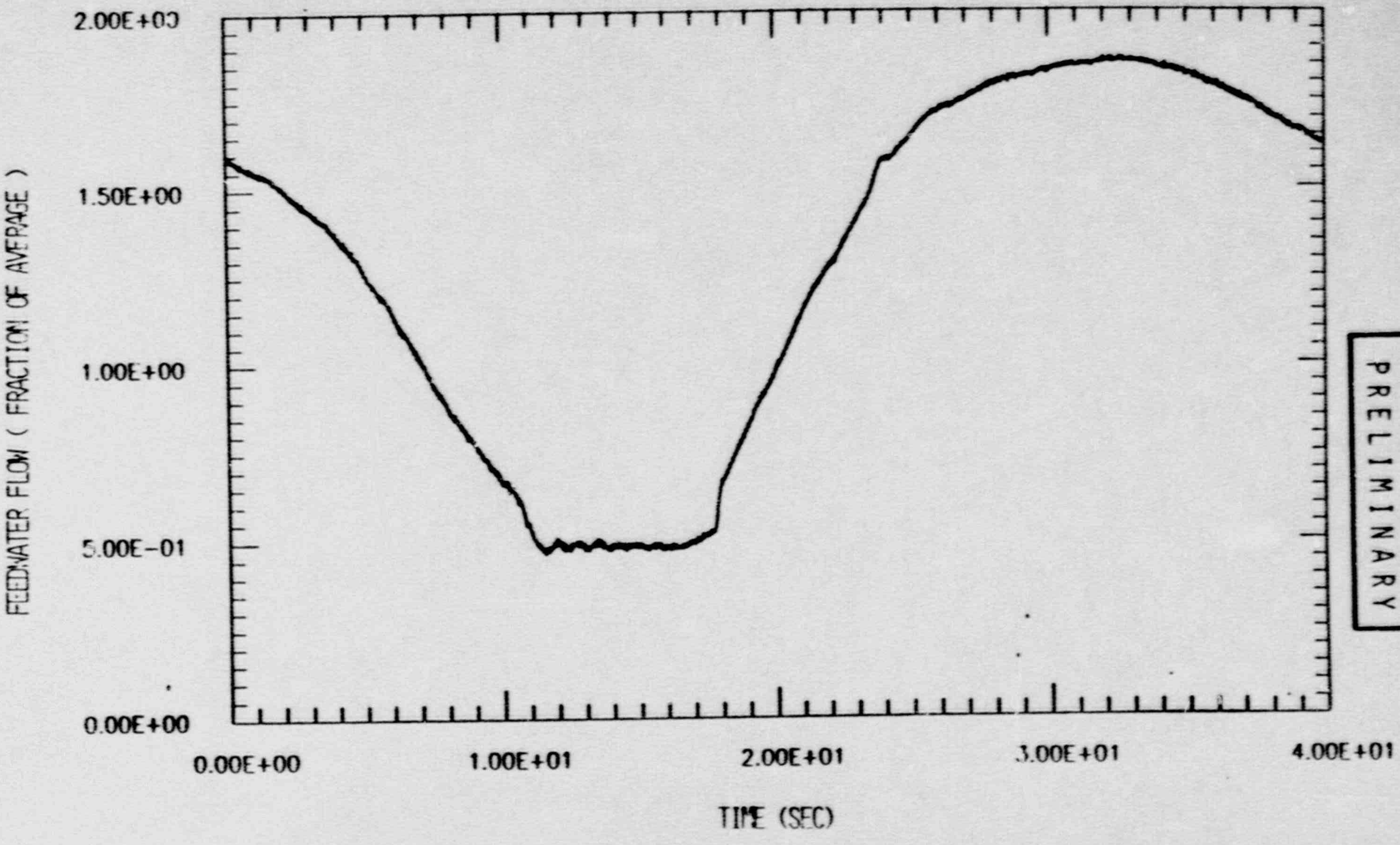
LASALLE PLANT DATA - CORE FLOW



TRACG CALCULATION - REACTOR POWER

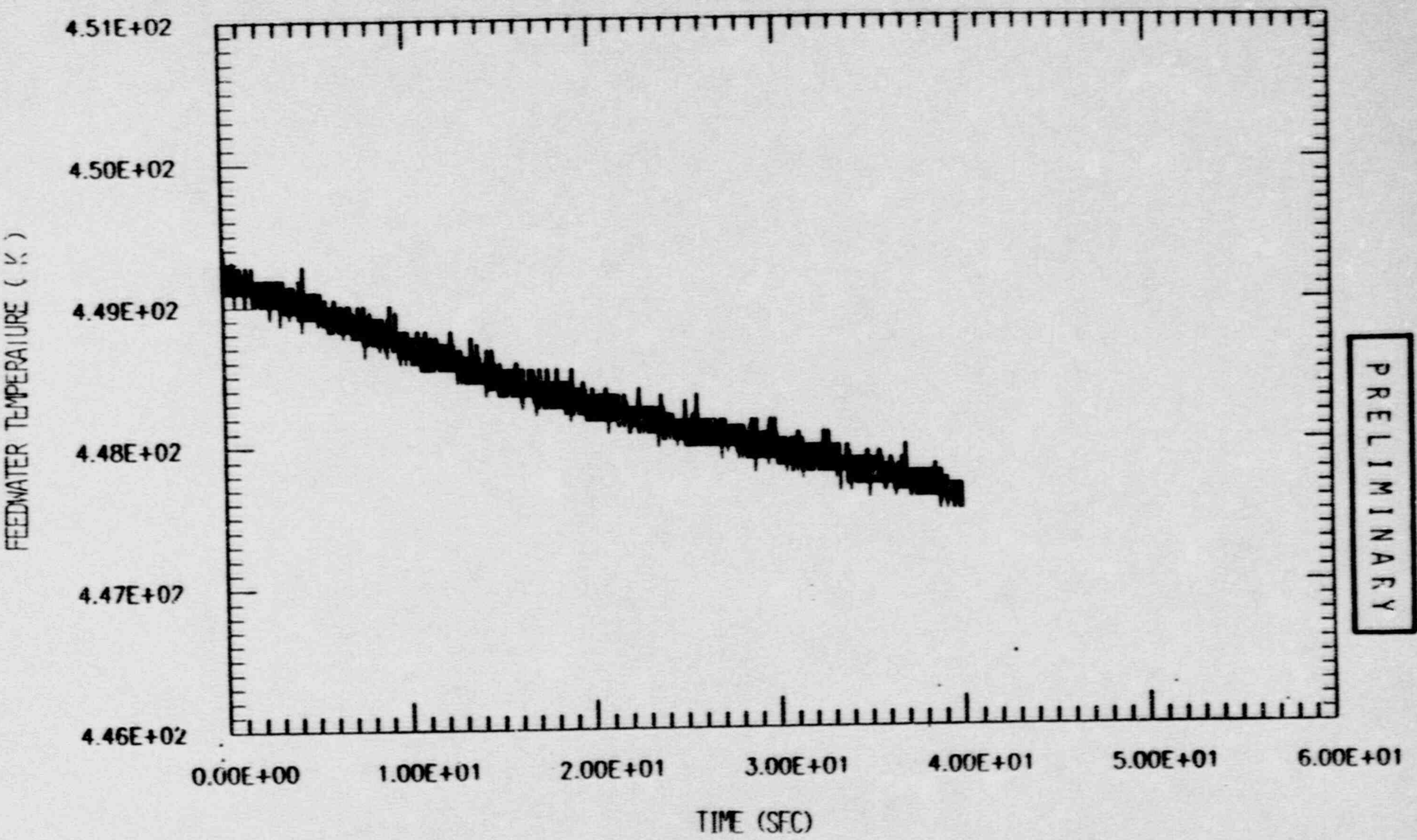


TRACG CALCULATION - REACTOR POWER

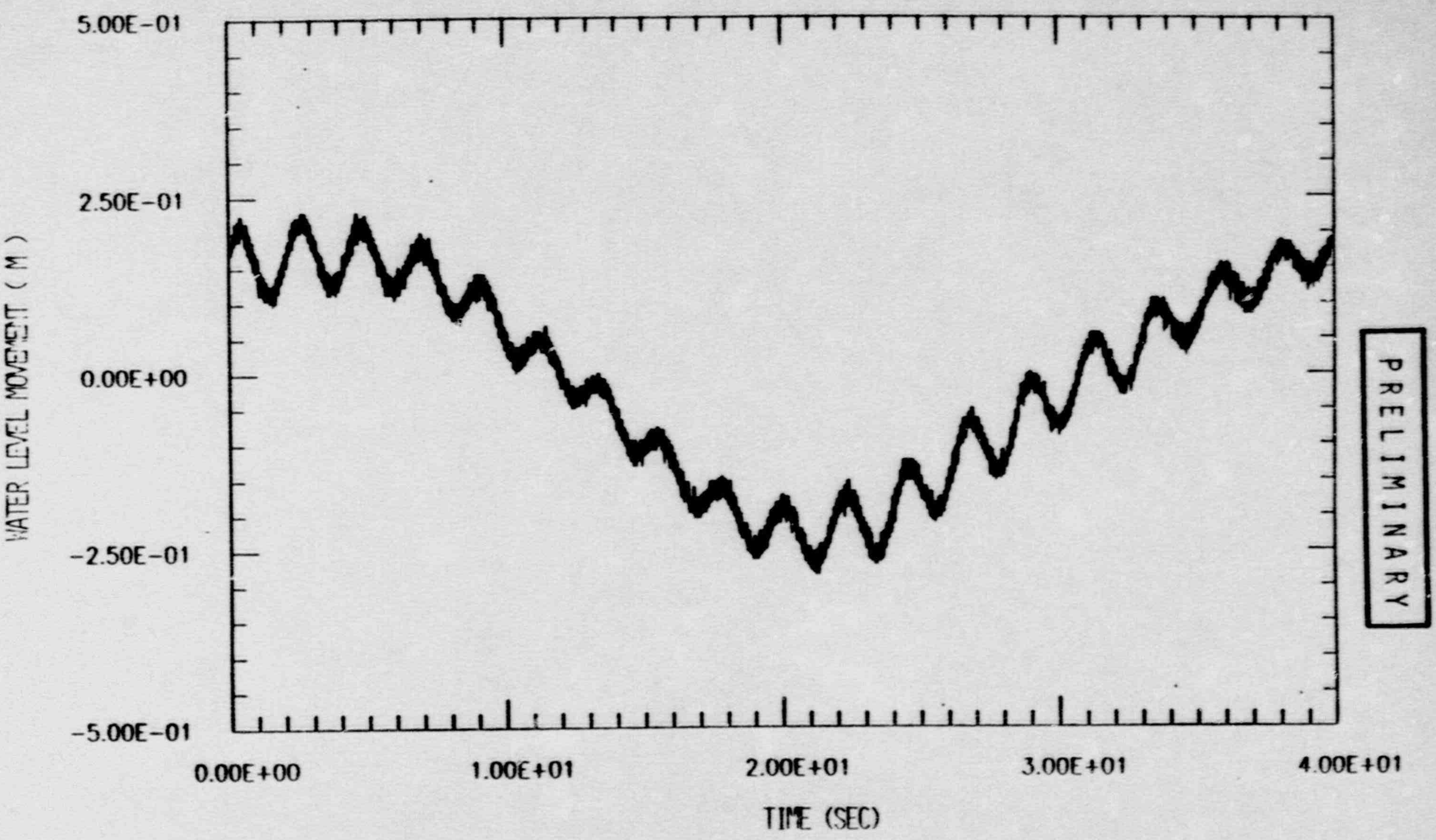


PRELIMINARY

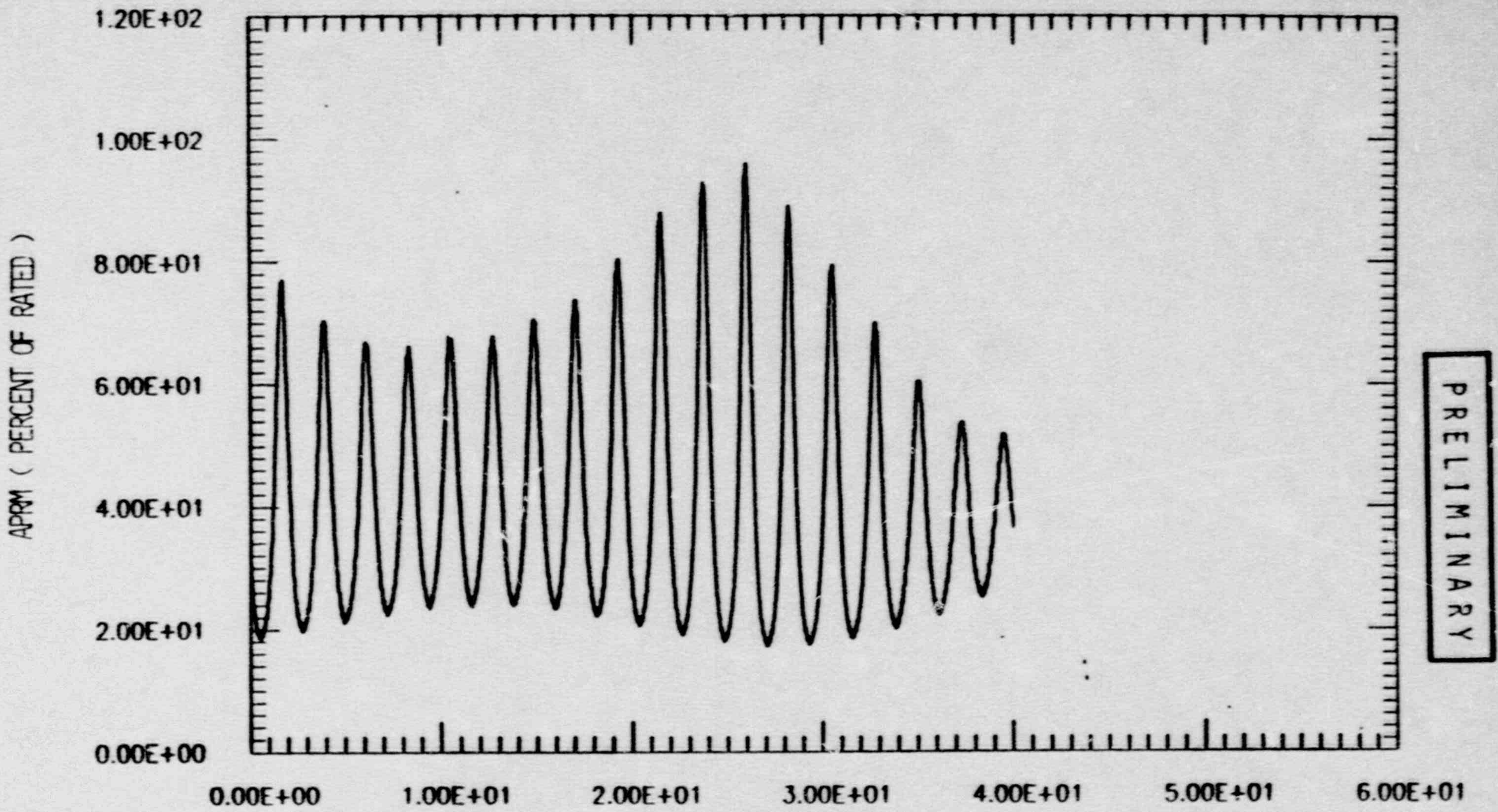
LASALLE PLANT DATA - FEEDWATER FLOW



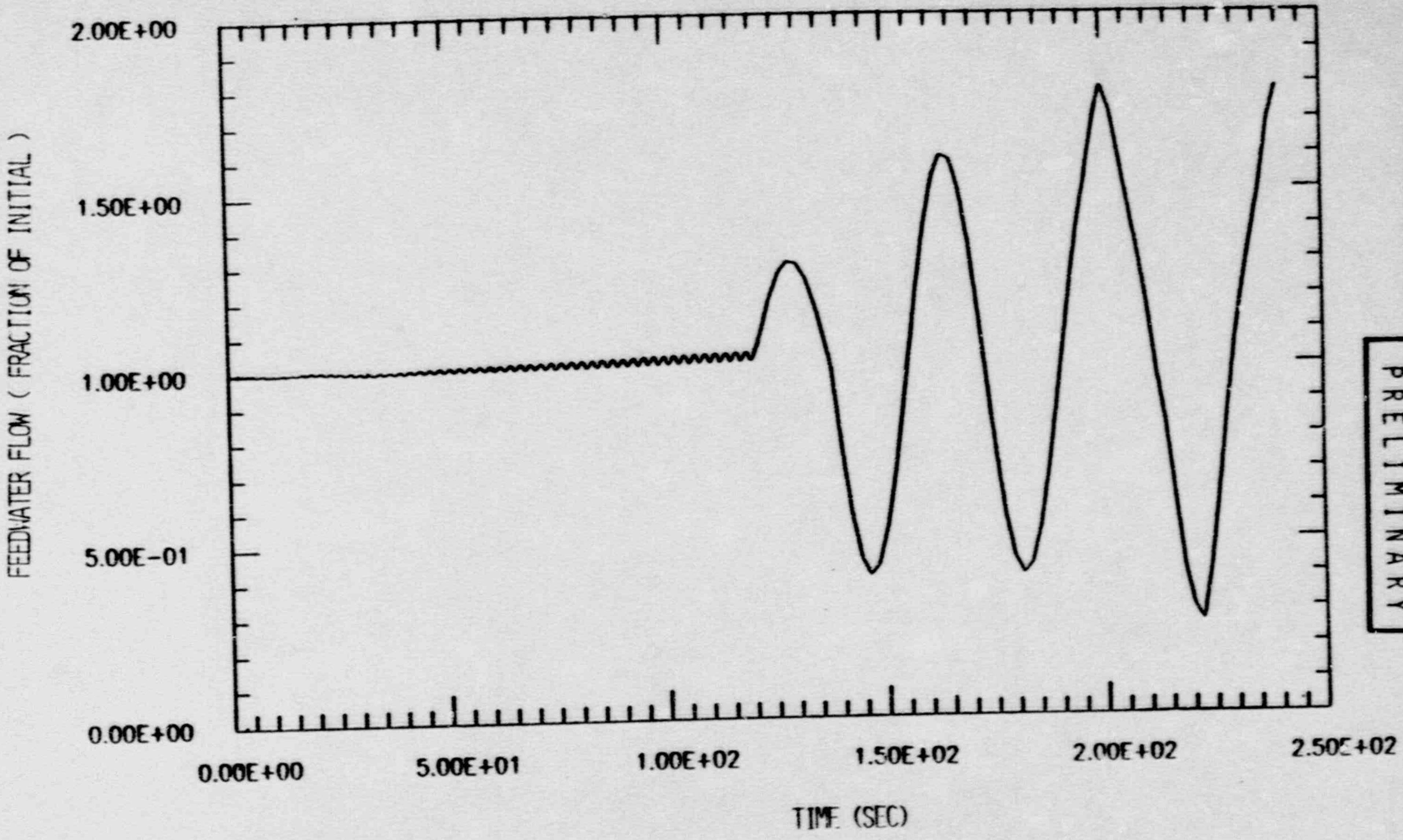
LASALLE PLANT DATA - FEEDWATER TEMPERATURE



LASALLE PLANT DATA - LEVEL MOVEMENT



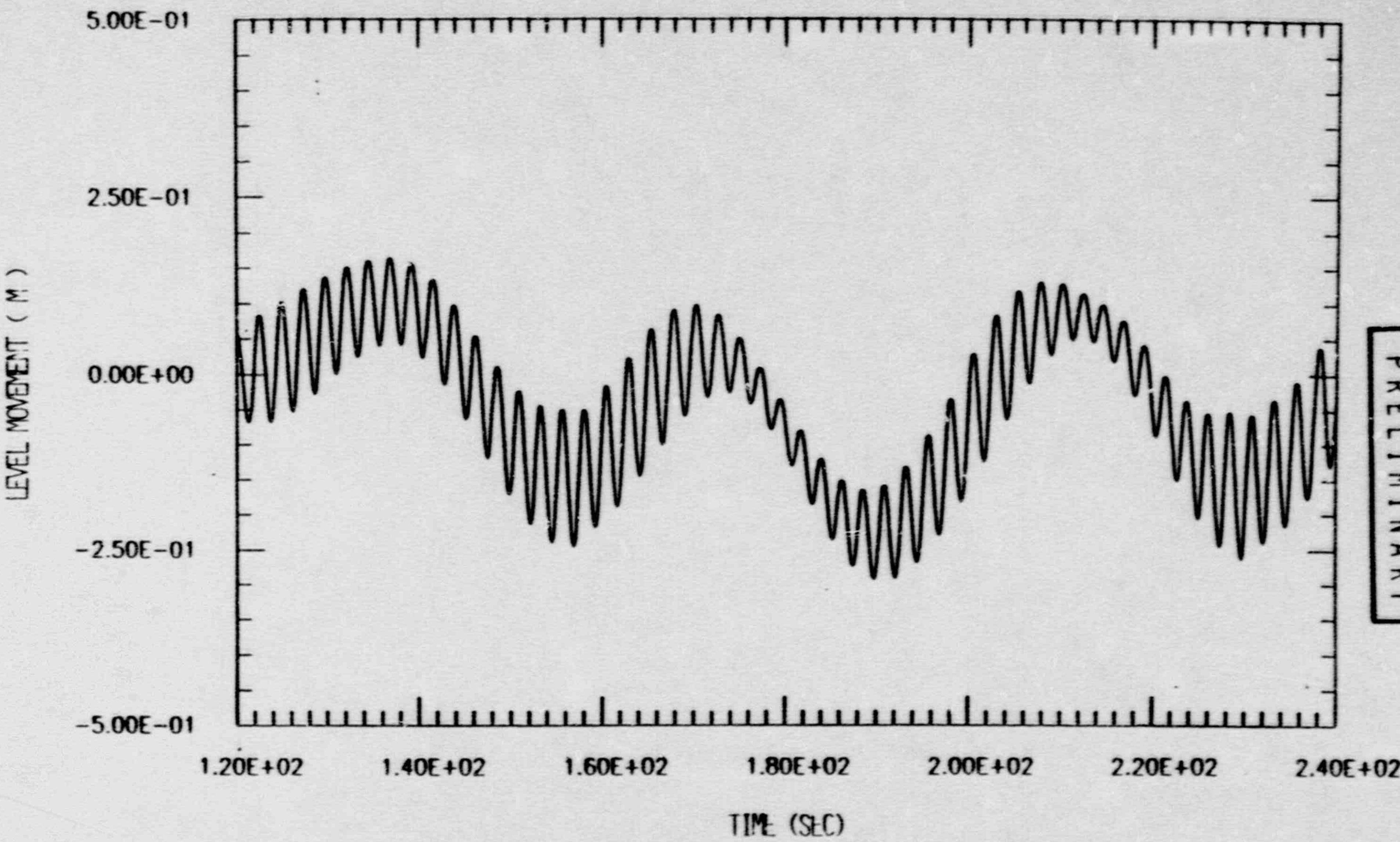
LASALLE PLANT DATA - APRM



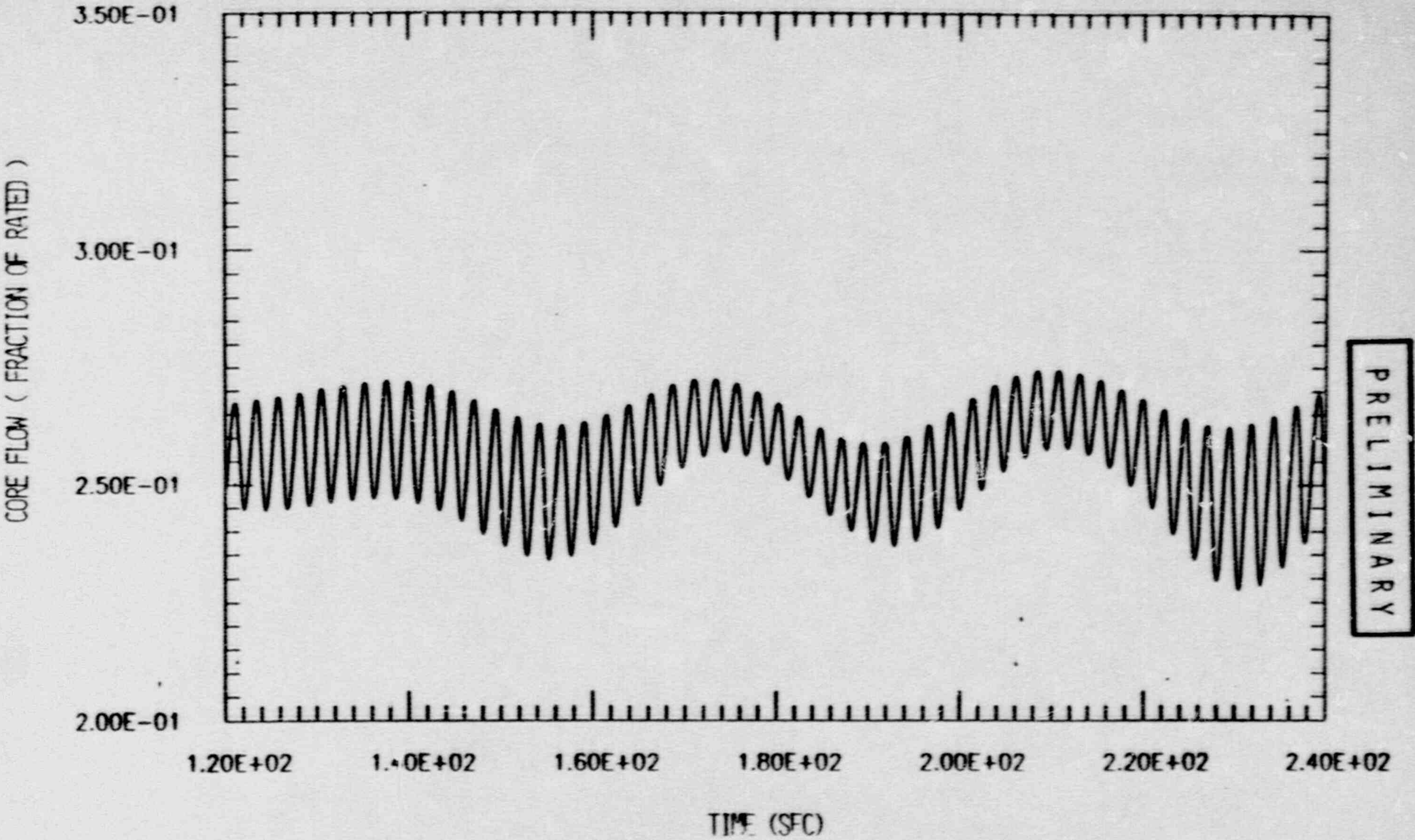
TRACG CALCULATION - FEEDWATER FLOW

PRELIMINARY

PRELIMINARY

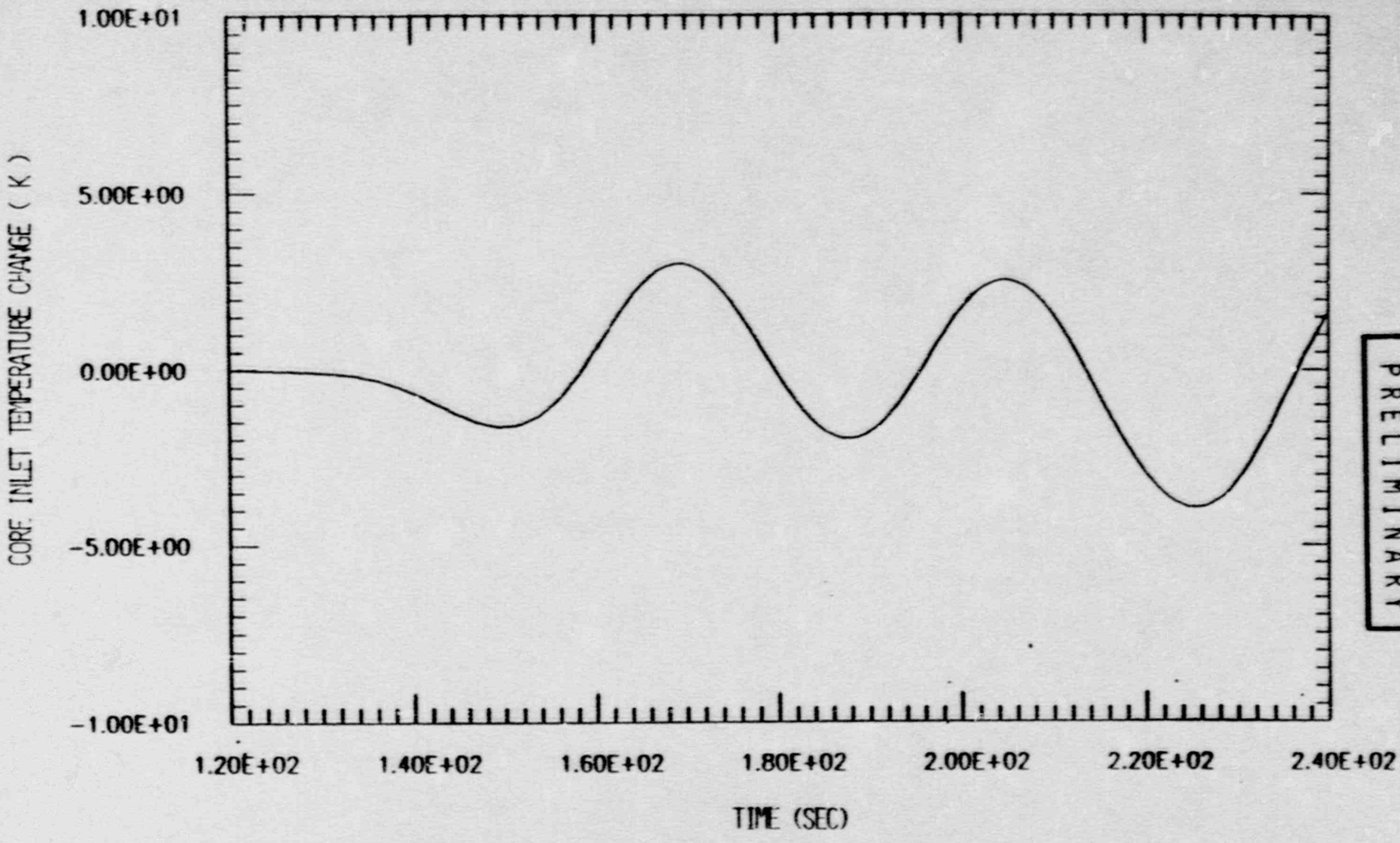


TRACG CALCULATION - LEVEL MOVEMENT

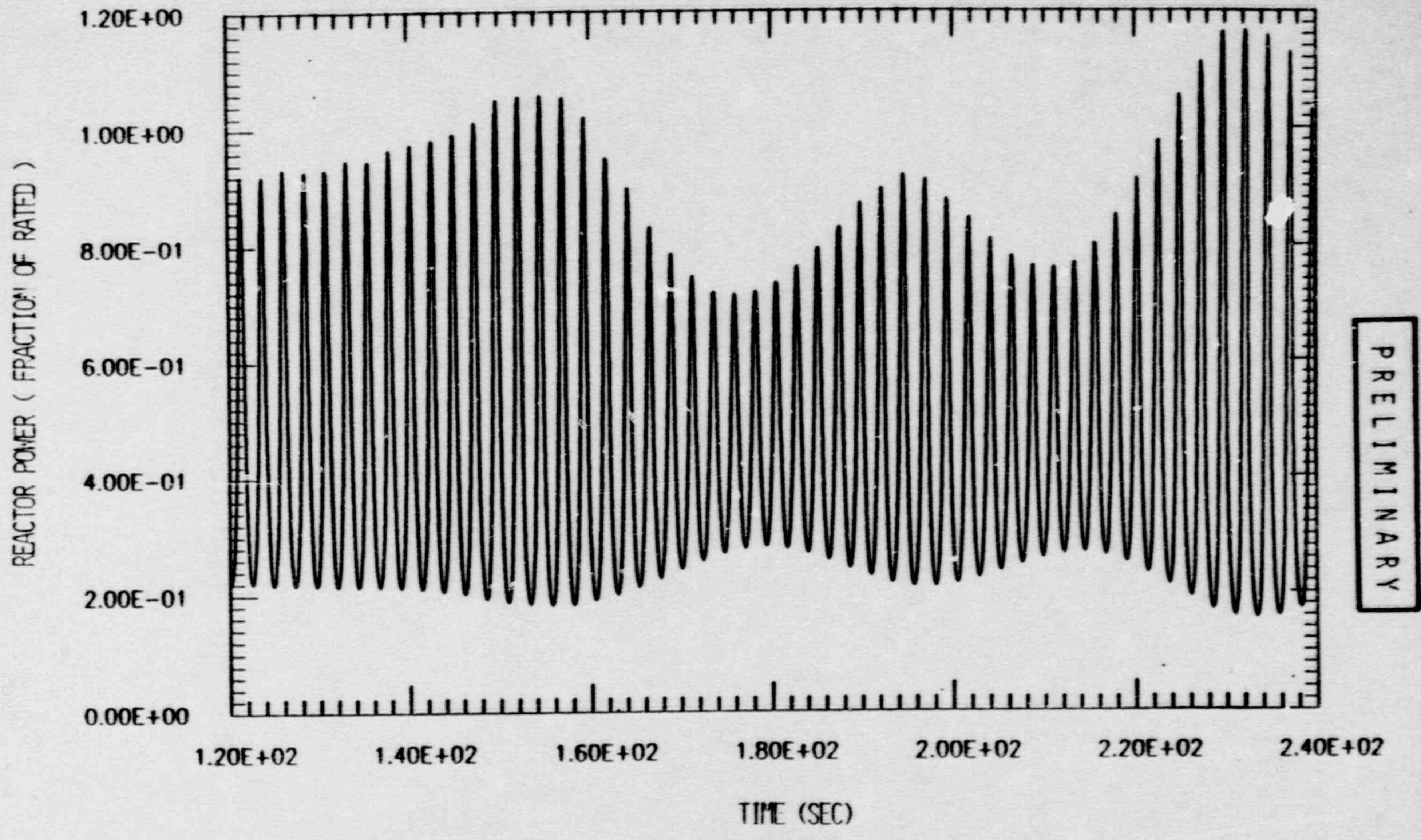


TRACG CALCULATION - CORE FLOW

PRELIMINARY

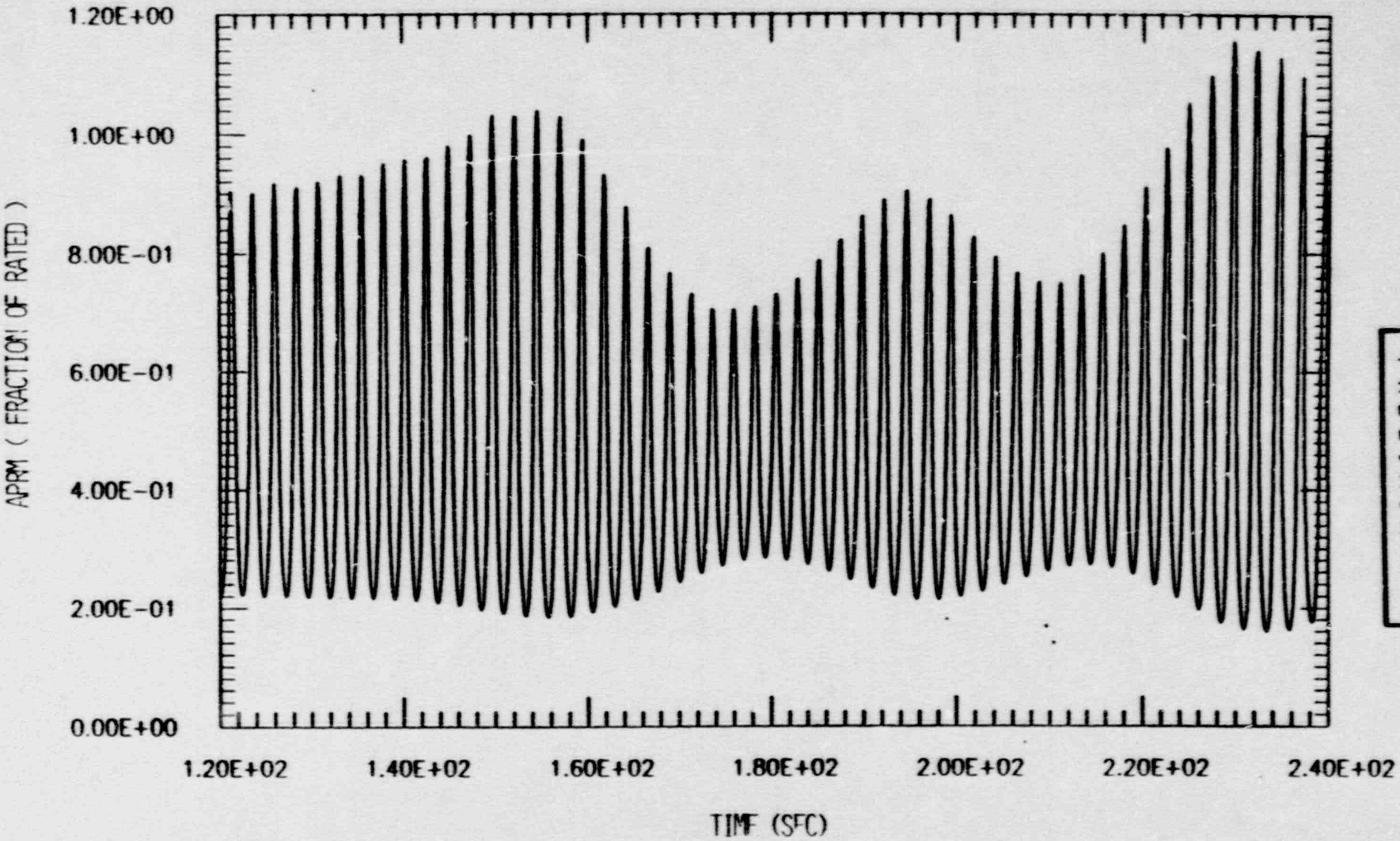


TRACG CALCULATION - CORE INLET TEMPERATURE



TRACG CALCULATION - REACTOR POWER

PRELIMINARY



TRACG CALCULATION - APRM

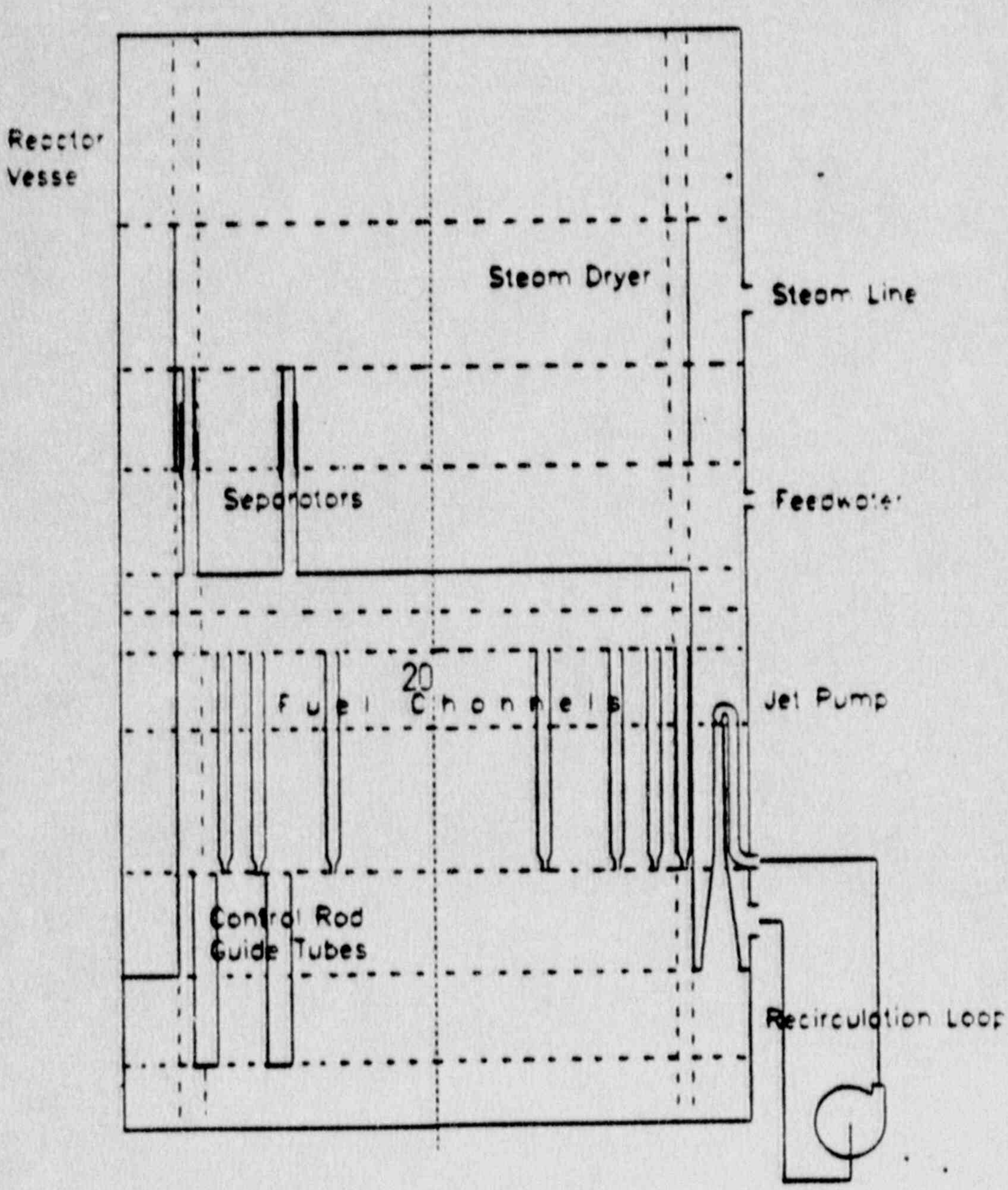
PRELIMINARY

CONCLUSIONS

- TRACG PREDICTS COREWIDE OSCILLATION AT LASALLE EVENT CONDITIONS WITH NO EXTERNAL FORCING PERTURBATION.
- OSCILLATION AMPLITUDES CONSISTENT WITH PLANT DATA CAN BE PREDICTED WITH TRACG.
- FREQUENCY OF POWER OSCILLATIONS AGREES WELL WITH DATA.
- FEEDWATER TRANSIENT PLAYED AN IMPORTANT ROLE IN THE EVENT CAUSING THE REACTOR SCRAM.
- WHEN THE FEEDWATER TRANSIENT IS SIMULATED, TRACG PREDICTS THE SYSTEM RESPONSE, INCLUDING REACTOR POWER, IN GOOD AGREEMENT WITH PLANT DATA.
- OSCILLATION AMPLITUDE FROM PLANT DATA AND TRACG SENSITIVE TO SMALL SYSTEM CHANGES.

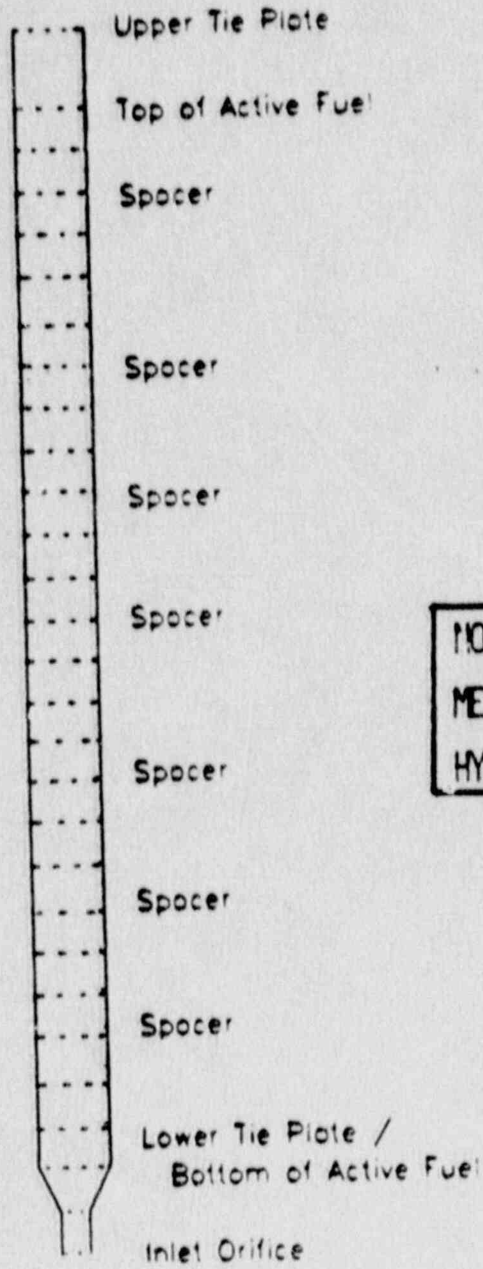
PRELIMINARY

LEIBSTADT STABILITY TESTS



REACTOR VESSEL NODALIZATION

PRELIMINARY

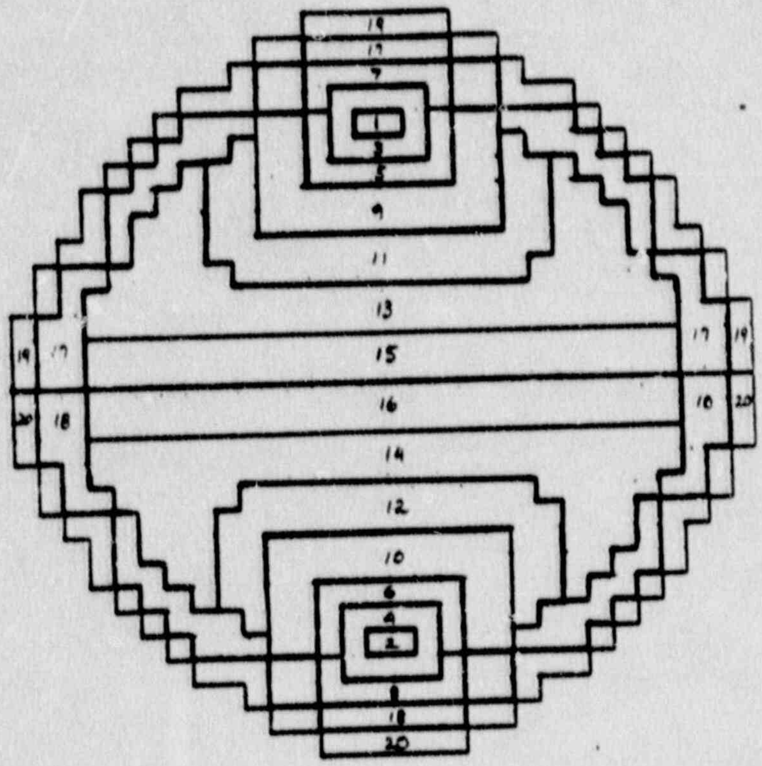


MODALIZATION AND NUMERICAL METHOD CONSISTENT WITH HYDRAULIC STABILITY TESTING

FUEL CHANNEL MODEL

PRELIMINARY

HYDRAULIC CHANNEL GROUPING

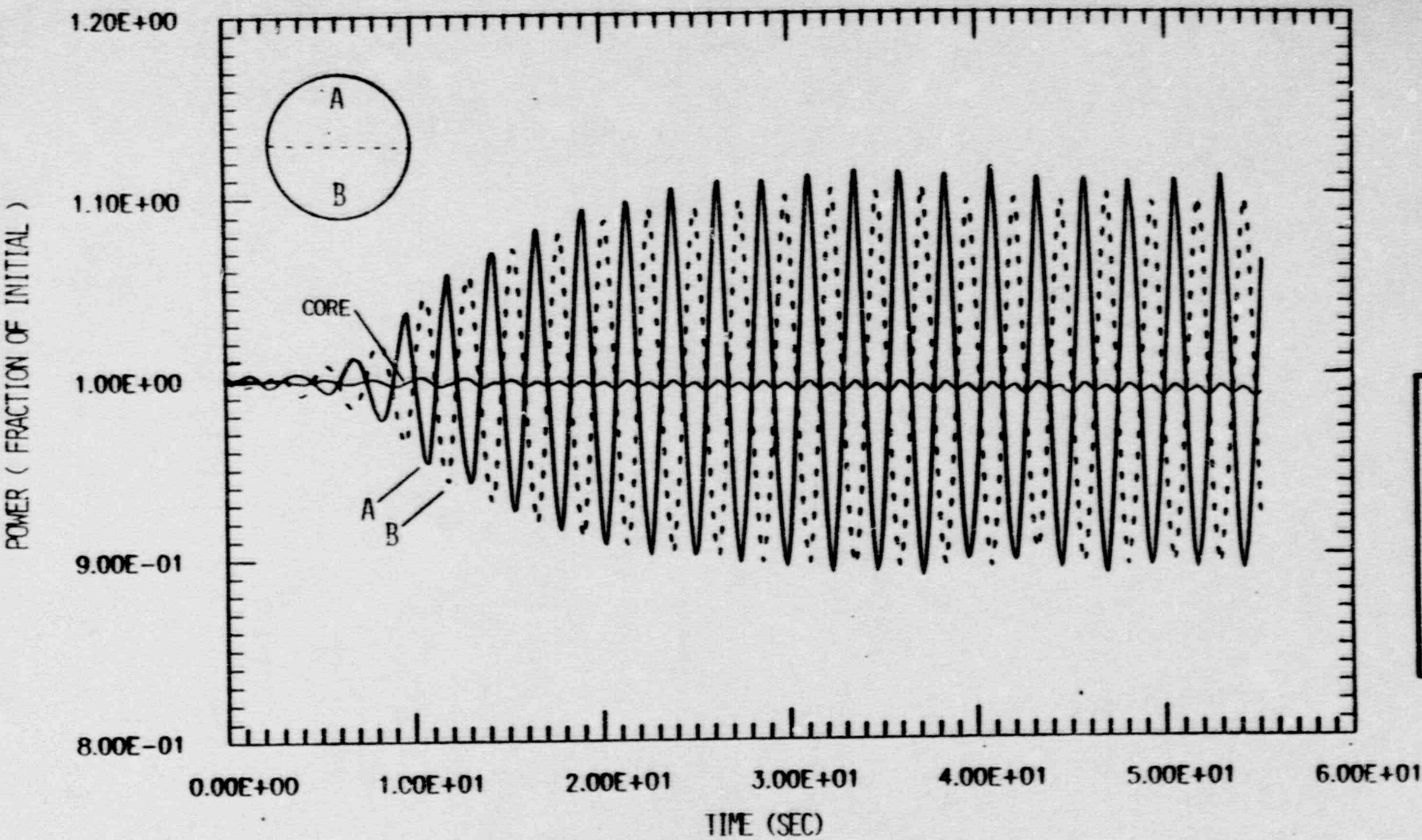


- 3D KINETICS FOR DISCRETE POWER CALCULATION FOR EACH BUNDLE.
- 20 CHARACTERISTIC HYDRAULIC CHANNELS.
- KINETICS COUPLED TO CONTROL SYSTEM TO PROVIDE SIMULATED LPRM AND APRM CALCULATIONS.

PRELIMINARY

TEST SIMULATION

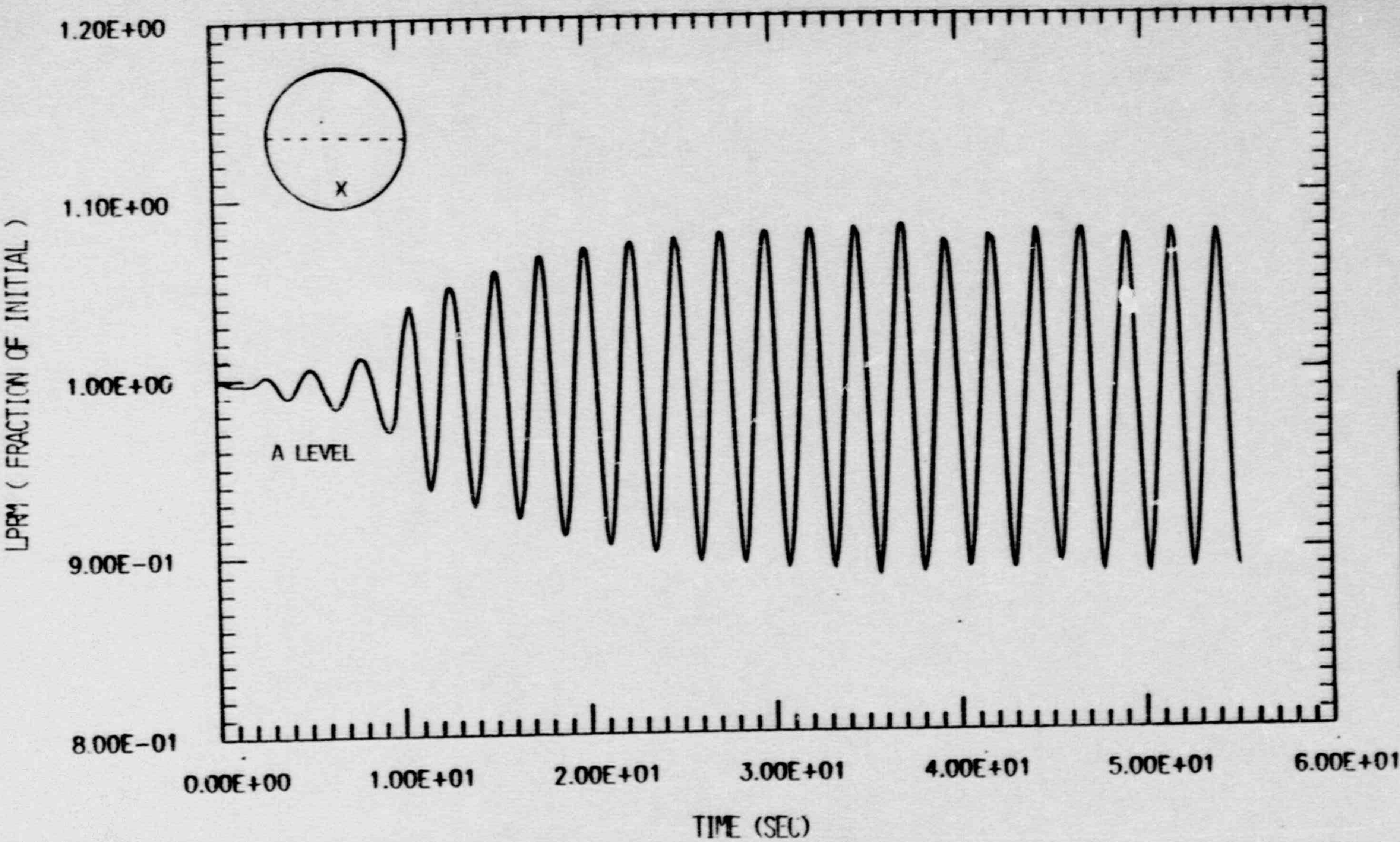
- INITIALIZE TRACG TO PRE TEST CONDITIONS.
- UTILIZE 3D BWR CORE SIMULATOR WRAPUP TO PROVIDE NUCLEAR DATA AND POWER SHAPE.
- UTILIZE PLANT DATA TO CHARACTERIZE HYDRAULIC CONDITIONS.



PRELIMINARY

LEIBSTADT TEST CONDITION 4 - POWER

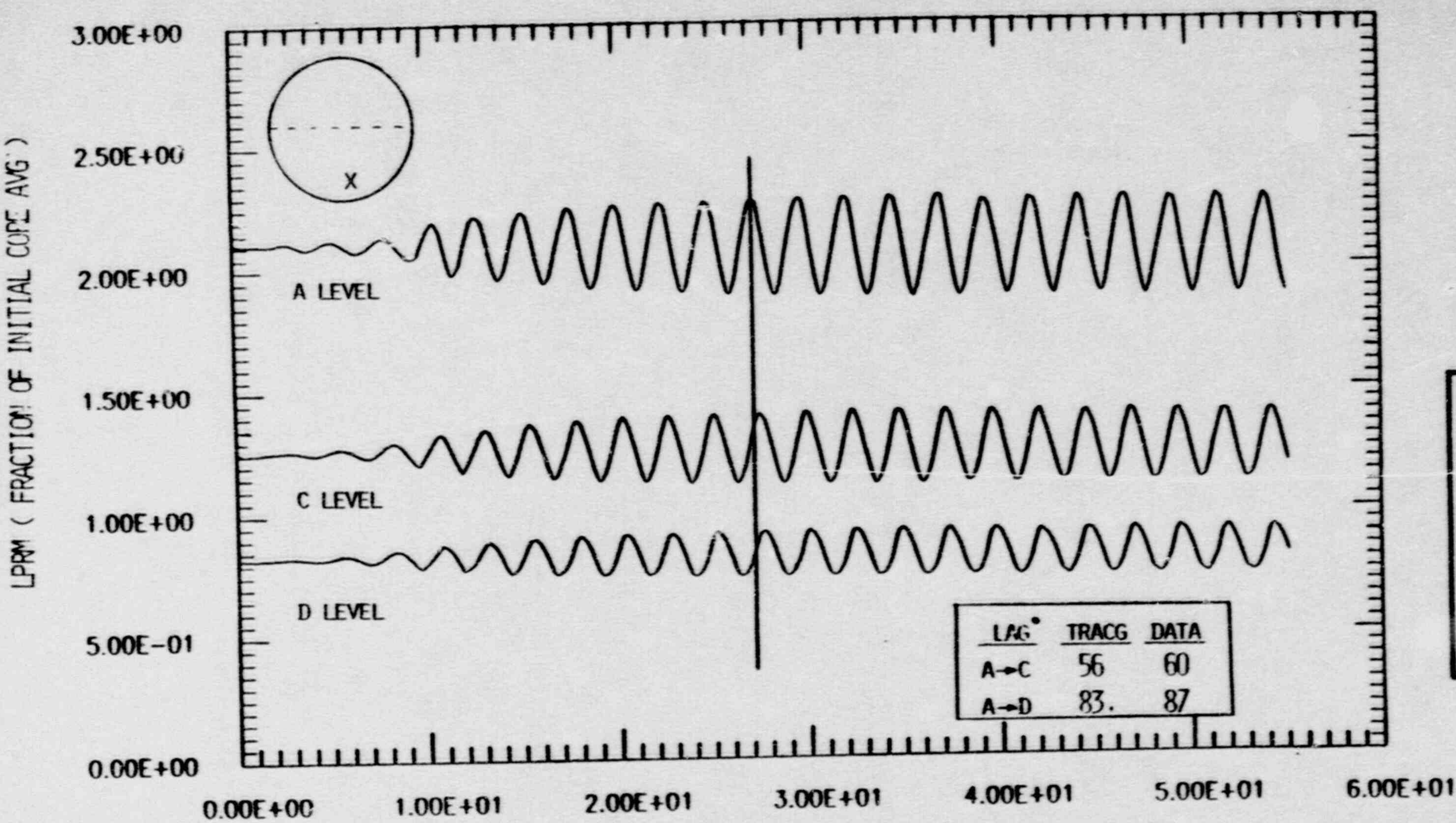
TRACG CALCULATION



LEIBSTADT TEST CONDITION 4 - LPRM

TRACG CALCULATION

PRELIMINARY

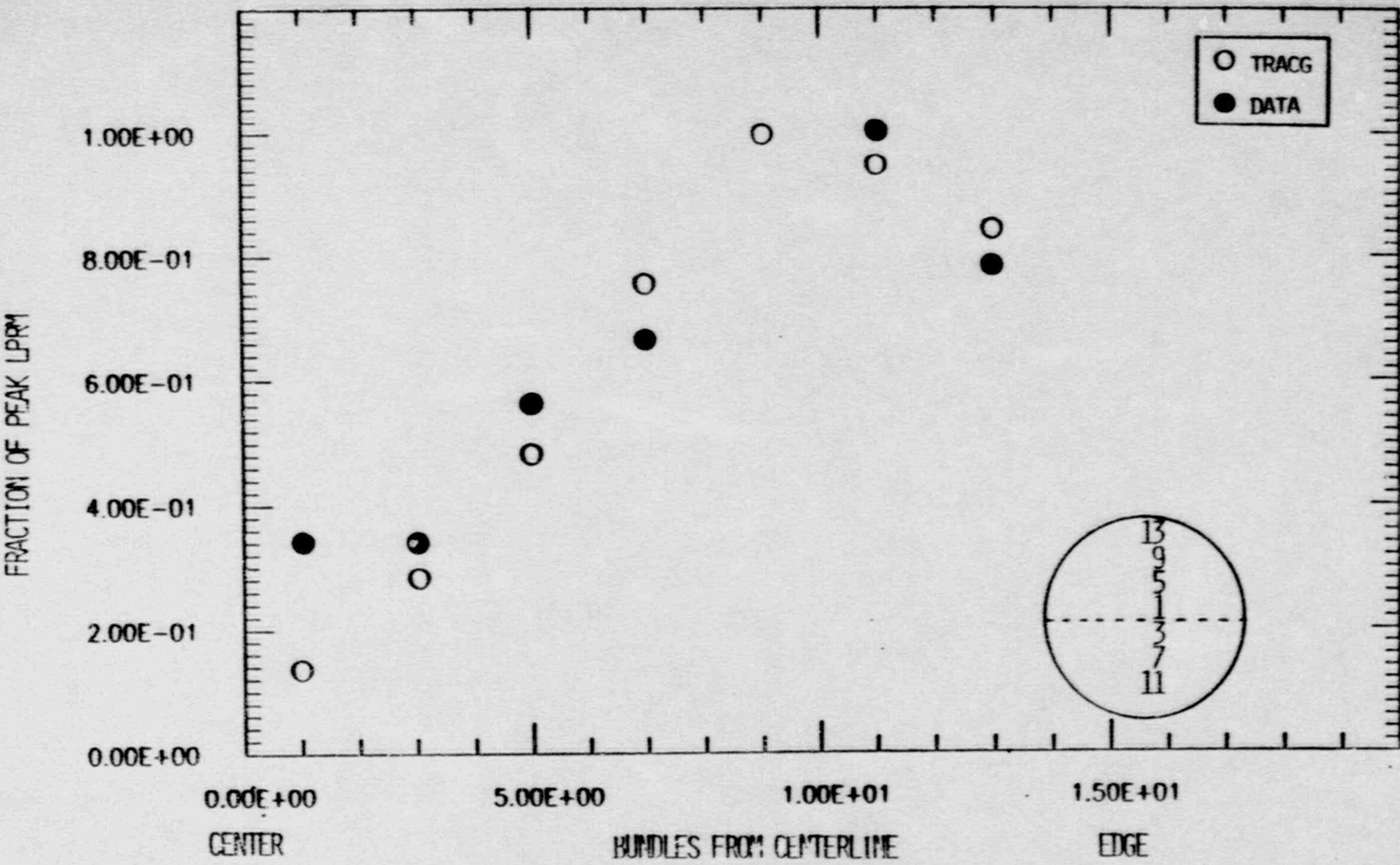


PRELIMINARY

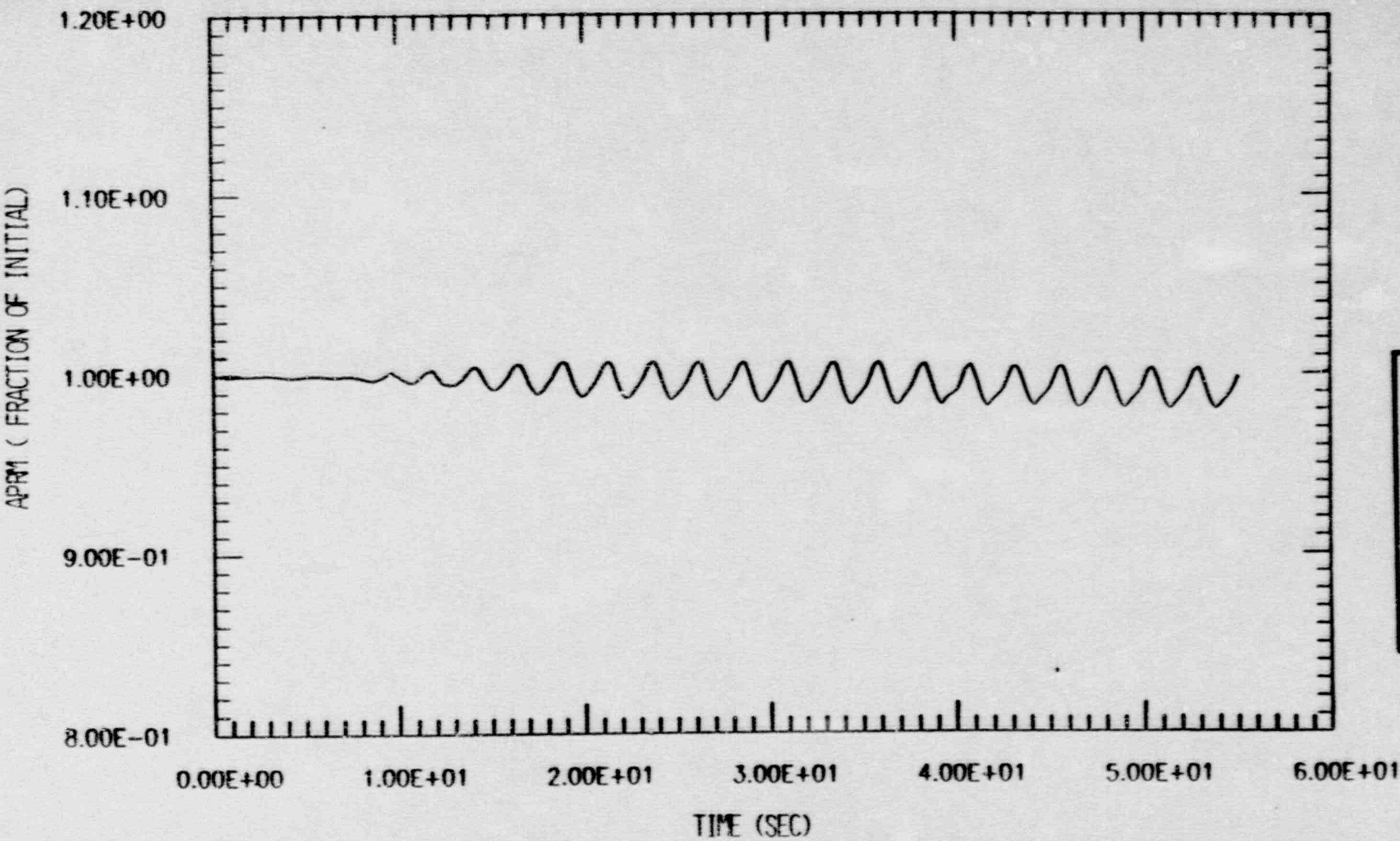
LEIBSTADT TEST CONDITION 4 - LPRM

TRACG CALCULATION

PRELIMINARY



LEIBSTADT TEST CONDITION 4 - POWER CONTOUR



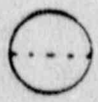
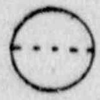
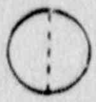
PRELIMINARY

LEIBSTADT TEST CONDITION 4 - APRM

IRACG CALCULATION

PRELIMINARY

CHANNEL GROUPING SENSITIVITY

GROUPING AXIS	GROUPING NUMBER	PEAK CHANNEL OSCILLATION
	20	1.0
	18	1.0
	10	0.88
	10	0.70

PRELIMINARY

LEIBSTADT DATA COMPARISON SUMMARY

TEST	LPRM (%) *		APRM (%) *		FREQ (HZ)	
	DATA	TRACG	DATA	TRACG	DATA	TRACG
4	14	19	4	3	.45	.41
4A	66	35	8	4	.45	.39
5	12	12	4	2	.45	.40
5A	12	12	4	2	.45	.39

* (P-P)/A

PRELIMINARY

CONCLUSIONS

- TRACG PREDICTS REGIONAL OSCILLATIONS OBSERVED UNDER TEST CONDITIONS WITH NO EXTERNAL FORCING PERTURBATION.
- LIMIT CYCLE OSCILLATIONS PREDICTED AT ALL TEST CONDITIONS ANALYZED.
- CONTOUR OF OSCILLATION AGREES FAVORABLY WITH TEST DATA.
- AXIAL CHARACTERISTIC OF DENSITY WAVE WELL PREDICTED.
- TRACG PREDICTION OF LPRM AND APRM OSCILLATION AMPLITUDE AND FREQUENCY IN GOOD AGREEMENT WITH DATA.
- IMPORTANT TO IDENTIFY DOMINANT CHANNEL AND AXIS OF OSCILLATION.
- LESS SENSITIVITY TO NUMBER OF HYDRAULIC CHANNELS USED TO SIMULATE CHANNELS AWAY FROM DOMINANT CHANNEL.

TRACG ANALYSIS LIMITATIONS

- o ONE-DIMENSIONAL BUNDLE REPRESENTATION
- o RESIDUAL NUMERICAL DAMPING/NODALIZATION SENSITIVITY
- o QUASI-STATIC PHENOMENOLOGICAL CORRELATIONS

GENERAL ANALYTICAL CONSIDERATIONS

- o COMPLEXITY OF PROCESS
  - Parameters Ranging From Gap Conductance To Loop Geometry Play A Role
- o SENSITIVITY TO PARAMETERS
  - (e.g., Core Flow, Power Distribution, Gap Conductance, Bundle Grouping).

FUTURE PLANS

- ADDITIONAL STABILITY QUALIFICATION

- QUANTIFY SENSITIVITIES

- MODAL ANALYSIS FOR BUNDLE GROUPING.

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TRAC–BF1 Description and Applicability to BWR Stability Analysis

S. Z. Rouhani

ACRS Thermal Hydraulic
Phenomena Subcommittee Meeting.
BWR T/H Stability Analysis Review
San Francisco, California
November 8, 1989

EG&G Idaho, Inc.

ECO01650

Background

- Start of TRAC-BWR development: INEL, 1979, TRAC-PD2 origin (LANL)
- Inclusion of BWR related models (TRAC-BD1)-1981
- Collaboration with GE (1979-1986)
- Inclusion of new constitutive relations (TRAC-BD1/MOD1, TRAC-BF0, and TRAC-BF1)

ECO01651

Main Features of TRAC–BF1 (cont'd)

- BWR–specific component models
  - Jet pump
  - Steam separator/dryer
  - Upper tie plate and SEO flooding (CCFL)
  - Balance–of–plant: turbines, condensors, feedwater heaters, valves

- Control system

- Containment modeling capability

Important Phenomena in Predicting BWR Instability

- Density wave propagation
  - Axial void propagation
  - Single-phase bundle friction
  - Two-phase bundle friction
  - Single and two-phase local losses
  - Direct deposition of power in coolant

- Reactor power
  - Void and Doppler feedback
  - Axial variation
  - Thermal attenuation through fuel cladding

ECO01655

TRAC–BF1 Models for Wall Friction and Local Losses

- Single–phase: laminar and turbulent (Pfann), accounting for wall roughness

- Two–phase: Martinelli–Nelson using Hancox multipliers

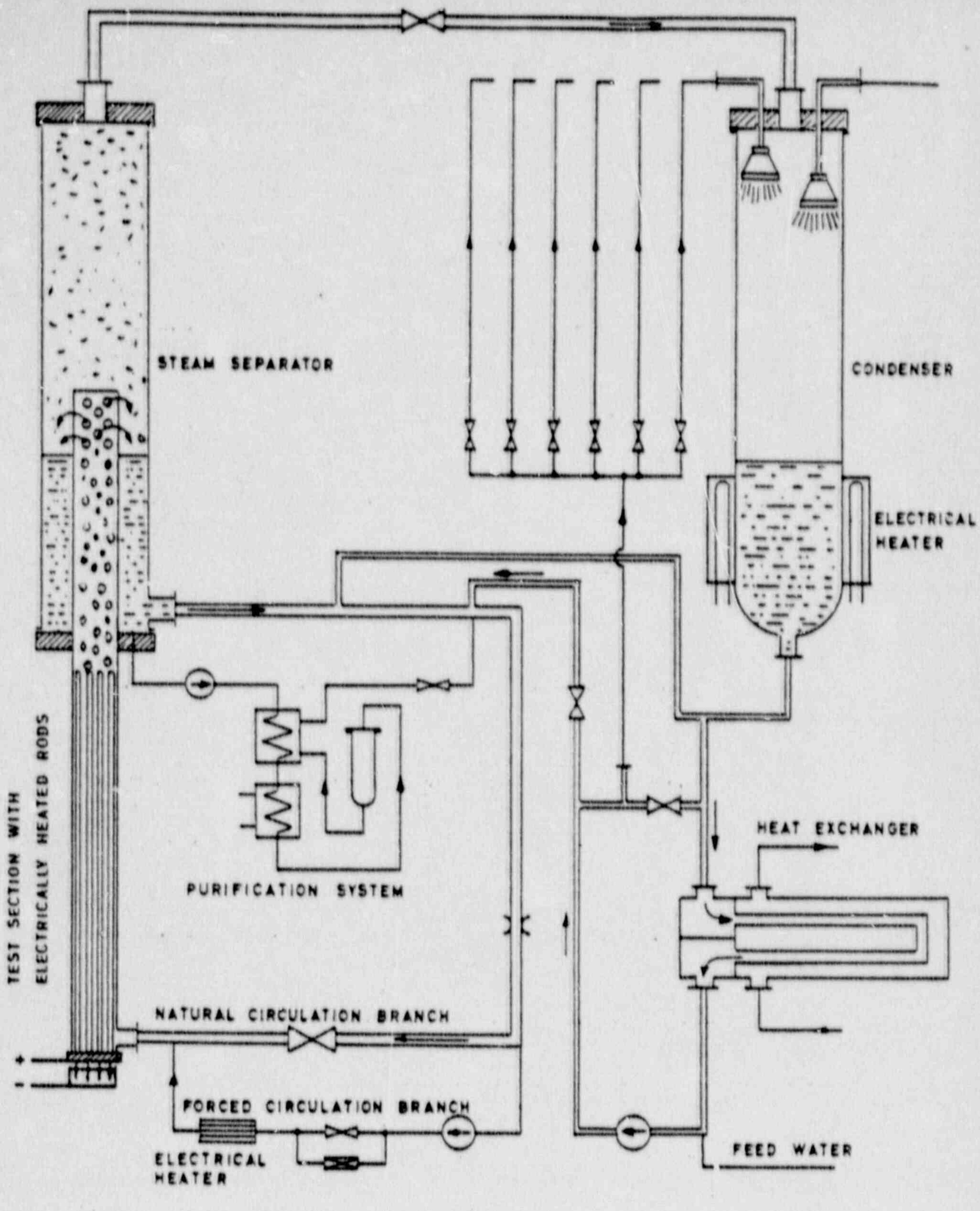
- Local losses
  - Single–phase: Darcy's equation using loss coefficients
  - Two–phase: Martinelli–Nelson using ρ_l/ρ_m multiplier (Kays and London)

ECO01656

Description of One–Dimensional Neutron Kinetics (cont'd)

- Polynomial neutron cross section model
- Multiple control rod banks
- Flexible nodalization
- Effect of inter–channel bypass can be included

ECO01073



CONSTRUCTION MATERIAL : CARBON STEEL

COOLING CAPACITY : 8 MW

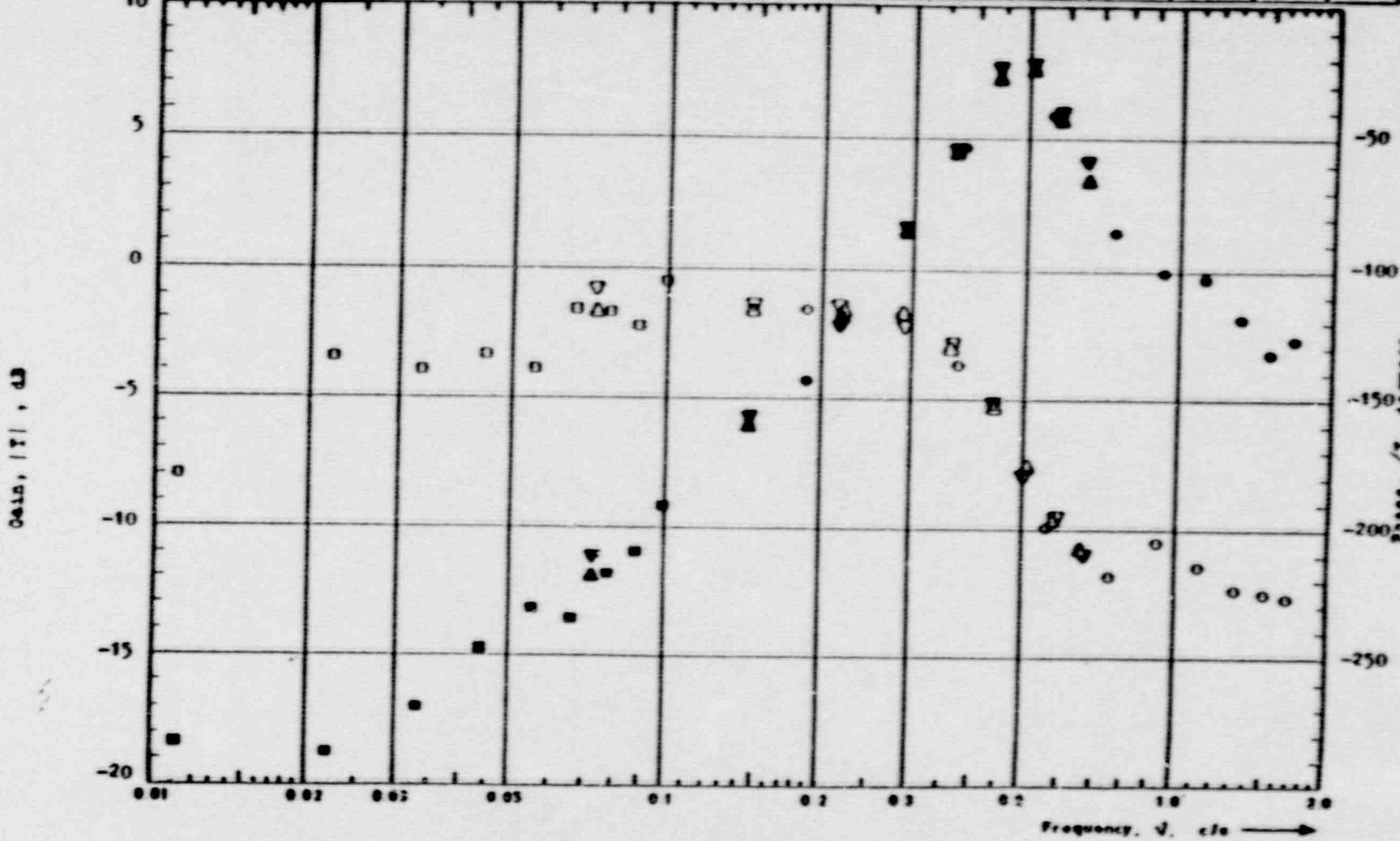
MAX. PRESSURE : 100 bars

FIG. 1 - SIMPLIFIED FLOW DIAGRAM FOR THE FRIGG LOOP.

Spectral Analysis of FRIGG Data

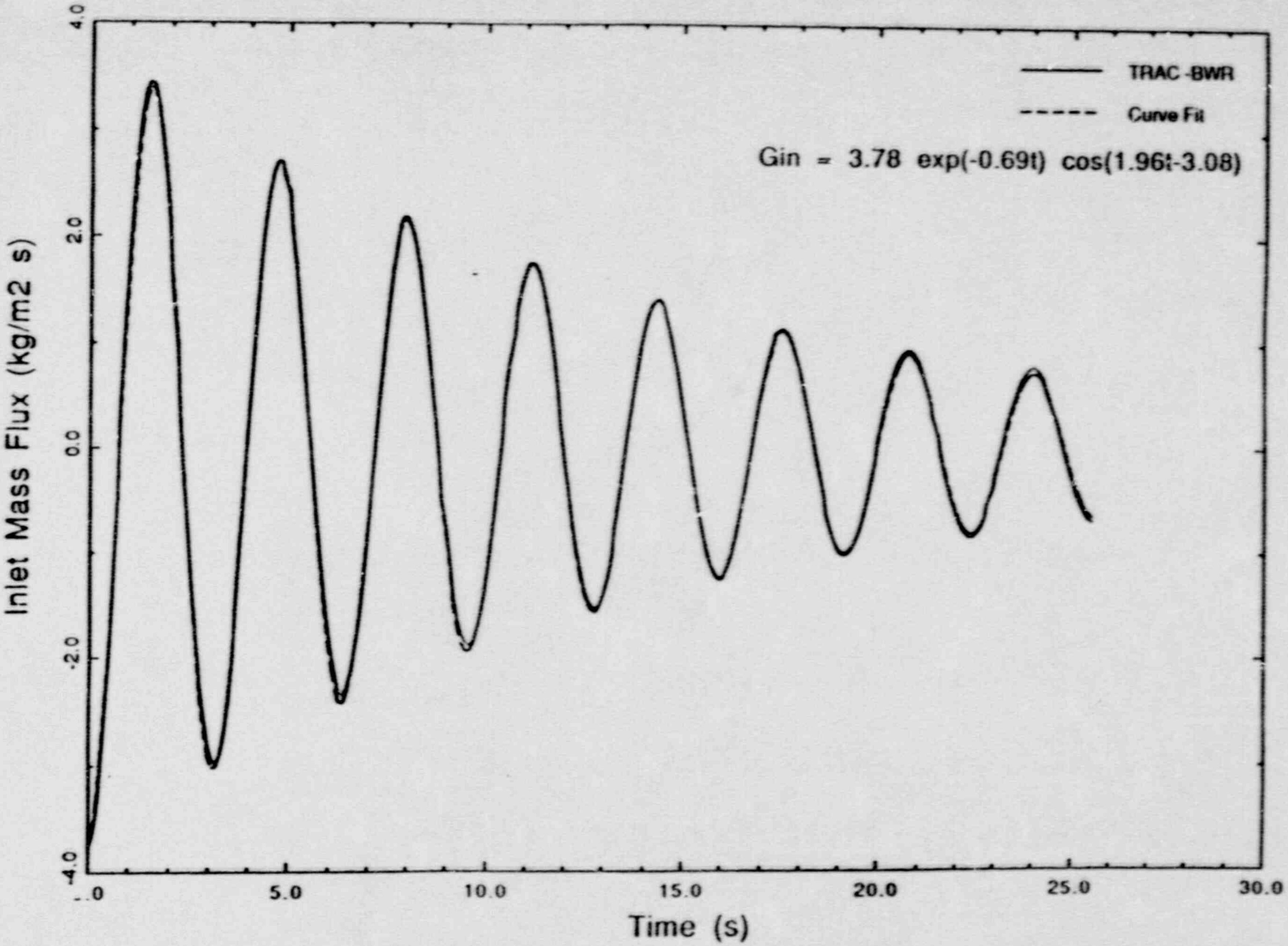
$$Y = \frac{\delta\Phi/\Phi}{\delta Q/Q}$$

Symbol $\lvert Y \rvert$	Symbol $\angle Y$	Run No	Test section	Perturbation δQ kW	Perturbation N	Perturbation T s	Sampling Δt_s s	Sampling ν_b c/s	Loop conditions h_{in}	Loop conditions p bars	Loop conditions $\Delta\vartheta_{sub}$ °C	Loop conditions Q kW	Loop conditions q/A W/cm²	Loop conditions G kg/m²s	Loop conditions x_{oe} %	Loop conditions α_{oe} %
■	□	362019	FT-36a	±50	19	91.2	0.6	0.3	14.1	49.7	4.6	3600	52.7	865	16.3	
▼	∇	362021	"	"	"	13.68	0.18	1.0	"	"	3.9	"	"	862	16.6	
▲	Δ	362022	"	"	"	"	"	"	"	"	"	"	"	"	"	
●	o	362023	"	"	"	5.32	0.14	3..	"	"	"	"	"	"	"	

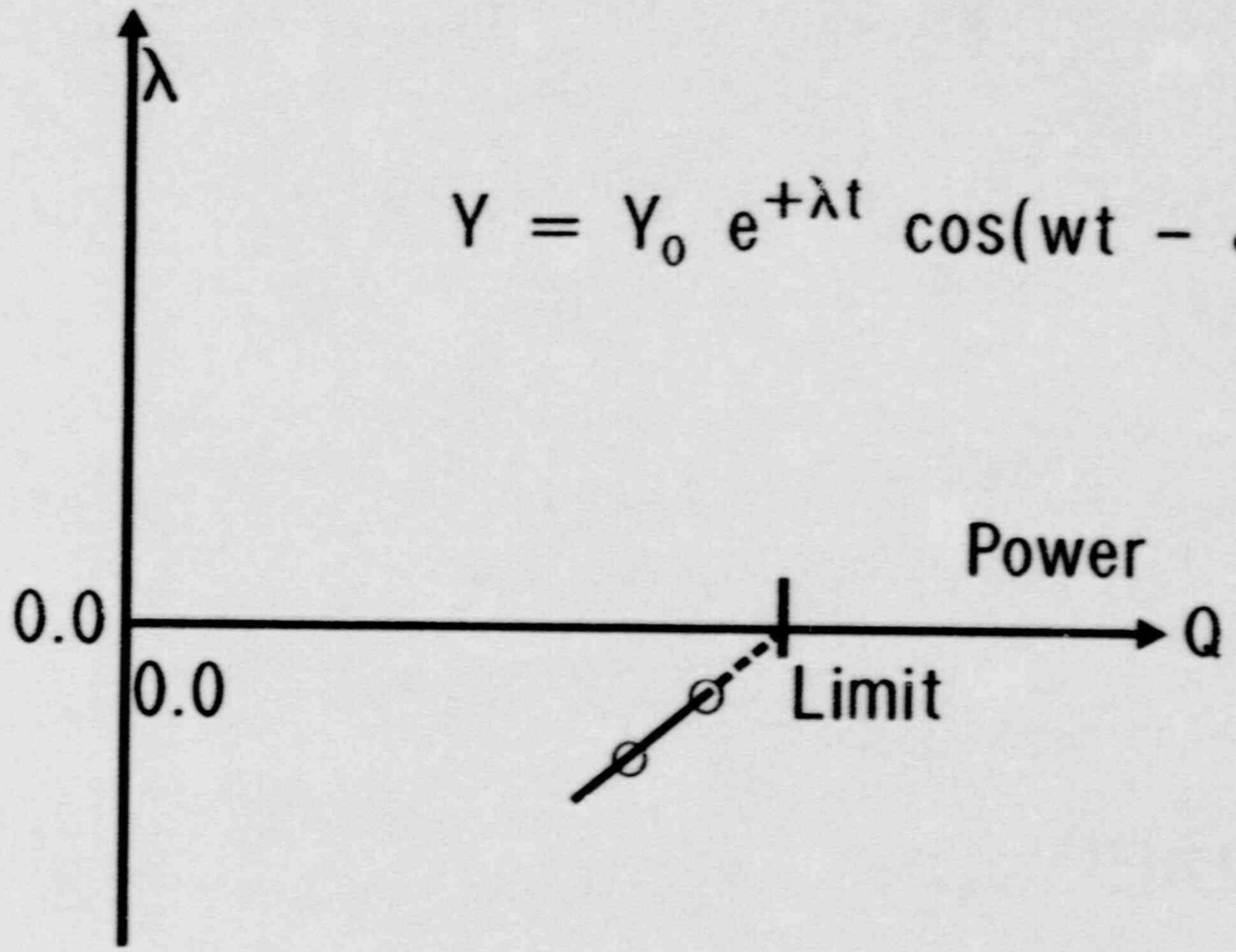


SAMPLE OF FRIGG DATA (FRIGG-2)

EXAMPLE OF FITTED FUNCTION TO TRAC-BF1 COMPUTED OSCILLATIONS



Extrapolation to Stability Limit



ECO01664

Summary and Conclusions

- TRAC-BF1 possesses the models needed to predict BWR instability behavior

- Assessment is ongoing, but is expected to demonstrate good agreement with data from a thermal-hydraulic perspective

ECO01666

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TRAC–BWR Assessment

W. L. Weaver

Presentation to the
ACRS Subcommittee on
Thermal Hydraulic Phenomena

November 8, 1989

EG&G Idaho, Inc.

ECO01609

TRAC-BF1 Description and Applicability to BWR Stability Analysis

S. Z. Rouhani

ACRS Thermal Hydraulic
Phenomena Subcommittee Meeting.
BWR T/H Stability Analysis Review
San Francisco, California
November 8, 1989

Idaho
National
Engineering
Laboratory

EG&G Idaho, Inc.

ECO01650

Outline

- Background
- TRAC-BF1 capabilities
- Important phenomena in predicting BWR instability
- TRAC-BF1 models relevant to instability
- Utilization of FRIGG data for TRAC-BF1 benchmarking
- Relation of TRACG
- Summary and conclusions

ECO01665

Background

- Start of TRAC-BWR development:
  INEL, 1979, TRAC-PD2 origin (LANL)

- Inclusion of BWR related models (TRAC-BD1)-1981

- Collaboration with GE (1979-1986)

- Inclusion of new constitutive relations
  (TRAC-BD1/MOD1, TRAC-BF0, and TRAC-BF1)

5CO01651

Main Features of TRAC–BF1

- Two–fluid, six equation model
  - 1–D loops
  - 3–D vessel
- 1–D channel model with multiple fuel rods
- Noncondensible gas transport
- Boron transport
- 1–D kinetics model
- Radiative heat transfer

ECO01652

Main Features of TRAC–BF1 (cont'd)

- BWR–specific component models
  - Jet pump
  - Steam separator/dryer
  - Upper tie plate and SEO flooding (CCFL)
  - Balance–of–plant: turbines, condensors, feedwater heaters, valves

- Control system

- Containment modeling capability

ECO01653

Main Features of TRAC–BF1 (cont'd)

- Level tracking model
- Dynamic slip critical flow model
- Heat conduction models (1–D) for fuel, gap, solid geometries
- Reflood heat transfer with moving mesh (including axial conduction)
- Courant–limit violating numerics

ECO01654

Important Phenomena in Predicting BWR Instability

- Density wave propagation
  - Axial void propagation
  - Single-phase bundle friction
  - Two-phase bundle friction
  - Single and two-phase local losses
  - Direct deposition of power in coolant

- Reactor power
  - Void and Doppler feedback
  - Axial variation
  - Thermal attenuation through fuel cladding

ECO01655

Axial Void Modeling in TRAC–BF1

- Subcooled void model from Lahey (modified Saha–Zuber)
- Interfacial shear model based on Ishii–Andersen's derivation, including flow regime effects and entrainment
- Interfacial heat transfer based on flow regime

ECO04657

TRAC–BF1 Models for Wall Friction and Local Losses

- Single–phase: laminar and turbulent (Pfann), accounting for wall roughness

- Two–phase: Martinelli–Nelson using Hancox multipliers

- Local losses
  - Single–phase: Darcy's equation using loss coefficients
  - Two–phase: Martinelli–Nelson using ρ_l/ρ_m multiplier (Kays and London)

ECO01656

Axial Void Modeling in TRAC–BF1 (cont'd)

- Wall heat transfer
  - Single–phase forced convection: Dittus–Boelter, McAdams (NC), Rohsenow and Choi (lam)
  - Nucleate boiling: Chen
  - Critical heat flux: critical quality concept for both Biasi and CISE–GE
  - Transition boiling: interpolation between critical heat flux and film boiling
  - Film boiling: Dougall–Rohsenow
  - Radiation: wall–to–wall and wall–to–coolant, gray body enclosure with user–specified cut–off void fraction
  - Condensation: Nusselt's condensation correlation
- Direct power deposition in coolant
  - User specified percent of fission and decay heat

ECO01658

Description of One-Dimensional Neutron Kinetics (cont'd)

- Polynomial neutron cross section model
- Multiple control rod banks
- Flexible nodalization
- Effect of inter-channel bypass can be included

Description of One–Dimensional Neutron Kinetics

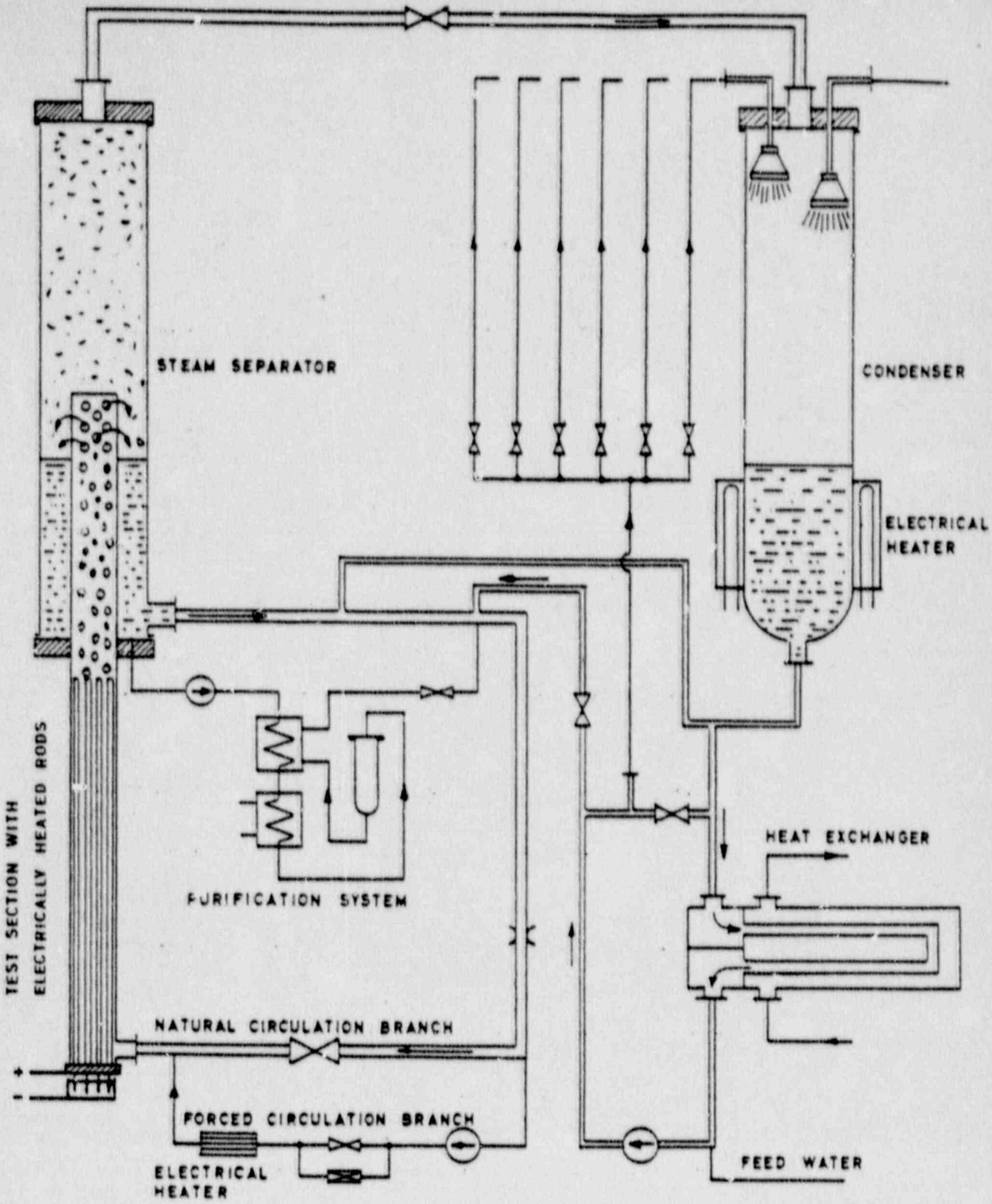
- Two–group, one–dimensional (axial) model based on Analytic Nodal method

- Albedo boundary conditions (axial)

- Buckling correction (radial)

- Static and dynamic modules
  - Improved time integration method
  - Crank–Nicholson to fully implicit

ECO01072



CONSTRUCTION MATERIAL : CARBON STEEL

COOLING CAPACITY : 8 MW

MAX. PRESSURE : 100 bars

FIG. 1 - SIMPLIFIED FLOW DIAGRAM FOR THE FRIGG LOOP.

TRAC–BF1 Assessment Using FRIGG Data

Objectives

- Demonstrate ability to predict test results
  - Axial void profile and inlet flow as a function of power during natural circulation (addressed separately)
  - Transfer function between power and void or flow
  - Limit of stability

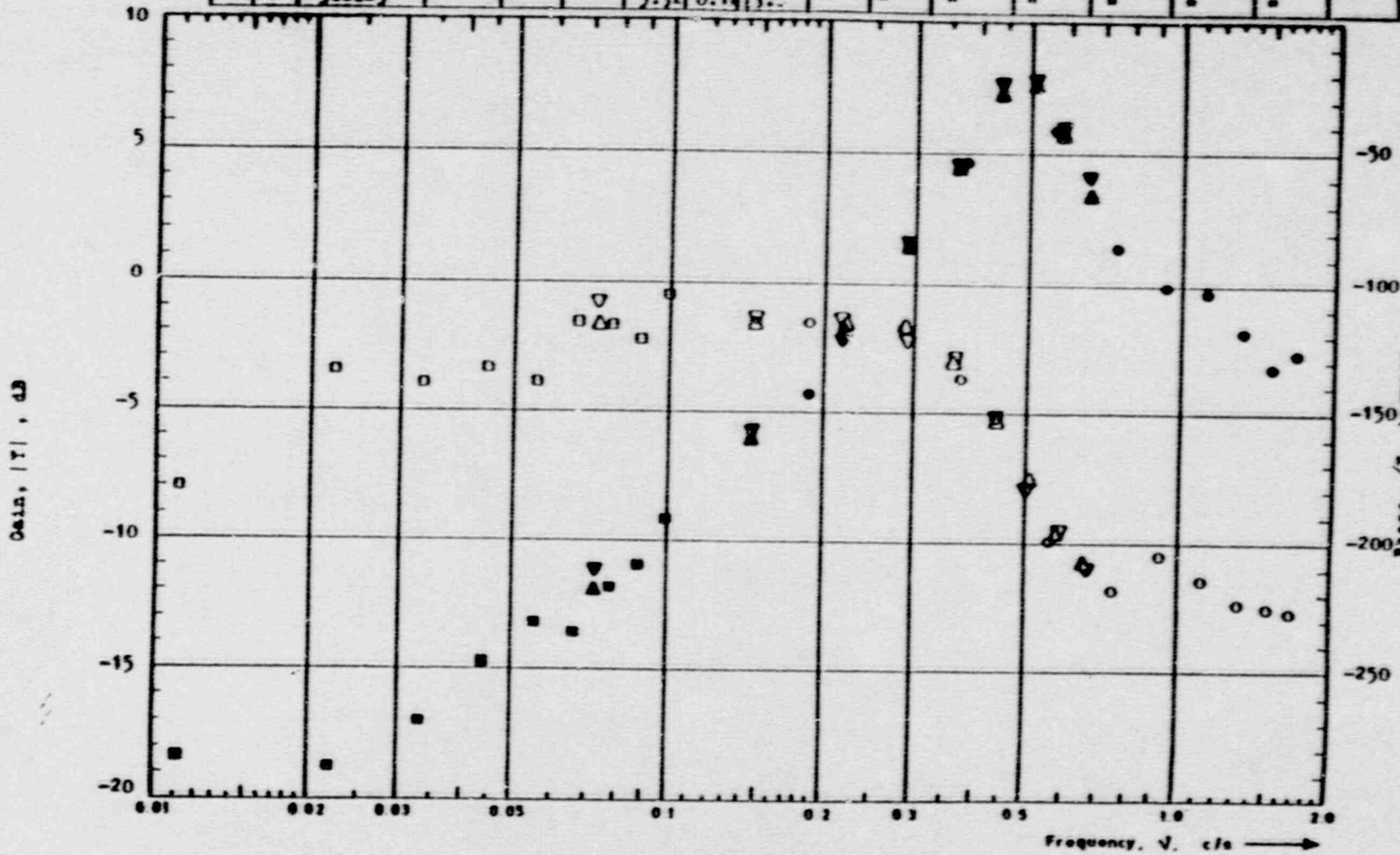
- Evaluate temporal and spatial convergence characteristics for damped and limit cycle oscillations (addressed separately)

ECO01659

Spectral Analysis of FRIGG Data

$$Y = \frac{\delta J/J}{\delta Q/Q}$$

Symbol $\lvert Y \rvert$	Symbol $\angle Y$	Run No	Test section	Perturbation δQ kW	Perturbation N	Perturbation T s	Sampling Δt_s s	Sampling ν_b c/s	Loop conditions h_{in}	Loop conditions p bars	Loop conditions $\Delta\vartheta_{sub}$ °C	Loop conditions Q kW	Loop conditions q/A W/cm²	Loop conditions G kg/m²s	Loop conditions x_{ex} %	Loop conditions α_{ex} %
■	□	362019	FT-36a	±50	19	91.2	0.6	0.3	14.1	49.7	4.6	3600	52.7	865	16.3	
▼	▽	362021	"	"	"	13.68	0.18	1.0	"	"	3.9	"	"	862	16.6	
▲	△	362022	"	"	"	"	"	"	"	"	"	"	"	"	"	
●	○	362023	"	"	"	5.32	0.14	3..	"	"	"	"	"	"	"	



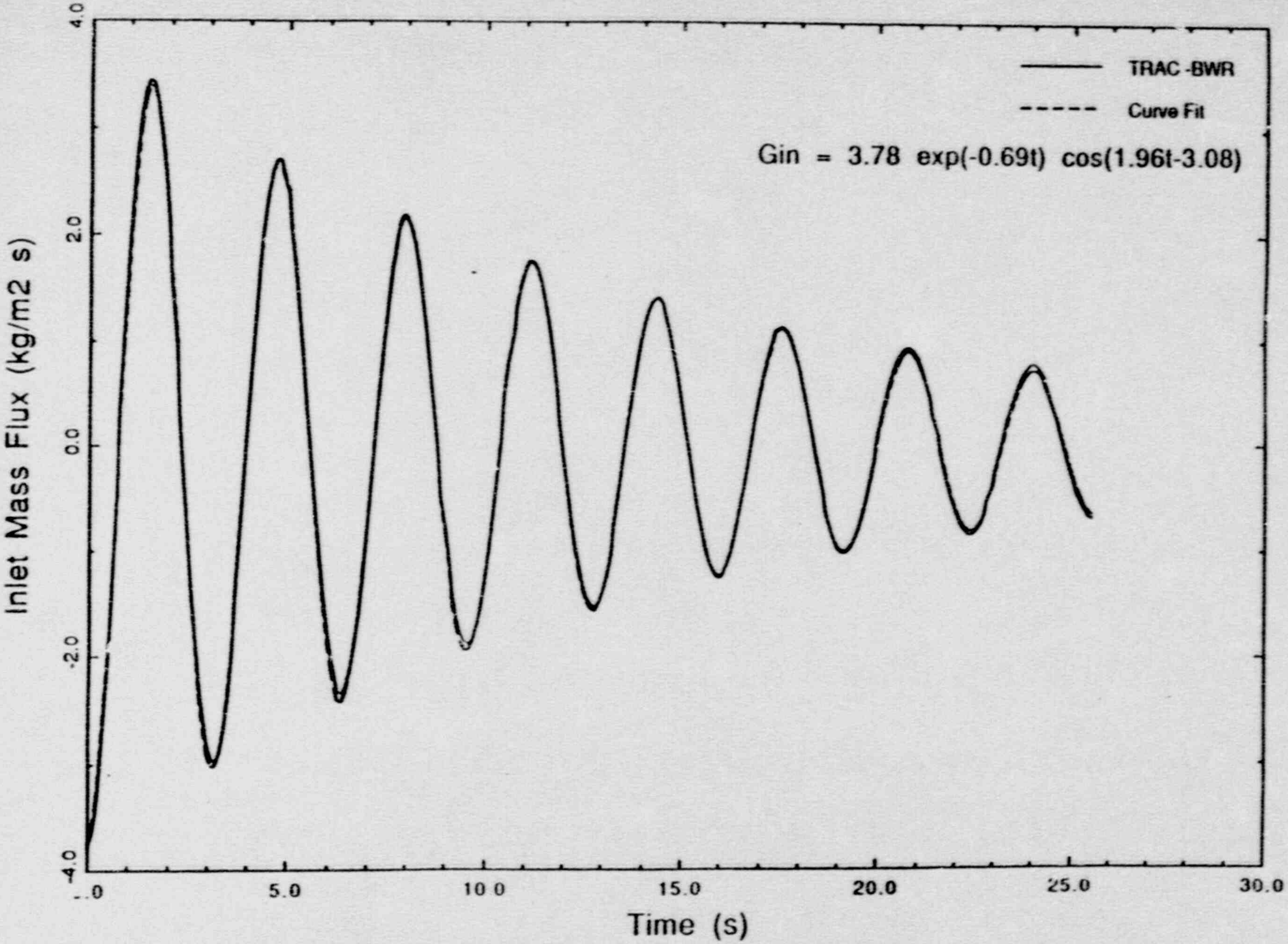
SAMPLE OF FRIGG DATA (FRIGG-2)

FRIGG Data

- Series of steady-state conditions
  - Flow, inlet subcooling, power were independent variables
  - Instability or dryout achieved under high power/low flow conditions but not sustained
- Transient tests
  - Power perturbations induced to permit spectral analysis of system response

ECO01660

EXAMPLE OF FITTED FUNCTION TO TRAC-BF1 COMPUTED OSCILLATIONS



Method for Comparing TRAC–BF1 and FRIGG Frequency Response

- Apply TRAC–BF1 to simulate a FRIGG run, with a small perturbation in power
- Approximate the calculated void and flow oscillations with

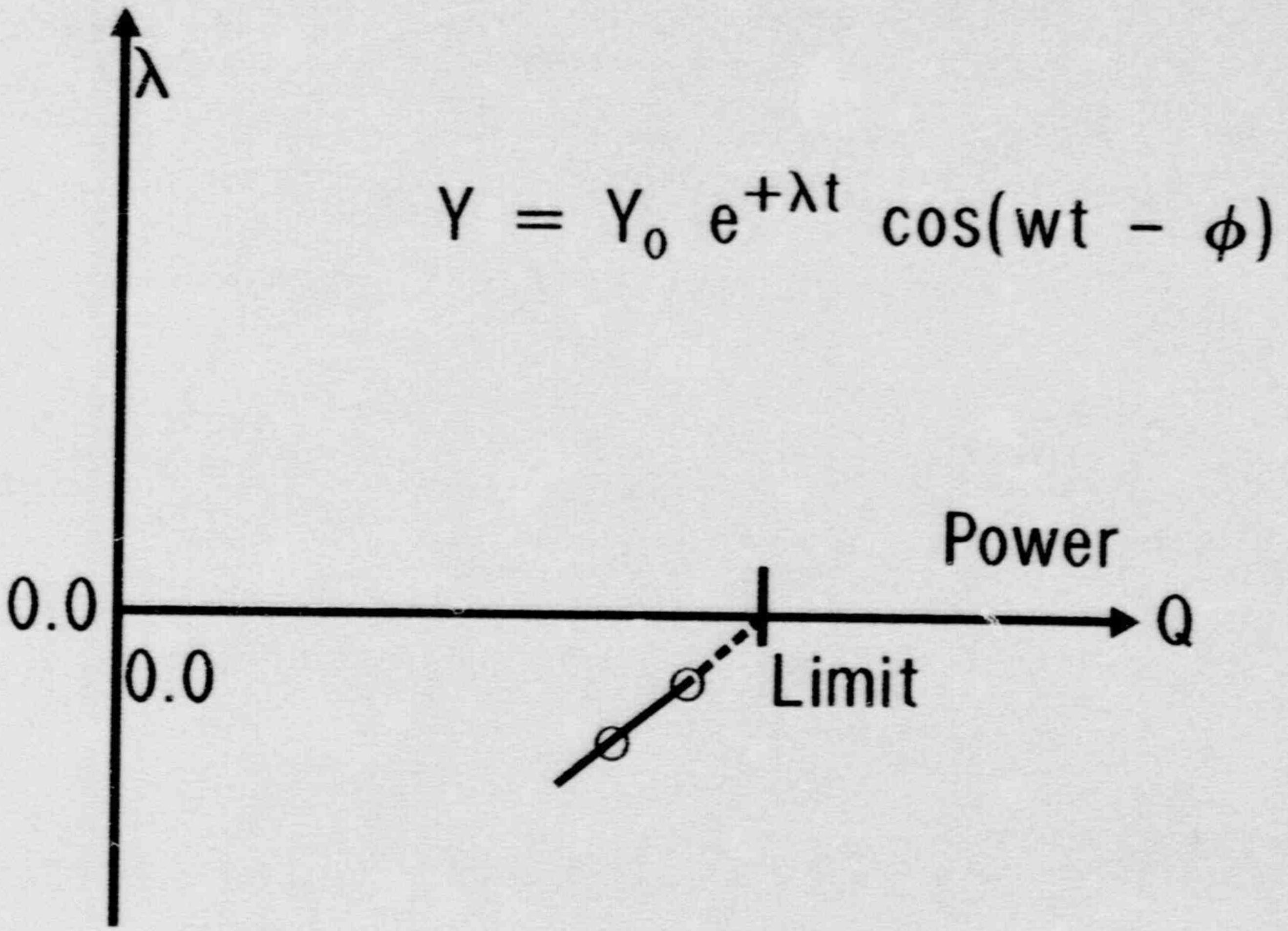
$$Y(t) = Y_0 \, e^{\lambda t} \cos(wt + \phi)$$

(finding Y_0, λ, w and ϕ)

- Calculate Fourier transforms of Y(t) and the perturbing signal (transform to frequency domain)
- Compare the gain and phase angle from TRAC–BF1 to FRIGG

ECO01661

Extrapolation to Stability Limit



ECO01664

Limit of Stability Determination

- Perform TRAC-B runs for two different power levels, each involving a perturbation

- Curve-fit the calculated oscillations of void or inlet flow with an exponential function

- Plot the exponents vs power and extrapolate to the power for zero exponent

ECO01663

Summary and Conclusions

- TRAC-BF1 possesses the models needed to predict BWR instability behavior

- Assessment is ongoing, but is expected to demonstrate good agreement with data from a thermal-hydraulic perspective

ECO01666

Relationship of TRAC–BF1 (NRC) to TRACG (GE)

- TRAC-BF1 and TRACG have basic similarities, but GE's code includes some proprietary correlations

- 24 common capabilities (some with slight differences)
  - 7 developed by INEL (e.g., fuel channel, critical flow, control systems)
  - 5 developed by GE (e.g., interphase shear, CCFL)
  - 10 developed jointly (e.g., level tracking, initialization)

ECO01667

Idaho
National
Engineering
Laboratory

TRAC–BWR Assessment

W. L. Weaver

Presentation to the
ACRS Subcommittee on
Thermal Hydraulic Phenomena

November 8, 1989

EG&G Idaho, Inc.

Outline

- Introduction
- TRAC-BWR assessment
- TRAC-BWR limitations
- Summary

ECO01610

TRAC–BWR Assessment

- Extensive assessment
  - Developmental assessment
  - Independent assessment
- Assessment of component models
  - Jet pump
- Assessment of process models
  - CCFL
  - Wall heat transfer including subcooled boiling model
  - Interfacial friction
  - Critical flow
  - Radiation heat transfer

ECO01611

TRAC–BWR Assessment (cont'd)

- Assessment using integral test data
  - Large break LOCA
  - Small break LOCA
  - Reflood
  - Startup tests
  - ATWS

TRAC–BWR Has Undergone Extensive Developmental and Independent Assessment:

Developmental Assessment	Independent Assessment [3]
INEL Jet Pump [1]	BWR/3 LBLOCA
8 x 8 Bundle CCFL Test	FIST 6PMC2, 6PNC1-2b, 6PNC1-4, 6PNC1-6
CISE Adiabatic Pipe Test [1]	BWR/6-218 LBLOCA
Dartmouth Air–Water Flooding	BWR/4 Startup Data (Browns Ferry)
GE Level Swell [1]	Dartmouth CCFL (BNL)
Marviken Tests 15 and 24	U. of Houston CCFL (BNL)
Christensen	THTF 3078H (BNL)
Marchaturre et al.	
Nylund et al. –Frigg Loop Project [1]	
Bennett et al. [1]	
Lehigh Tests	

(1) BD1/MOD1 and BF-1

(2) BF-1

(3) BD-1/MOD1

ECO01050

TRAC–BWR Has Undergone Extensive Developmental and Independent Assessment:

Developmental Assessment	Independent Assessment [3]
Heinemann et al.	Marviken Test 18
Nilsson et al–GOTA Radiation	SSTF EA3.1, Run 111
GOTA Test 42	ROSA-III, Run 912
THTF Test Cases [1]	FIX-II, 3025
BWR/6 Large Breaks [1]	Neptune Boiloff, Reflood (Switzerland)
BWR/4 MSIV Trip Transient ATWS (w/o BOP)	
TLTA Test 6423 [1]	
BWR/4 Feedwater Control Failure ATWS	
Loss of Feedwater Heater Transient	
FIST Power Transient 6PMC2 [1]	
BWR/6 Small Break [2]	
FIST Small Break 6SB1 [2]	

(1) BD1/MOD1 and BF-1

(2) BF-1

(3) BD-1/MOD1

ECO01051

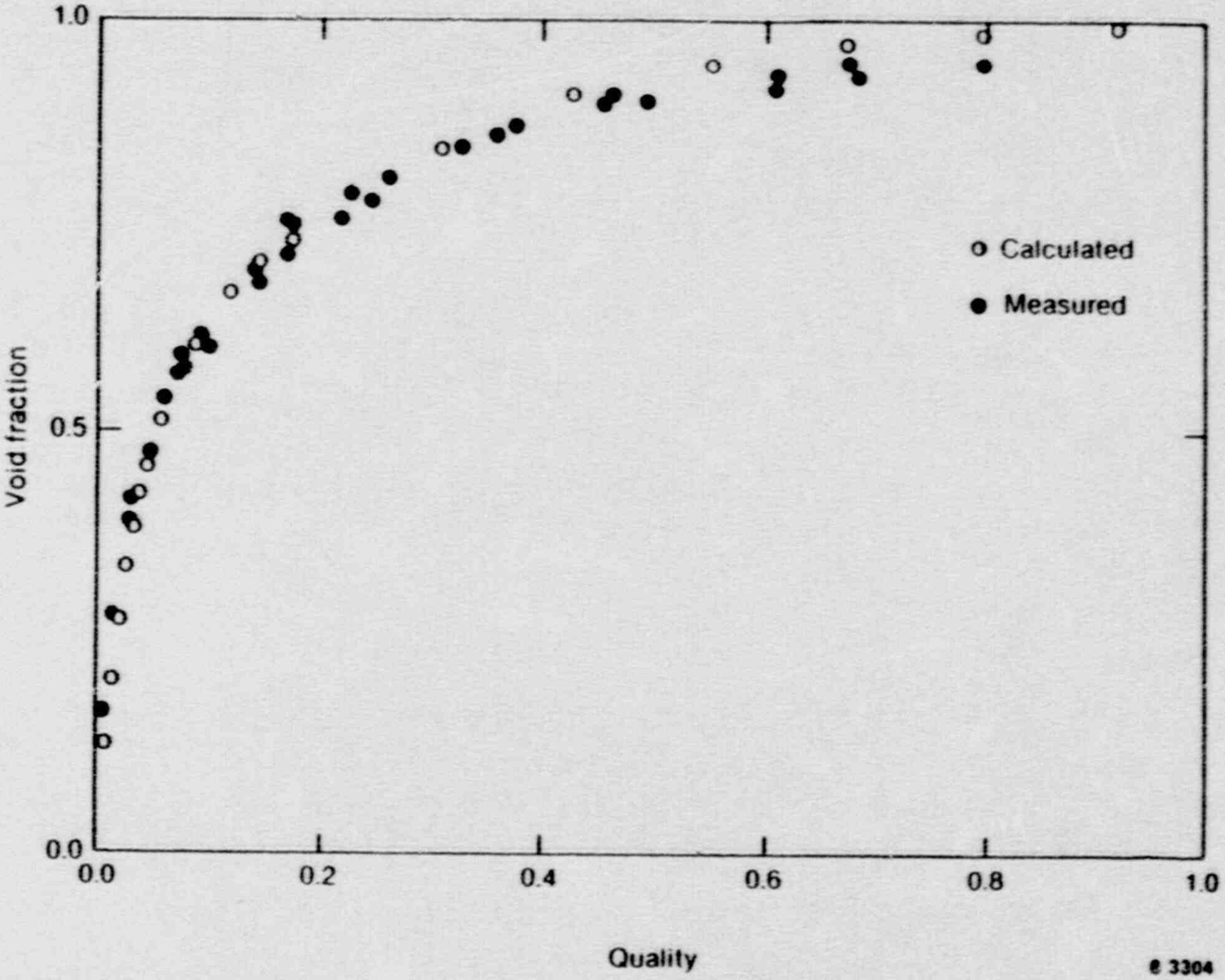
TRAC–BWR Assessment (cont'd)
Stability Related Assessments

- Void profile assessment tests
  - CISE adiabatic pipe
  - GE level swell
  - Christensen, Marchature, Bennett heated test section tests
  - THTF boiloff tests

- Two-phase pressure drop tests
  - FRIGG natural circulation flow tests (Flow rate vs power level)

ECO01613

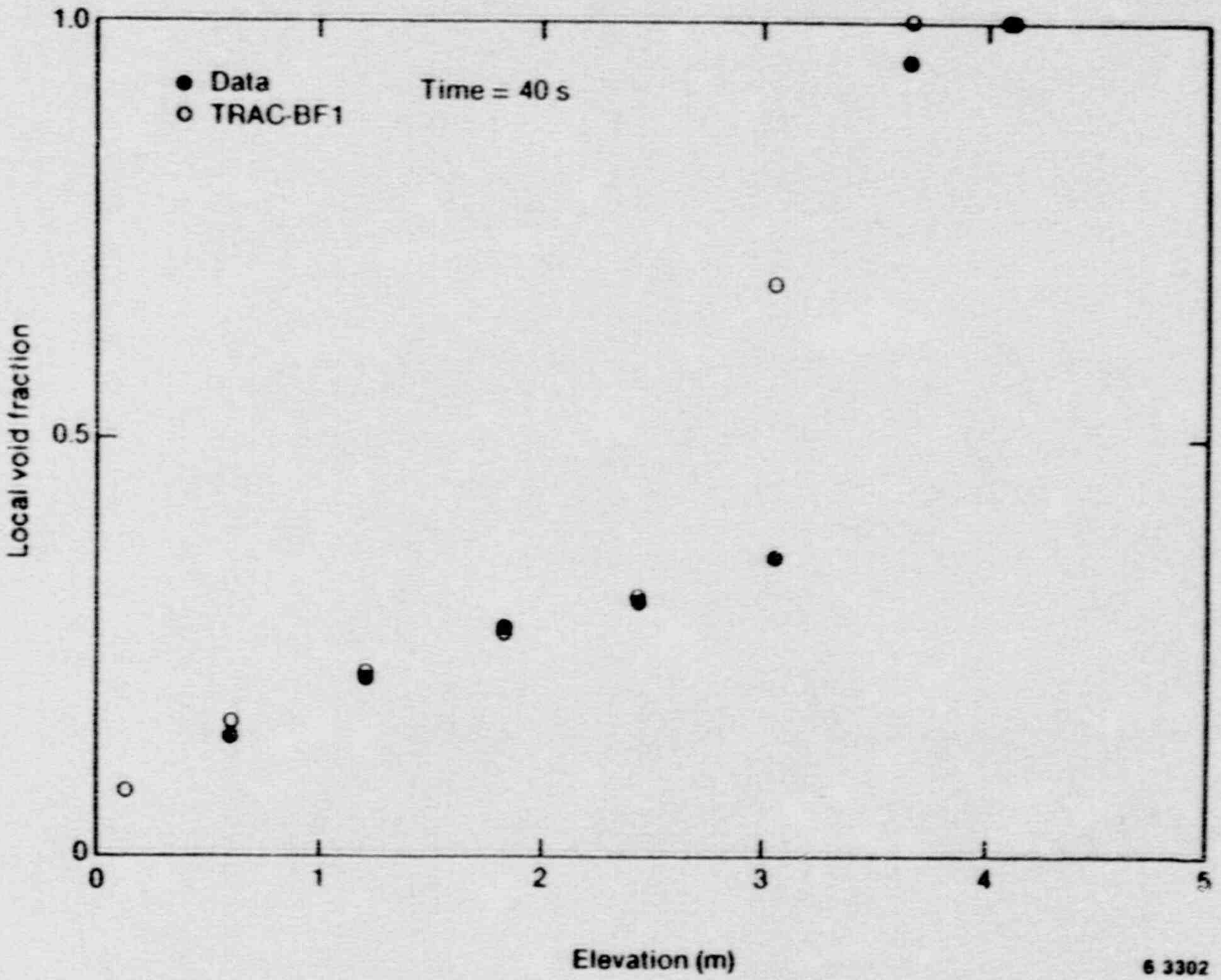
CISE Adiabatic Pipe Test



e 3304

ECO01625

GE Level Swell Test



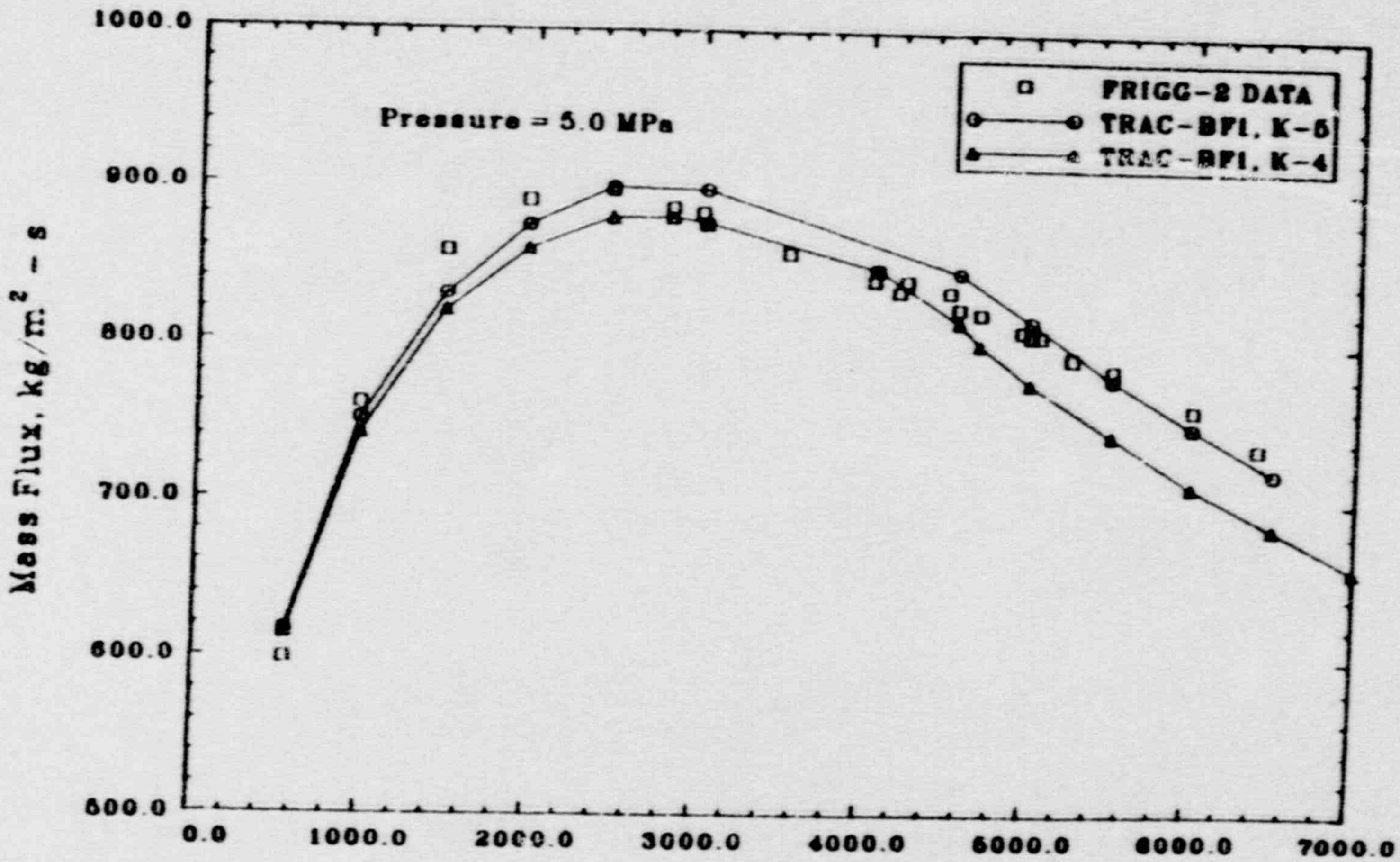
ECO01624

TRAC–BWR Assessment (cont'd)
Stability Specific Assessment

- FRIGG natural circulation tests
- FRIGG stability tests (ongoing)
  - Frequency domain analysis
  - Decay ratio studies

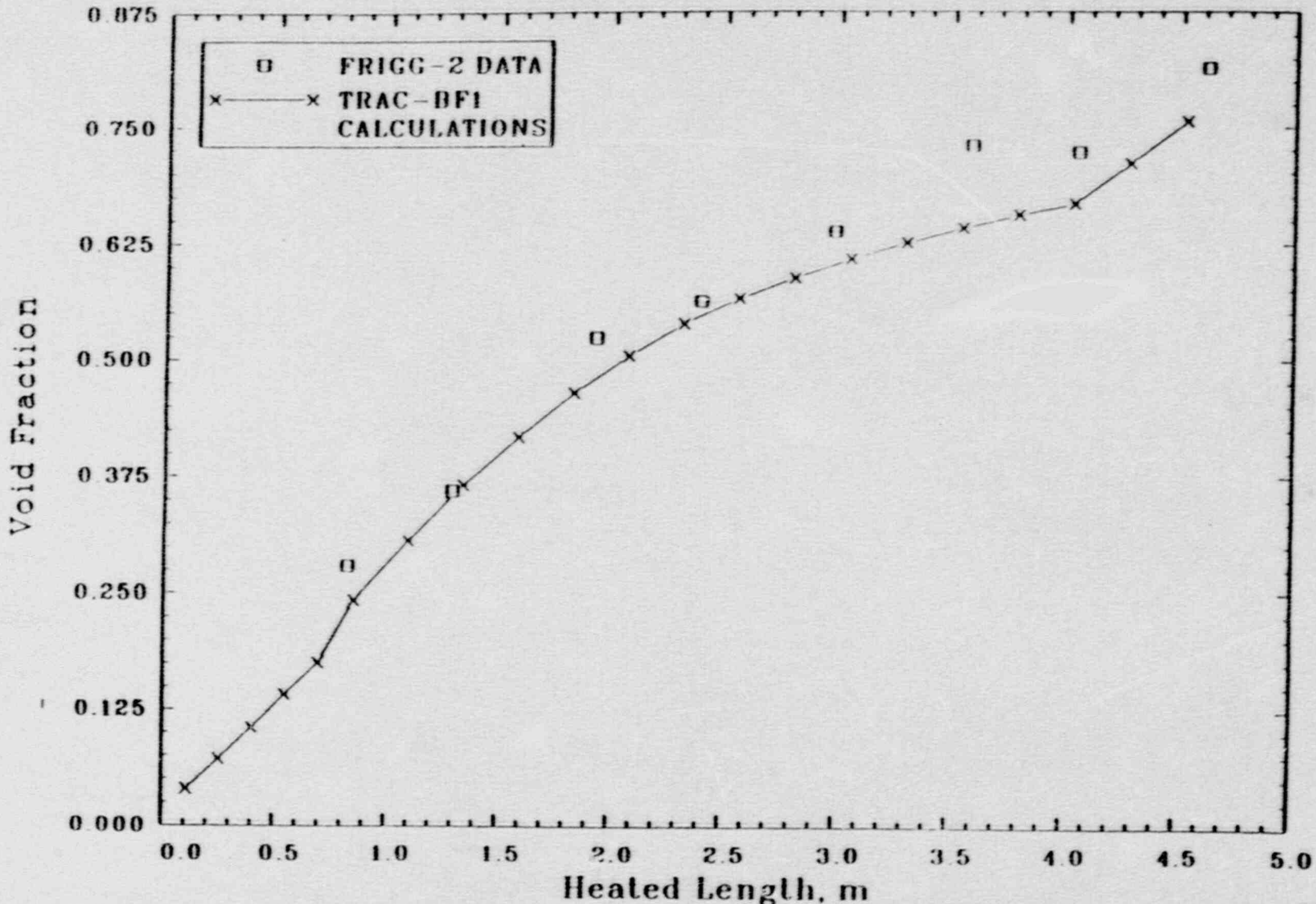
ECO01614

FRIGG Natural Circulation Tests



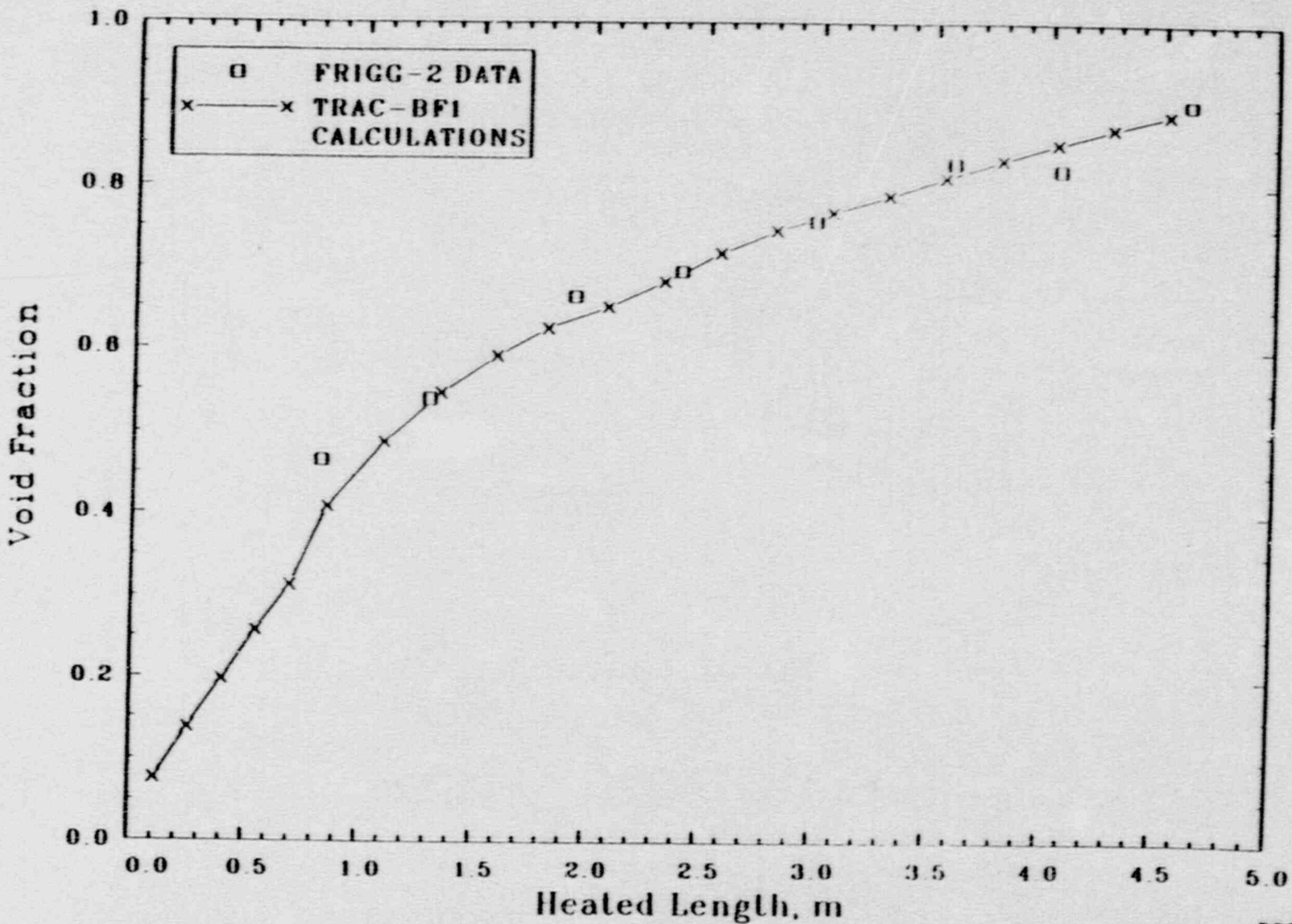
ECO01623

FRIGG Void Profile – Test 313027, 2.82 MW



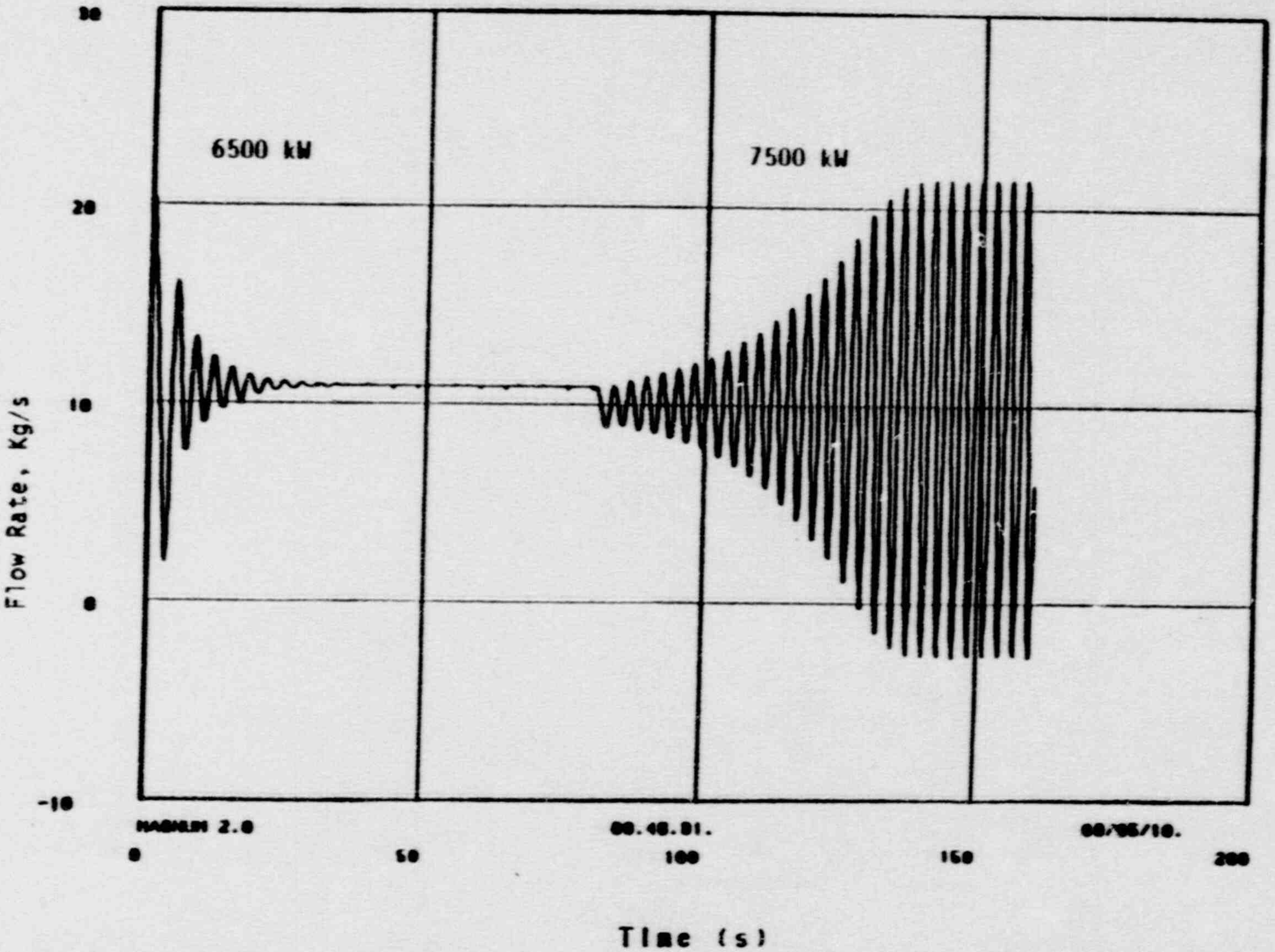
ECO01622

FRIGG Void Profile – Test 313030, 4.56 MW

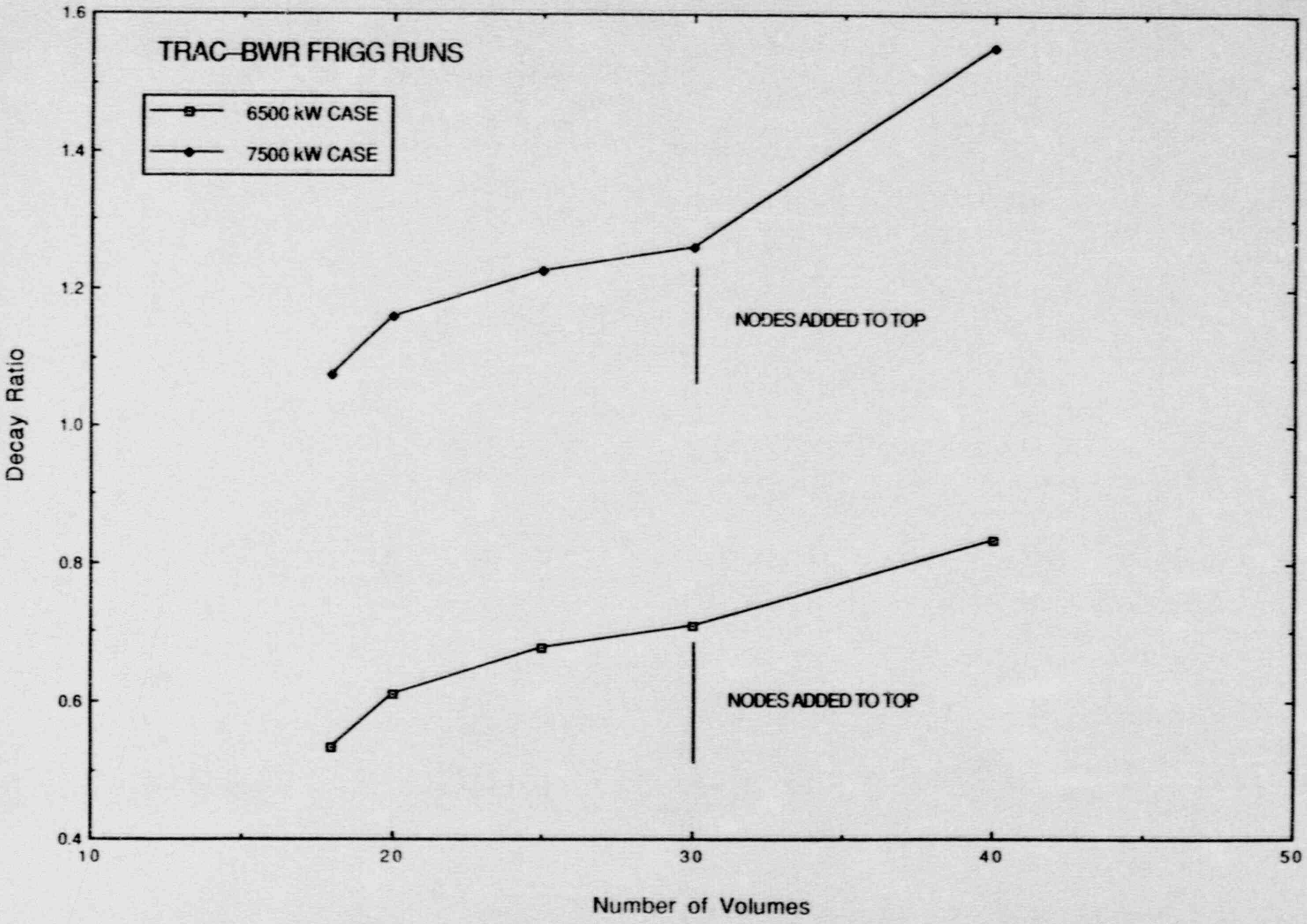


ECO01621

TRAC–BWR Test Section Inlet Flow Rate



EC001620



TRAC-BWR FRIGG RUNS
6500 kW CASE
7500 kW CASE
NODES ADDED TO TOP
NODES ADDED TO TOP
Decay Ratio
Number of Volumes
1.6
1.4
1.2
1.0
0.8
0.6
0.4
10
20
30
40
50

Numerical Damping of First Order Numerics

$$\frac{\partial \phi}{\partial t} + V \frac{\partial \phi}{\partial x} = 0$$

$$(\phi_j^{n+1} - \phi_j^{n}) = \frac{V\Delta t}{\Delta x} (\phi_j^{n} - \phi_{j-1}^{n})$$

$$\phi\ (t,x) = \xi(t)e^{-ikx}$$

$$\xi^2 = (2C^2 - 2C + 1) + 2C(1-C) \cos (k\Delta x)$$

ξ = damping over one time step

C = Courant number

k = wave number

ECO01615

Numerical Damping of First Order Numerics (cont'd)

$k = \frac{\pi}{N}$ for most unstable wave

$\frac{N}{C} =$ number of time steps for this wave to propigate thru test section

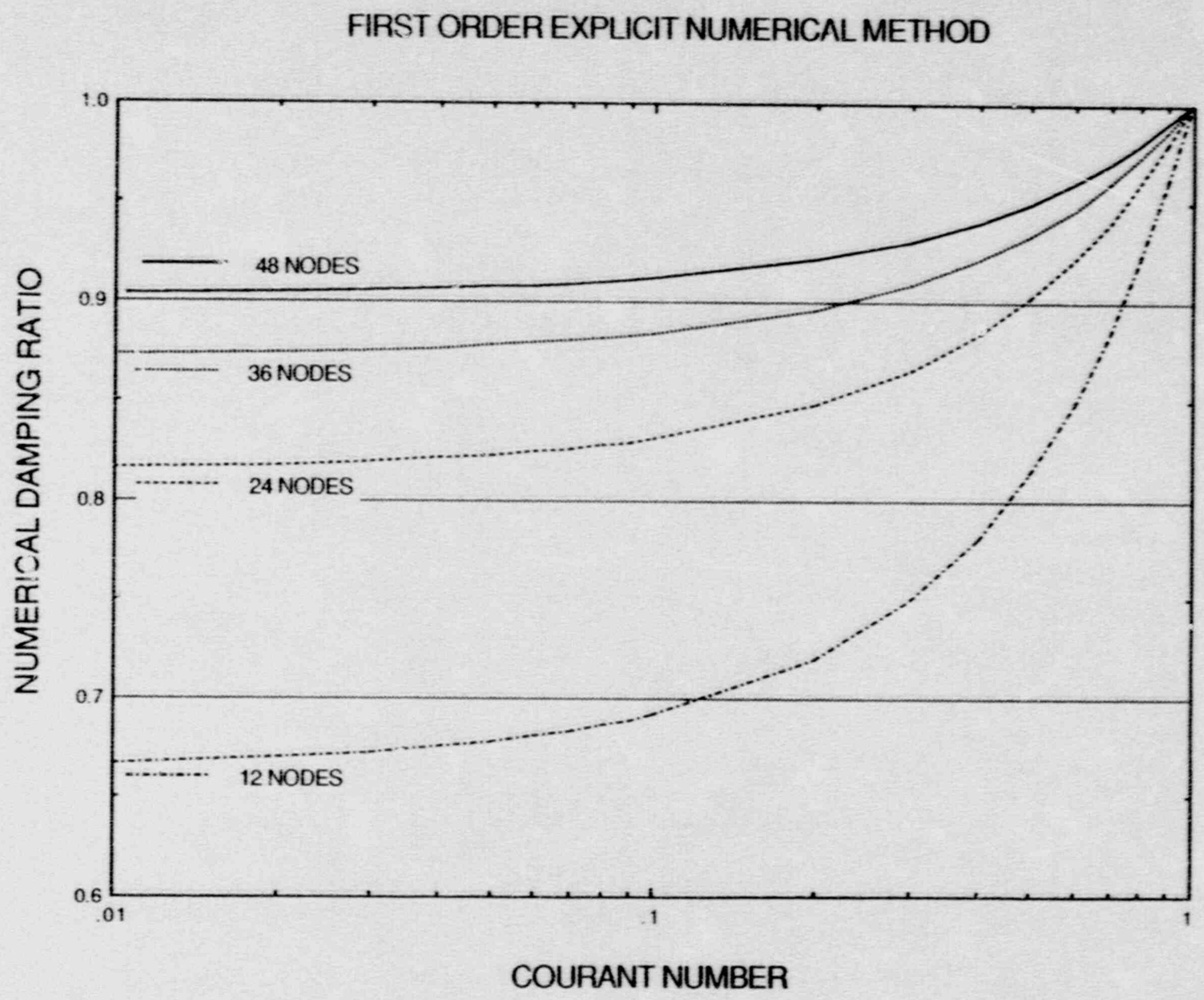
$$DR = \left[(2C^2 - 2C+1) + 2C(1-C)\cos\frac{\pi}{N}\right]^{\frac{N}{2C}}$$

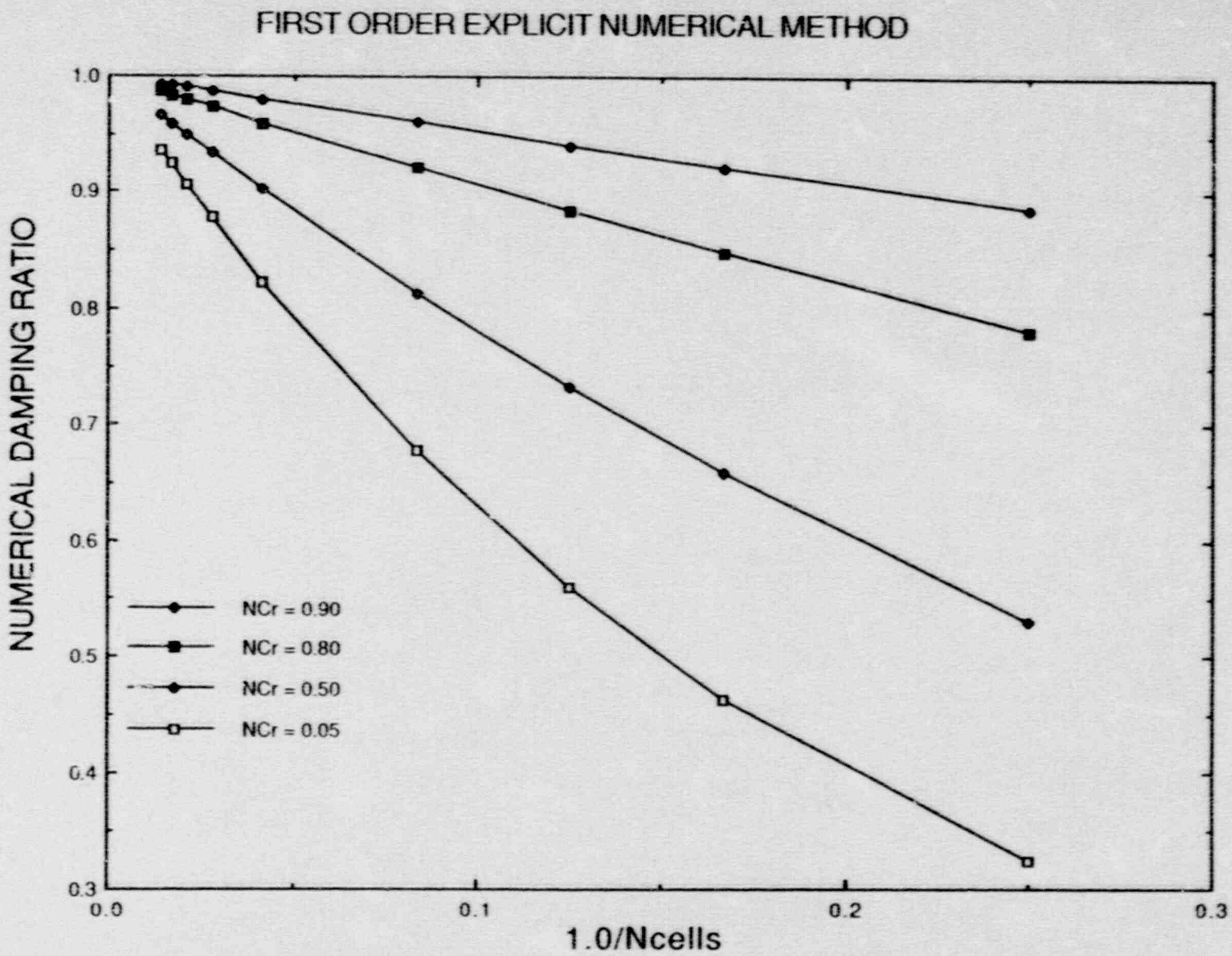
Can plot several ways

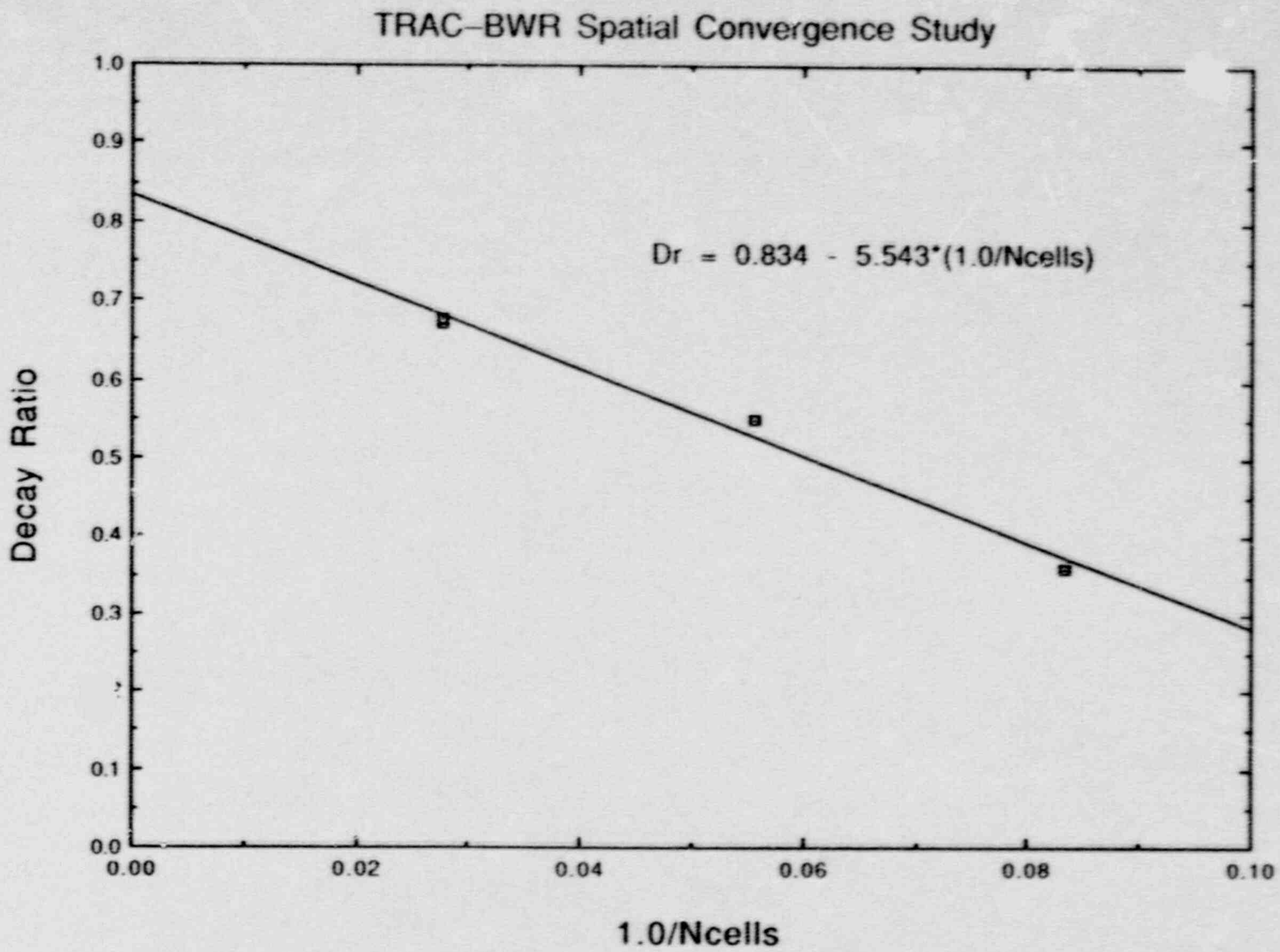
I. Fix N and vary C (varies Δt)

II. Fix C and vary N (varies Δt and Δx simultaneously)

EC001616

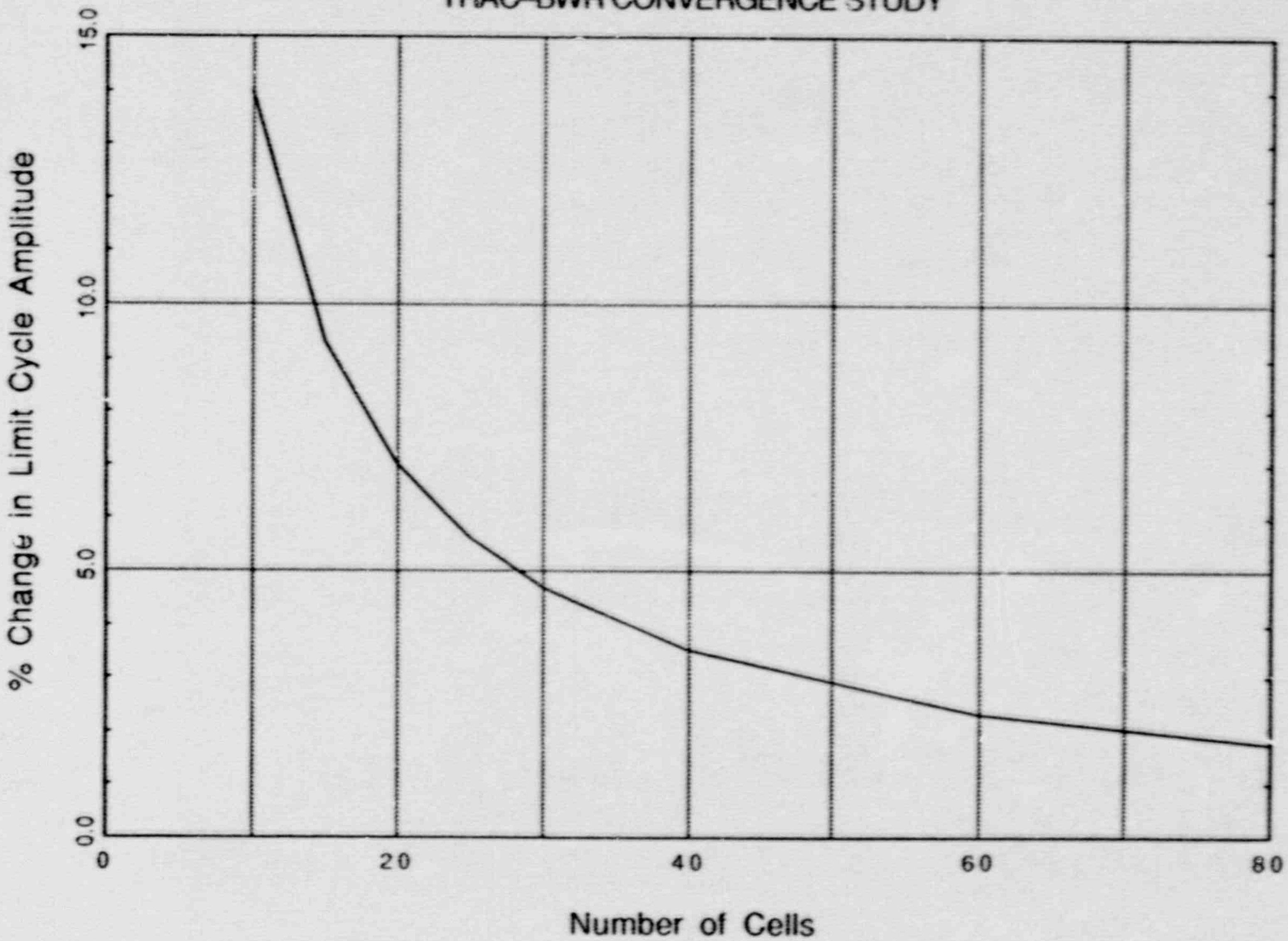

FIRST ORDER EXPLICIT NUMERICAL METHOD
NUMERICAL DAMPING RATIO
1.0
0.9
0.8
0.7
0.6
.01
.1
1
COURANT NUMBER
48 NODES
36 NODES
24 NODES
12 NODES


FIRST ORDER EXPLICIT NUMERICAL METHOD
NUMERICAL DAMPING RATIO
1.0/Ncells
1.0
0.9
0.8
0.7
0.6
0.5
0.4
0.3
0.0
0.1
0.2
0.3
NCr = 0.90
NCr = 0.80
NCr = 0.50
NCr = 0.05



TRAC-BWR Spatial Convergence Study
Dr = 0.834 - 5.543*(1.0/Ncells)
Decay Ratio
1.0/Ncells
0.0
0.1
0.3
0.4
0.5
0.6
0.7
0.8
0.9
1.0
0.00
0.02
0.04
0.06
0.08
0.10

TRAC-BWR CONVERGENCE STUDY



TRAC–BWR Numerical Convergence Study Problems and Limitations

- Each cell has different Courant numbers
  - Fix only maximum Courant number
- Decay is a group phenomena so decay ratio depends upon damping for all cells
- Decay ratio computed by hand
- Costs increase rapidly (i.e., factor of 4 per test run)

ECO01617

TRAC–BWR Limitations

- 1–D neutron kinetics restricts applicability of TRAC–BWR to in–phase oscillations

- Lack of assessment for magnitude of limit cycle
  - Lack of separate effects data

ECO01618

Summary

- TRAC-BWR has been assessed against a wide range of steady-state and transient test data

- Stability related assessment shows that there are no fundamental limitations to the use of TRAC-BWR for stability analysis

- Stability specific assessment is ongoing

- Developed a methodology to remove the effects of numerical damping from code results and confirmatory investigations underway

F^O01619

IDAHO NATIONAL ENGINEERING LABORATORY

TRAC-BF1
BASIC & STABILITY RELATED CAPABILITIES

PRESENTED BY: GARY E. WILSON
ZIA ROUHANI
WALT WEAVER

ACRS T/H PHENOMENA SUBCOMMITTEE MEETING
NOVEMBER 1989

EG&G Idaho, Inc.

THE PRESENTATION STRUCTURE FOR THIS TOPIC IS:

- o INTRODUCTION & CODE USE (GARY E. WILSON)
- o BASIC & STABILITY RELATED CODE FEATURES (ZIA ROUHANI)
- o BASIC & NUMERICAL DAMPING ASSESSMENT (WALT WEAVER)

THE ROLE OF TRAC-BF1 IN THE NRC'S BWR STABILITY ANALYSIS IS TO HELP EVALUATE THE EFFECTIVENESS OF ATWS EOPs TO PREVENT OR MITIGATE LIMIT CYCLE OSCILLATIONS

- o STUDY OBJECTIVES:
  - * DETERMINE INSTABILITY INITIATION & OSCILLATION AMPLITUDE
  - * DETERMINE SUPPRESSION POOL LOADING

- o TYPES OF STUDIES
  - * WATER LEVEL CONTROL
  - * FEEDWATER FLOW CONTROL
  - * PRESSURE EFFECTS
  - * BORON INJECTION EFFECTS

- o INTERFACES WITH OTHER NRC CODES
  - * LAPUR & EPA FOR MAPPING NECESSARY ANALYSIS SPACE & SELECTED CODE-TO-CODE BENCHMARKS
  - * TRAC-BF1 FOR IN-PHASE STUDIES & CODE-TO-CODE BENCHMARKS
  - * RAMONA SIMILAR TO TRAC-BF1, BUT FOR MULTI-D OSCILLATION MODES

THE STRATEGY TO ACCOMPLISH THE STABILITY RESEARCH OBJECTIVES INCLUDES THE FOLLOWING ELEMENTS:

- o TRAC-BF1 VALIDATION
  - * CRITICAL EVALUATION OF MODELS
  - * FRIGG ASSESSMENT (T/H OSCILLATIONS & FREQUENCY RESPONSE)
  - * PREVIOUS STABILITY RELATED ASSESSMENT
  - * CONVERGENCE STUDY (SPATIAL & TEMPORAL)
  - * LAPUR/EPA/TRAC-BF1 BENCHMARKS
  - * TRAC-BF1 LA SALLE EVENT BENCHMARK

- o TRAC-BF1 APPLICATION
  - * ATWS EOPs (OPERATOR ACTIONS)
  - * LA SALLE MODEL
  - * ANALYSIS SPACE PROVIDED BY LAPUR & EPA
  - * RAMONA/TRAC-BF1 COMPARISONS

IDAHO NATIONAL ENGINEERING LABORATORY

OVERVIEW
1989 STABILITY SYMPOSIUM

PRESENTED BY. GARY E. WILSON

CONTRIBUTORS: GERRY LELLOUCHE
ZIA ROUHANI
WALT WEAVER

ACRS T/H PHENOMENA SUBCOMMITTEE MEETING
NOVEMBER 1989

THE MAJOR TOPICS ADDRESSED IN THIS PRESENTATION INCLUDE

- o SYMPOSIUM OBJECTIVES
- o SYMPOSIUM STRUCTURE
- o SIGNIFICANT RESULTS

THE SYMPOSIUM WAS CONDUCTED IN CONJUNCTION WITH A TRAC-BF1 WORKSHOP WITH THE OBJECTIVE OF PROVIDING AN INTERNATIONAL FORUM FOR:

- o PERSPECTIVES OF VARIOUS ORGANIZATIONS INVOLVED IN REACTOR SAFETY
- o PRESENTATION OF RECENT STUDIES RELATING TO STABILITY
- o OPEN DISCUSSION OF COMMON PROBLEMS, QUESTIONS, APPROACHES, ETC.

THE SYMPOSIUM STRUCTURE STATISTICS INCLUDE:

- o HOSTED BY EG&G IDAHO WITH SUPPORT FROM NRC & DOE-ID
- o APPROXIMATELY 60 PARTICIPANTS FROM 20 ORGANIZATIONS IN SIX COUNTRIES
- o FOUR KEYNOTE SPEAKERS OFFERING PERSPECTIVES OF REGULATION, VENDORS, UTILITIES & UTILITY SUPPORTED RESEARCH
- o 12 PRESENTATIONS COVERING RESEARCH IN EXPERIMENTATION, GENERAL ANALYTICAL STUDIES AND CODE SIMULATIONS
- o LIVELY PLENARY SESSION WITH OPEN DISCUSSION ON COMMON PROBLEMS

THE INFORMATION PRESENTED TENDED TO CONFIRM EXISTING OPINIONS IN THE FOLLOWING IMPORTANT AREAS

- o SENSITIVITY OF TIME DOMAIN CODE SIMULATIONS TO NODALIZATION & TIME STEP
- o SENSITIVITY OF TIME DOMAIN CODE SIMULATIONS TO TRACKING OF THE BOILING BOUNDARY
- o AVERAGE POWER LEVEL DEPENDENCY ON OSCILLATION AMPLITUDE
- o POTENTIAL INTERACTIONS BETWEEN LOCAL CHANNEL AND CORE-WIDE HYDRAULIC OSCILLATIONS
- o PROTOTYPICAL DATA FOR ASSESSMENT OF CODE CALCULATED OSCILLATION AMPLITUDE

NEARLY ALL OF THE CODE APPLICATION STUDIES DEMONSTRATED A DEPENDENCY OF THE INITIATION OF INSTABILITY AND OSCILLATION AMPLITUDE ON NODALIZATION AND TIME STEP

- o HENTZEN'S STUDIES WITH TRAC-BF1 USING FRIGG DATA SHOWED TYPICAL DEPENDENCIES FOR TIME DOMAIN CODES
- o ANDERSEN FOCUSED ON EXPLICIT AND IMPLICIT NUMERICAL SOLUTIONS TO SHOW:
  - * BOTH SCHEMES PRODUCE NUMERICAL DAMPING FOR COURANT NO. UNEQUAL TO ONE
  - * ONLY EXPLICIT SOLUTION AT COURANT NO. OF 1 IS FREE OF DAMPING
- o THESE STUDIES PROVIDED MOTIVATION FOR ADDITIONAL WORK JUST REPORTED BY WEAVER

GALER & JENSEN STUDY WITH RETRAN INDICATED TIME DOMAIN CODE SENSITIVITY TO BOILING BOUNDARY TRACKING THAT WAS INDEPENDENT OF NUMERICAL INTEGRATION SCHEME & TIME STEP

- o CALCULATED DECAY RATIO SIGNIFICANTLY INFLUENCED BY LOCATION OF BOILING BOUNDARY IN LARGE INLET NODE
- o STUDY RESULTS INDICATE NEED FOR FINE NODALIZATION IN ABSENCE OF SPECIFIC MODEL TO TRACK BOILING BOUNDARY LOCATION IN LARGE NODES

MARCH-LEUBA'S STUDIES OF BWR LIMIT CYCLES WITH THE FREQUENCY DOMAIN CODE LAPUR INDICATE:

- o A LIMIT CYCLE BOUNDS THE POWER OSCILLATIONS
- o TYPICALLY, THE AVERAGE POWER INCREASE IS 1.5% TO 2% OF PEAK POWER OSCILLATION
- o LIMIT CYCLES CAN BECOME UNSTABLE AND BIFURCATE, ULTIMATELY LEADING TO APERIODIC (CHAOTIC) REGIMES FOR PEAK POWERS GREATER THAN 500% OF STEADY STATE
- o THE LIMIT CYCLE INSTABILITY AND BIFURCATION HAS NOT YET BEEN AS EXTENSIVELY ANALYZED WITH THE TIME DOMAIN CODES AS WITH LAPUR. INTERACTIONS BETWEEN CHANNEL AND CORE-WIDE OSCILLATIONS ARE OF PARTICULAR INTEREST, WITH RESPECT TO MODES (IN & OUT OF PHASE, ETC.)

THE PROTOTYPICAL DATA BASE FOR BWR STABILITY CODE ASSESSMENT HAS CERTAIN LIMITATIONS

- THE DATA BASE IS CONSIDERED REASONABLE FOR ASSESSMENT OF THE PREDICTION OF ONSET OF INSTABILITY IN SINGLE CHANNELS
- THE DATA BASE FOR ASSESSMENT OF LIMIT CYCLE AMPLITUDE IS, AT BEST, NOT READILY AVAILABLE AND IS LIKELY INSUFFICIENT

BNL ENGINEERING PLANT ANALYZER (EPA) ANALYSES OF BWR STABILITY

1. EPA DESCRIPTION
2. EPA ASSESSMENT
3. EPA OBJECTIVES FOR BWR STABILITY ANALYSES
4. EPA RESULTS FROM BWR STABILITY ANALYSES
5. EPA LIMITATIONS
6. FUTURE PLANS ON EPA ACTIVITIES

BROOKHAVEN NATIONAL LABORATORY
ASSOCIATED UNIVERSITIES, INC.

BWR STABILITY ANALYSIS

WITH

BNL ENGINEERING PLANT ANALYZER

W. WULFF

BROOKHAVEN NATIONAL LABORATORY

PRESENTED BEFORE

ACRS SUBCOMMITTEE ON THERMAL HYDRAULICS

NOVEMBER 8-9, 1989

SAN FRANCISCO, CA

BROOKHAVEN NATIONAL LABORATORY
ASSOCIATED UNIVERSITIES, INC.

- EPA: ADI SIMULATION SYSTEM
  HIPA CODE.

- HIPA SYSTEMS CODE WITH

  - POINT KINETICS,
    1 - GROUP OF PROMPT NEUTRONS,
    6 DELAYED NEUTRON GROUPS,
    DECAY HEAT AS ANS STANDARD 5.1,
    SEVEN REACTIVITY FEEDBACK MECHANISMS.
  - INTEGRAL METHODS FOR CONDUCTION IN FUEL
  - THERMOHYDRAULICS
    NONHOMOGENEOUS FLOW/PHASE SEPARATION (DRIFT FLUX, ISHII 1977),
    NONEQUILIBRIUM FLOW (SCANDPOWER Γ_v)
    3 PARALLEL CHANNELS IN CORE,
    ONE-DIM. FLOW IN REST OF SYSTEM.
    FOUR-EQUATION DF MODEL WITH:
    - MIXTURE LOOP MOMENTUM BALANCES,
    - MIXTURE VOLUMETRIC FLUX DIVERGENCE EQUATION,
    - MIXTURE ENERGY BALANCE, AND
    - VAPOR MASS BALANCE.

    VAPOR AT SATURATION CONDITIONS
    LIQUID SUBCOOLED, SATURATED OR SUPERHEATED.

1. EPA DESCRIPTION

1.1 REFERENCE: NUREG/CR-3943 (1984) (CODE STATUS AS OF JUNE 1984)

1.2 MAJOR EPA CHARACTERISTICS

- SIMULATION FACILITY

SPECIAL-PURPOSE MINICOMPUTER (AD10)
SYSTEMS SOFTWARE PROVIDING SIMULATION ENVIRONMENT, SIMULATION LANGUAGE,
6 MODELING PRINCIPLES:
- •• MODEL SELECTION,
- •• RELEVANCE OF PROCESSES,
- •• ANALYTICAL INTEGRATION WHERE POSSIBLE,
- •• ELIMINATION OF ITERATION,
- •• USE OF PRETABULATED FUNCTIONS,
- •• SELECTION OF ALGORITHM.

OPTIMIZATION OF
MACHINE ARCHITECTURE +
MODELING +
NUMERICAL METHODS
AS A WHOLE.

EPA CHARACTERISTICS (CONT.)

- SOLUTION METHODS IN EPA

  - IMPLICIT INTEGRATION FOR
    PROMPT NEUTRON ODE

  - EXPLICIT INTEGRATION FOR
    ALL OTHER ODEs: $\dot{P}$, $\dot{M}_i$, $\dot{m}_{vj}$, $\dot{E}_{MJ}$, $\dot{\omega}$, ETC.
    MIX OF FIRST-ORDER EULER AND
    THIRD-ORDER ADAMS-BASHFORD;
    BUILT-IN, STANDARD TEXT BOOK METHODS

  - QUADRATURES IN SPACE FOR
    - MIXTURE MASS BALANCE (FLUX DIVERGENCE EQUATION,
    - LOOP MOMENTUM BALANCE:

    TRAPEZOIDAL RULE
    (FOR GIVEN MEAN VALUES)
    SIMPSON RULE
    (FOR GIVEN DISCRETE VAL.)

EPA CHARACTERISTICS (CONT.)

●● SIMULATION SCOPE

NSSS: RPV, RCP, STEAM LINE (ACOUSTIC EFFECTS), 67 CONTROL VOLUMES.

BOP: TURBINE, GENERATOR, FW TURBINE, FW PUMP, FW PREHEATERS, CONDENSER, RCP MOTOR/GENERATOR SET.

CONTROLS: PRESSURE REGULATOR, FW CONTROL, RECIRC. FLOW CONTROL. } GE TRANSFER FUNCTIONS

SAFETY SYSTEMS: NINE AUTOMATIC SCRAM TRIPS, PUMP TRIPS, TURBINE GENERATOR TRIPS, SAFETY AND RELIEF VALVES, ECCS, BORON INJECTION AND TRANSPORT.

CONTAINMENT: DRY WELL, WETWELL AND SUPPRESSION POOL (N_2/H_2O ATMOSPHERE, CONDENSATION ETC.).

FAILURES: PUMPS, HEATERS, VALVES, SCRAM, CONTROL SYSTEMS AND TRIPS.

ON-LINE, INTERACTIVELY IMPOSED.

1.3 EPA MODIFICATIONS FOR BWR INSTABILITY ANALYSES

- INTEGRATORS FOR AVG. POWER,
  AVG. TOT. REACTIVITY.

- MULTISTEPPING FOR KINETICS CALCULATIONS
  (PROMPT AND DELAYED)
  INTERPOLATING TOT. REACTIVITY

- MULTISTEPPING FOR CONDUCTION IN FUEL
  INTERPOLATING VOID REACTIVITY
  (DOPPLER FFEDBACK IN EVERY SUBSTEP)

- RESULT FROM CHANGES
  - oo DECREASE IN PEAK POWER (-25%)
  - oo INCREASE OF MEAN POWER (+ 53%)

AFTER SCRAM FAILURE/INVENTORY MAINTAINED

SOLUTION METHODS IN EPA (CONT.)

- ●● NO LINEARIZATION OF EQUATIONS
- ●● COMPUTATIONAL ERRORS FROM NUMERICAL DIFFUSION (2EQS.), QUADRATURE, ODE INTEGRATION, COVARIANCE TERMS.
- ●● DIFFUSION, CONTROLLED BY EXPLICIT INTEGRATION WITH MAX. COURANT NO. $<$ 1.0.
- ●● COMPUTATIONAL MODELS AND METHODS AND COMPUTAITONAL ERRORS FOR THERMOHYDRAULICS ARE THE SAME IN RAMONA-3B AND EPA.

BROWNS FERRY TURBINE TRIP

- COMPARISON OF EPA RESULTS WITH TEST DATA

PUMP SPEED	WITHIN PLOTTING ACCURACY
CORE FLOW	+ 4% OF INITIAL VALUES
POWER	± 4%
STEAM FLOW	± 6%
PRESSURE	+ 1%
COLLAPSED LIQ. LEVEL	± 10%

2. EPA ASSESSMENT

2.1 DEVELOPMENTAL ASSESSMENT
(NUREG/CR-3943)

- COMPARISONS WITH FRIGG TESTS
  (G. P. COMPUTER)

- CHAPTER 15 TRANSIENTS AND
  ATWS : GE NEDO-2422

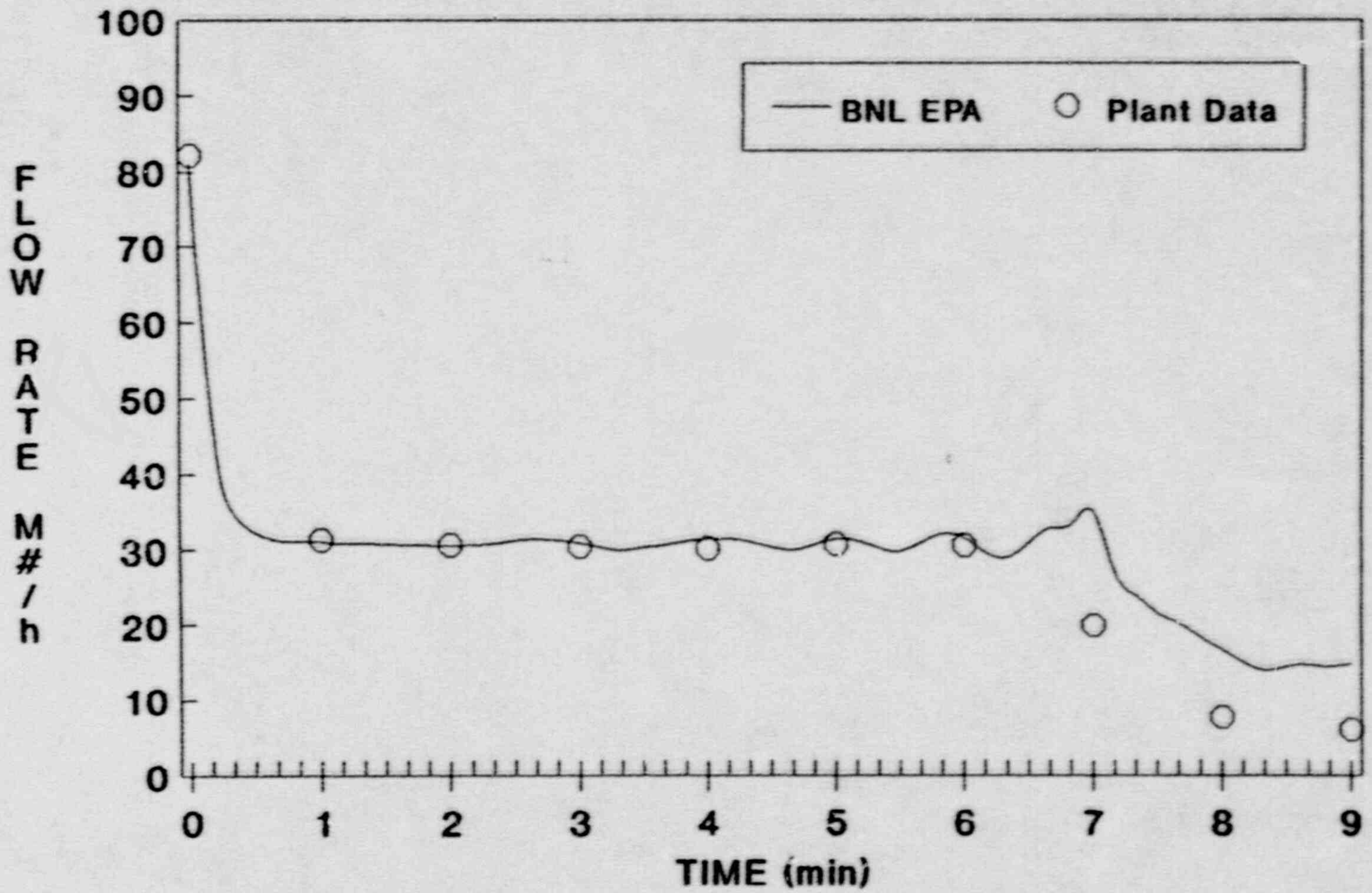
- COMPARISON WITH TRAC, RELAP5, RAMONA:
  MSIV CLOSURE ATWS

2.2 LASALLE RELATED TRANSIENTS:

- BROWNS FERRY-1 RCP TRIP TEST
  (H. S. CHENG MEMO, AUG. 18, 1987)

- LASALLE TRANSIENT
  UP TO SCRAM TRIP:
  - STARTREC DATA VS. CALCULATION
  - BE PLANT DATA FROM GE

REACTOR CORE FLOW RESPONSE
BNL EPA vs. Plant Data



LASALLE-2 TRANSIENT

	RESULT
• BE CALCUL. + W_{FW} IMPOSED FROM STAR TREC	NO. OSCIL.
• BE CALCUL. + FW CONTROLLER INTACT (BF CONTR. PARAM.)	OSCIL., SCRAM
• UNCERTAINTIES FOR RHOV, LEXT, LENT, + W_{FW} IMPOSED FROM STAR TREC	OSCIL., SCRAM

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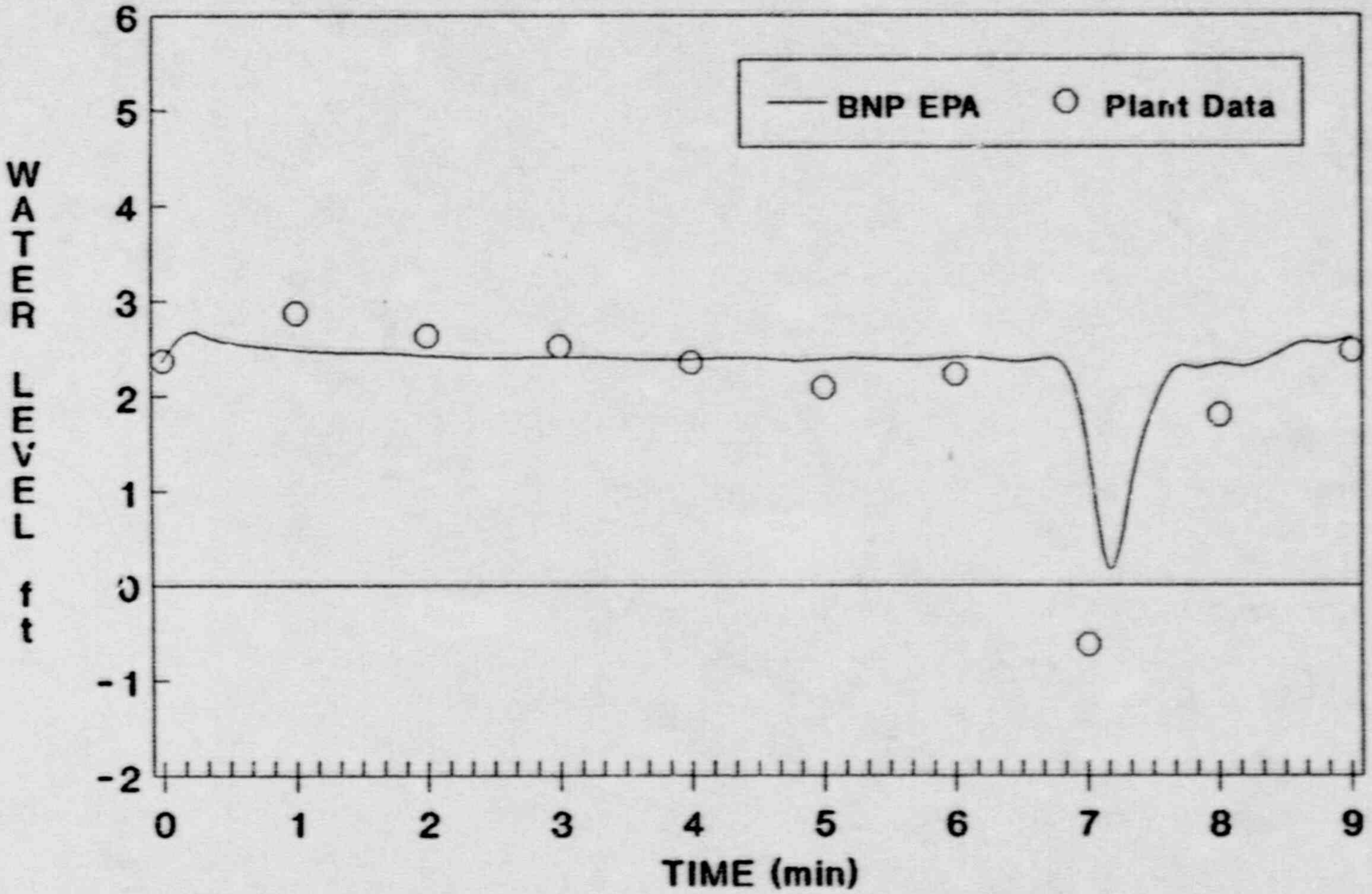
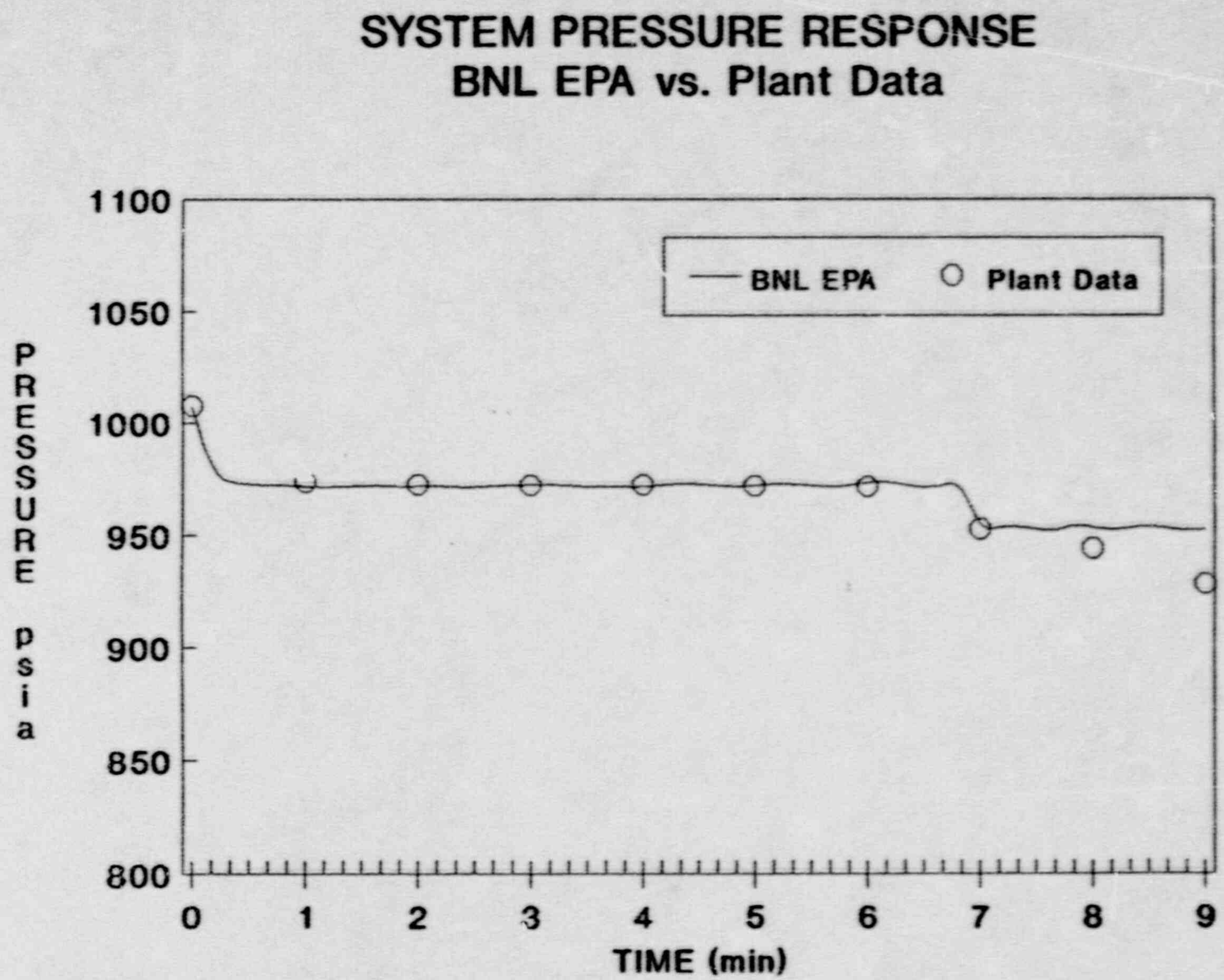
*LASALLE FW REGULATOR VALVE FAILED DURING LASALLE EVENT.

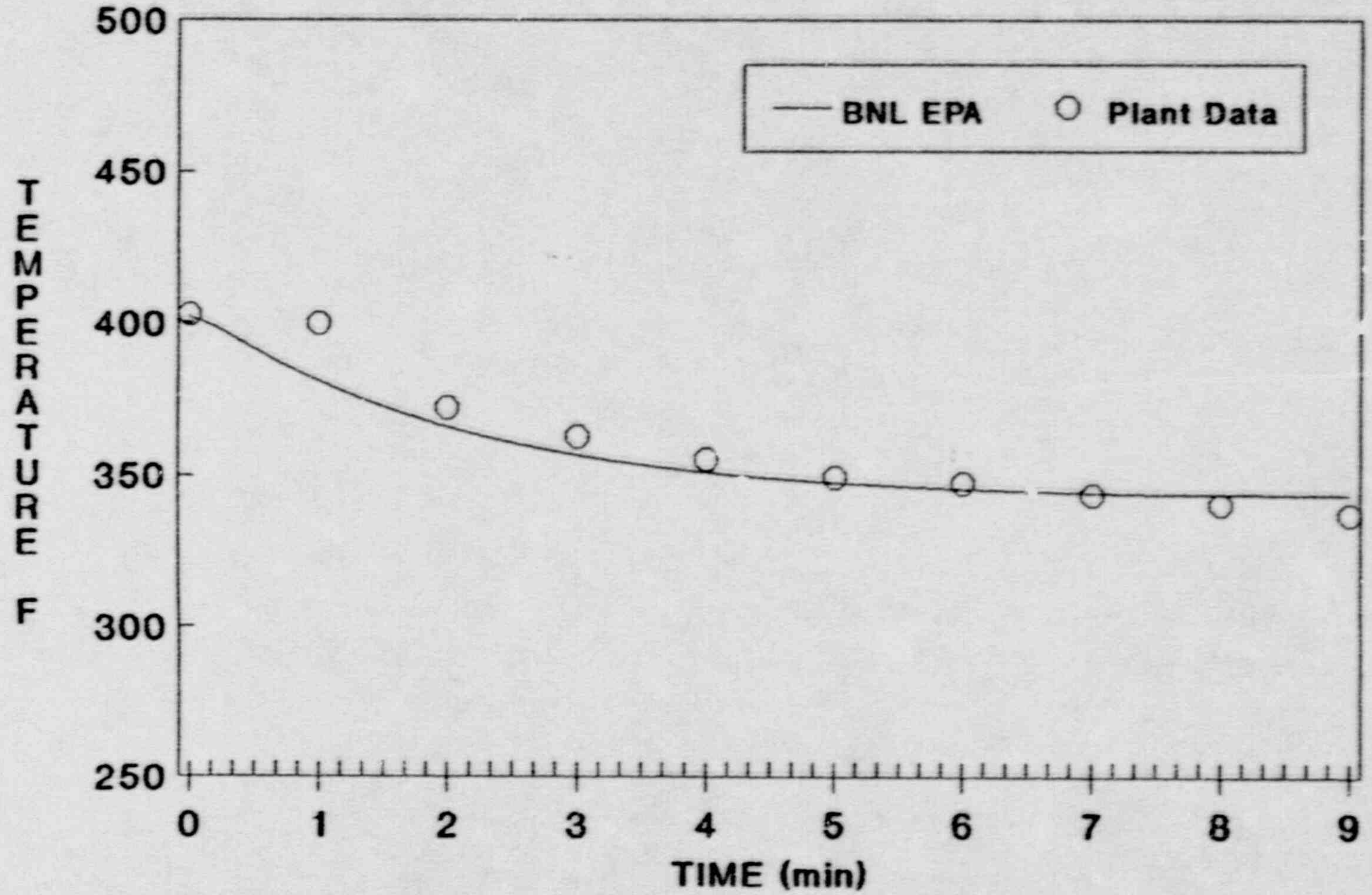
REACTOR WATER LEVEL RESPONSE
BNL EPA vs. Plant Data



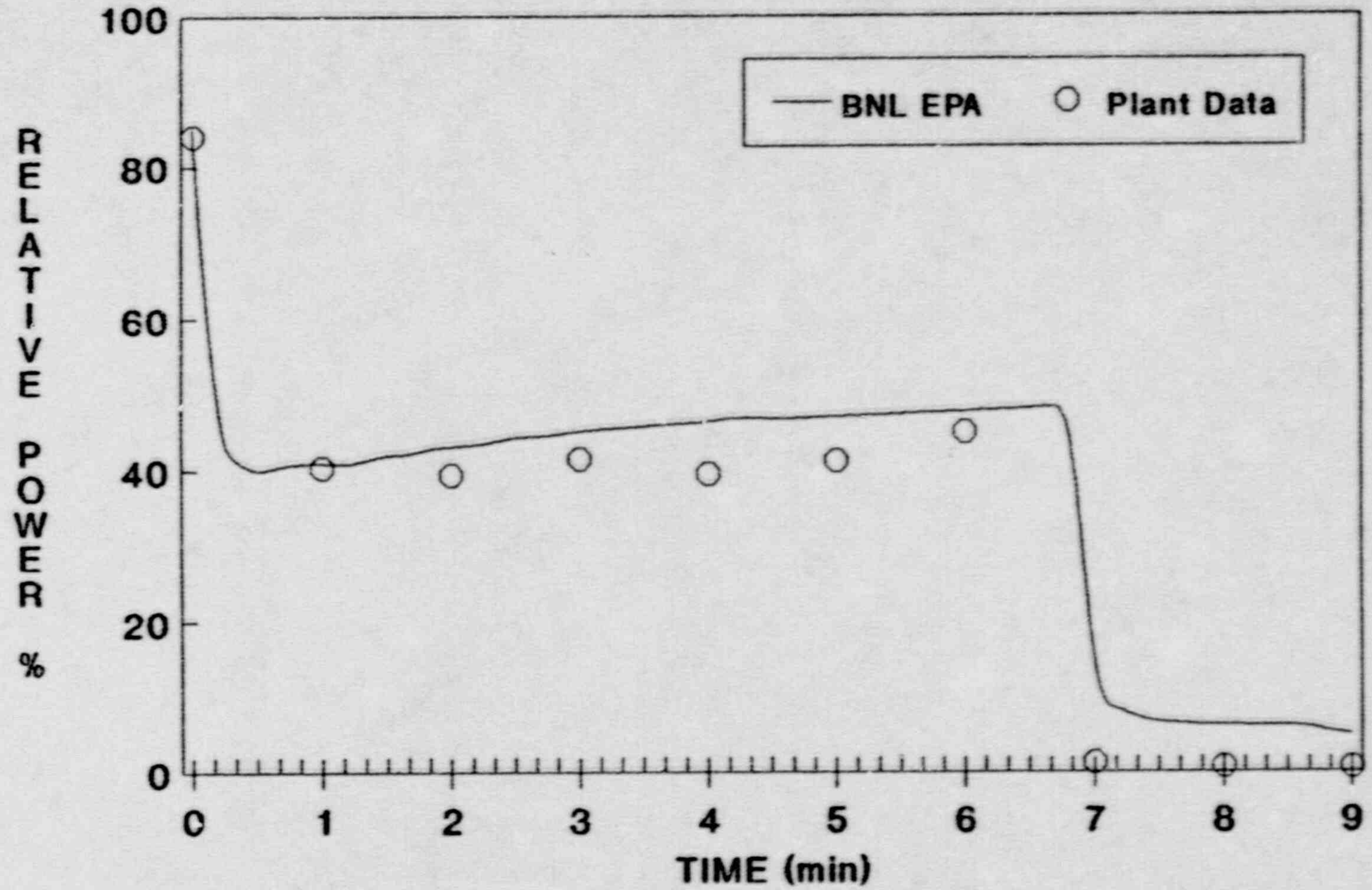


SYSTEM PRESSURE RESPONSE
BNL EPA vs. Plant Data
BNL EPA
Plant Data
PRESSURE psia
1100
1050
1000
950
900
850
800
0
1
2
3
4
5
6
7
8
9
TIME (min)

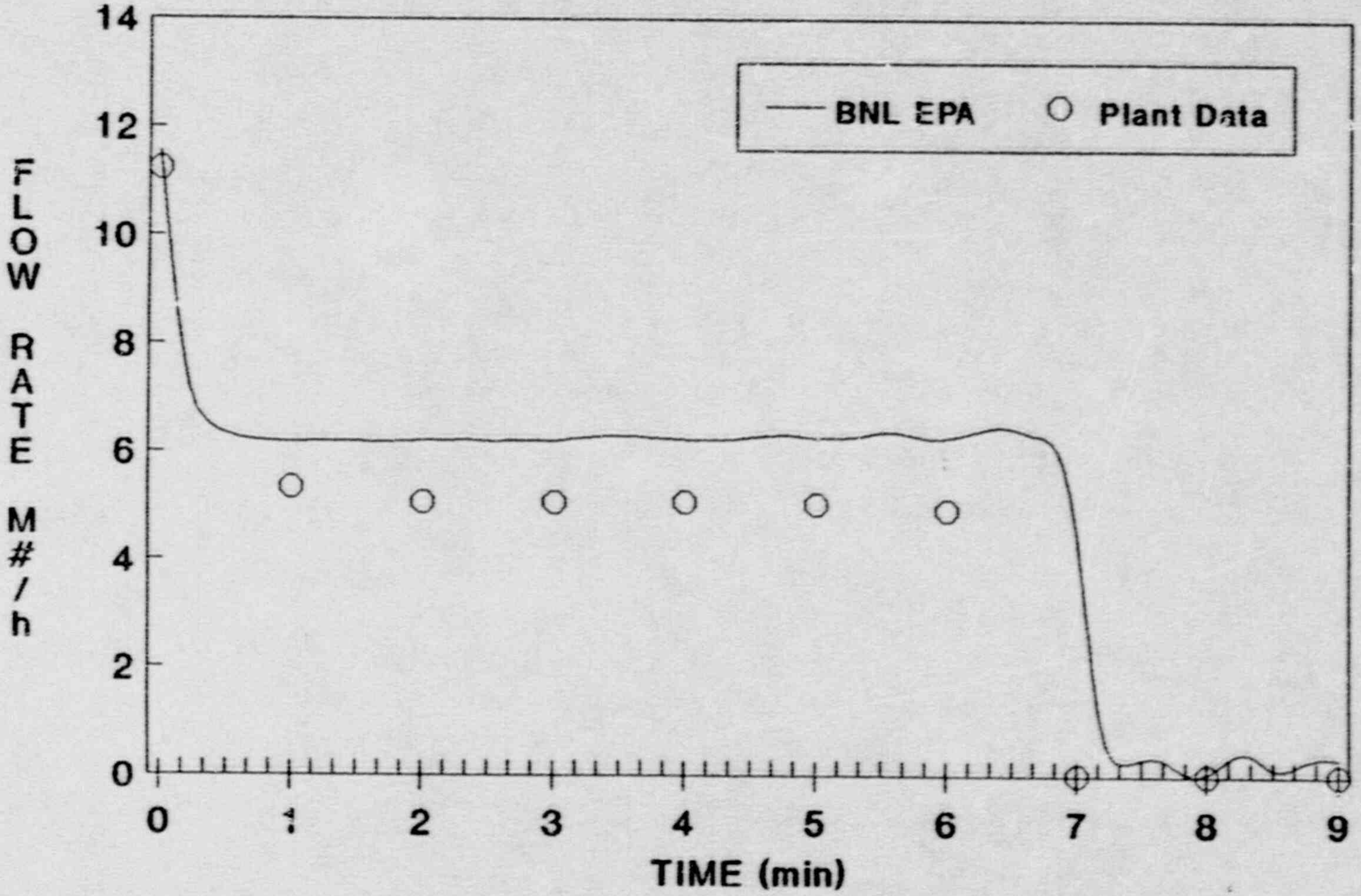
FEEDWATER TEMPERATURE RESPONSE
BNL EPA vs. Plant Data



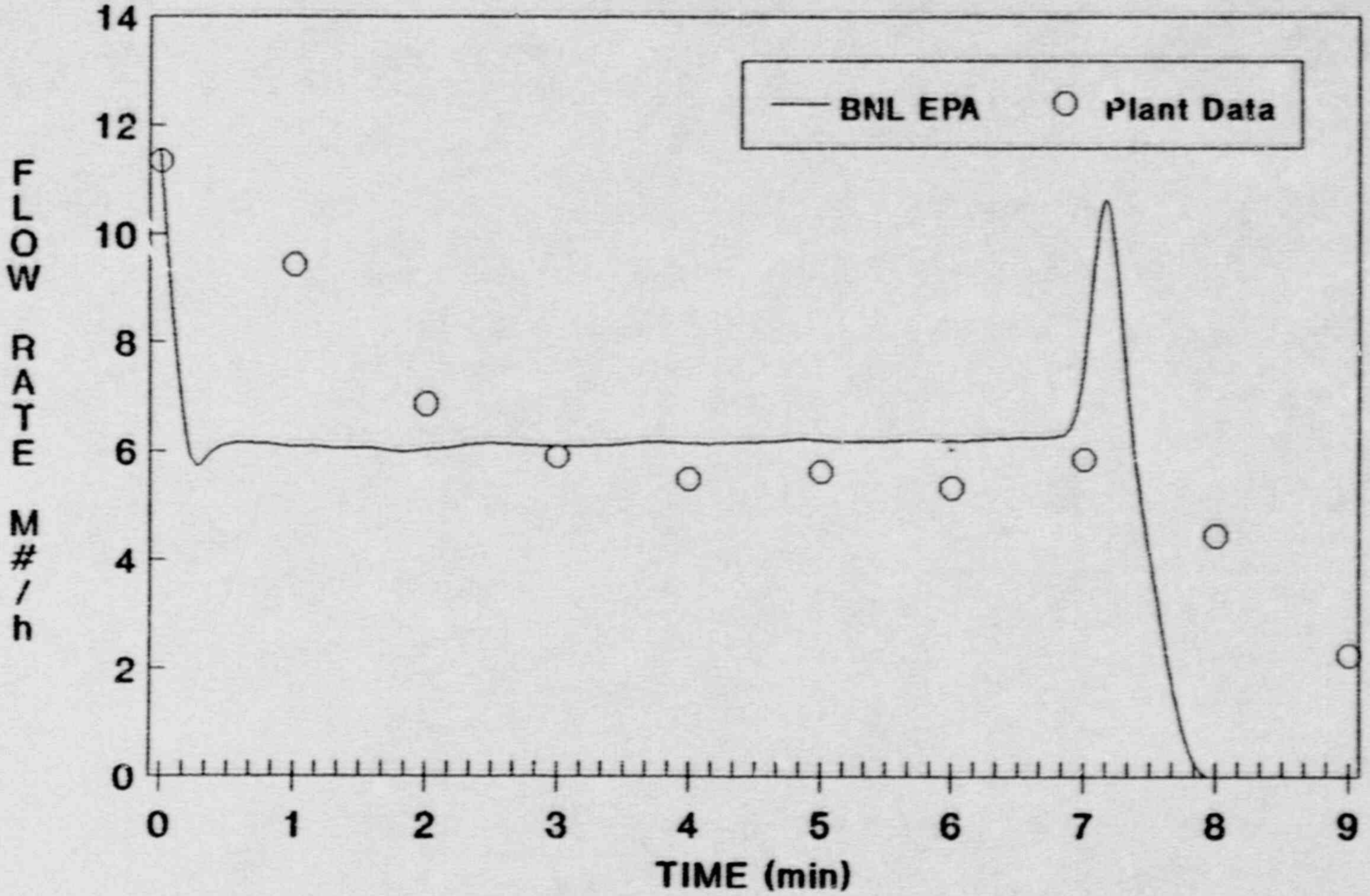
AVERAGE POWER RESPONSE
BNL EPA vs. Plant Data

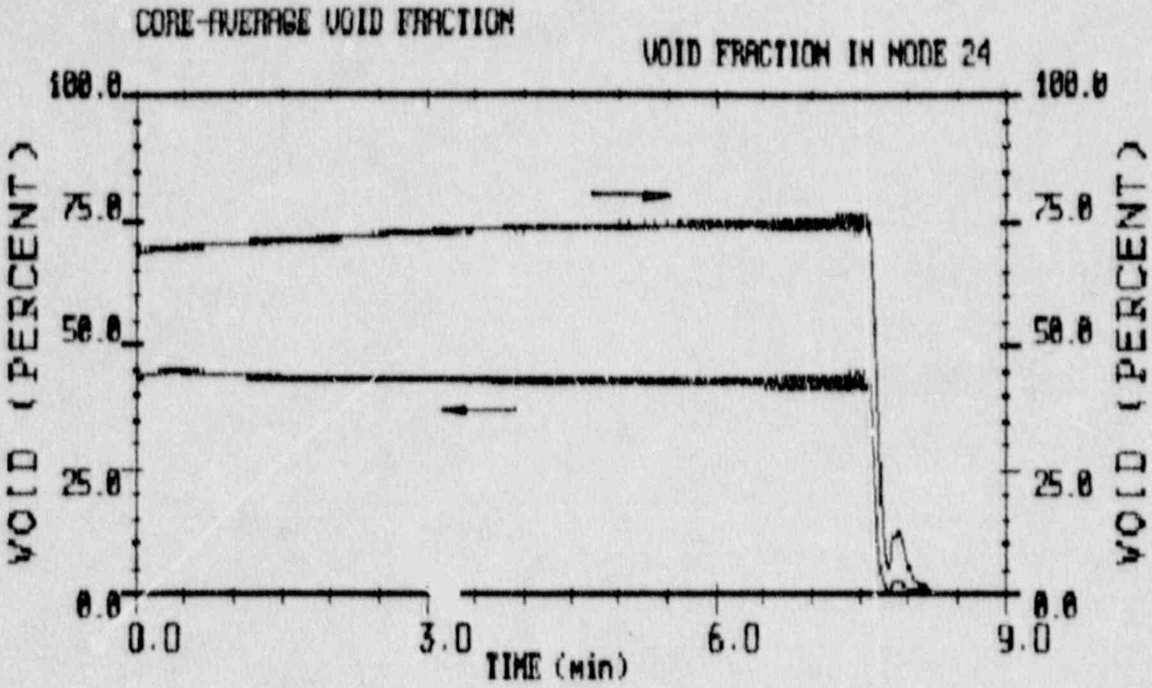


TOTAL STEAM FLOW RESPONSE
BNL EPA vs. Plant Data



FEEDWATER FLOW RESPONSE
BNL EPA vs. Plant Data





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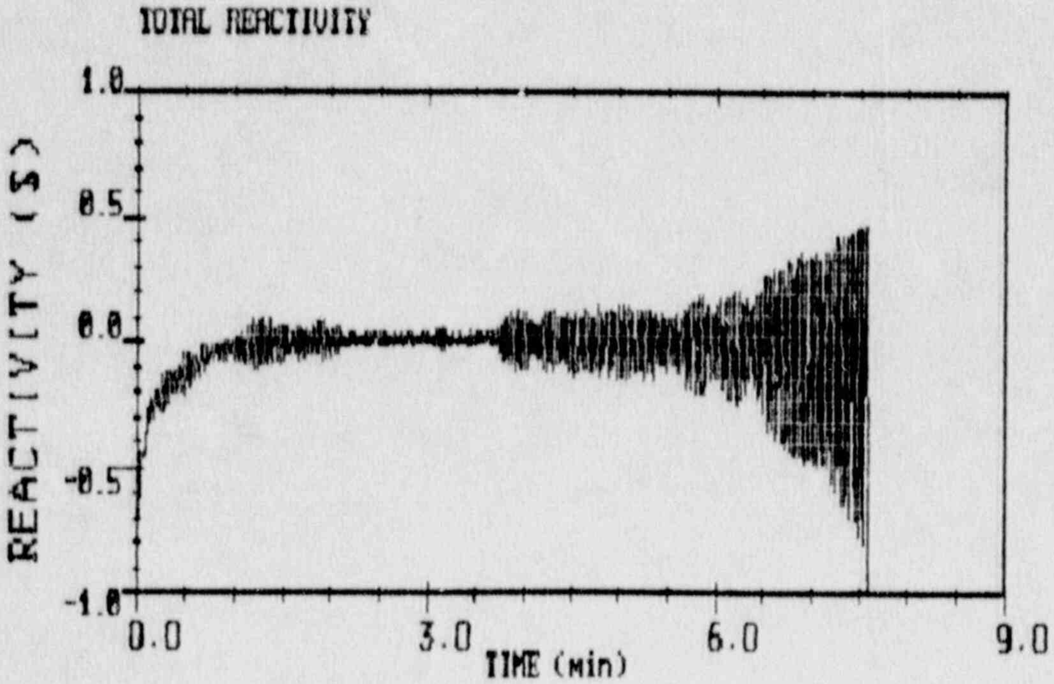

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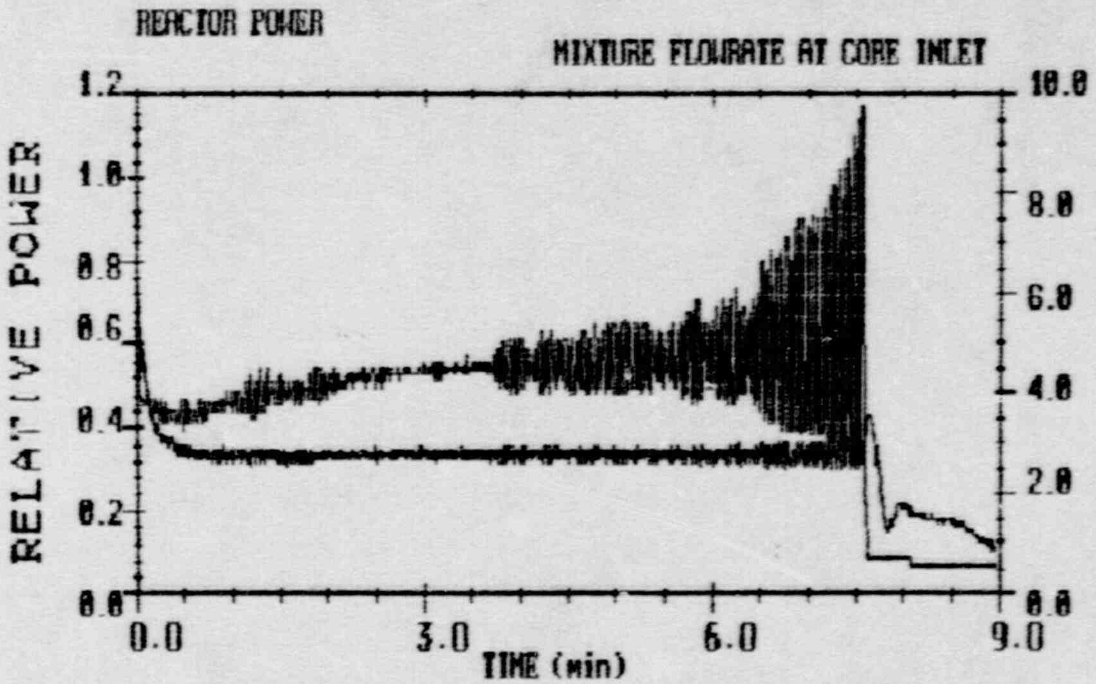

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3 EPA OBJECTIVES FOR BWR STABILITY (CONT.)

(7) CAN SUPPRESSION POOL TEMPERATURE AND PRESSURE EXCEED ALLOWED LIMITS?

3.2 RANK MODELING PARAMETERS ACCORDING TO THEIR IMPORTANCE TO STABILITY.

3.3 DETERMINE CONSEQUENCES FROM POSTULATED OPERATOR ACTIONS.

3.4 PROVIDE AUDIT AND ANALYSIS CAPABILITIES TO NRC.

- NONLINEAR EFFECTS ON INSTABILITY!
- SYSTEMS EFFECTS ON INSTABILITY!

3. EPA OBJECTIVES FOR BWR STABILITY

3.1 ANSWERS TO THESE SEVEN QUESTIONS:

(1) WHAT ARE THE CAUSES OF LARGE AMPLITUDE OSCILLATIONS AND UNDER WHAT CONDITIONS CAN THEY OCCUR IN A BWR?

(2) WHAT ARE THE INHERENT LIMITS, IF ANY, ON THE AMPLITUDE OF POWER AND FUEL TEMPERATURE OSCILLATIONS IN THE CASE OF SCRAM FAILURE?

(3) CAN CORE-WIDE POWER AND FLOW OSCILLATIONS OCCUR DURING ANY TYPE OF ANTICIPATED TRANSIENT WITHOUT SCRAM (ATWS)?

(4) WHAT ARE THE AMPLITUDES OF FUEL PELLET AND CLADDING TEMPERATURE OSCILLATIONS ASSOCIATED WITH LIMIT-CYCLE POWER OSCILLATIONS?

(5) CAN THE SAFETY LIMIT OF MINIMUM CRITICAL POWER RATIO (MCPR = 1.05) BE VIOLATED DURING LIMIT-CYCLE OSCILLATIONS?

(6) HOW DO THE TIME RATES OF SUPPRESSION POOL TEMPERATURE AND OF CONTAINMENT ATMOSPHERE TEMPERATURE RISE DEPEND ON THE AMPLITUDE OF LIMIT-CYCLE POWER OSCILLATIONS?

4. EPA RESULTS TO DATE

CORE-WIDE OSCILLATIONS

(i) LASALLE-2 EXPERIENCED
THERMOHYDRAULIC INSTABILITY,
ENHANCED BY VOID REACTIVITY FEEDBACK.

(ii) LASALLE-2 EXPERIENCED
LIMIT-CYCLE FLOW AND POWER
OSCILLATIONS, TERMINATED BY SCRAM.
WITHOUT SCRAM, POWER PEAKS MUCH HIGHER
THAN RATED POWER COULD HAVE BEEN
REACHED.

(iii) THREE CAUSES FOR INSTABILITY, ALL NEEDED:

- FLOW REDUCTION (TWO RCP TRIPPED)
- REACTIVITY INSERTION (COLD FEEDWATER)
- EXTREME RADIAL AND AXIAL POWER PEAKING.

(iv) SLOW RESTORATION OF RECIRCULATION FLOW
RESTABILIZES POWER AND CORE FLOW.

4. EPA RESULTS TO DATE (CONT.)

(v) TRANSITION FROM LINEAR TO NONLINEAR POWER OSCILLATIONS IS ACCOMPANIED BY PERIOD-DOUBLING BIFURCATION AND AMPLITUDE GROWTH.

(vi) MEAN POWER INCREASES WITH INCREASE IN AMPLITUDE OF POWER OSCILLATIONS.

INCREASE DEPENDS ON REACTIVITY (I.E. FEEDWATER MASS FLOW RATE AND TEMPERATURE).

(vii) EPA SIMULATIONS
(MORE THAN 60 TRANSIENTS, 15 MINUTES LONG)

- PRIMARY MODELING PARAMETERS:
  VOID REACTIVITY COEFFICIENTS,
  POWER PEAKING FACTORS, POWER SHAPE,
  SUBCOOLING,
  EXIT FLOW RESISTANCE,
  ENTRANCE FLOW RESISTANCE,
  FUEL THERMAL RESPONSE.

4. EPA RESULTS TO DATE (CONT.)

- RESULTS PRESENTED ON
  MCPR
  FUEL TEMPERATURE
  POWER VS FLOW MAP
    100%, 80% CONTROL ROD LINES
    NATURAL CIRCULATION
    STABILITY BOUNDARY (± 20%)
  ATWS TRANSIENTS AFTER ESTABLISHED OSCILLATIONS:
    TURBINE TRIP, WITH/WITHOUT FW PUMP TRIP
    TURBINE TRIP, WITH/WITHOUT BYPASS
    MSIV CLOSURE
    RESTART OF RCP
    RESTART OF RCP AND MSIV CLOSURE.

GRAPHS FROM EPA

NEW CALCULATIONS

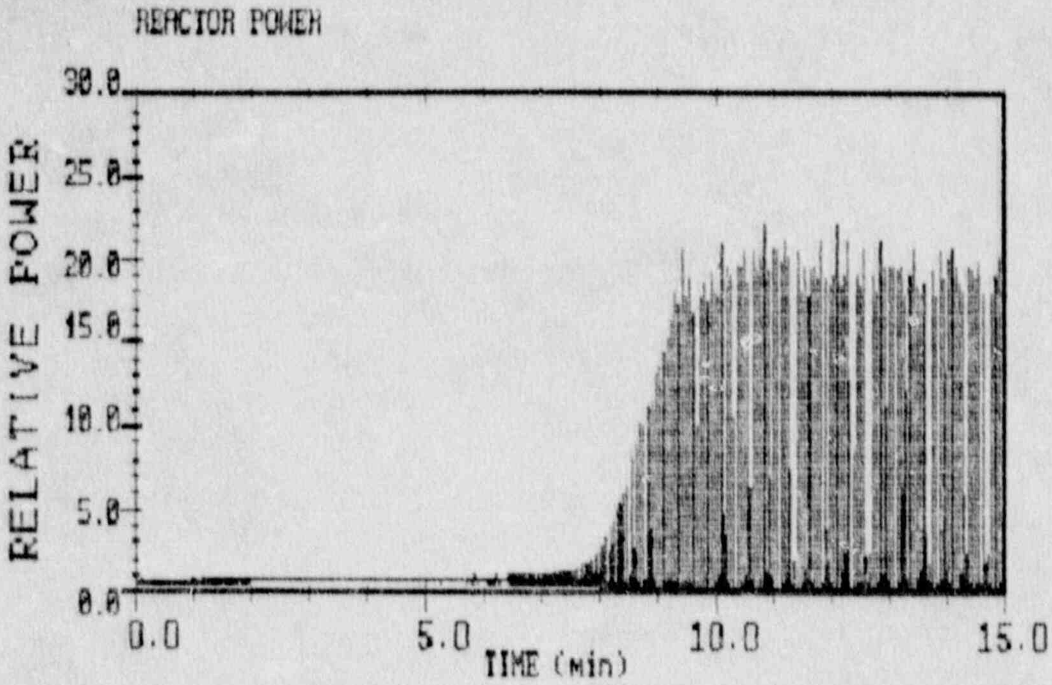
(i) WITH FW CONTROLLER IN NORMAL OPERATION

(ii) WITH MANUAL FW FLOW CONTROL

(ii) WITH STARTREC FW FLOW

ZOOM OF POWER AND BIFURCATION

POWER VS FLOW MAP



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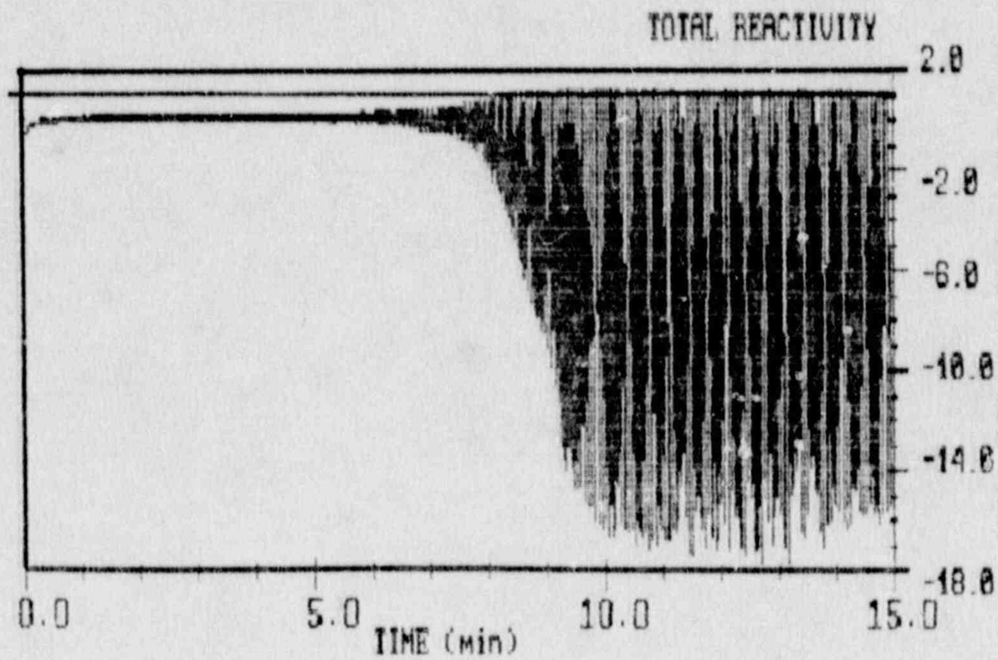

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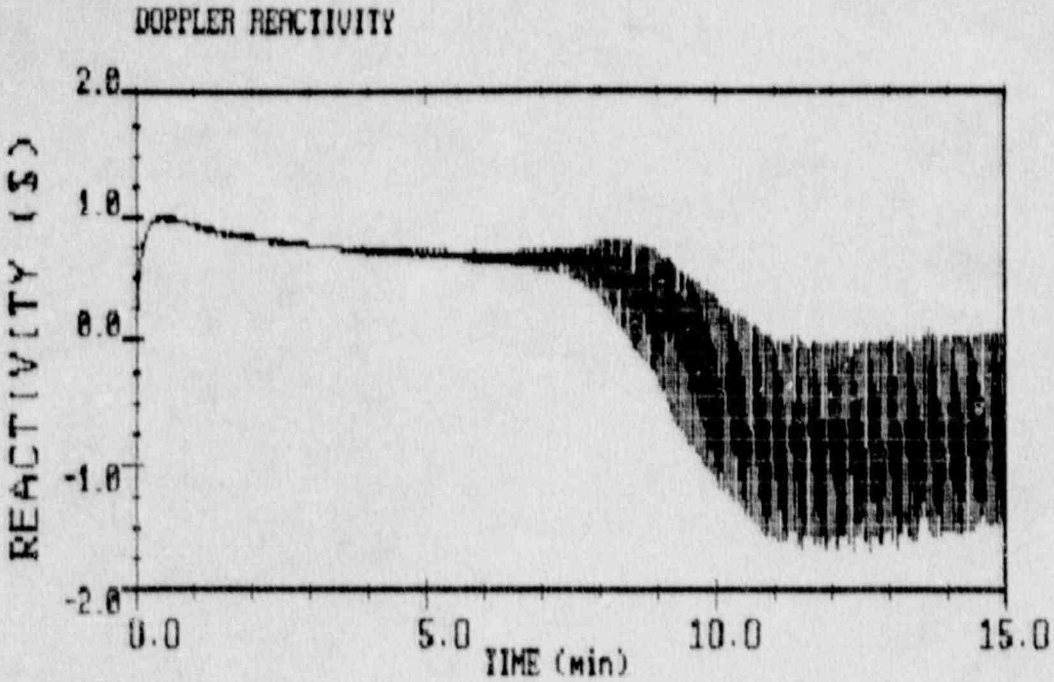

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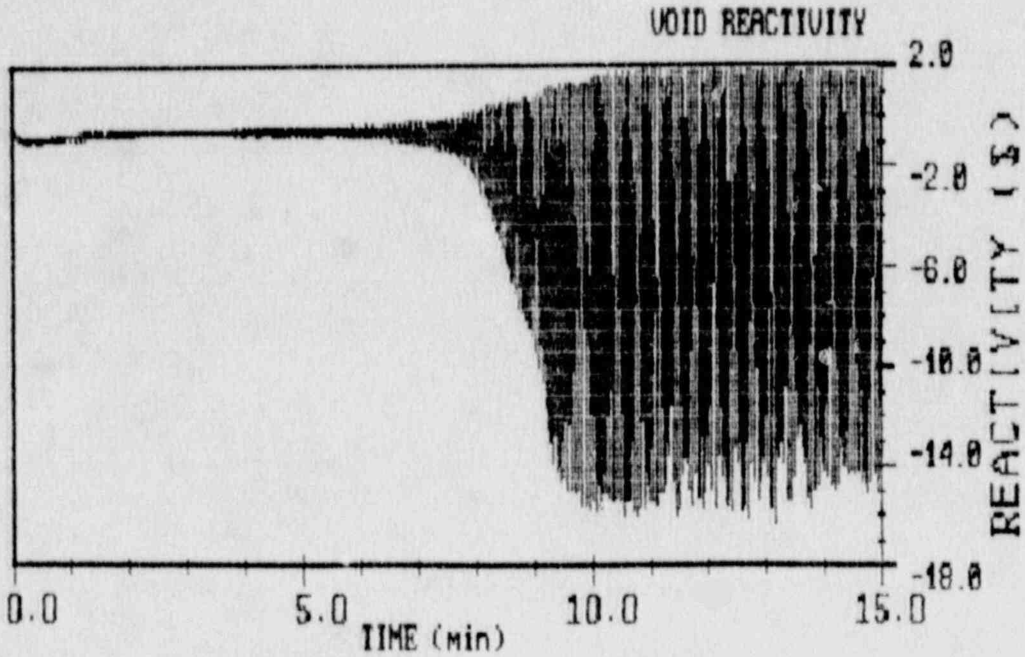

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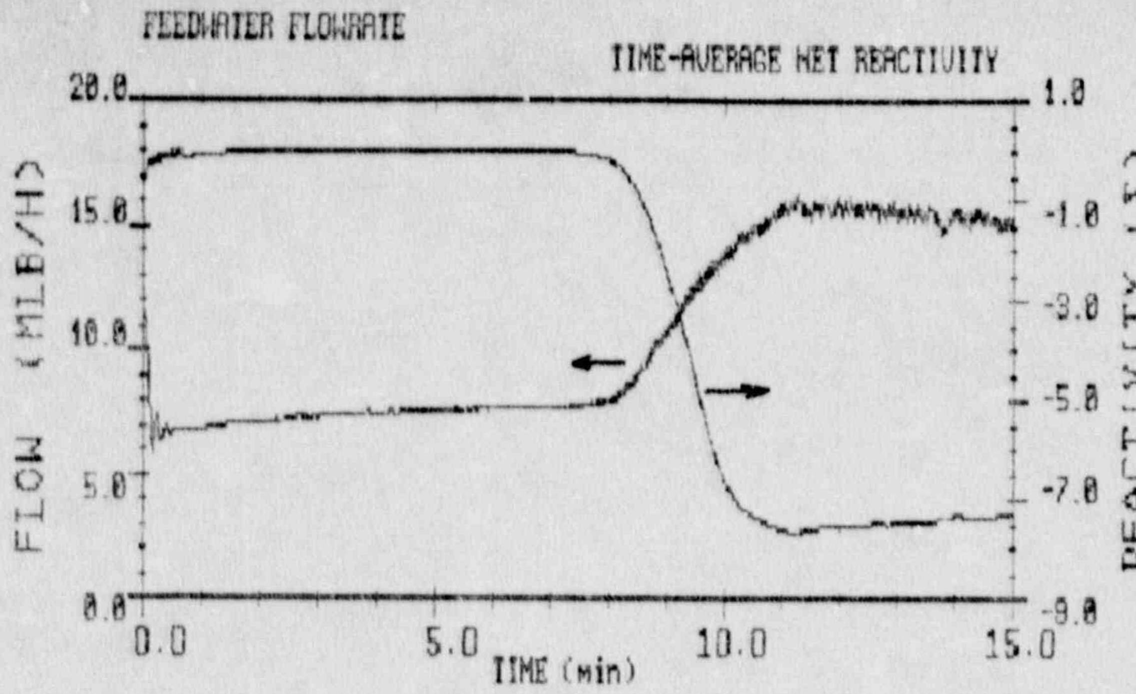

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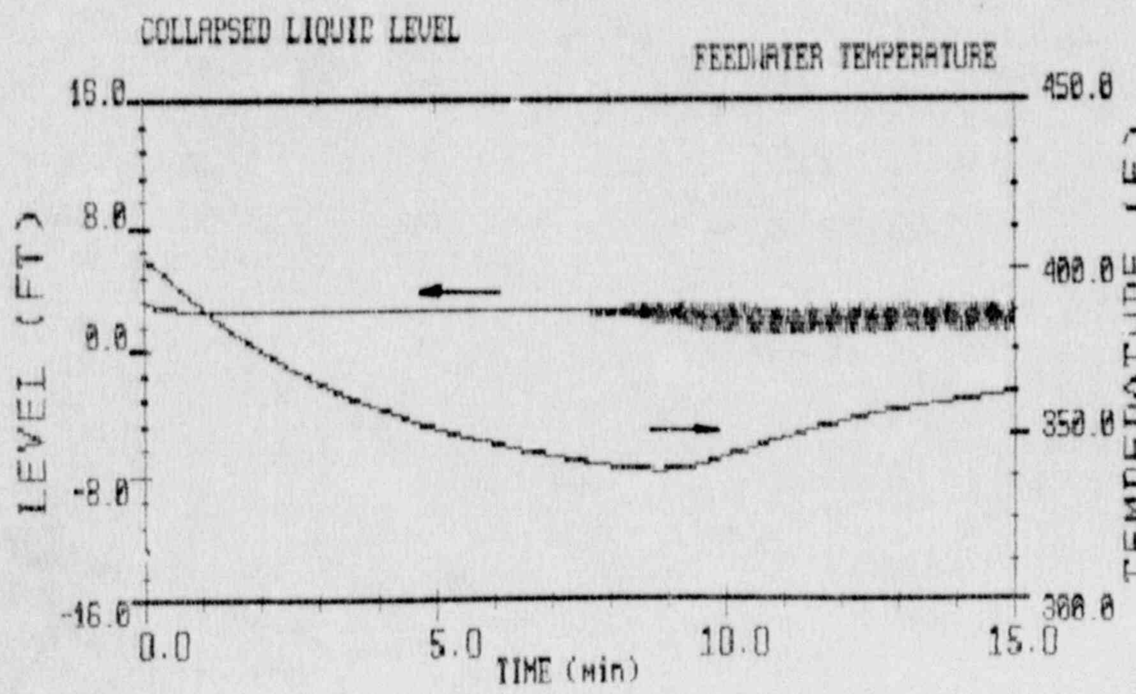

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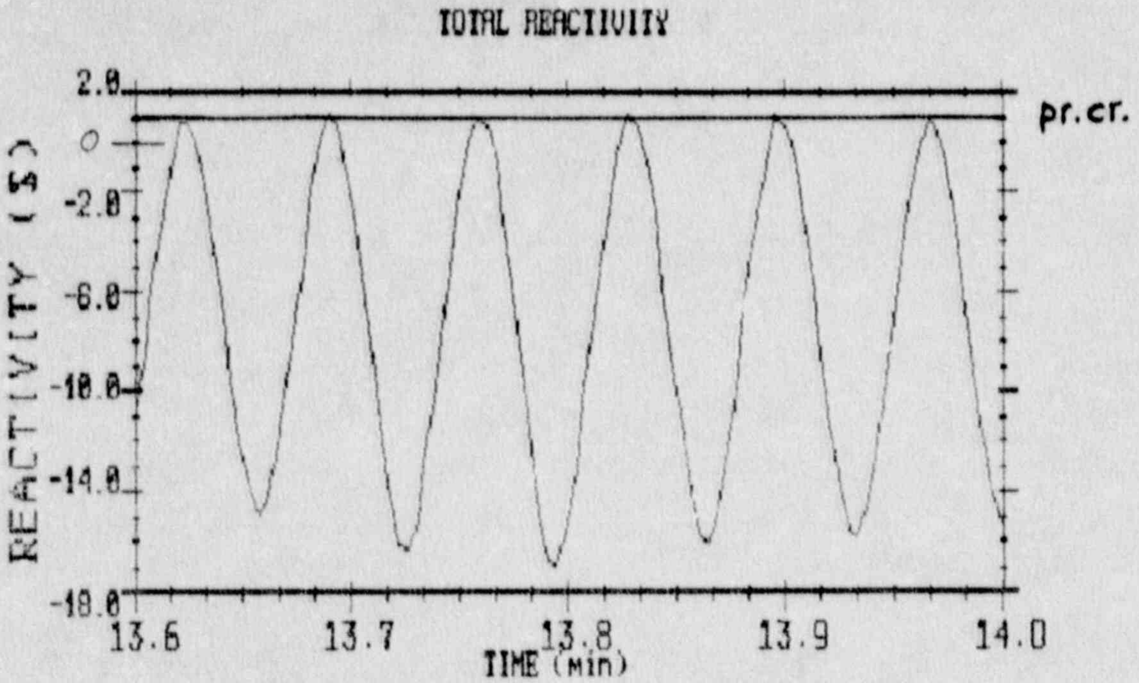

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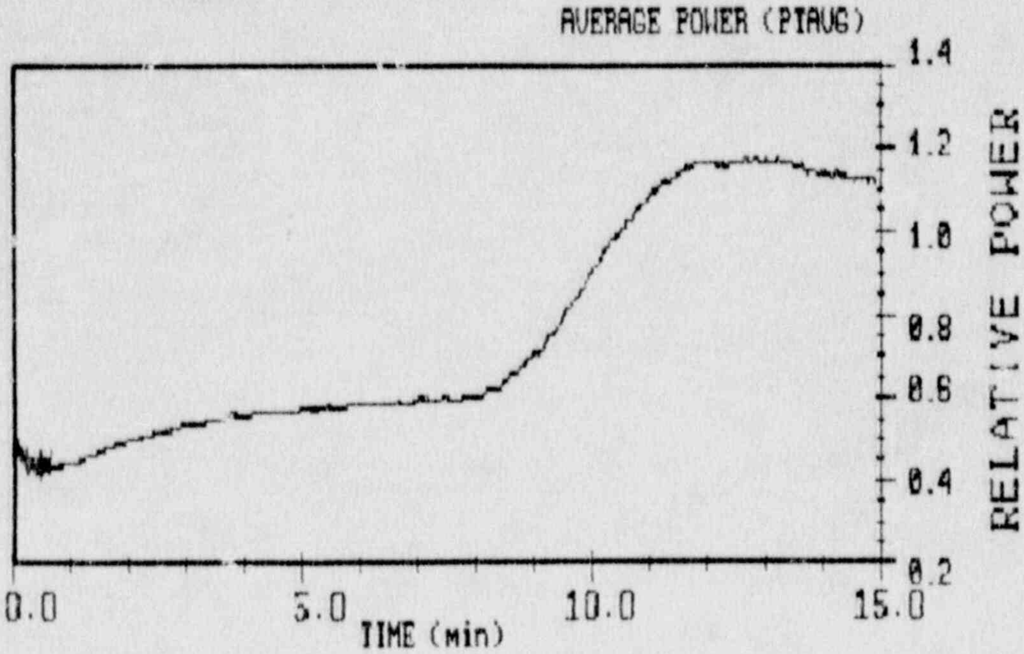
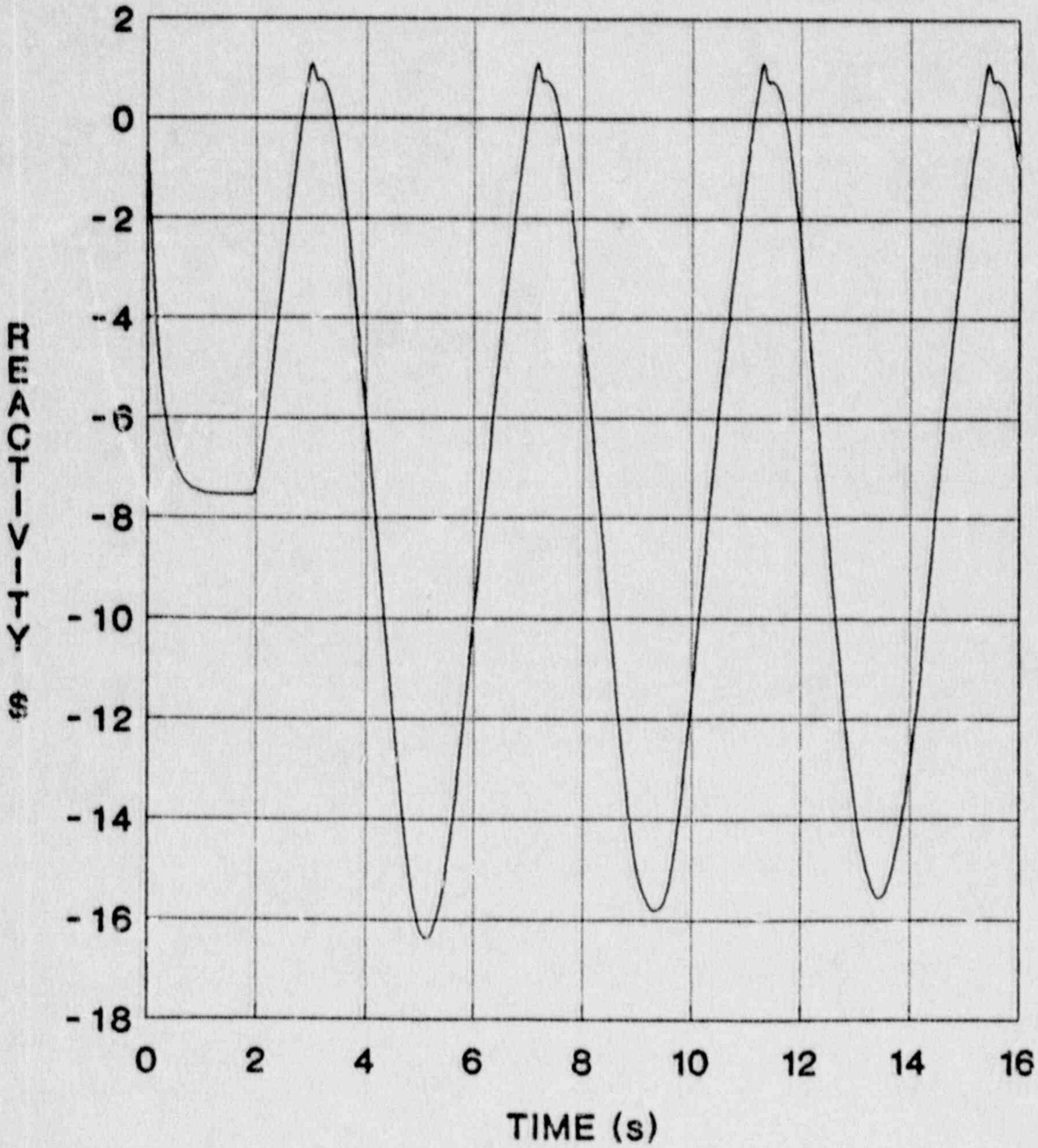
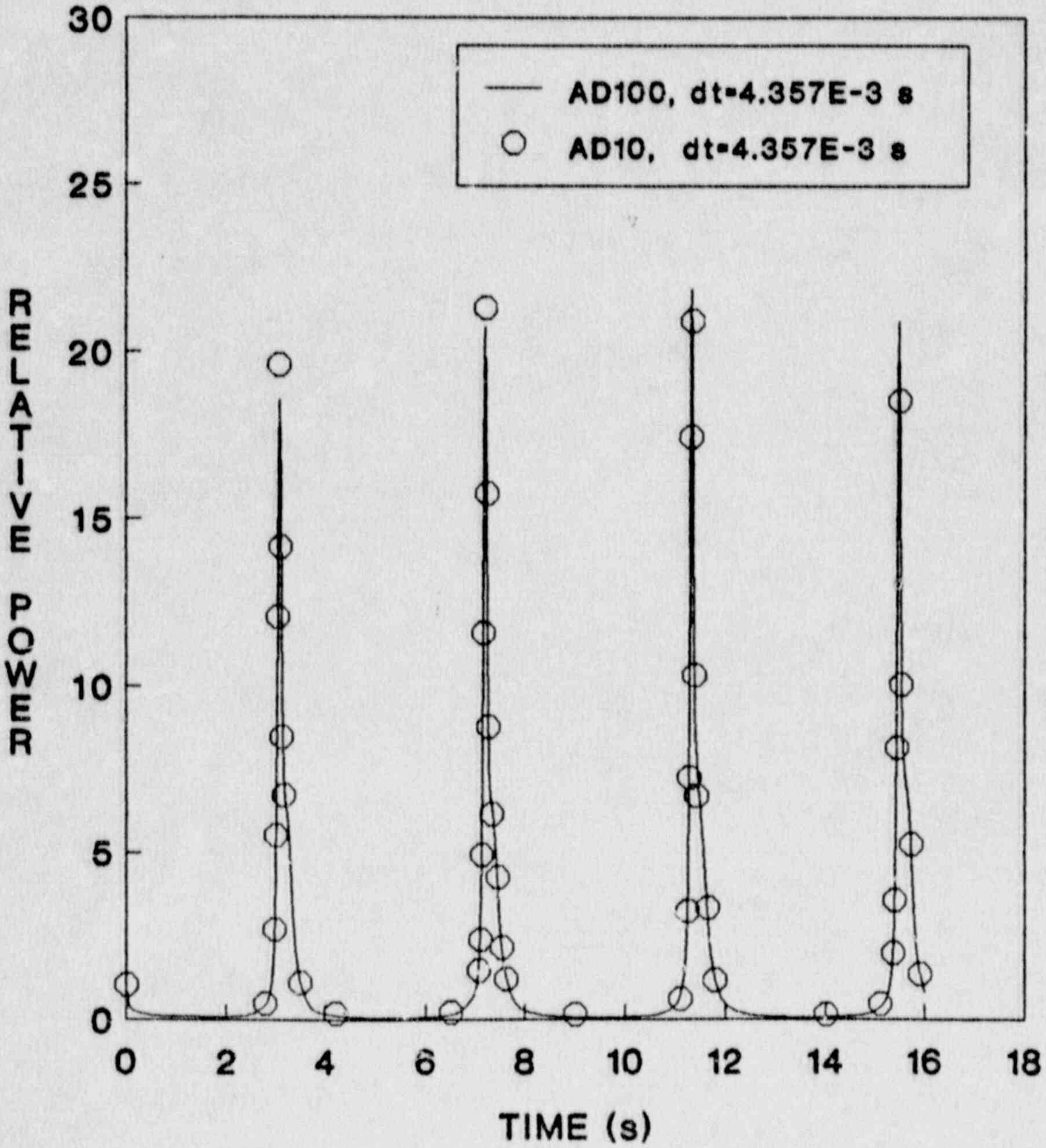


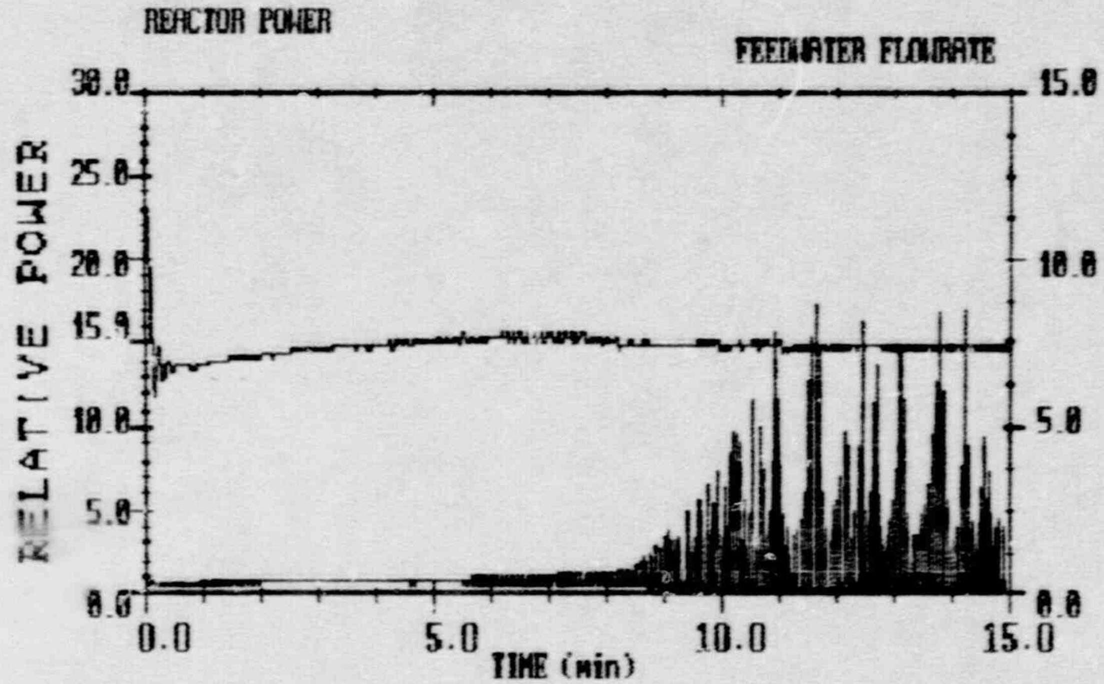
SLV3H04 Ic=8 ØRES Sp=5 MuPW=7 BNL Plant Analyzer 27-OCT-89 12:44

LaSalle-2 Total Reactivity Behavior
With Scram Failure and Auto FW Control
AD10 Calculated Results for EPA



LaSalle-2 Power Oscillations
With Scram Failure & Auto FW Control
Comparison of AD-10 and AD-100 Results





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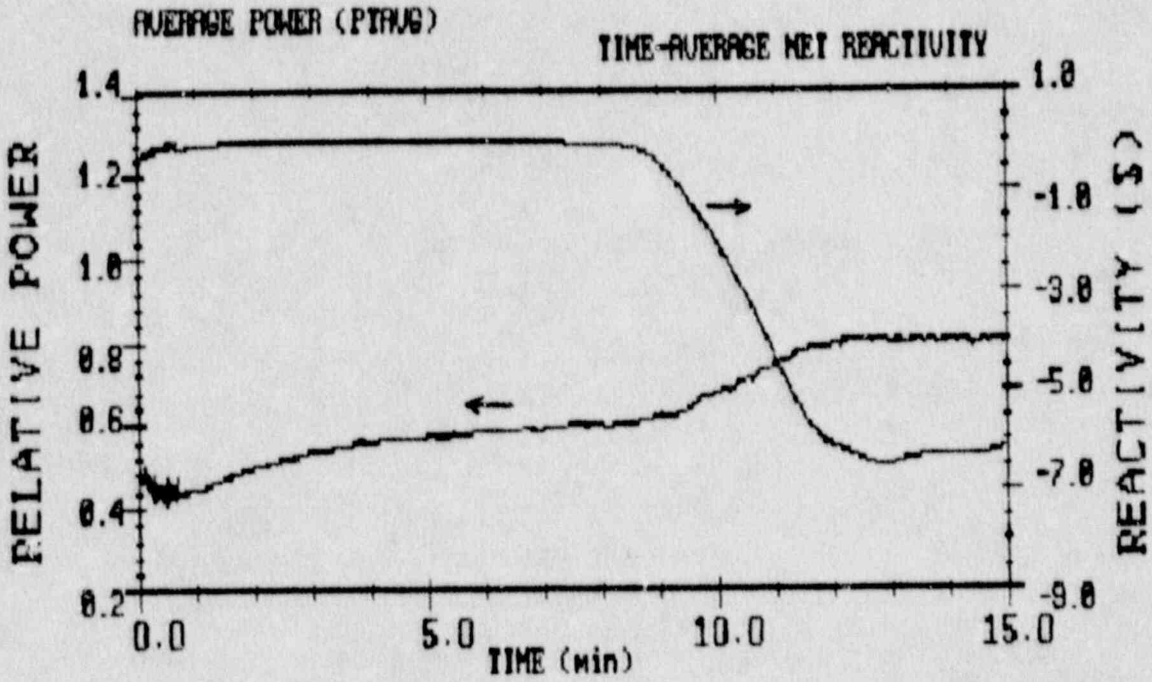

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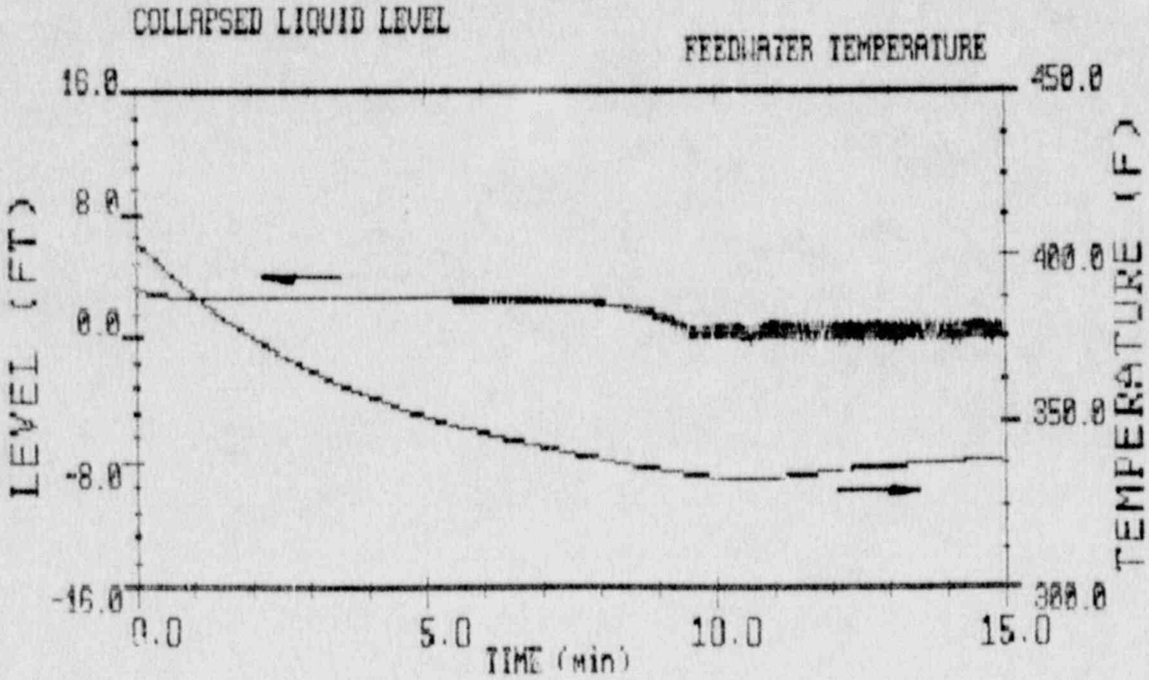

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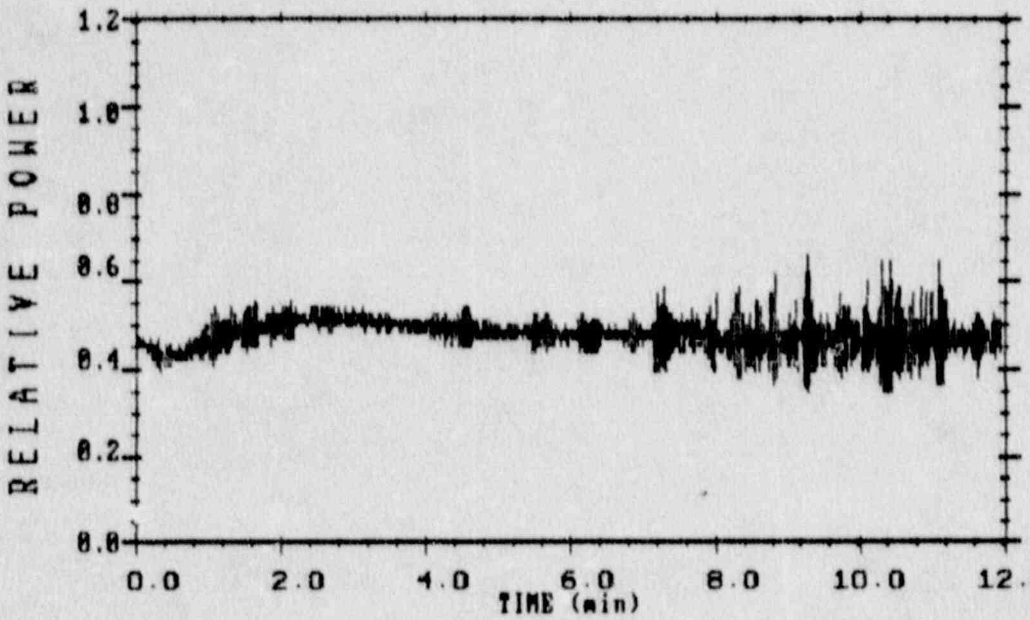
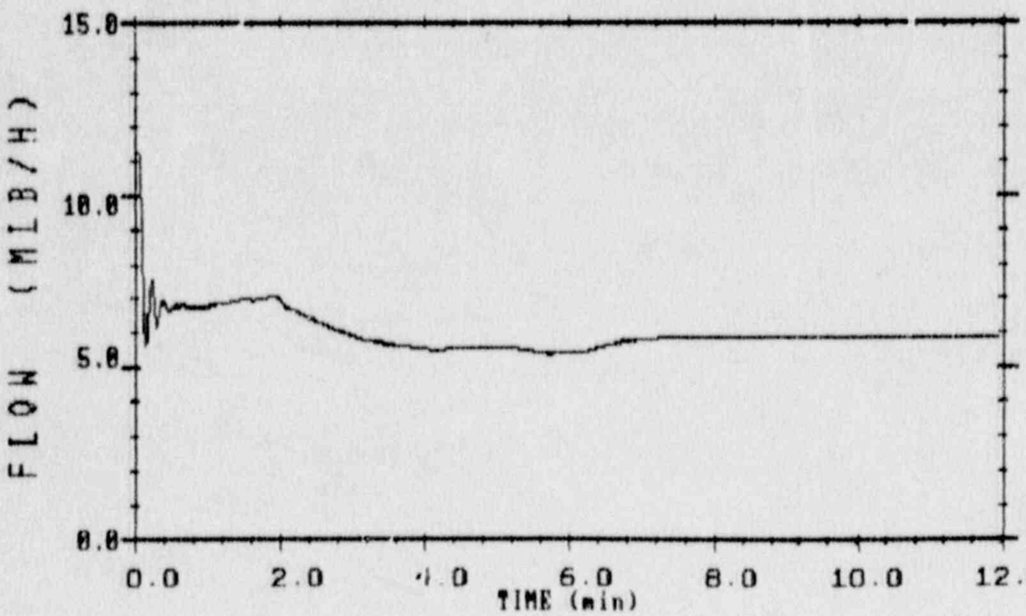
REACTOR POWER

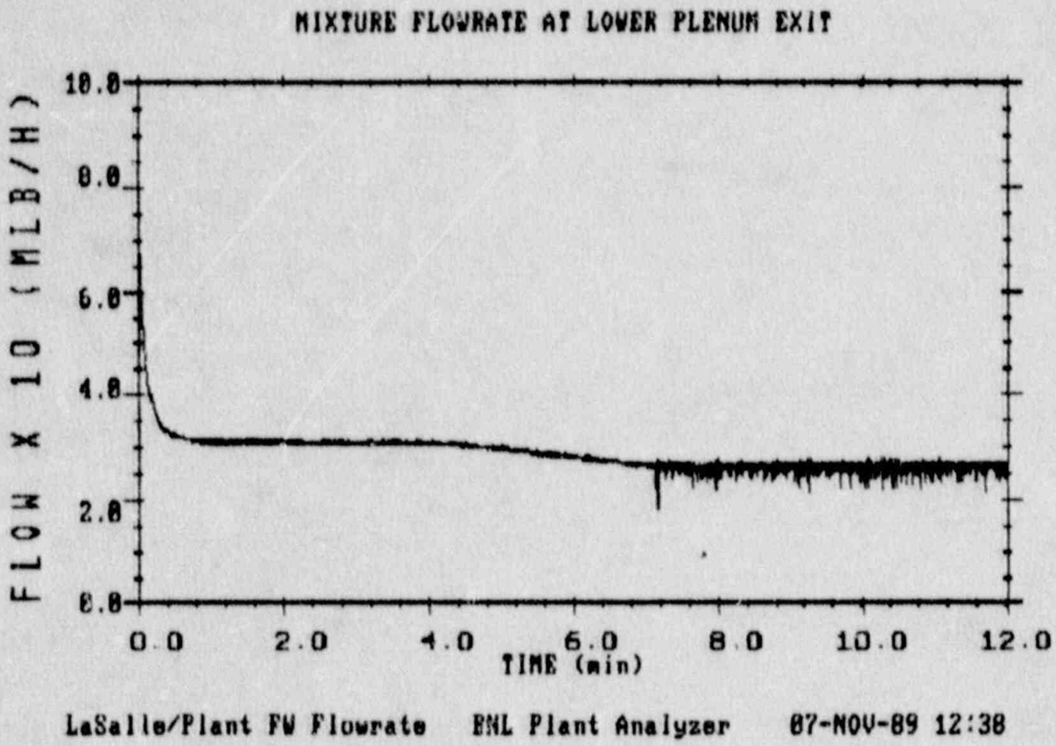


LaSalle/Plant FW Flowrate BNL Plant Analyzer 07-NOV-89 12:38

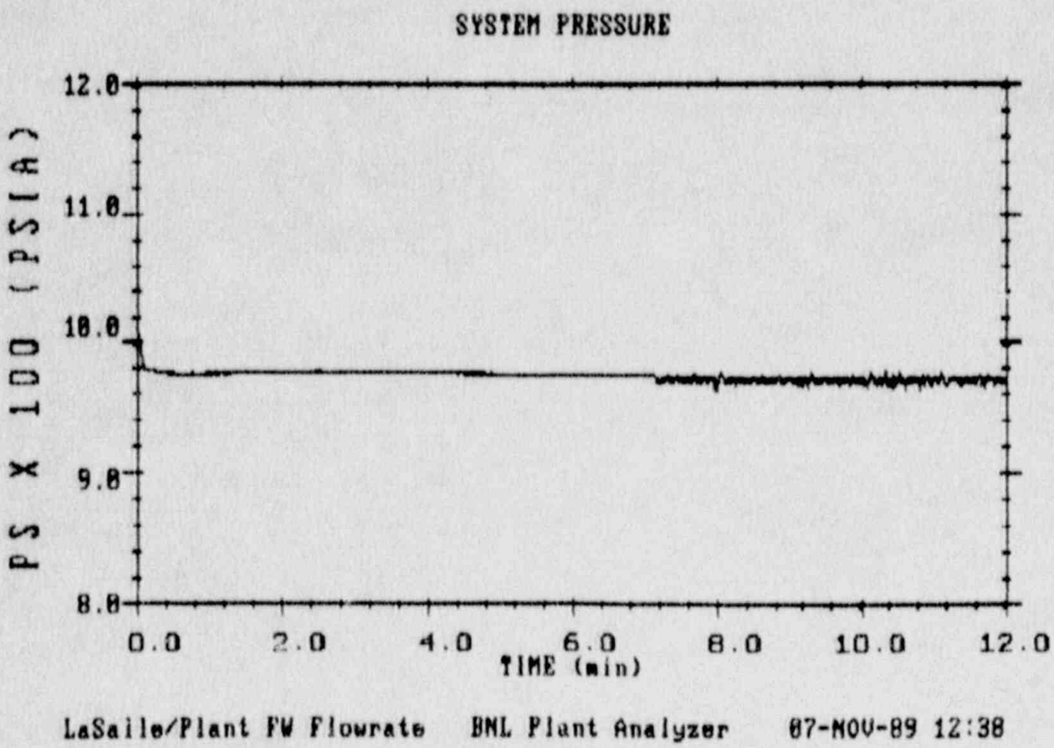
FEEDWATER FLOWRATE



LaSalle/Plant FW Flowrate BNL Plant Analyzer 07-NOV-89 12:38

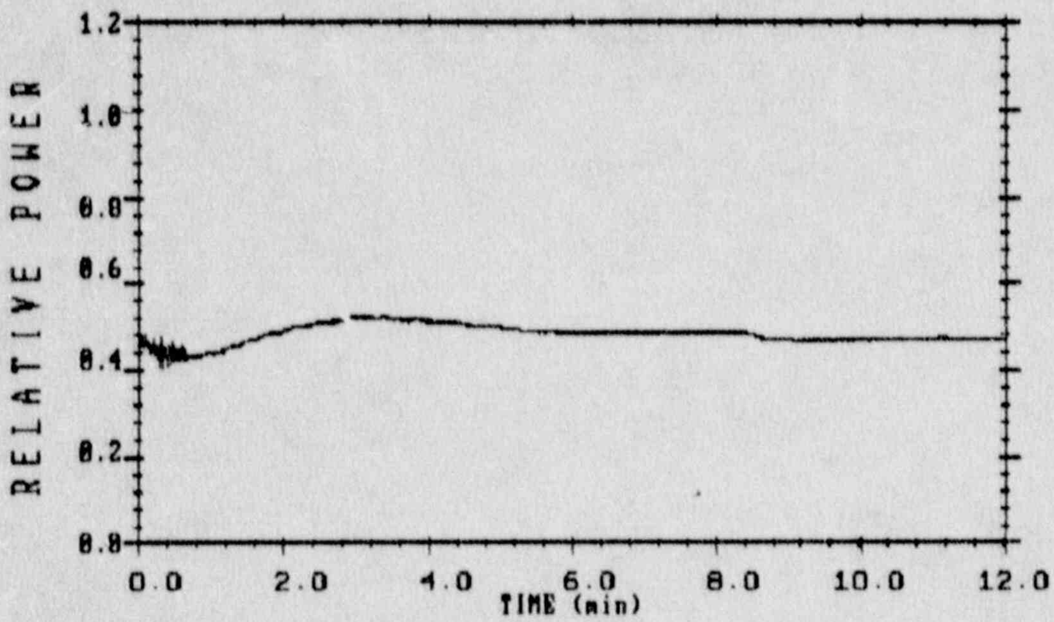


MIXTURE FLOWRATE AT LOWER PLENUM EXIT
FLOW X 10 (MLB/H)
10.0
8.0
6.0
4.0
2.0
0.0
0.0 2.0 4.0 6.0 8.0 10.0 12.0
TIME (min)
LaSalle/Plant FW Flowrate BNL Plant Analyzer 07-NOV-89 12:38



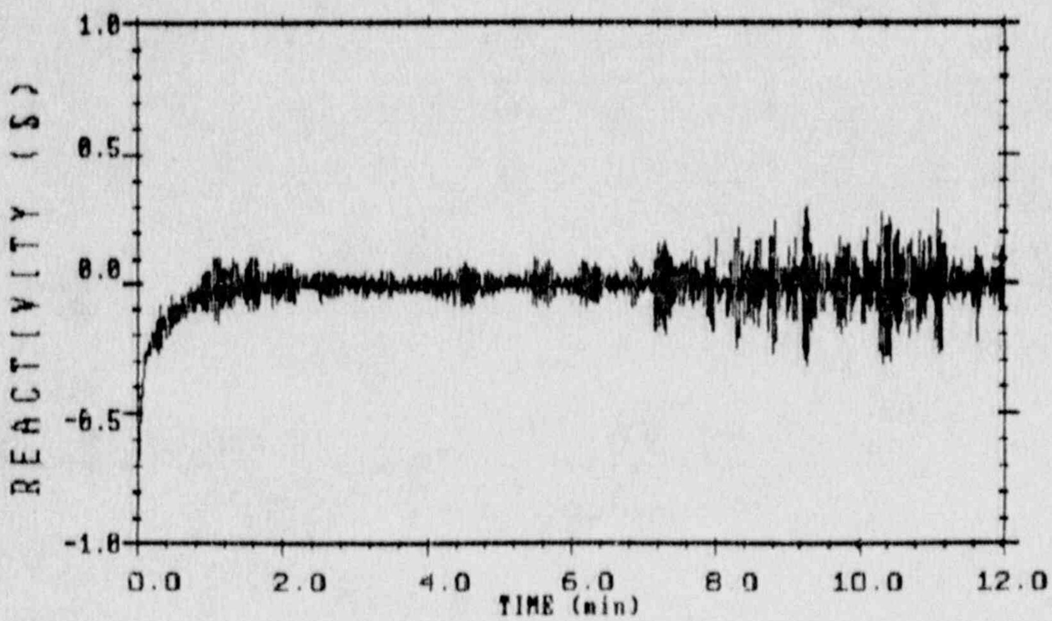
SYSTEM PRESSURE
PS X 100 (PSIA)
12.0
11.0
10.0
9.0
8.0
0.0 2.0 4.0 6.0 8.0 10.0 12.0
TIME (min)
LaSalle/Plant FW Flowrate BNL Plant Analyzer 07-NOV-89 12:38

AVERAGE POWER (PTAVG)



LaSalle/Plant FW Flowrate BNL Plant Analyzer 07-NOV-89 12:38

TOTAL REACTIVITY



LaSalle/Plant FW Flowrate BNL Plant Analyzer 07-NOV-89 12:38

5. EPA LIMITATIONS

5.1 MODELING LIMITATIONS

- POINT KINETICS
  - AXIAL POWER SHAPE FROM TRANSIENT LASALLE DATA (STARTREC)
  - RADIAL POWER SHAPE THROUGH FIXED PEAKING FACTOR IN HOT CHANNEL AND SPATIAL WEIGHT FACTOR
- ONE-DIMENSIONAL CORE FLOW (ONLY FOR IN-PHASE OSCILLATIONS)
- NO MODEL FOR SUPERHEATED VAPOR/POST-CHF (FUEL COOLED BY FORCED CONVECTION VAPOR FLOW)
- NO TRACKING OF BOILING BOUNDARY (BOILING BOUNDARY IS 1 TO 7 CM FROM ENTRANCE FOR LASALLE CONDITIONS PRIOR TO SCRAM)
- FUEL CONDUCTION MODEL FOR THERMALLY THIN CONDUCTION REGIME

6. FUTURE PLANS FOR EPA ANALYSES

- PARAMETRIC STUDIES IN SUPPORT OF TPG ON BWR STABILITY (AS SPECIFIED BY RES)
- ANALYSES REQUESTED BY NRR
- COMPLETION OF COMPUTATIONAL ERROR ANALYSIS
- COMPLETION/REVISION OF DRAFT DOCUMENTATION ON EPA ANALYSES OF BWR INSTABILITY.

5.2 MODELING PARAMETER UNCERTAINTIES

- VOID REACTIVITY COEFFICIENT (30-50%)
- FORM LOSS COEFFICIENTS, 2-Ø FLOW (± 30%)
- FUEL-CLAD GAP CONDUCTANCE (± 45%)

5.3 TOTAL UNCERTAINTY (TIME DOMAIN CODE)

± 20% FOR DECAY RATIO (FREQ. CODE)

- 20% FOR GAIN (EXPERIMENT)

BWR STABILITY ANALYSIS

WITH

RAMONA-3B

W. WULFF

BROOKHAVEN NATIONAL LABORATORY

PRESENTED BEFORE

ACRS SUBCOMMITTEE ON THERMAL HYDRAULICS

NOVEMBER 8-9, 1989

SAN FRANCISCO, CA

BROOKHAVEN NATIONAL LABORATORY bnl
ASSOCIATED UNIVERSITIES, INC. aui

RAMONA-3B ANALYSES OF BWR STABILITY

1. RAMONA-3B DESCRIPTIONS

2. RAMONA-3B ASSESSMENT

3. RAMONA-3B OBJECTIVES FOR BWR STABILITY

4. RAMONA-3B RESULTS TO DATE

5. RAMONA-3B LIMITATIONS

6. FUTURE PLANS FOR RAMONA-3B ANALYSES

RAMONA-3B DESCRIPTION

1.1 REFERENCE: NUREG/CR-3664 (1984)
GIVES CODE STATUS AS OF OCT. 1981.

1.2 MAJOR CHARACTERISTICS OF RAMONA-3B

- BWR SYSTEMS CODE WITH

  - 3-DIMENSIONAL NEUTRON KINETICS
    - 1-1/2 GROUP COARSE MESH DIFFUSION MODEL
    - 6 DELAYED NEUTRON GROUPS
    - DECAY HEAT: ANS STANDARD 5.1 (1978)
    - ALL REACTIVITY FEEDBACKS FROM THERMOHYDRAULICS ARE MODELED
    - RECTANGULAR COORDINATES

  - THERMOHYDRAULICS
    - NONHOMOGENOUS FLOW (DRIFT FLUX)
    - NONEQUILIBRIUM FLOW (SCANDPOWER Γ_V)
    - PARALLEL CHANNEL FLOW
    - ONE-DIM.FLOW IN DOWNCOMER,
PLENA,
RISER.

RAMONA-3B CHARACTERISTICS (CONT.)

- THERMOHYDRAULICS (CONT.)
  FOUR-EQUATION DRIFT FLUX
  MODEL WITH:
  - LOOP MOMENTUM BALANCES,
  - MIXTURE VOLUMETRIC FLUX DIVERGENCE EQUATION,
  - MIXTURE ENERGY BALANCE, AND
  - VAPOR MASS BALANCE.

  VAPOR AT SATURATION,
  LIQUID SUBCOOLED, SATURATED OR SUPERHEATED.

- RECIRCULATION FLOW SYSTEM WITH:
  JET PUMPS,
  RECIRCULATION PUMP/MOTOR GENERATOR DYNAMICS.

- SAFETY AND RELIEF VALVES,

- STEAM LINE DYNAMICS (ACOUSTICS),
  SIMPLIFIED PRESSURE REGULATOR

- FEEDWATER CONDITIONS IMPOSED AS B.C.

- SOLUTION METHODS IN RAMONA-3B

  - IMPLICIT INTEGRATION FOR
    PROMPT NEUTRON EQUATION

  - EXPLICIT INTEGRATION FOR
    DELAYED NEUTRON EQUATIONS,
    ODES OF LOOP MOMENTUM BALANCES,
    ODES OF GLOBAL MASS AND ENERGY
    BALANCE FOR SYSTEM PRESSURE,
    ODES OF VAPOR MASS BALANCE,
    ODES OF MIXTURE ENERGY BALANCE,
    ODES OF STEAM LINE DYNAMICS,
    ODES OF ROTATING MACHINERY,
    CONTROLS.

  - QUADRATURES OVER SPACE FOR
    MIXTURE VOLUMETRIC FLUX
    DIVERGENCE EQUATION,
    LOOP MOMENTUM BALANCES (GRAVITY
    TERMS).

- SOLUTION METHODS IN RAMONA-3B
  - NO LINEARIZATION OF EQUATIONS
  - COMPUTATIONAL ERRORS FROM
    - NUMERICAL DIFFUSION: VAPOR MASS, MIXTURE ENERGY.
    - NUMERICAL QUADRATURE: (SIMPSON, TRAPEZOIDAL RULES)
    - NUMERICAL INTEGRATION OF ODE'S (1ST. ORDER EULER, 4TH ORDER RUNGE-KUTTA)
  - NUMERICAL DIFFUSION IS
    - MORE IMPORTANT THAN OTHER COMP. ERRORS,
    - REDUCED BY USING MAX. COURANT NO. < 1.0.

1.3 RECENT MODIFICATIONS OF RAMONA-3B

- CHANGE FROM SLIP (SCANDPOWER) TO DRIFT VELOCITIES (ISHII, ANL-77-47)

- CAPABILITY TO COMPUTE FLOW REVERSAL: UPWIND DIFFERENCING FOR REVERSE FLOW, FLOW BRANCHING FOR REVERSED FLOW.

2. RAMONA-3B ASSESSMENT

2.1 DEVELOPMENTAL ASSESSMENT
(NUREG/CR-3664, P. 315)

- STEADY-STATE BOILING IN HEATED CHANNEL
- PEACH-BOTTOM 2: SAFETY AND RELIEF VALVE TEST, TURBINE TRIP WITH DELAYED SCRAM.
- BROWNS-FERRY: GENERATOR LOAD REJECTION, FW PUMP TRIP TEST, RECIRCULATION PUMP TRIP TEST.
- KRB (GE): PRESSURE SET POINT OSCILLATION.
- VERMONT YANKEE: GENERATOR LOAD REJECTION
- OYSTER CREEK: RECIRCULATION PUMP TRIP
- COMPARISONS WITH ANALYTICAL SOLUTIONS FOR STEAM LINE DYNAMICS

2.2 ASSESSMENT FOR INSTABILITY ANALYSES

- FRIGG-3 TESTS (UNIFORM AXIAL POWER DISTR.)
- FRIGG-4 TESTS (NONUNIFORM AXIAL POWER DISTR.)

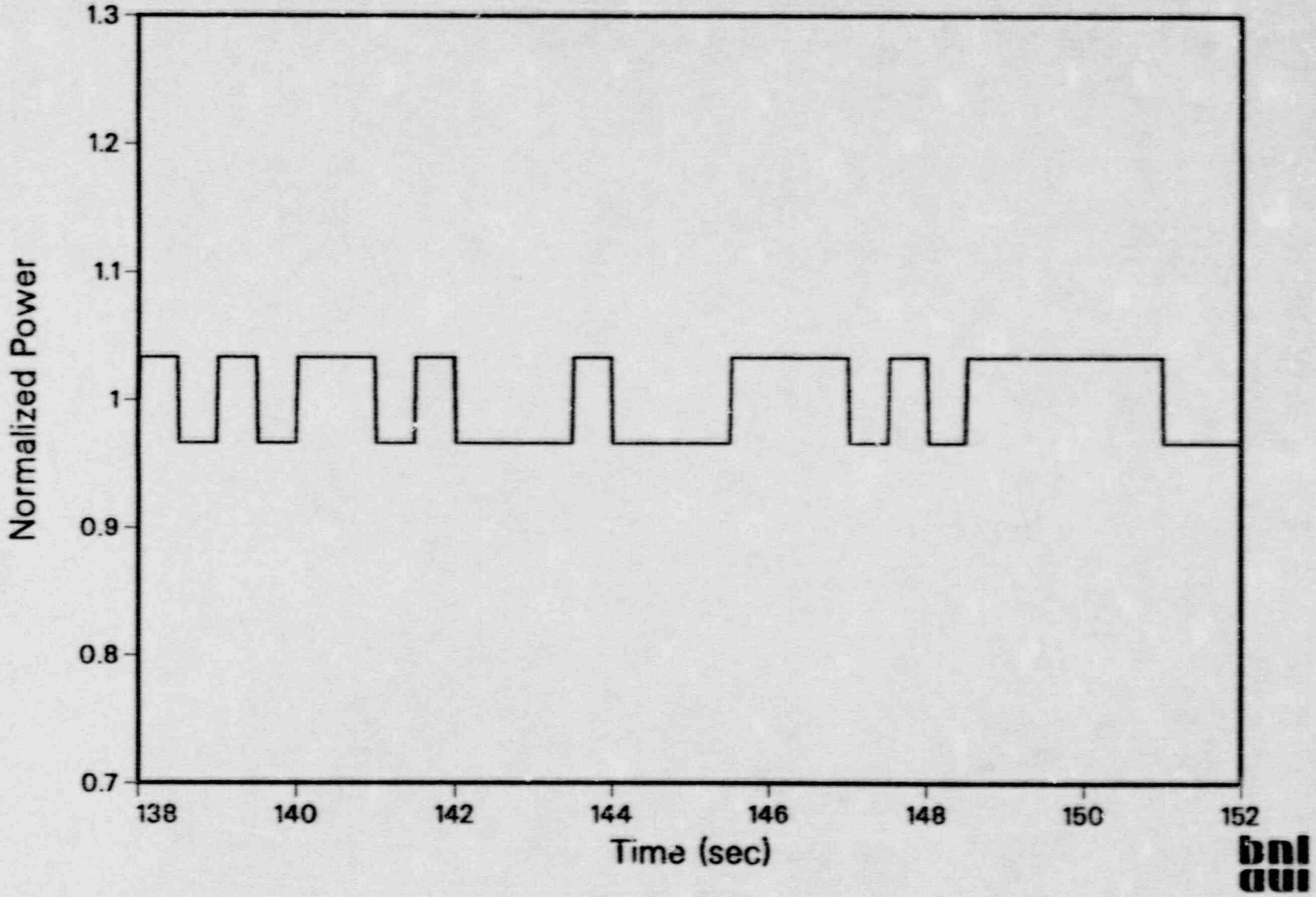
TEST CHARACTERISTICS:

BASE POWER IMPOSED (3000-5000 KW)
PSEUDO-RANDOM BINARY SEQUENCE WITH 100 KW AMPLITUDE (±1% OF POWER).
CHANNEL INLET MASS VELOCITY MEASUREMENT,
CHANNEL EXIT VOID FRACTION MEASUREMENT,
RESULTS EVALUATED TO GET TRANSFER FUNCTIONS → GAIN AND PHASE SHIFT.

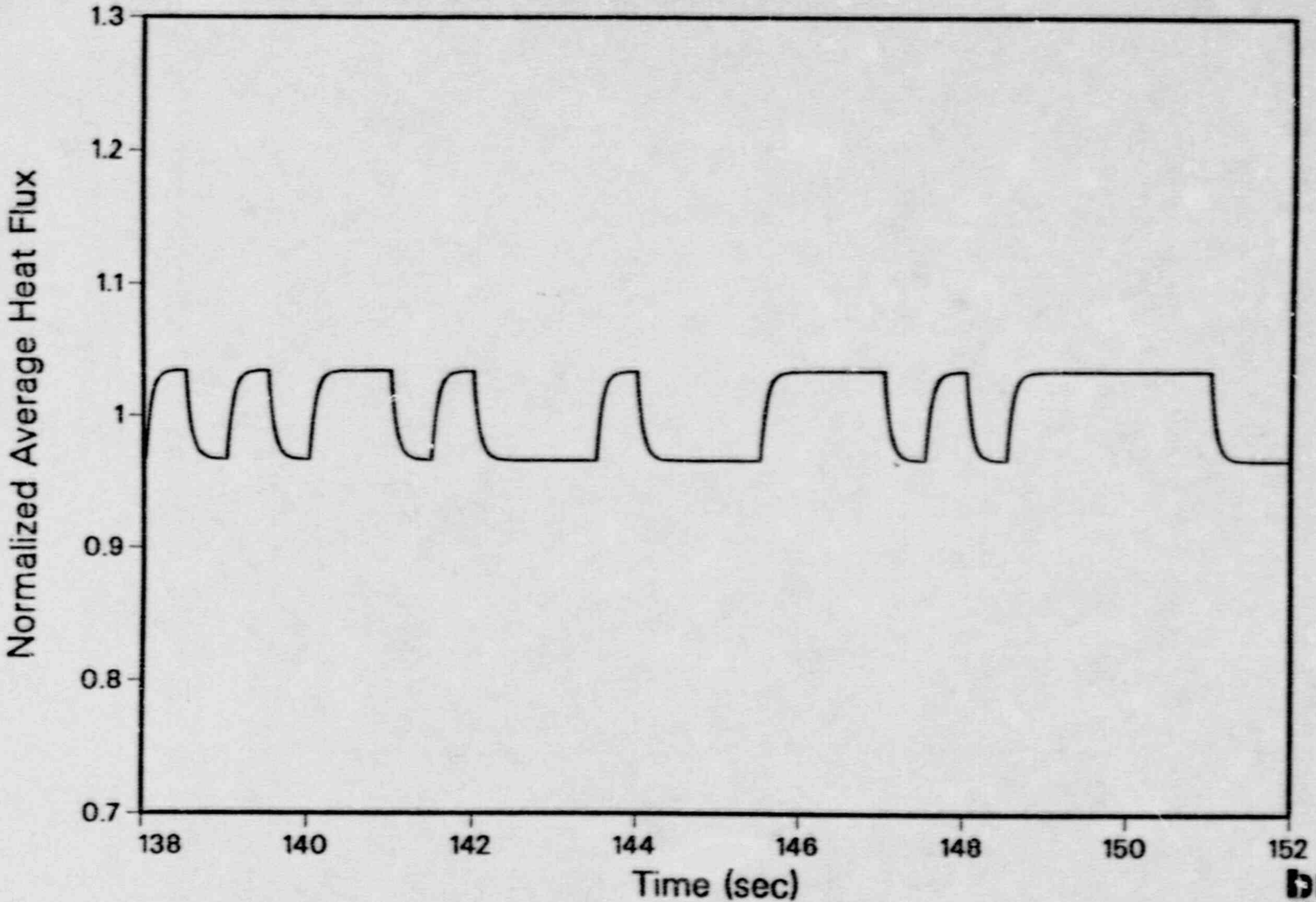
RAMONA-3B COMPUTATIONS:

PSEUDO-RANDOM BINARY SEQUENCE WITH 100 KW POWER AMPLITUDE (AS IN TEST)
SIMULATED HEATING ELEMENT WITH FIRST-ORDER LAG,
DELAY COMPUTED FROM CURRENT HEAT TRANSFER COEFFICIENT AND THERMAL INERTIA.
CALCULATE TRANSFER FUNCTIONS FOR POWER TO VOID FRACTION AND FLOW,
→ GAIN AND PHASE SHIFT.

RAMONA CALCULATION
Imposed Core Power
FRIGG-4 (662101)



RAMONA CALCULATION
Average Heat Flux
FRIGG-4 (662101)



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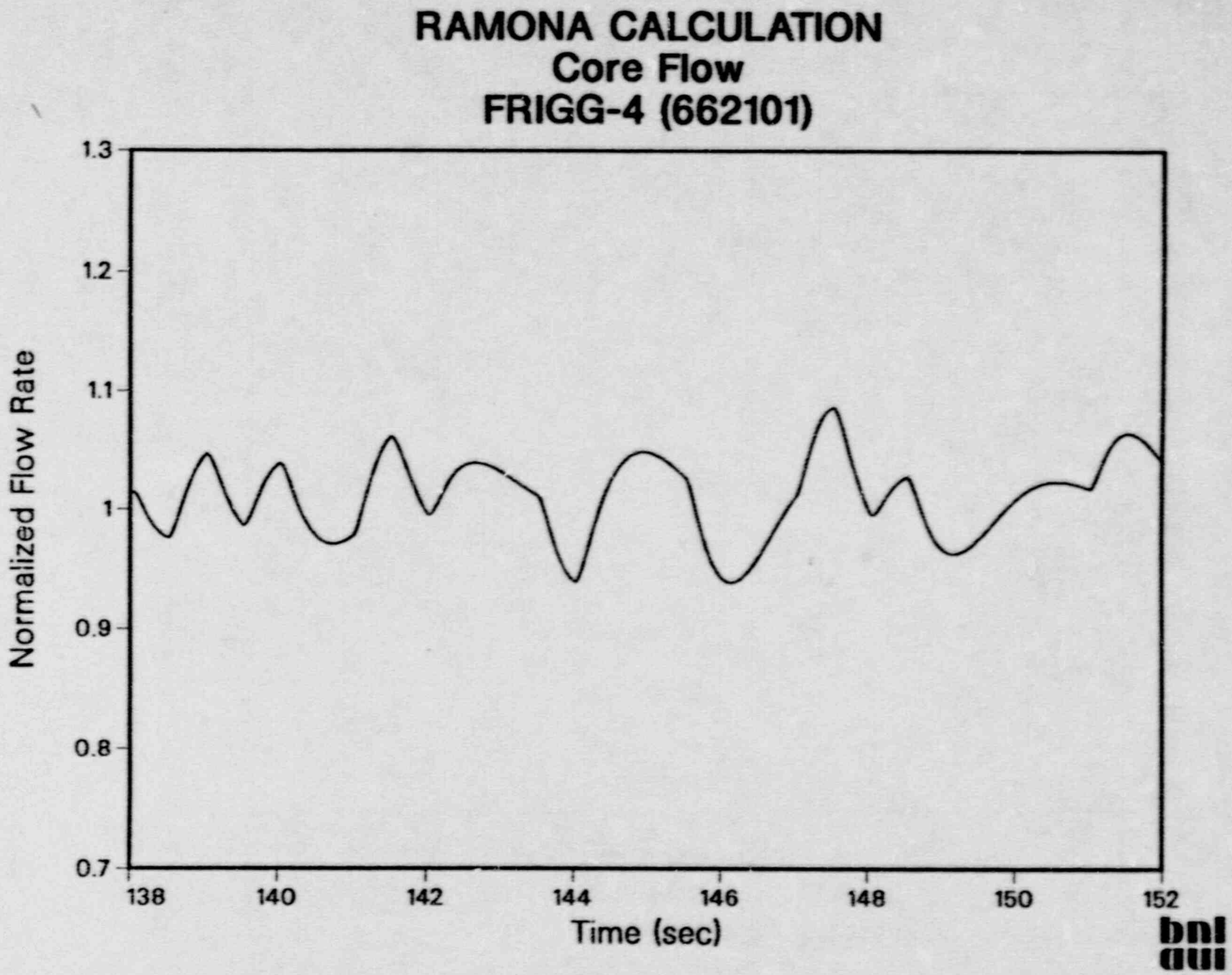
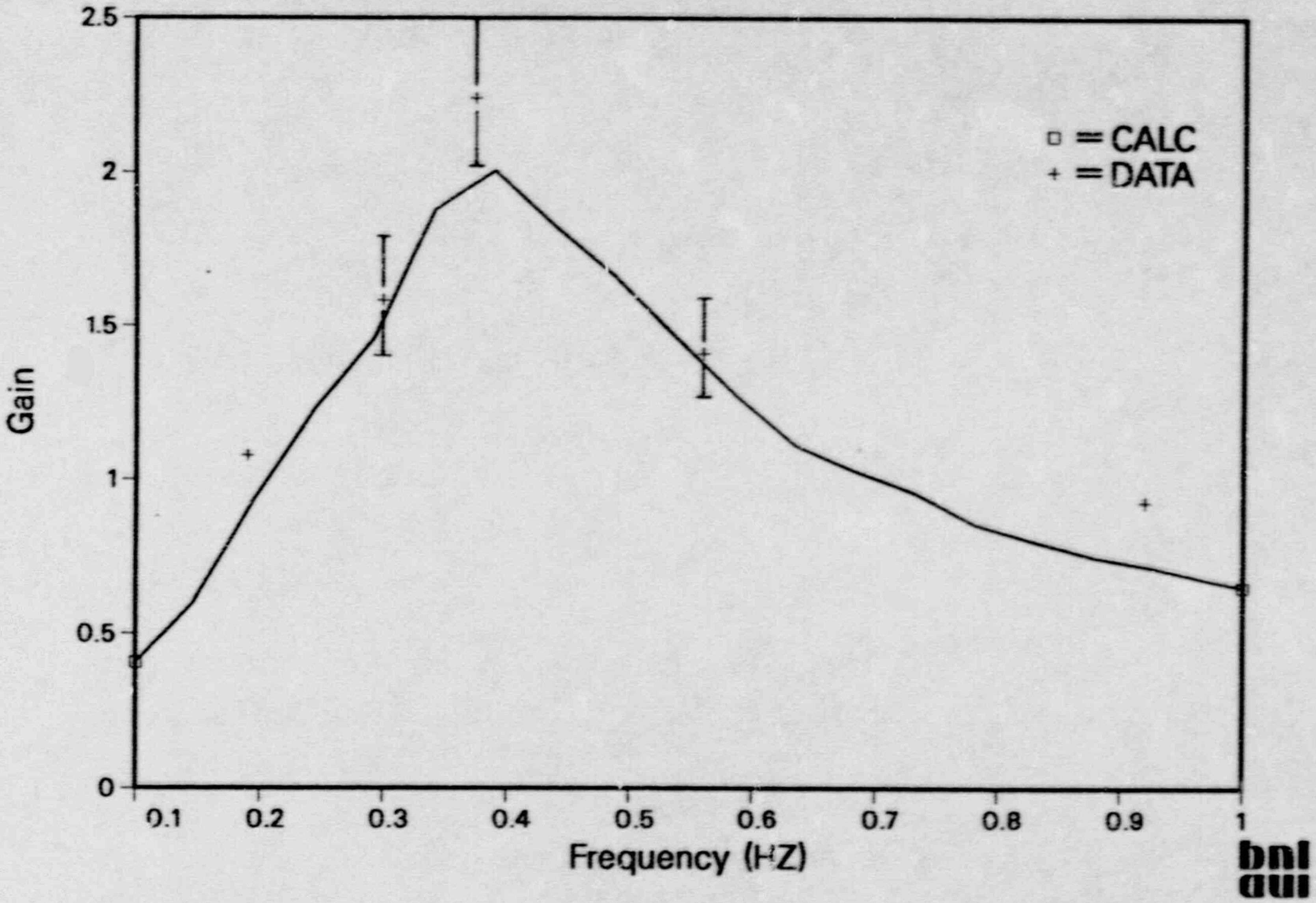
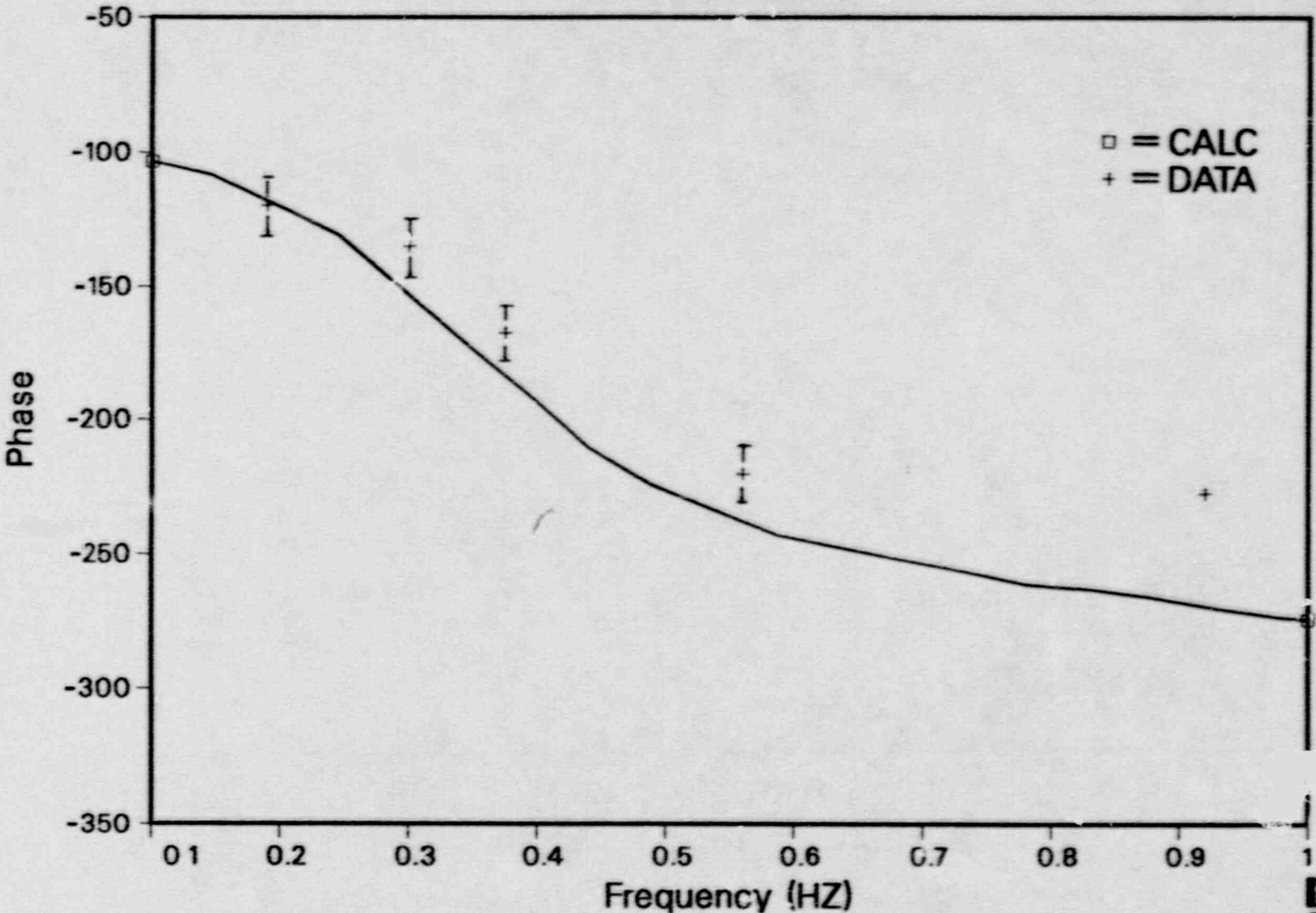

RAMONA CALCULATION
Core Flow
FRIGG-4 (662101)
1.3
1.2
1.1
1
0.9
0.8
0.7
Normalized Flow Rate
138
140
142
144
146
148
150
152
Time (sec)
bnl
aui

RAMONA CALCULATION
Gain (|Transfer Func|)
FRIGG-4 (6S2101)



RAMONA CALCULATION
Phase
FRIGG-4 (662101)



bnl

3. RAMONA-3B OBJECTIVES FOR BWR STABILITY

3.1 IDENTIFY
CAUSES (MECHANISMS) OF,
CONDITION FOR

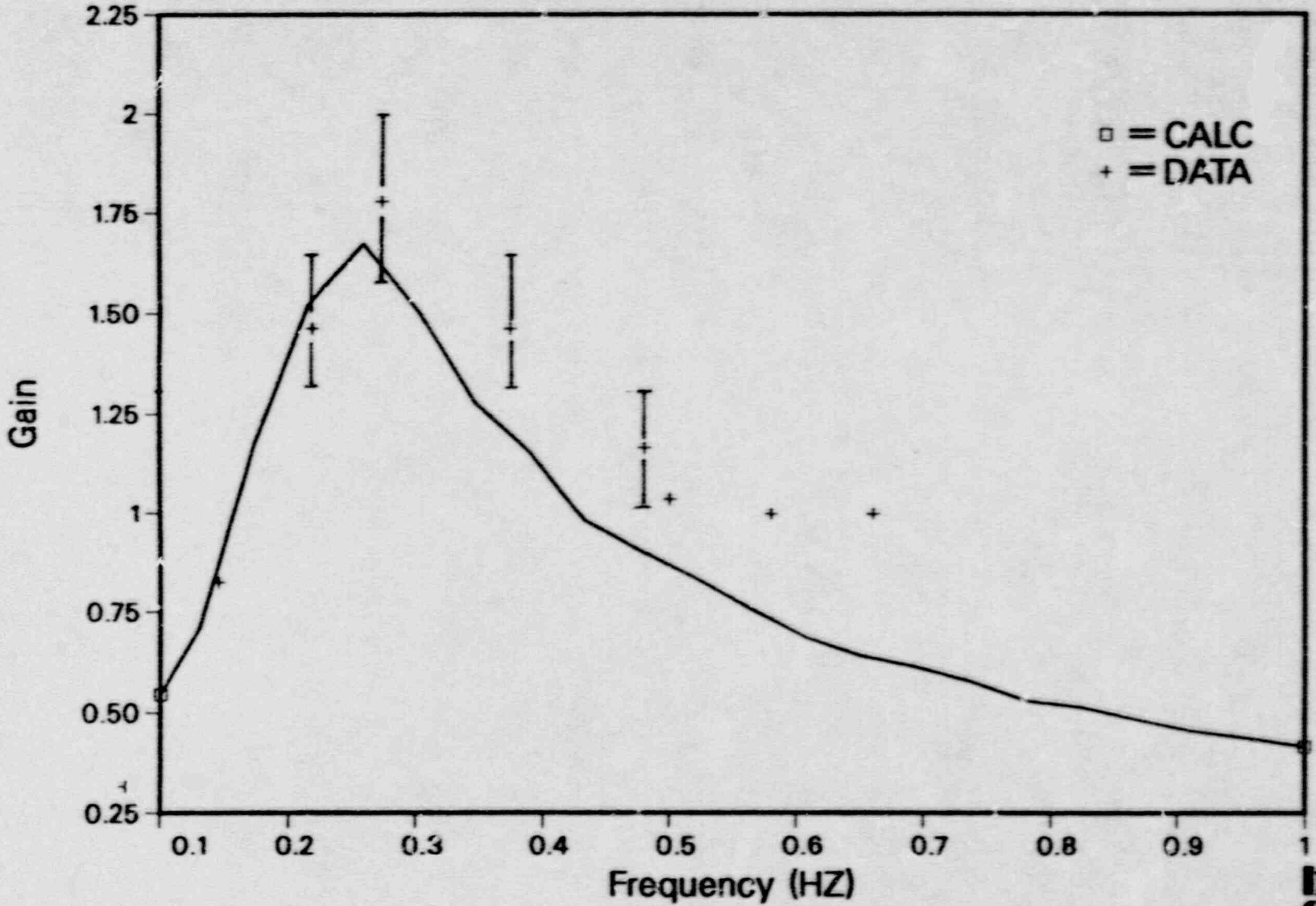
- OUT-OF PHASE, REGION-WISE POWER OSCILLATIONS.

3.2 DETERMINE INHERENT AMPLITUDE LIMITS, IF ANY, ON REGION-WISE OSCILLATIONS.

3.3 IDENTIFY CONTROL ROD PATTERNS THAT CAUSE REGION-WISE OSCILLATIONS.

3.4 PROVIDE AUDIT CAPABILITY AND ANALYSIS CAPABILITY TO NRC.

RAMONA CALCULATION
Gain (|Transfer Func|)
FRIGG-4 (662113)



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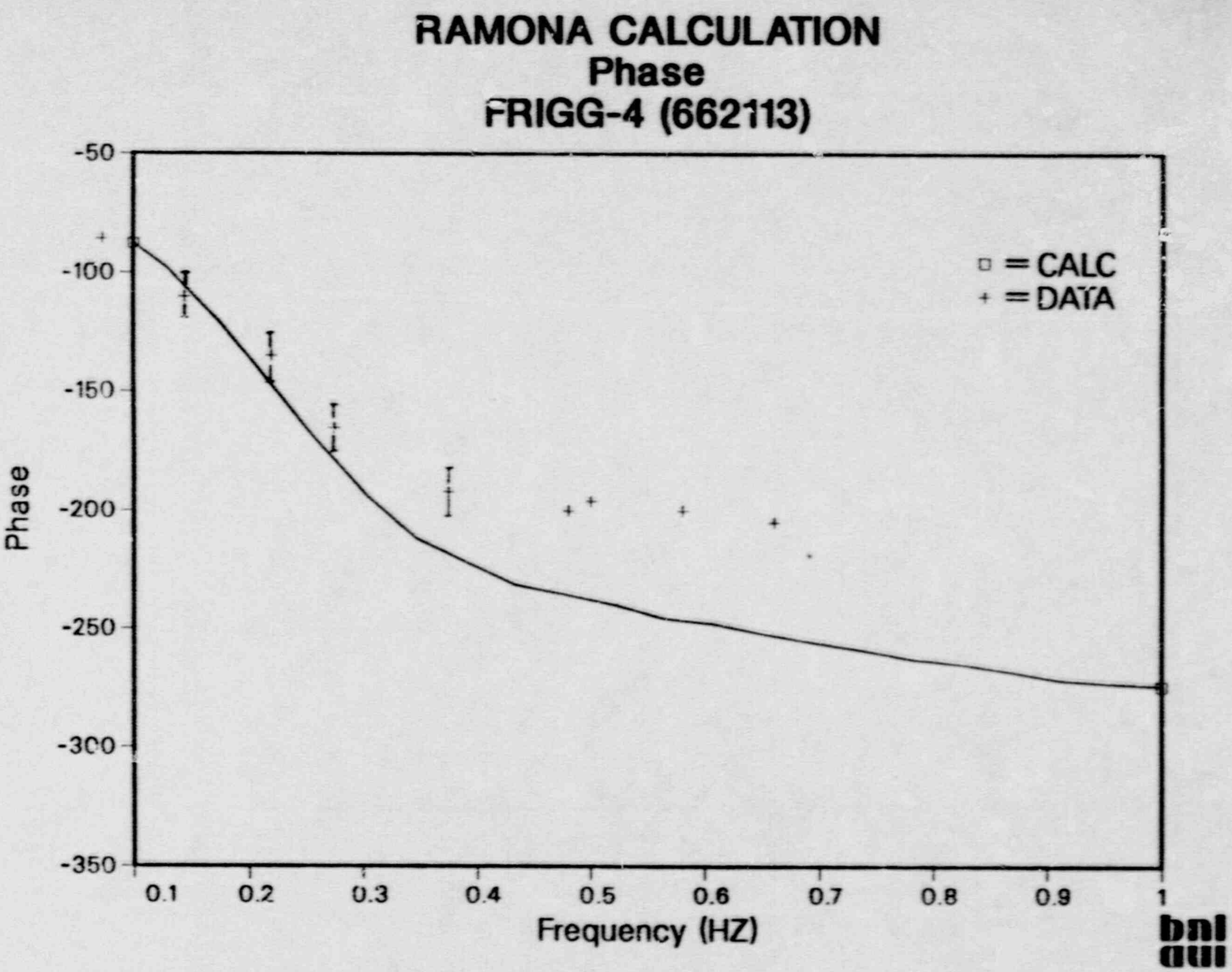
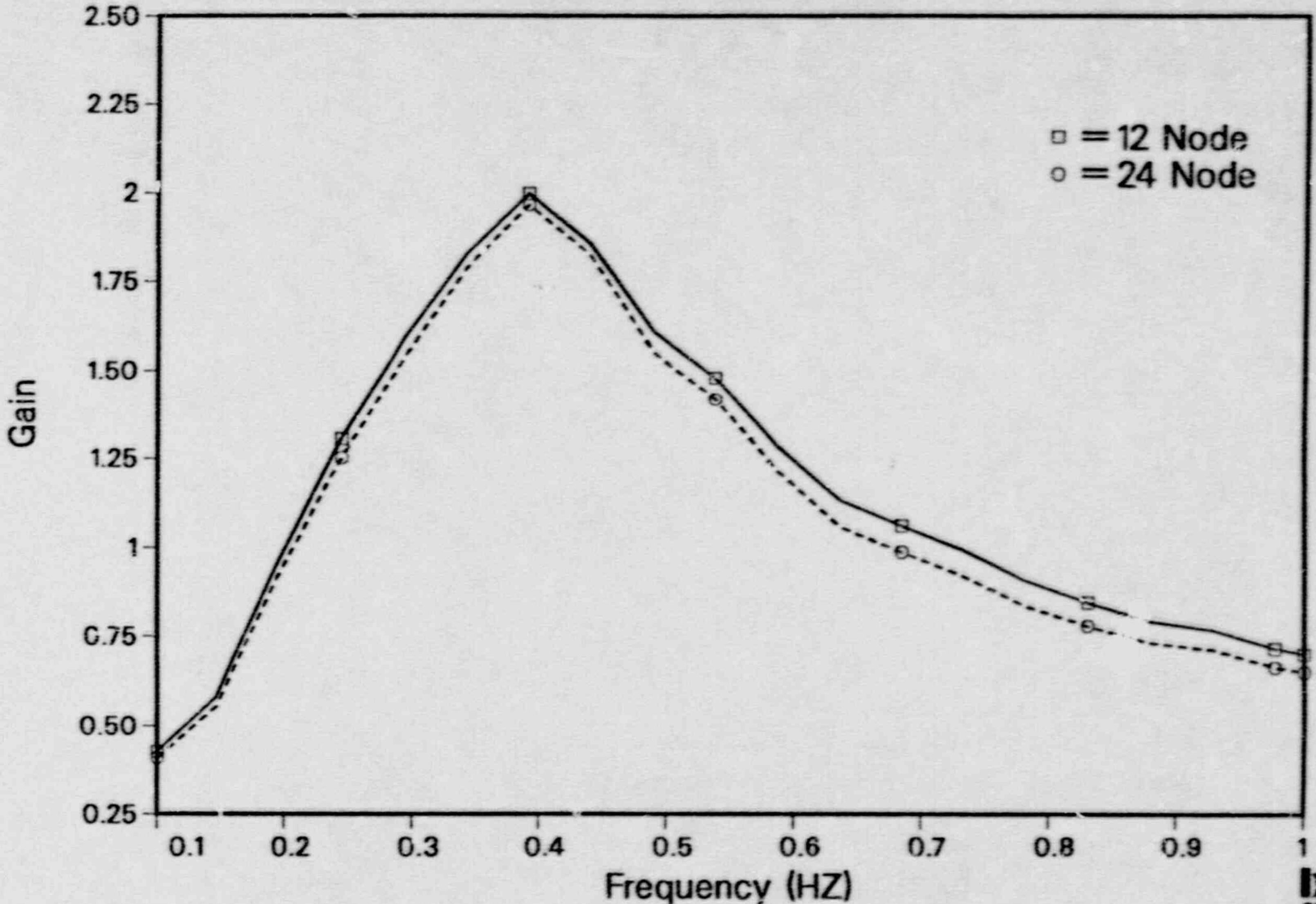
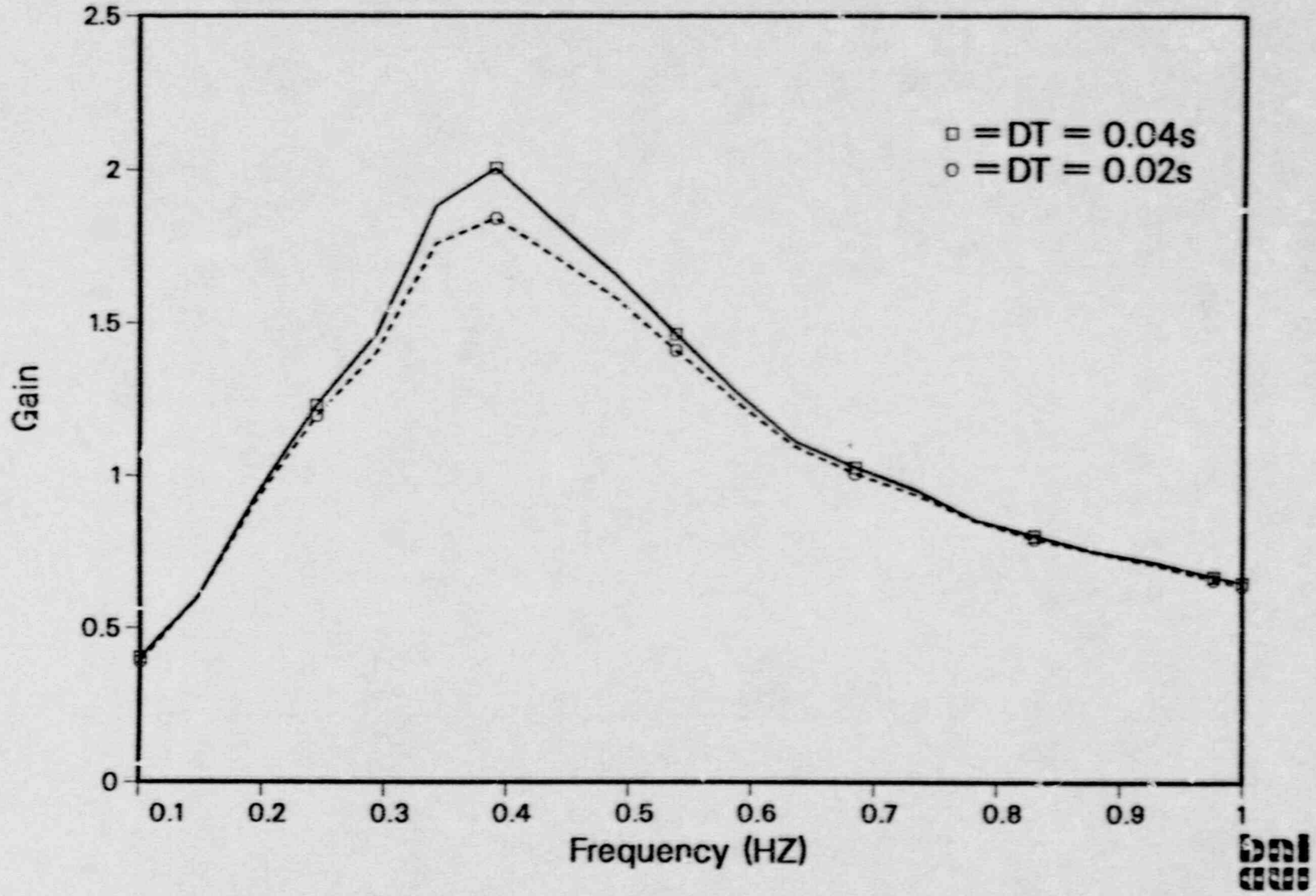


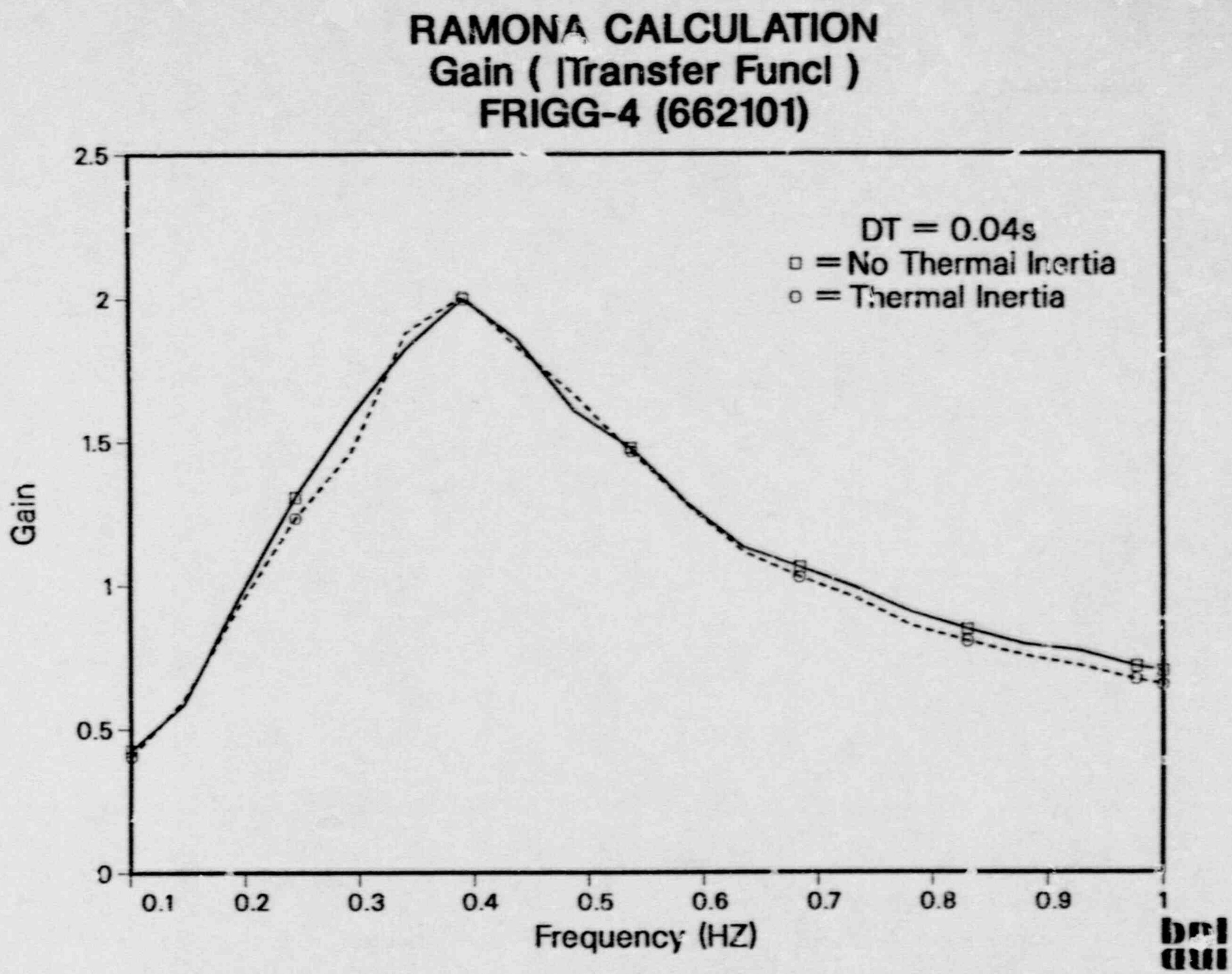
RAMONA CALCULATION
Phase
FRIGG-4 (662113)
-50
-100
-150
-200
-250
-300
-350
Phase
□ = CALC
+ = DATA
0.1
0.2
0.3
0.4
0.5
0.6
0.7
0.8
0.9
1
Frequency (HZ)

RAMONA CALCULATION
Gain (|Transfer Func|)
FRIGG-4 (662101)



RAMONA CALCULATION
Gain (|Transfer Func|)
FRIGG-4 (662101)




RAMONA CALCULATION
Gain (|Transfer Funcl)
FRIGG-4 (662101)
DT = 0.04s
□ = No Thermal Inertia
○ = Thermal Inertia
Gain
2.5
2
1.5
1
0.5
0
0.1
0.2
0.3
0.4
0.5
0.6
0.7
0.8
0.9
1
Frequency (HZ)

4. RAMONA RESULTS TO DATE

REGION-WISE OSCILLATIONS

- PRELIMINARY SCOPING CALCULATIONS SHOW, RAMONA-3B PRODUCES OUT-OF-PHASE OSCILLATIONS

- CAUSE OF OSCILLATIONS IS A SMALL NUMBER OF UNSTABLE FUEL CHANNELS WITH HIGH POWER AND LOW FLOW

- COMPUTED POWER AMPLITUDES:
  LOCAL: 300% OF RATED POWER, RISING.
  GROBAL: ~ZERO

- COMPUTED TEMPERATURE:
  FUEL: <200°C

- COMPUTED PERIOD 2.6-2.9 SECONDS

CALCULATIONS WERE PERFORMED WITH EXISTING BROWNS FERRY 3, CYCLE 5 DATA SET AND ATYPICAL BOUNDARY CONDITIONS.

5. RAMONA-3B LIMITATION

- COMPUTATIONAL RESOLUTION

  MAX. 200 SETS OF FUEL BUNDLES
  MAX. 24 AXIAL SEGMENTS

- COMPUTING TIME REQUIREMENTS

  FOR 200 NEUTRONIC CHANNELS
  35 HYDRAULIC CHANNELS

  ~120 TIMES SLOWER THAN REAL-TIME SPEED
  ~ 1 MIN. OF REAL TIME PER DAY

- DETAILED KINETICS DATA REQUIRED

- SUPER HEATED VAPOR NOT SIMULATED AT THIS TIME

- NO TRACKING OF BOILING BOUNDARY (IMPORTANT FOR EXPERIMENT SIMULATION, NOT FOR LASALLE)

- ONE-DIMENSIONAL FLOW MODELS FOR PLENA AND DOWNCOMER

6. FUTURE PLANS FOR RAMONA-3B ANALYSES

- PREPARATIONS FOR LASALLE SIMULATION
  - CROSS-SECTION PROCESSING,
  - PLANT PARAMETER IDENTIFICATION.
- DOCUMENTATION OF CODE MODIFICATION.

BWR STABILITY ANALYSIS

WITH

BNL ENGINEERING PLANT ANALYZER

W. WULFF

BROOKHAVEN NATIONAL LABORATORY

PRESENTED BEFORE

ACRS SUBCOMMITTEE ON THERMAL HYDRAULICS

NOVEMBER 8-9, 1989

SAN FRANCISCO, CA

BROOKHAVEN NATIONAL LABORATORY

ASSOCIATED UNIVERSITIES, INC.

BNL ENGINEERING PLANT ANALYZER (EPA) ANALYSES OF BWR STABILITY

1. EPA DESCRIPTION
2. EPA ASSESSMENT
3. EPA OBJECTIVES FOR BWR STABILITY ANALYSES
4. EPA RESULTS FROM BWR STABILITY ANALYSES
5. EPA LIMITATIONS
6. FUTURE PLANS ON EPA ACTIVITIES

1. EPA DESCRIPTION

1.1 REFERENCE: NUREG/CR-3943 (1984) (CODE STATUS AS OF JUNE 1984)

1.2 MAJOR EPA CHARACTERISTICS

- SIMULATION FACILITY

  SPECIAL-PURPOSE MINICOMPUTER
  (AD10)
  SYSTEMS SOFTWARE PROVIDING
  SIMULATION ENVIRONMENT,
  SIMULATION LANGUAGE,
  6 MODELING PRINCIPLES:
  - MODEL SELECTION,
  - RELEVANCE OF PROCESSES,
  - ANALYTICAL INTEGRATION WHERE POSSIBLE,
  - ELIMINATION OF ITERATION,
  - USE OF PRETABULATED FUNCTIONS,
  - SELECTION OF ALGORITHM.

  OPTIMIZATION OF
  MACHINE ARCHITECTURE +
  MODELING +
  NUMERICAL METHODS
  AS A WHOLE.

- EPA: ADI SIMULATION SYSTEM
  HIPA CODE.

- HIPA SYSTEMS CODE WITH

  - POINT KINETICS,
    1 - GROUP OF PROMPT NEUTRONS,
    6 DELAYED NEUTRON GROUPS,
    DECAY HEAT AS ANS STANDARD 5.1,
    SEVEN REACTIVITY FEEDBACK MECHANISMS.
  - INTEGRAL METHODS FOR CONDUCTION IN FUEL
  - THERMOHYDRAULICS
    NONHOMOGENOUS FLOW/PHASE SEPARATION (DRIFT FLUX, ISHII 1977),
    NONEQUILIBRIUM FLOW (SCANDPOWER Γ_v)
    3 PARALLEL CHANNELS IN CORE,
    ONE-DIM. FLOW IN REST OF SYSTEM.
    FOUR-EQUATION DF MODEL WITH:
    MIXTURE LOOP MOMENTUM BALANCES,
    MIXTURE VOLUMETRIC FLUX DIVERGENCE EQUATION,
    MIXTURE ENERGY BALANCE, AND VAPOR MASS BALANCE.
    VAPOR AT SATURATION CONDITIONS
    LIQUID SUBCOOLED, SATURATED OR SUPERHEATED.

EPA CHARACTERISTICS (CONT.)

- SIMULATION SCOPE

NSSS:	RPV, RCP, STEAM LINE (ACOUSTIC EFFECTS), 67 CONTROL VOLUMES.
BOP:	TURBINE, GENERATOR, FW TURBINE, FW PUMP, FW PREHEATERS, CONDENSER, RCP MOTOR/GENERATOR SET.
CONTROLS:	PRESSURE REGULATOR, FW CONTROL, RECIRC. FLOW CONTROL. } GE TRANSFER FUNCTIONS
SAFETY SYSTEMS:	NINE AUTOMATIC SCRAM TRIPS, PUMP TRIPS, TURBINE GENERATOR TRIPS, SAFETY AND RELIEF VALVES, ECCS, BORON INJECTION AND TRANSPORT.
CONTAINMENT:	DRY WELL, WETWELL AND SUPPRESSION POOL (N_2/H_2O ATMOSPHERE, CONDENSATION ETC.).
FAILURES:	PUMPS, HEATERS, VALVES, SCRAM, CONTROL SYSTEMS AND TRIPS.

ON-LINE, INTERACTIVELY IMPOSED.

EPA CHARACTERISTICS (CONT.)

- SOLUTION METHODS IN EPA

  - IMPLICIT INTEGRATION FOR
    PROMPT NEUTRON ODE

  - EXPLICIT INTEGRATION FOR
    ALL OTHER ODEs: $\dot{P}$, $\dot{M}_i$, $\dot{m}_{vj}$, $\dot{E}_{MJ}$, $\dot{\omega}$, ETC.
    MIX OF FIRST-ORDER EULER AND
    THIRD-ORDER ADAMS-BASHFORD;
    BUILT-IN, STANDARD TEXT BOOK METHODS

  - QUADRATURES IN SPACE FOR
    - MIXTURE MASS BALANCE (FLUX DIVERGENCE EQUATION,
    - LOOP MOMENTUM BALANCE:

    TRAPEZOIDAL RULE
    (FOR GIVEN MEAN VALUES)
    SIMPSON RULE
    (FOR GIVEN DISCRETE VAL.)

SOLUTION METHODS IN EPA (CONT.)

- NO LINEARIZATION OF EQUATIONS
- COMPUTATIONAL ERRORS FROM NUMERICAL DIFFUSION (2EQS.), QUADRATURE, ODE INTEGRATION, COVARIANCE TERMS.
- DIFFUSION, CONTROLLED BY EXPLICIT INTEGRATION WITH MAX. COURANT NO. < 1.0.
- COMPUTATIONAL MODELS AND METHODS AND COMPUTAITONAL ERRORS FOR THERMOHYDRAULICS ARE THE SAME IN RAMONA-3B AND EPA.

1.3 EPA MODIFICATIONS FOR BWR INSTABILITY ANALYSES

- INTEGRATORS FOR AVG. POWER
  AVG. TOT. REACTIVITY.

- MULTISTEPPING FOR KINETICS CALCULATIONS
  (PROMPT AND DELAYED)
  INTERPOLATING TOT. REACTIVITY

- MULTISTEPPING FOR CONDUCTION IN FUEL
  INTERPOLATING VOID REACTIVITY
  (DOPPLER FEEDBACK IN EVERY SUBSTEP)

- RESULT FROM CHANGES
  - oo DECREASE IN PEAK POWER (-25%)
  - oo INCREASE OF MEAN POWER (+53%)

  AFTER SCRAM FAILURE/INVENTORY MAINTAINED

2. EPA ASSESSMENT

2.1 DEVELOPMENTAL ASSESSMENT (NUREG/CR-3943)

- COMPARISONS WITH FRIGG TESTS (G. P. COMPUTER)
- CHAPTER 15 TRANSIENTS AND ATWS : GE NEDO-2422
- COMPARISON WITH TRAC, RELAP5, RAMONA: MSIV CLOSURE ATWS

2.2 LASALLE RELATED TRANSIENTS:

- BROWNS FERRY-1 RCP TRIP TEST (H. S. CHENG MEMO, AUG. 18, 1987)
- LASALLE TRANSIENT UP TO SCRAM TRIP:
  - STARTREC DATA VS. CALCULATION
  - BE PLANT DATA FROM GE

BROWNS FERRY TURBINE TRIP

- COMPARISON OF EPA RESULTS WITH TEST DATA

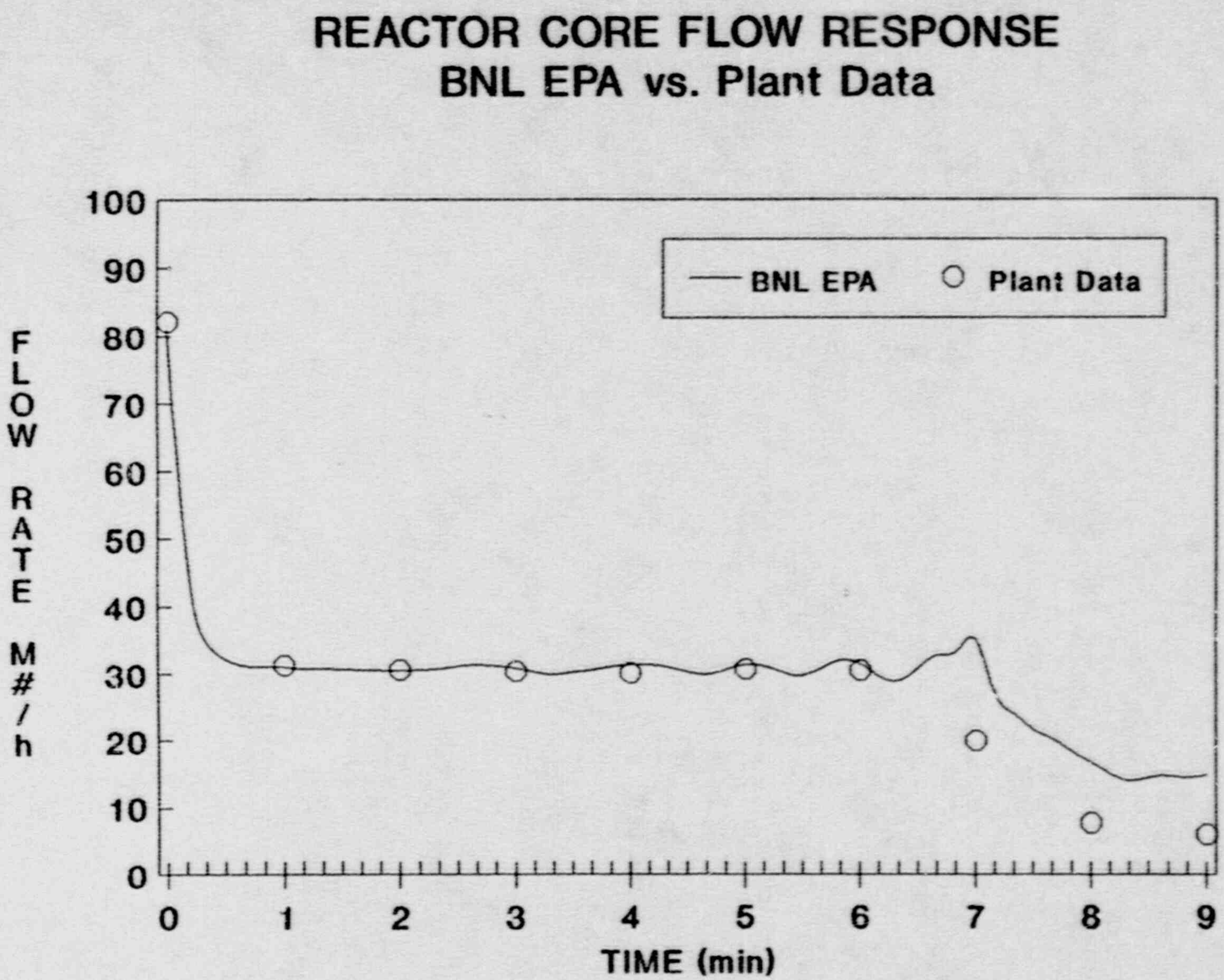
PUMP SPEED	WITHIN PLOTTING ACCURACY
CORE FLOW	+ 4% OF INITIAL VALUES
POWER	± 4%
STEAM FLOW	± 6%
PRESSURE	+ 1%
COLLAPSED LIQ. LEVEL	± 10%

LASALLE-2 TRANSIENT

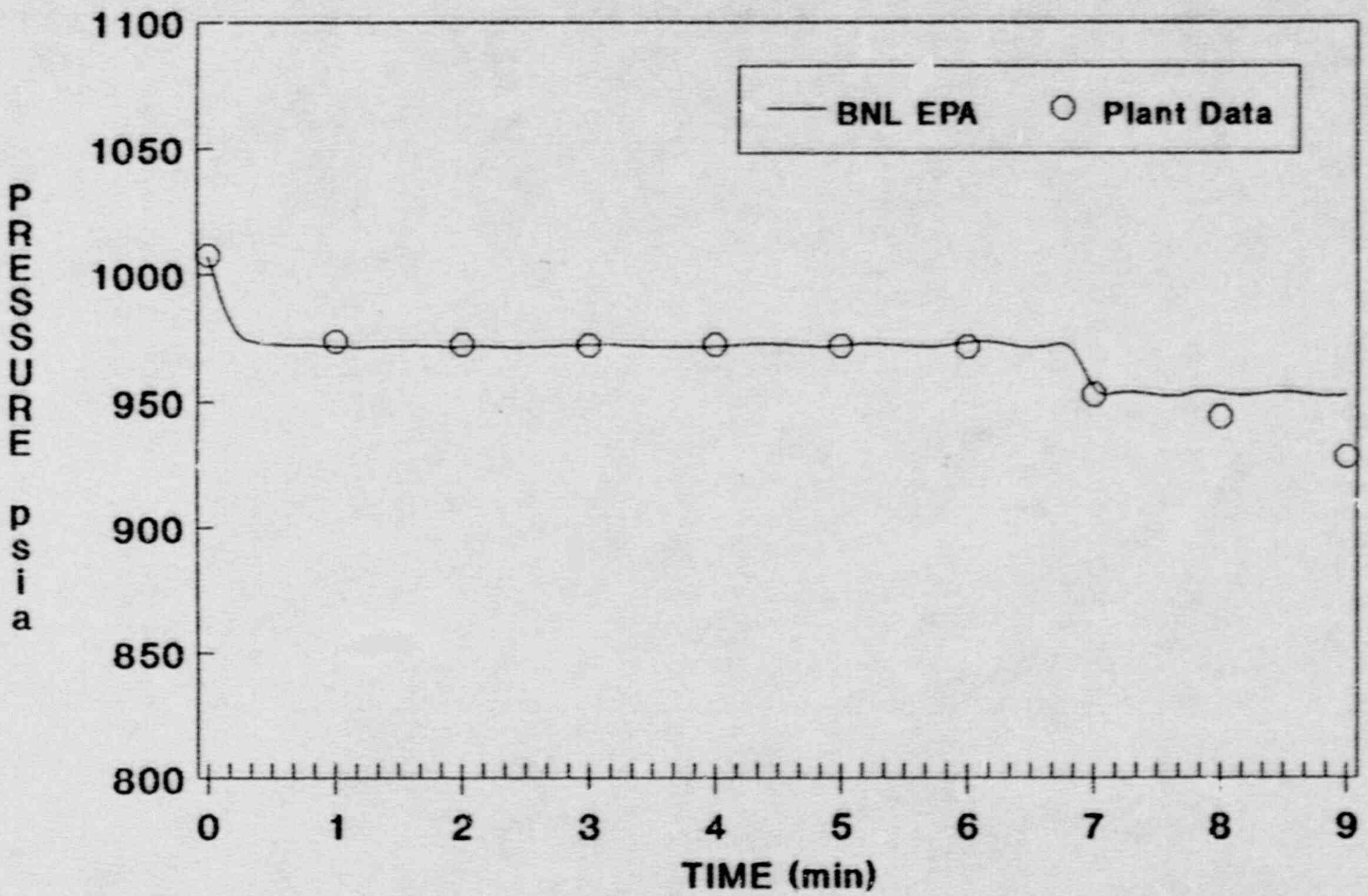
	RESULT
• BE CALCUL. + W_{FW} IMPOSED FROM STAR TREC	NO. OSCIL.
• BE CALCUL. + FW CONTROLLER INTACT (BF CONTR. PARAM.)	OSCIL., SCRAM
• UNCERTAINTIES FOR RHOV, LEXT, LENT, + W_{FW} IMPOSED FROM STAR TREC	OSCIL., SCRAM

---

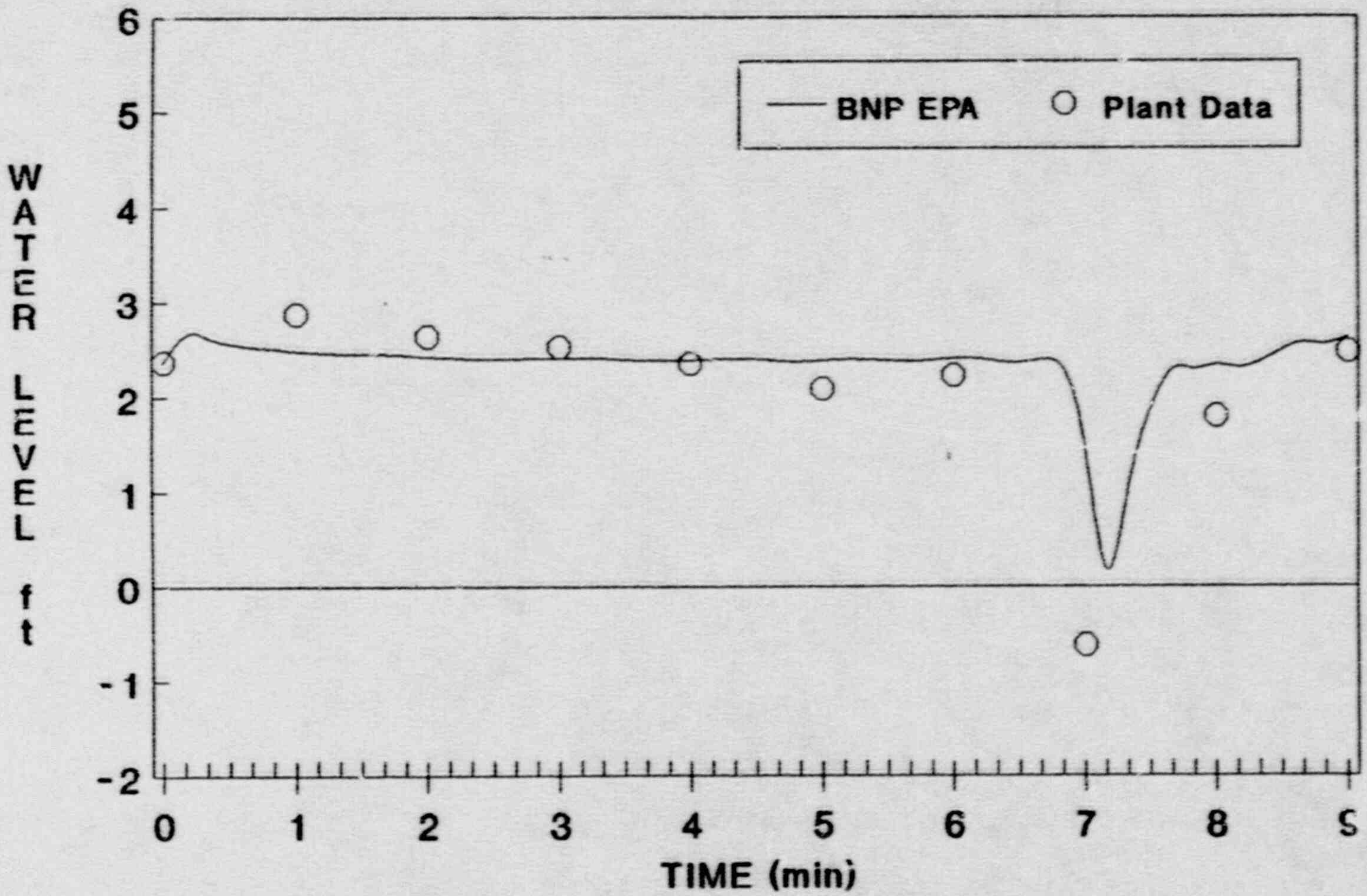
*LASALLE FW REGULATOR VALVE FAILED DURING LASALLE EVENT


REACTOR CORE FLOW RESPONSE
BNL EPA vs. Plant Data
BNL EPA
Plant Data
FLOW RATE M#/h
TIME (min)
100
90
80
70
60
50
40
30
20
10
0
0
1
2
3
4
5
6
7
8
9

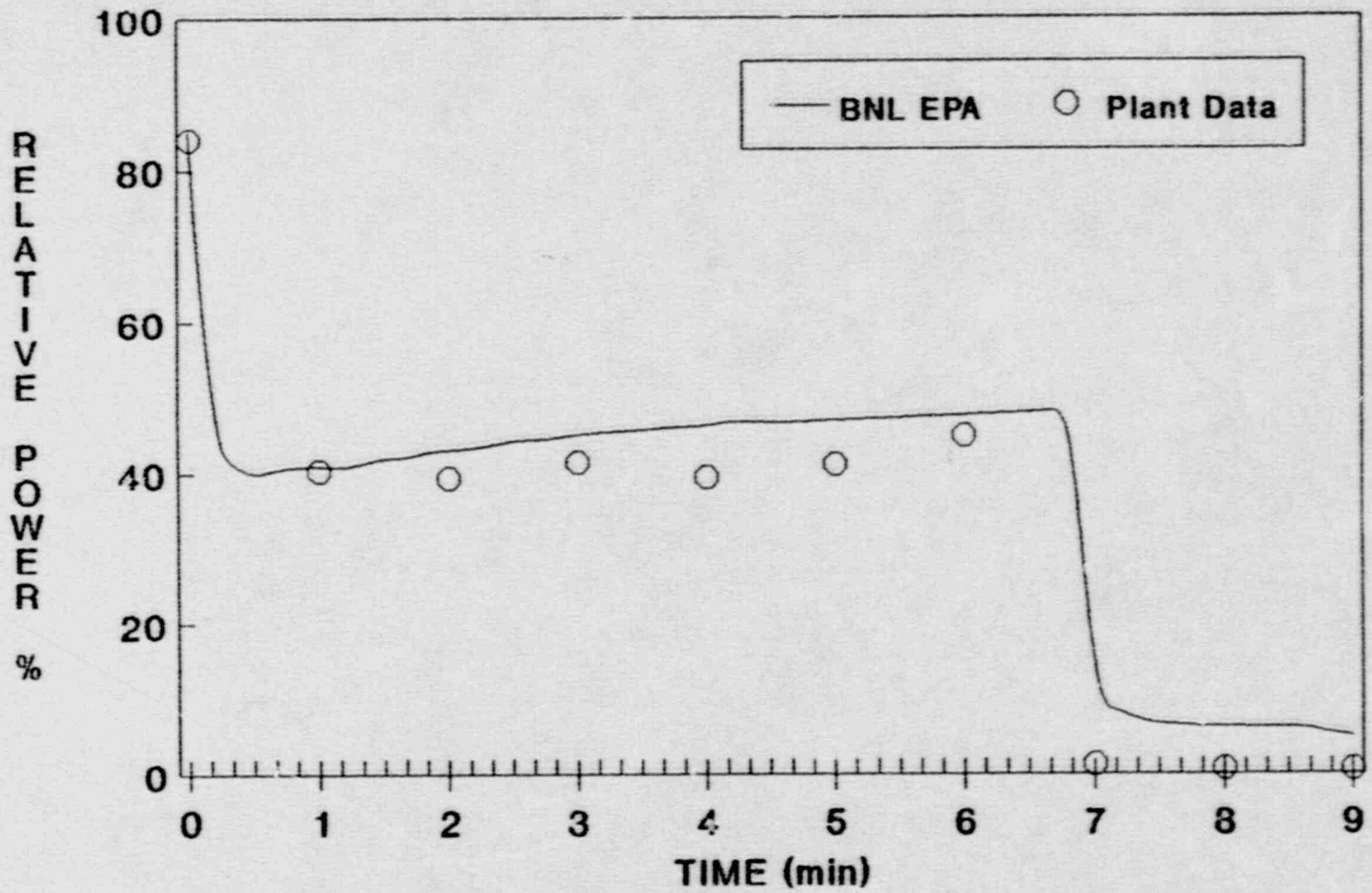
SYSTEM PRESSURE RESPONSE
BNL EPA vs. Plant Data

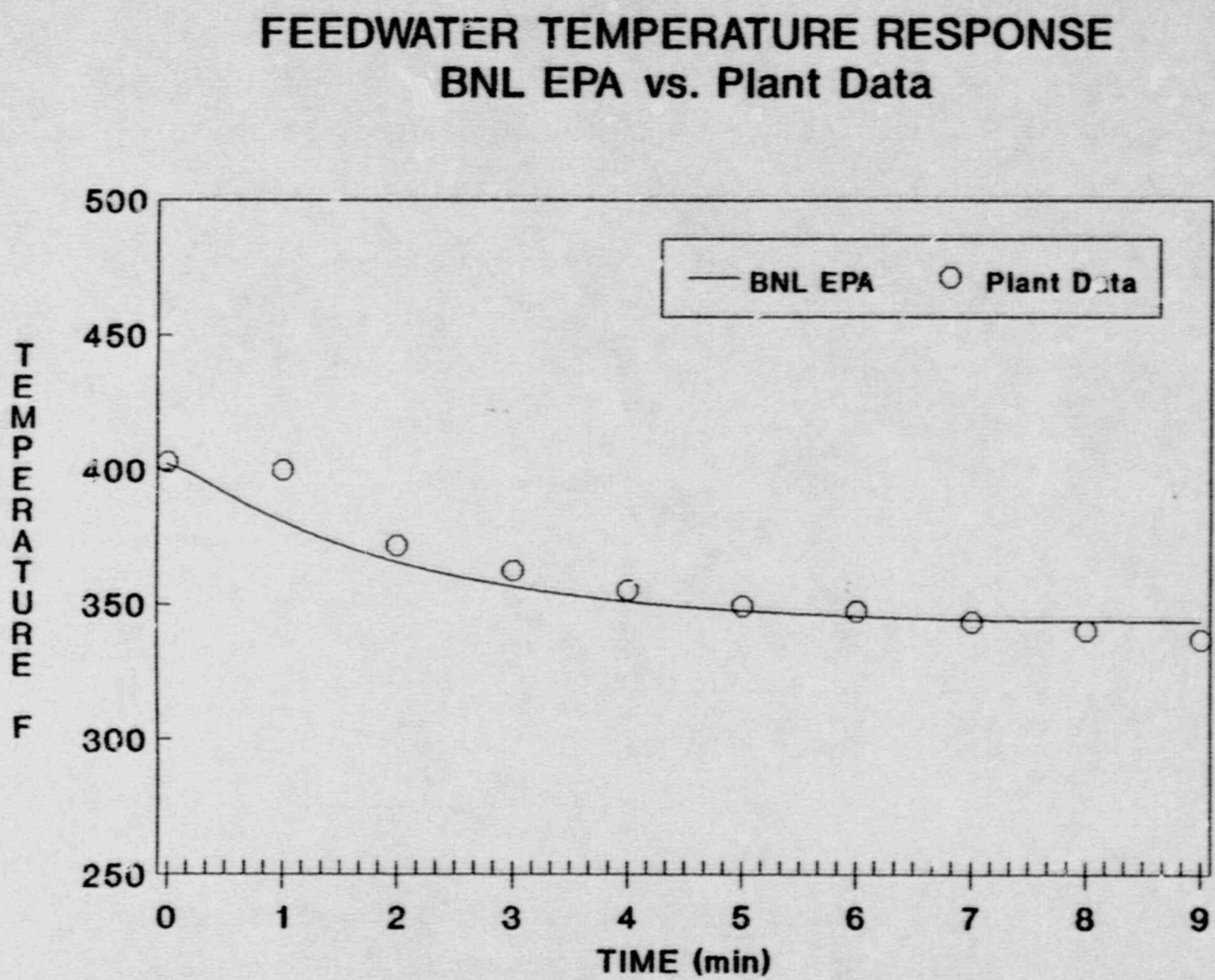


REACTOR WATER LEVEL RESPONSE
BNL EPA vs. Plant Data

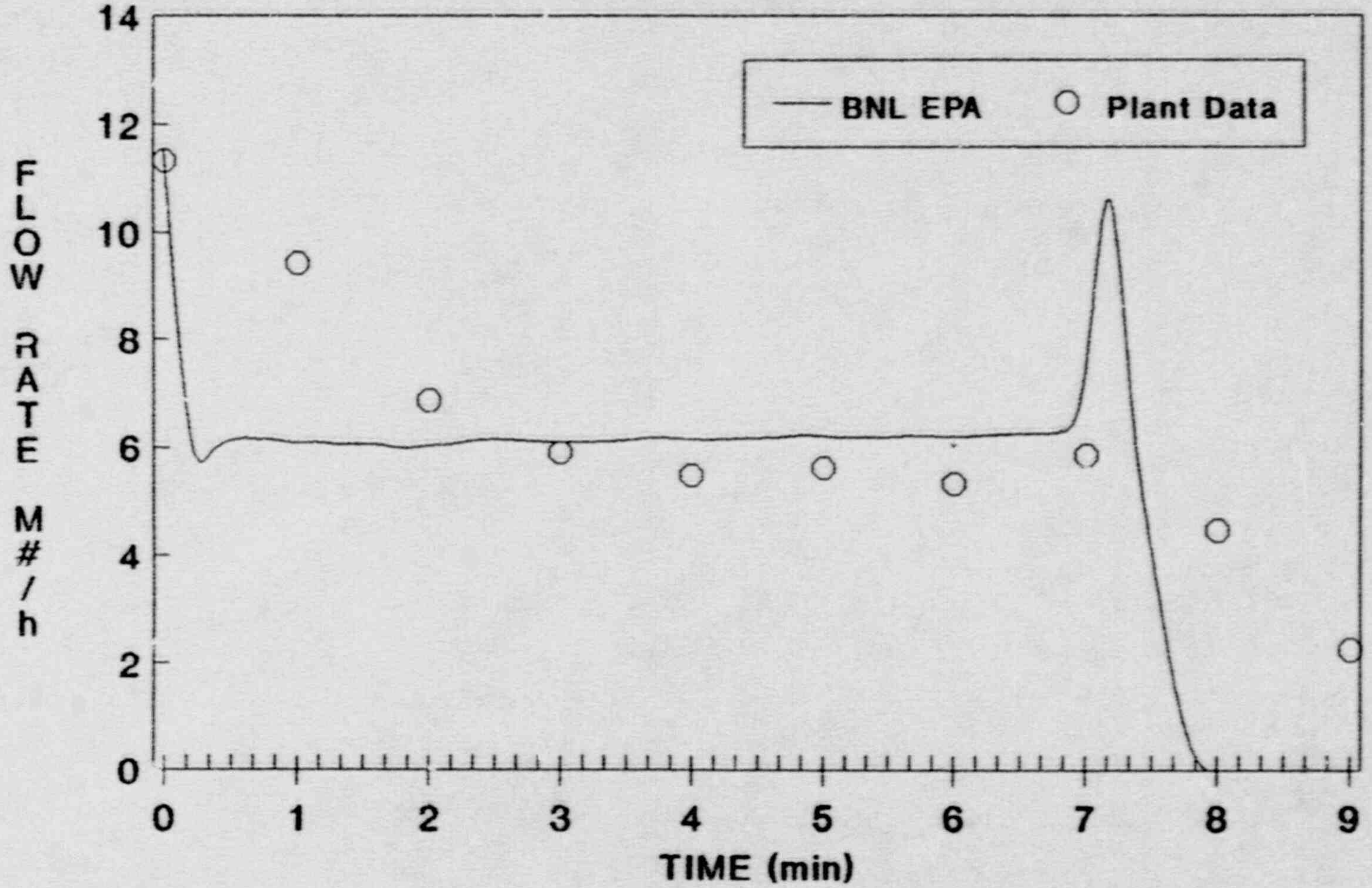


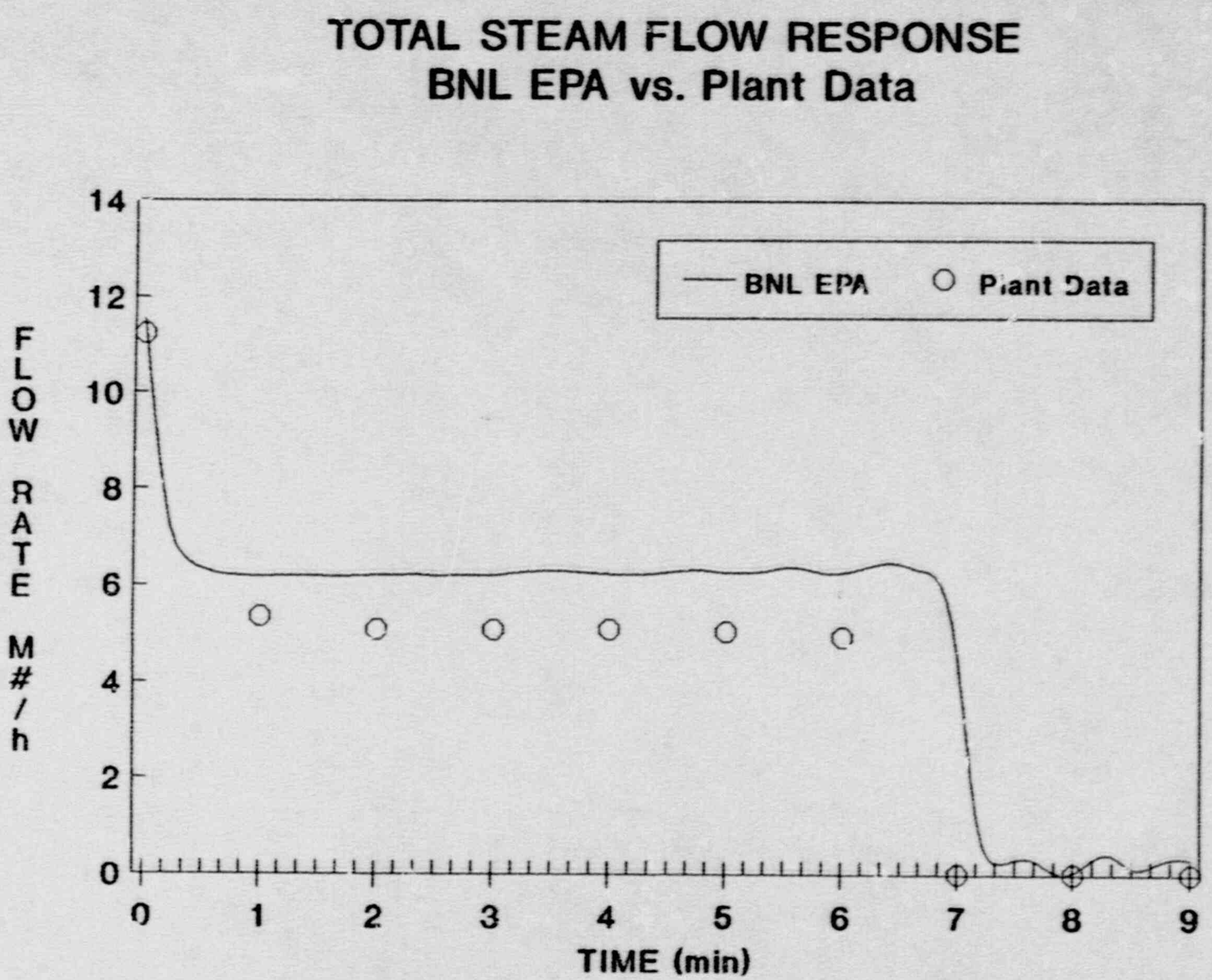
AVERAGE POWER RESPONSE
BNL EPA vs. Plant Data



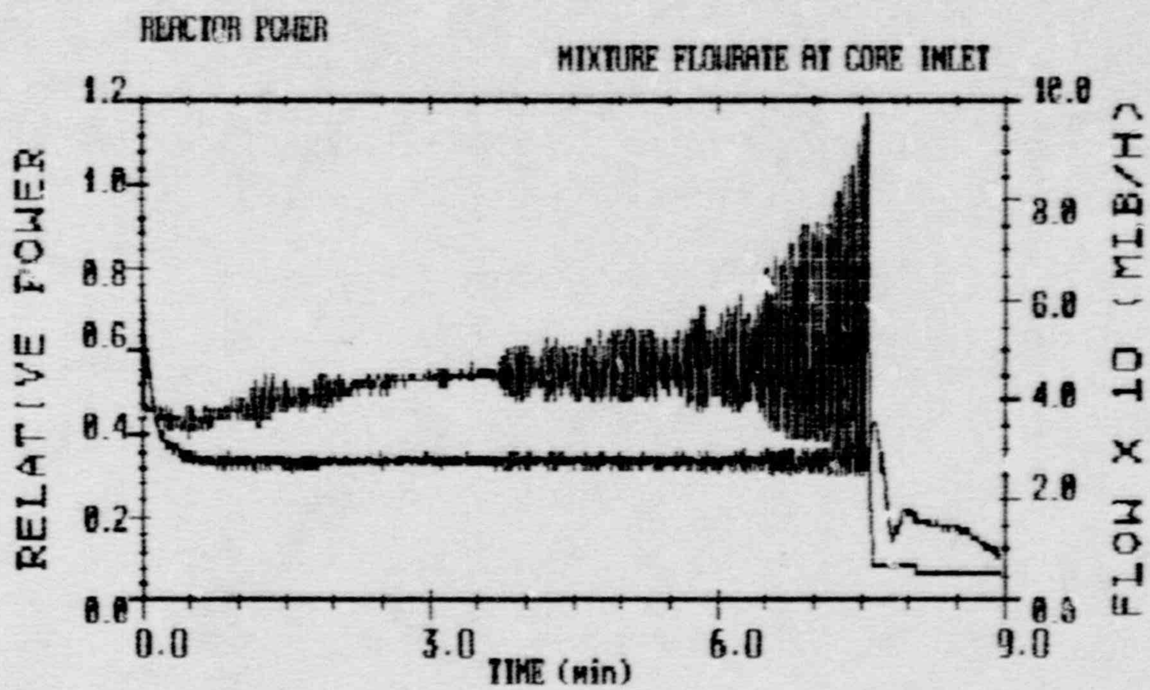

FEEDWATER TEMPERATURE RESPONSE
BNL EPA vs. Plant Data
BNL EPA
Plant Data
TEMPERATURE F
500
450
400
350
300
250
0
1
2
3
4
5
6
7
8
9
TIME (min)

FEEDWATER FLOW RESPONSE
BNL EPA vs. Plant Data





TOTAL STEAM FLOW RESPONSE
BNL EPA vs. Plant Data
BNL EPA
Plant Data
FLOW RATE M#/h
TIME (min)
0
2
4
6
8
10
12
14
0
1
2
3
4
5
6
7
8
9



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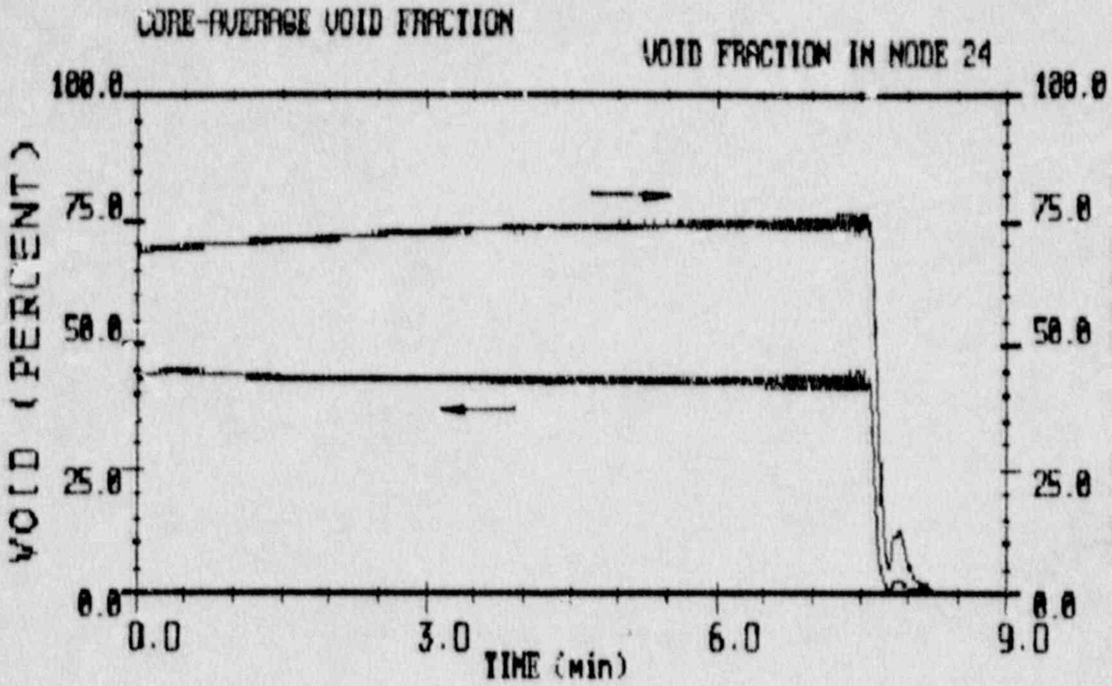

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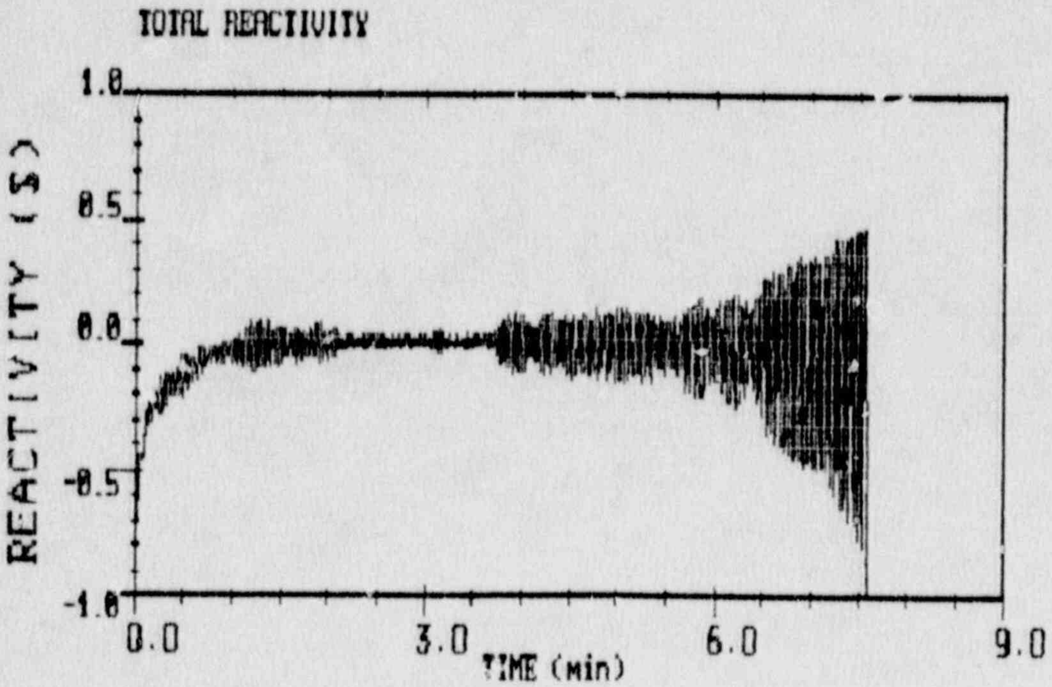

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3. EPA OBJECTIVES FOR BWR STABILITY

3.1 ANSWERS TO THESE SEVEN QUESTIONS:

(1) WHAT ARE THE CAUSES OF LARGE AMPLITUDE OSCILLATIONS AND UNDER WHAT CONDITIONS CAN THEY OCCUR IN A BWR?

(2) WHAT ARE THE INHERENT LIMITS, IF ANY, ON THE AMPLITUDE OF POWER AND FUEL TEMPERATURE OSCILLATIONS IN THE CASE OF SCRAM FAILURE?

(3) CAN CORE-WIDE POWER AND FLOW OSCILLATIONS OCCUR DURING ANY TYPE OF ANTICIPATED TRANSIENT WITHOUT SCRAM (ATWS)?

(4) WHAT ARE THE AMPLITUDES OF FUEL PELLET AND CLADDING TEMPERATURE OSCILLATIONS ASSOCIATED WITH LIMIT-CYCLE POWER OSCILLATIONS?

(5) CAN THE SAFETY LIMIT OF MINIMUM CRITICAL POWER RATIO (MCPR = 1.05) BE VIOLATED DURING LIMIT-CYCLE OSCILLATIONS?

(6) HOW DO THE TIME RATES OF SUPPRESSION POOL TEMPERATURE AND OF CONTAINMENT ATMOSPHERE TEMPERATURE RISE DEPEND ON THE AMPLITUDE OF LIMIT-CYCLE POWER OSCILLATIONS?

3 EPA OBJECTIVES FOR BWR STABILITY (CONT.)

(7) CAN SUPPRESSION POOL TEMPERATURE AND PRESSURE EXCEED ALLOWED LIMITS?

3.2 RANK MODELING PARAMETERS ACCORDING TO THEIR IMPORTANCE TO STABILITY.

3.3 DETERMINE CONSEQUENCES FROM POSTULATED OPERATOR ACTIONS.

3.4 PROVIDE AUDIT AND ANALYSIS CAPABILITIES TO NRC.

- NONLINEAR EFFECTS ON INSTABILITY!
- SYSTEMS EFFECTS ON INSTABILITY!

4. EPA RESULTS TO DATE

CORE-WIDE OSCILLATIONS

(i) LASALLE-2 EXPERIENCED
THERMOHYDRAULIC INSTABILITY,
ENHANCED BY VOID REACTIVITY FEEDBACK.

(ii) LASALLE-2 EXPERIENCED
LIMIT-CYCLE FLOW AND POWER
OSCILLATIONS, TERMINATED BY SCRAM.
WITHOUT SCRAM, POWER PEAKS MUCH HIGHER
THAN RATED POWER COULD HAVE BEEN
REACHED.

(iii) THREE CAUSES FOR INSTABILITY, ALL NEEDED:
- FLOW REDUCTION (TWO RCP TRIPPED)
- REACTIVITY INSERTION (COLD FEEDWATER)
- EXTREME RADIAL AND AXIAL POWER PEAKING.

(iv) SLOW RESTORATION OF RECIRCULATION FLOW
RESTABILIZES POWER AND CORE FLOW.

4. EPA RESULTS TO DATE (CONT.)

(v) TRANSITION FROM LINEAR TO NONLINEAR POWER OSCILLATIONS IS ACCOMPANIED BY PERIOD-DOUBLING BIFURCATION AND AMPLITUDE GROWTH.

(vi) MEAN POWER INCREASES WITH INCREASE IN AMPLITUDE OF POWER OSCILLATIONS.

INCREASE DEPENDS ON REACTIVITY (I.E. FEEDWATER MASS FLOW RATE AND TEMPERATURE).

(vii) EPA SIMULATIONS
(MORE THAN 60 TRANSIENTS, 15 MINUTES LONG)

- PRIMARY MODELING PARAMETERS:
  VOID REACTIVITY COEFFICIENTS,
  POWER PEAKING FACTORS, POWER SHAPE,
  SUBCOOLING,
  EXIT FLOW RESISTANCE,
  ENTRANCE FLOW RESISTANCE,
  FUEL THERMAL RESPONSE.

4. <u>EPA RESULTS TO DATE</u> (CONT.)

- RESULTS PRESENTED ON
  MCPR
  FUEL TEMPERATURE
  POWER VS FLOW MAP
    - 100%, 80% CONTROL ROD LINES
    - NATURAL CIRCULATION
    - STABILITY BOUNDARY (± 20%)

  ATWS TRANSIENTS AFTER ESTABLISHED OSCILLATIONS:
    - TURBINE TRIP, WITH/WITHOUT FW PUMP TRIP
    - TURBINE TRIP, WITH/WITHOUT BYPASS
    - MSIV CLOSURE
    - RESTART OF RCP
    - RESTART OF RCP AND MSIV CLOSURE.

GRAPHS FROM EPA

NEW CALCULATIONS

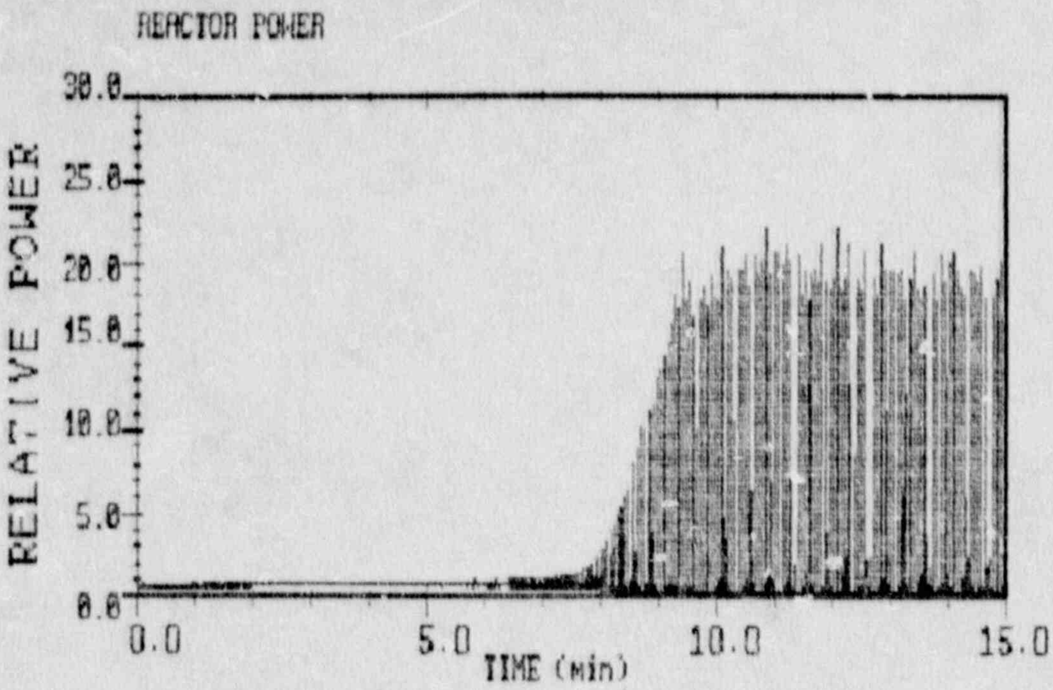
(i) WITH FW CONTROLLER IN NORMAL OPERATION

(ii) WITH MANUAL FW FLOW CONTROL

(iii) WITH STARTREC FW FLOW

ZOOM OF POWER AND BIFURCATION

POWER VS FLOW MAP



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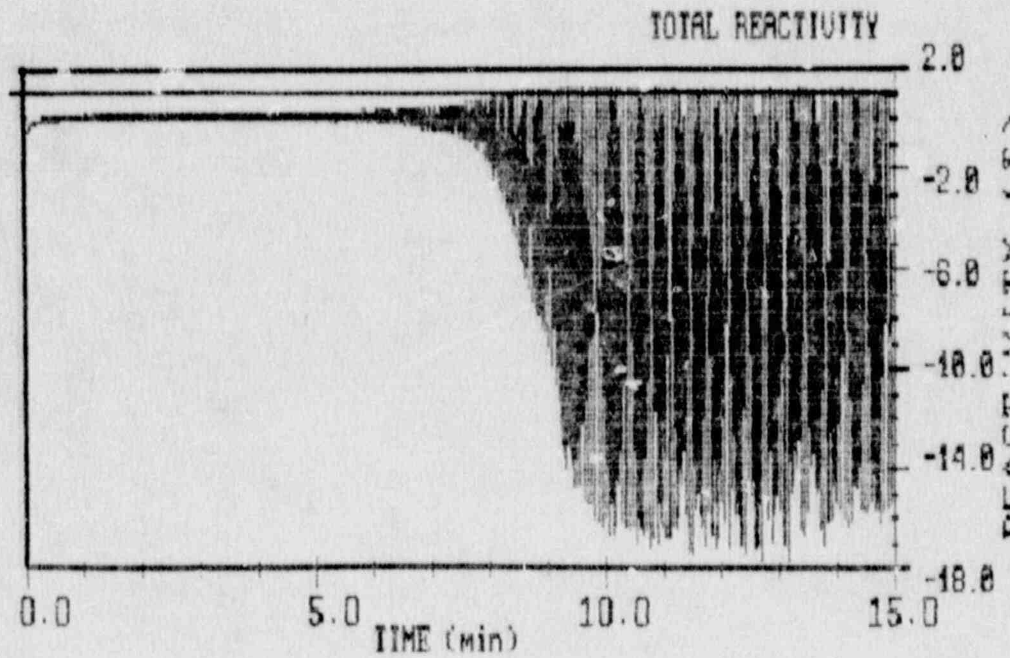

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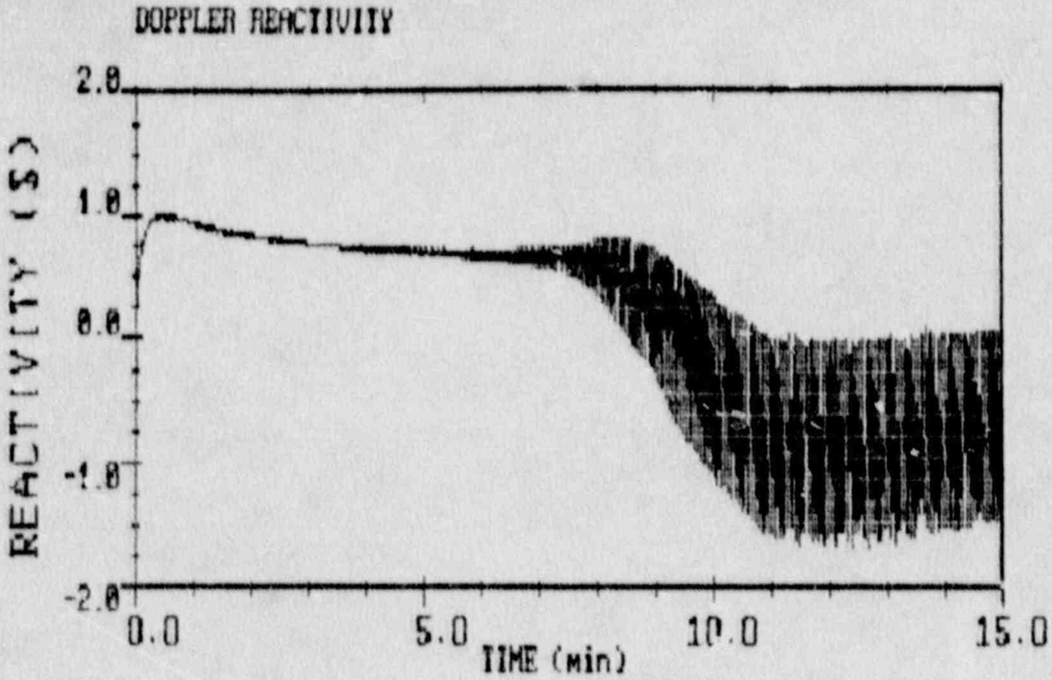

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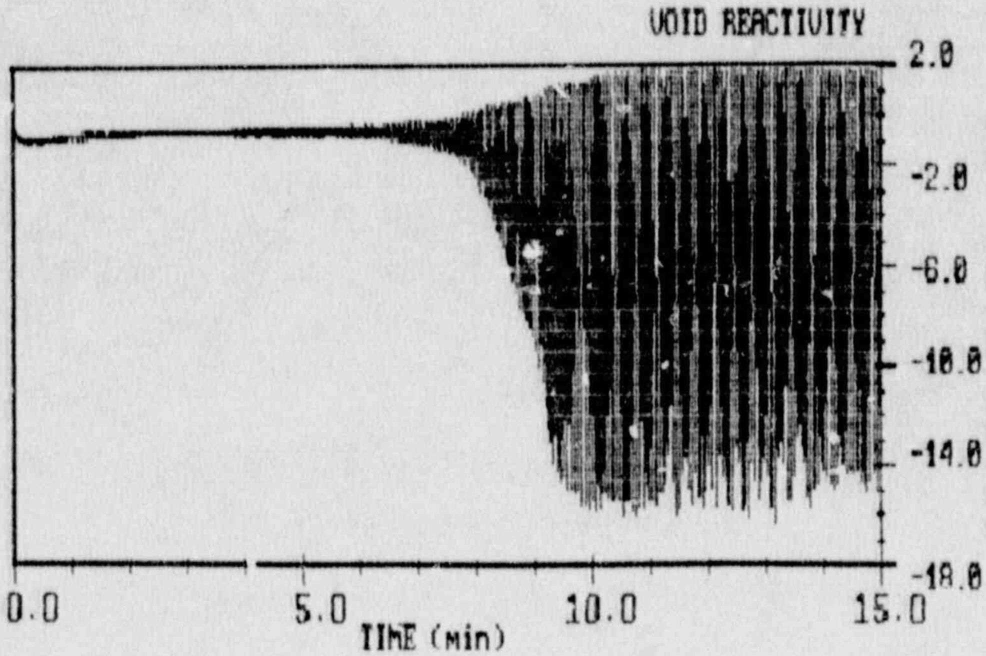
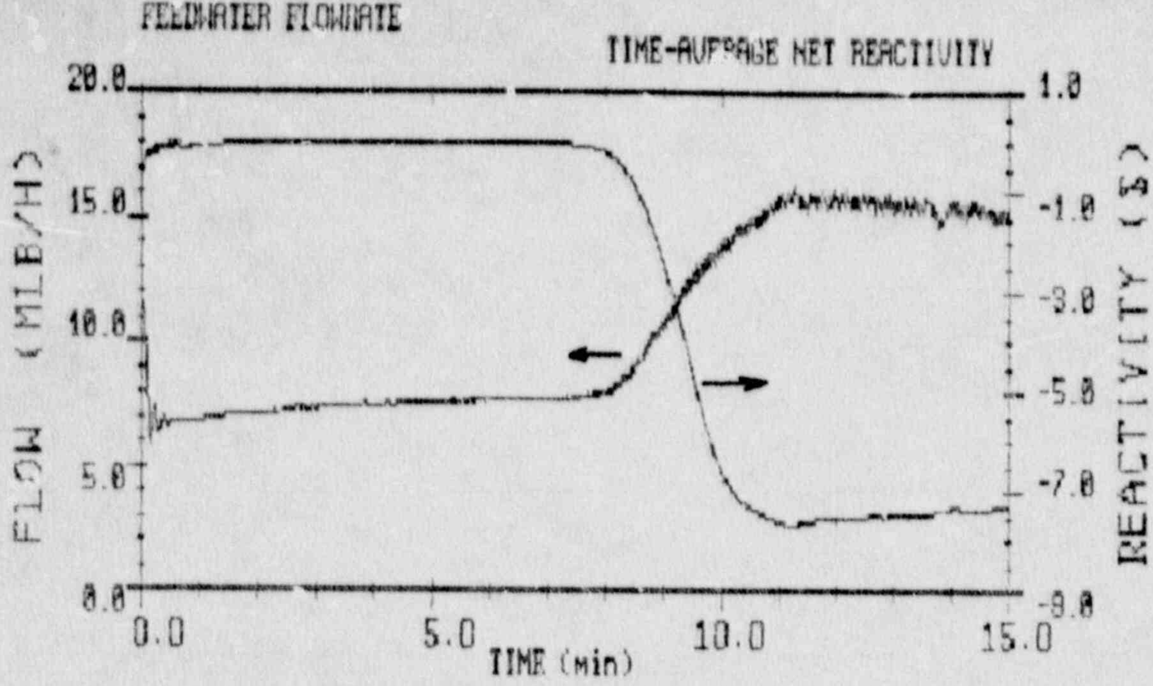


SLV3H03 Tc=8 @RES Sp=5 MuPW=7 BNL Plant Analyzer 27-OCT-89 12:44



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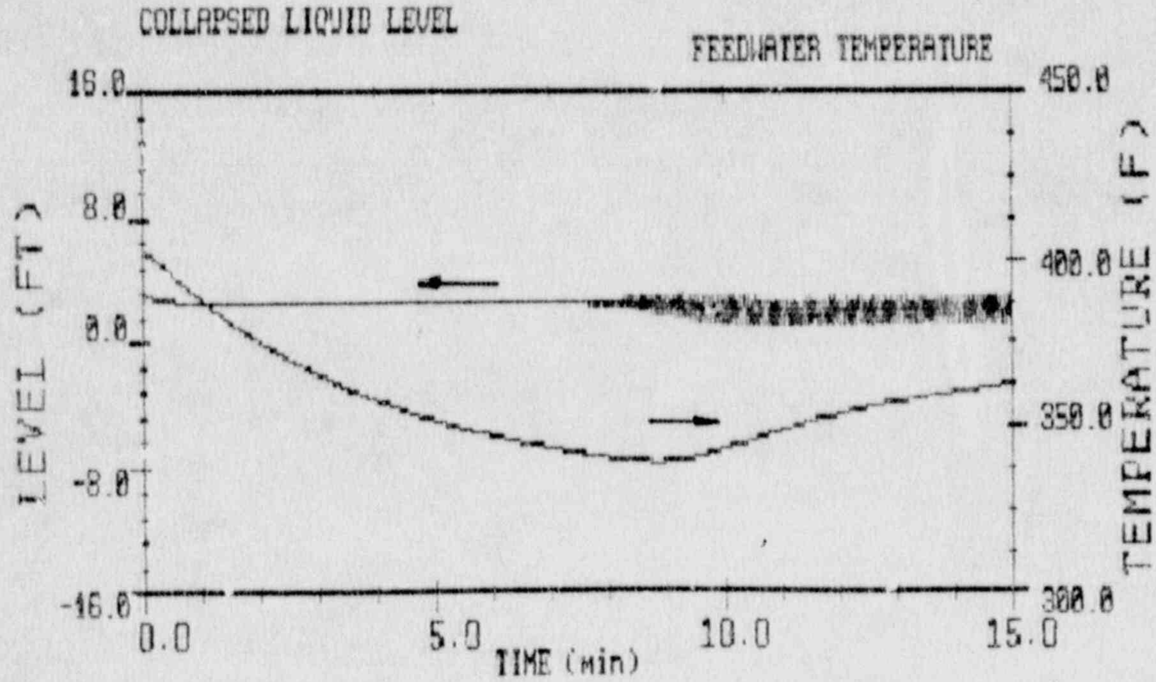
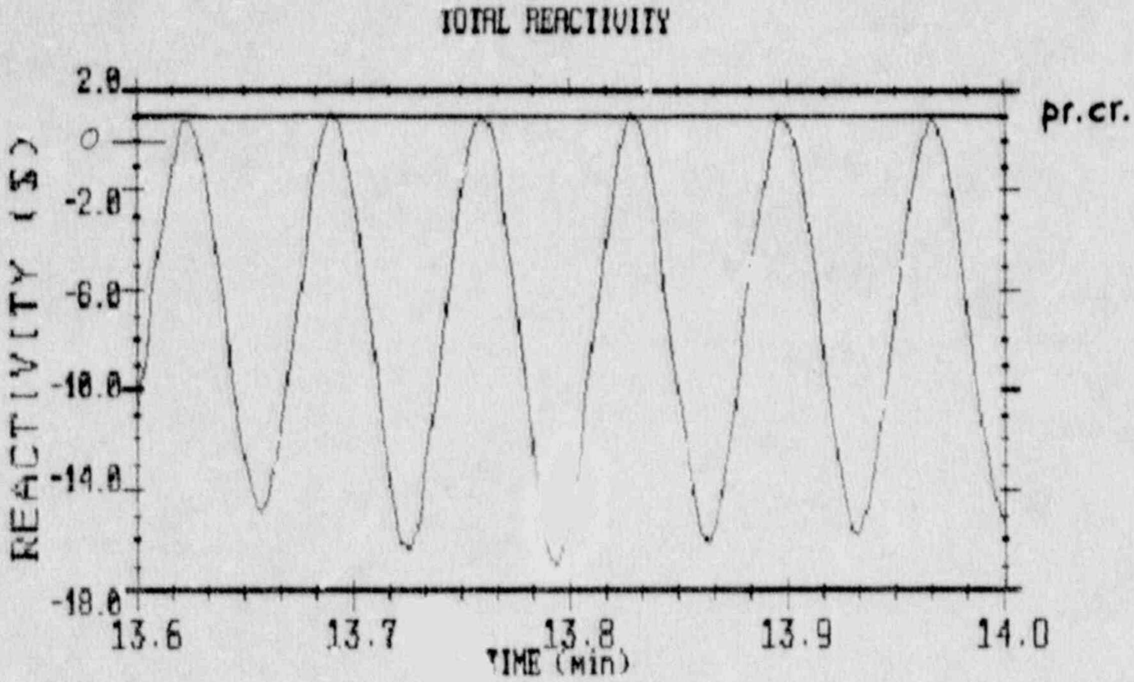


SLV3H8A Ic=8 ØRES Sp=5 MuPW=7 BNL Plant Analyzer 27-OCT-89 12:44



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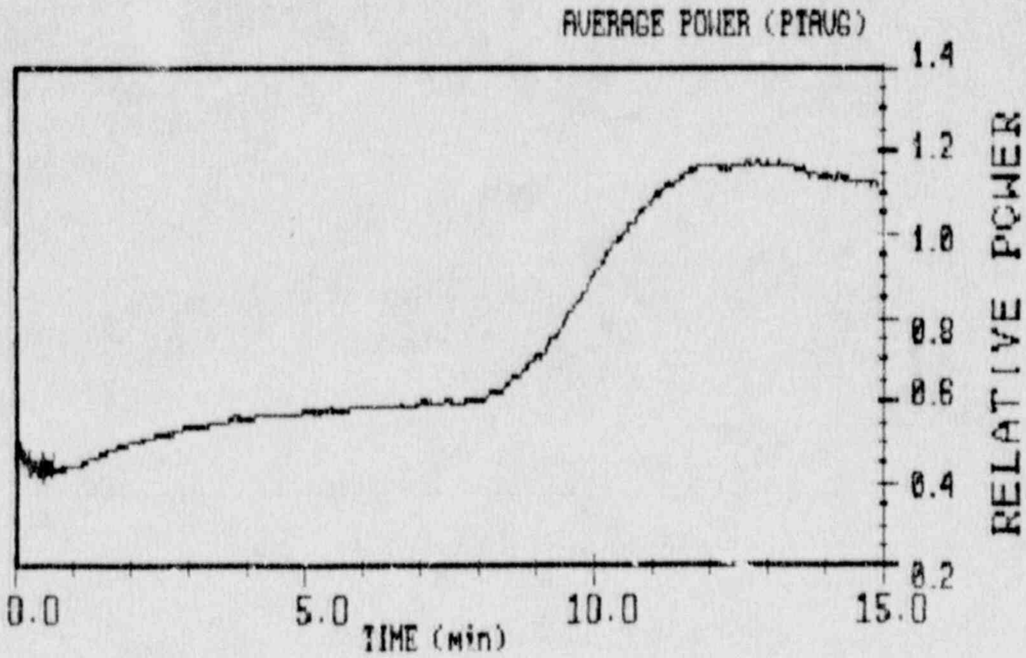

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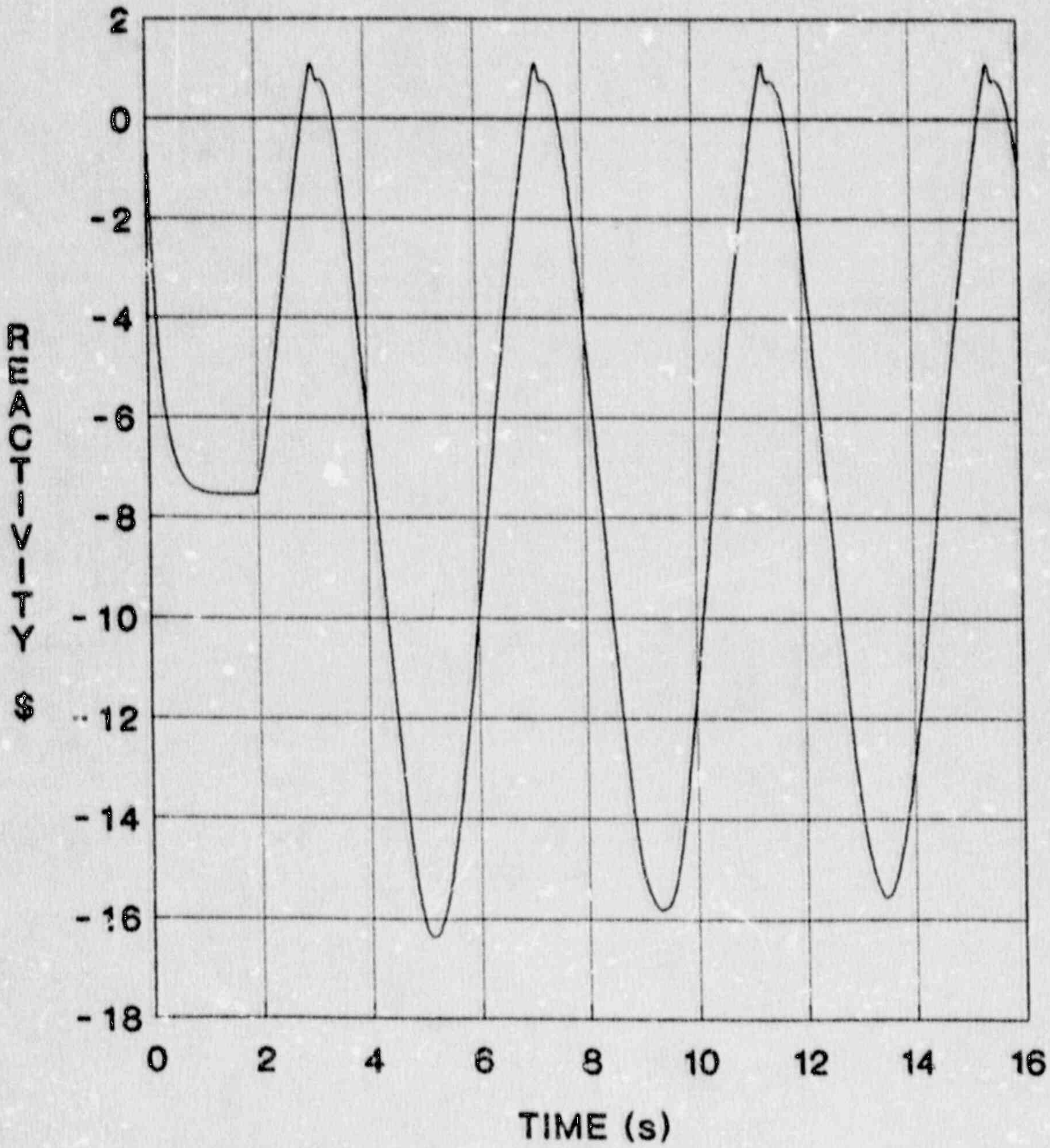
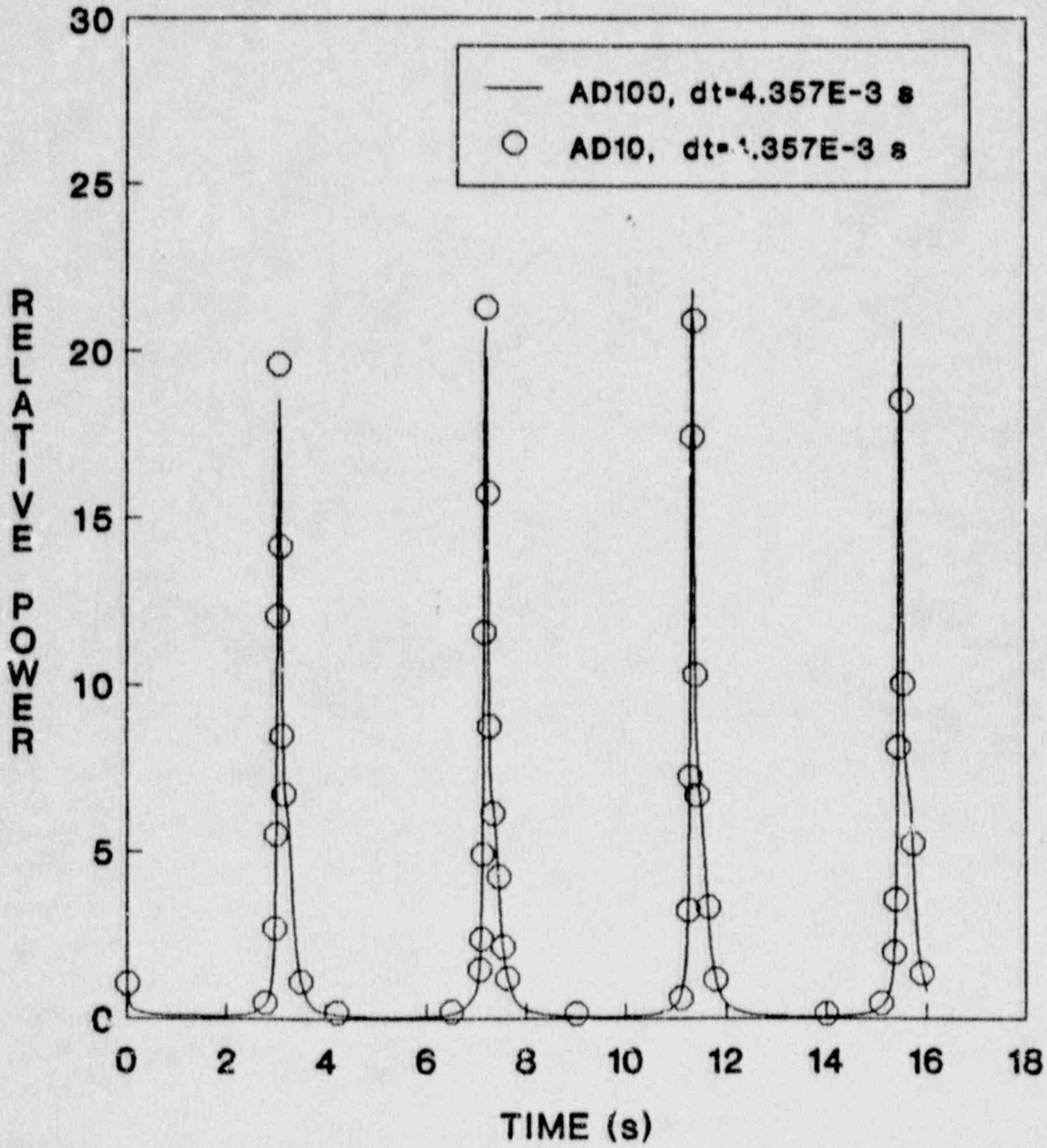
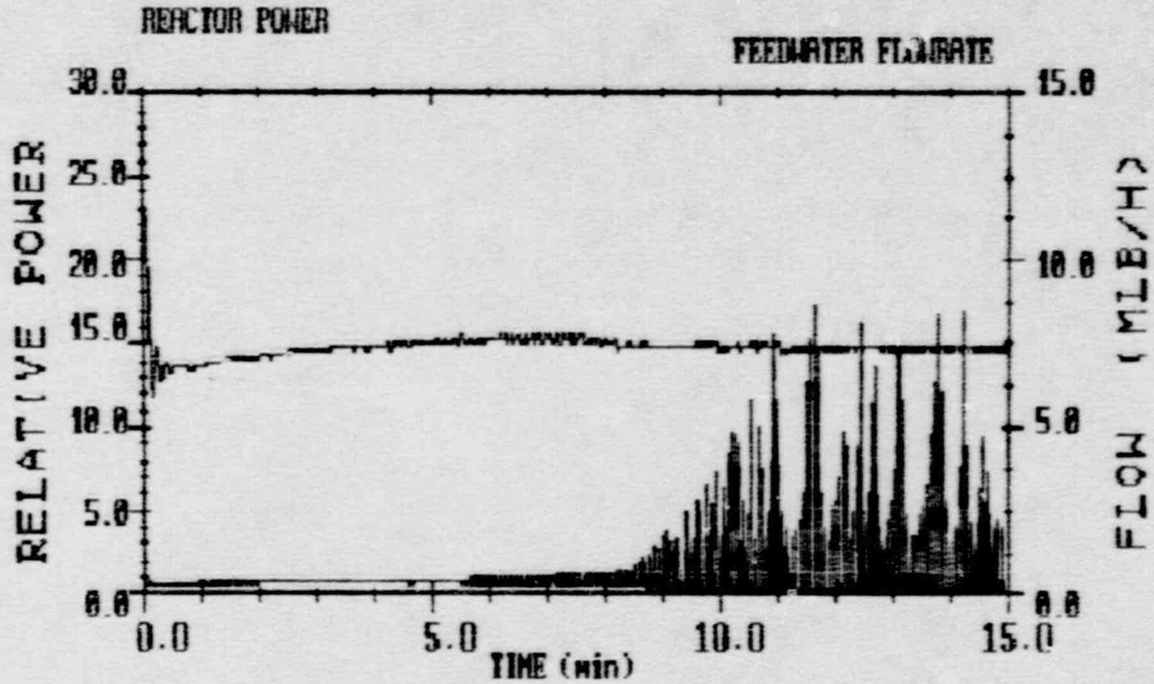
LaSalle-2 Total Reactivity Behavior
With Scram Failure and Auto FW Control
AD10 Calculated Results for EPA



LaSalle-2 Power Oscillations
With Scram Failure & Auto FW Control
Comparison of AD-10 and AD-100 Results





SLV3H72 Ic=8 ØRES Sp=5 MuPW=7 BNL Plant Analyzer 26-OCT-89 16:58

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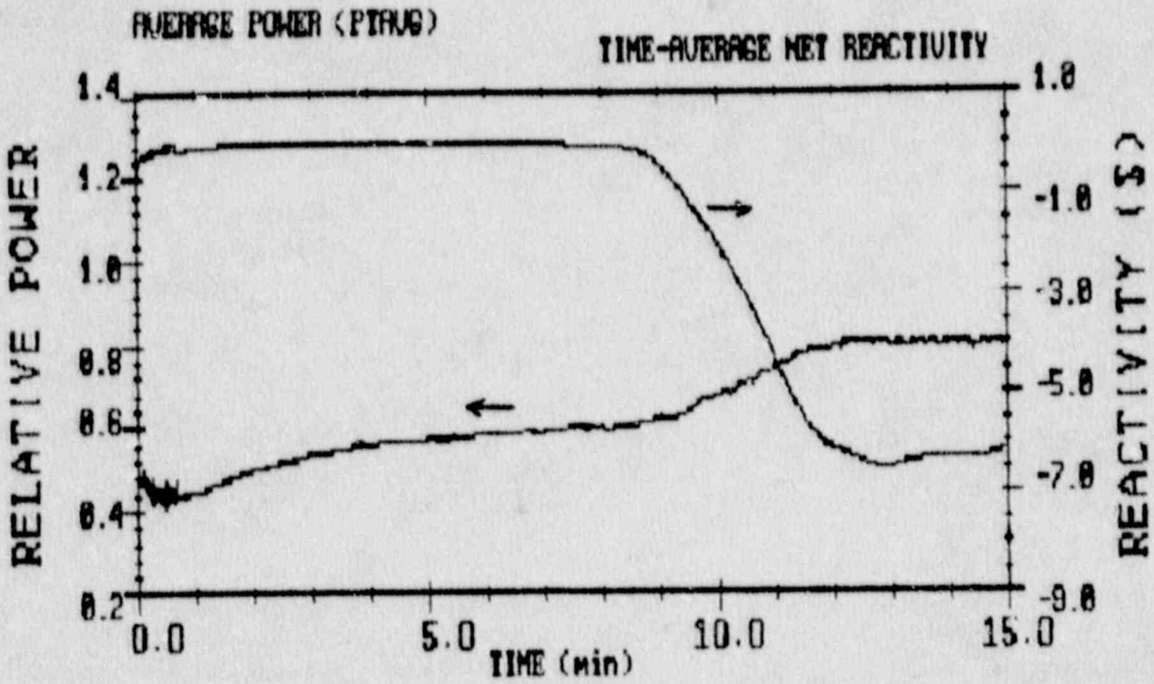

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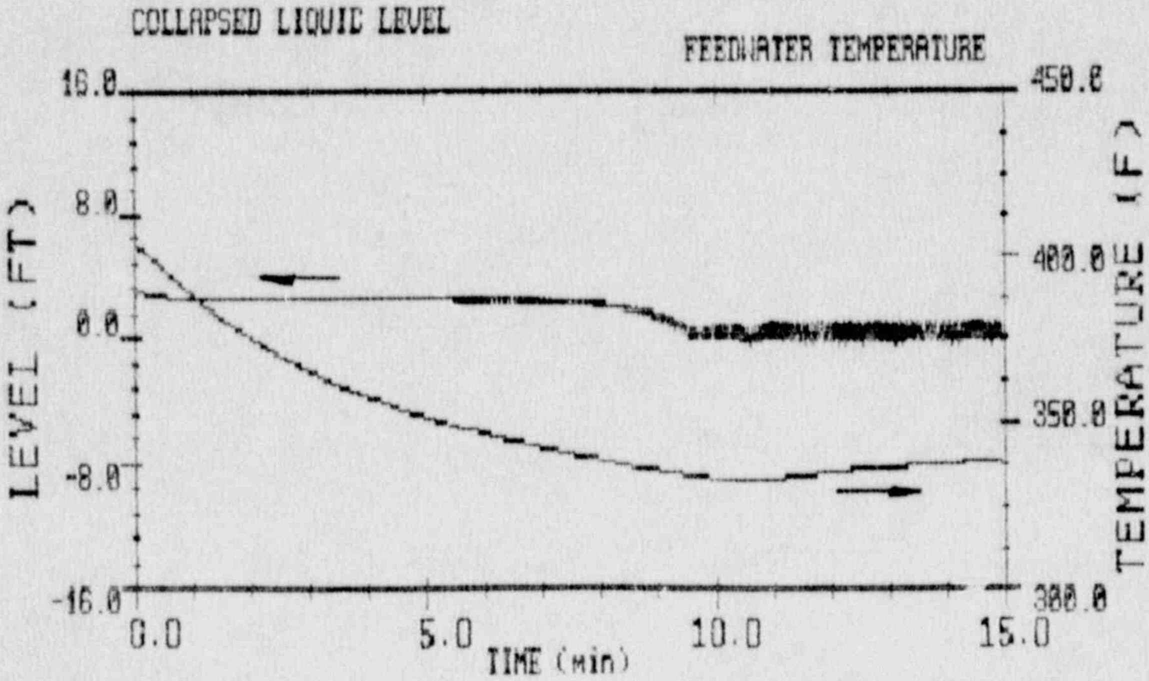

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1

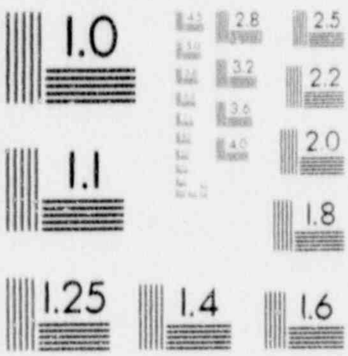
IMAGE EVALUATION TEST TARGET (MT-3)

150mm

6"

1

IMAGE EVALUATION TEST TARGET (MT-3)

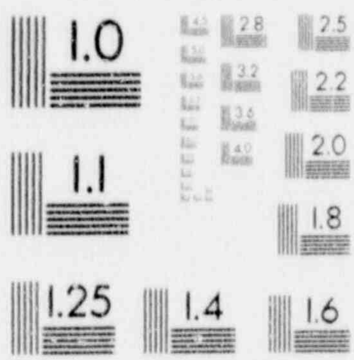


150mm

6"

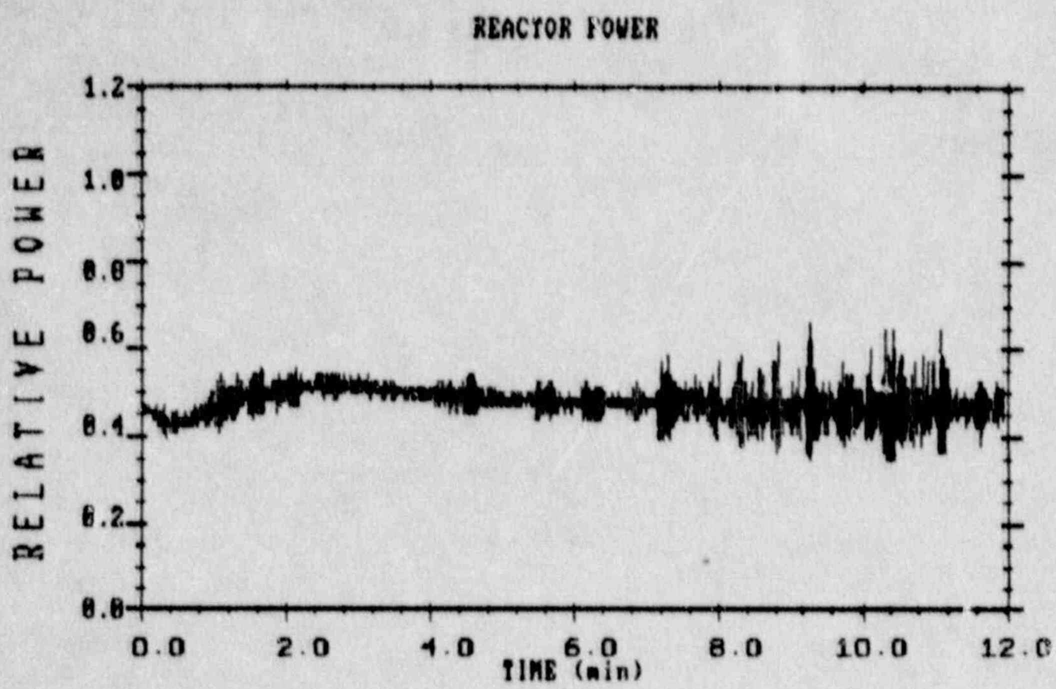
1

IMAGE EVALUATION TEST TARGET (MT-3)

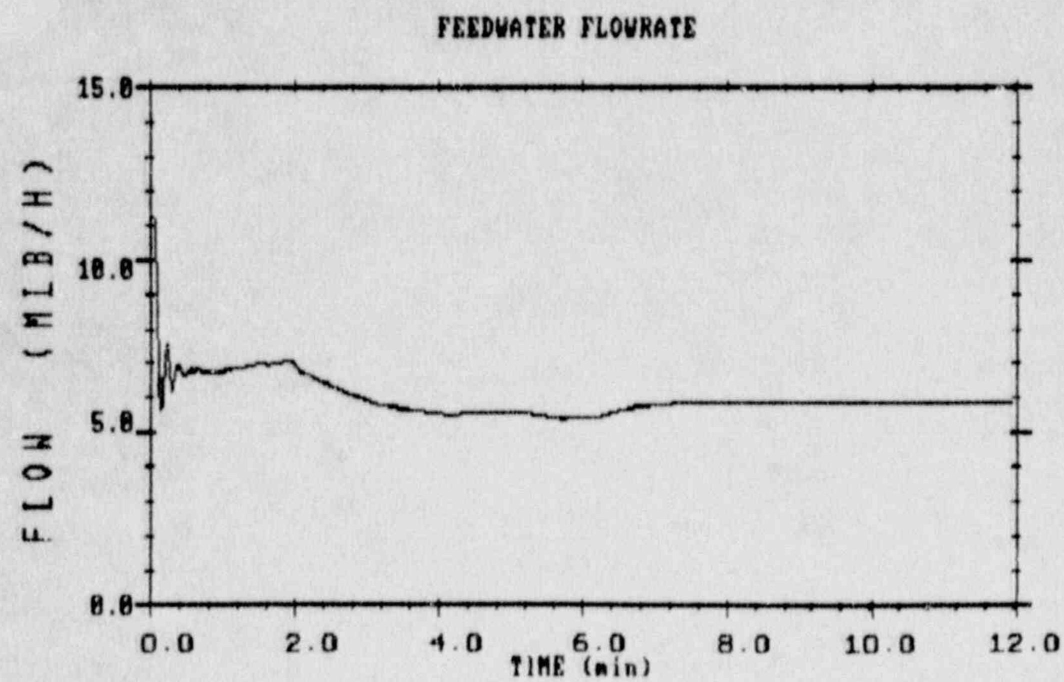


150mm

6"

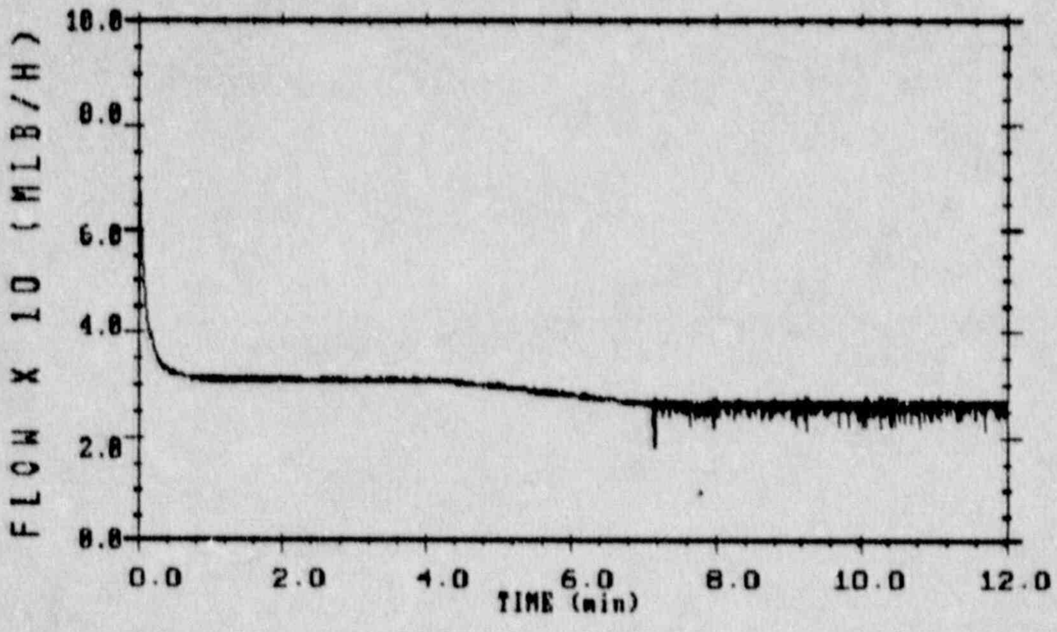


LaSalle/Plant FW Flowrate BNL Plant Analyzer 07-NOV-89 12:38



LaSalle/Plant FW Flowrate BNL Plant Analyzer 07-NOV-89 12:38

MIXTURE FLOWRATE AT LOWER PLENUM EXIT



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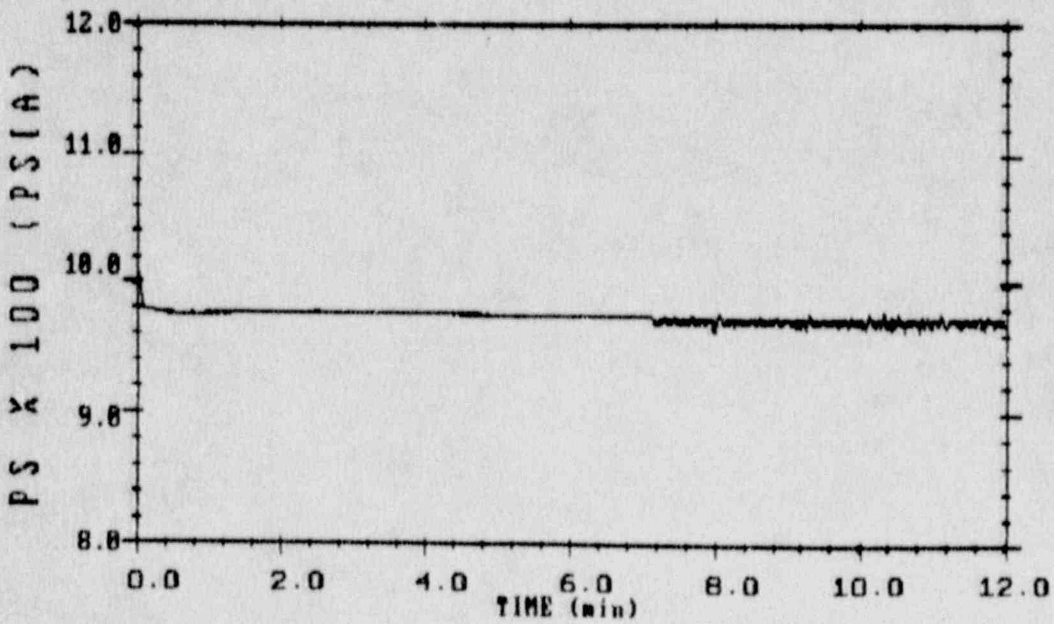
SYSTEM PRESSURE



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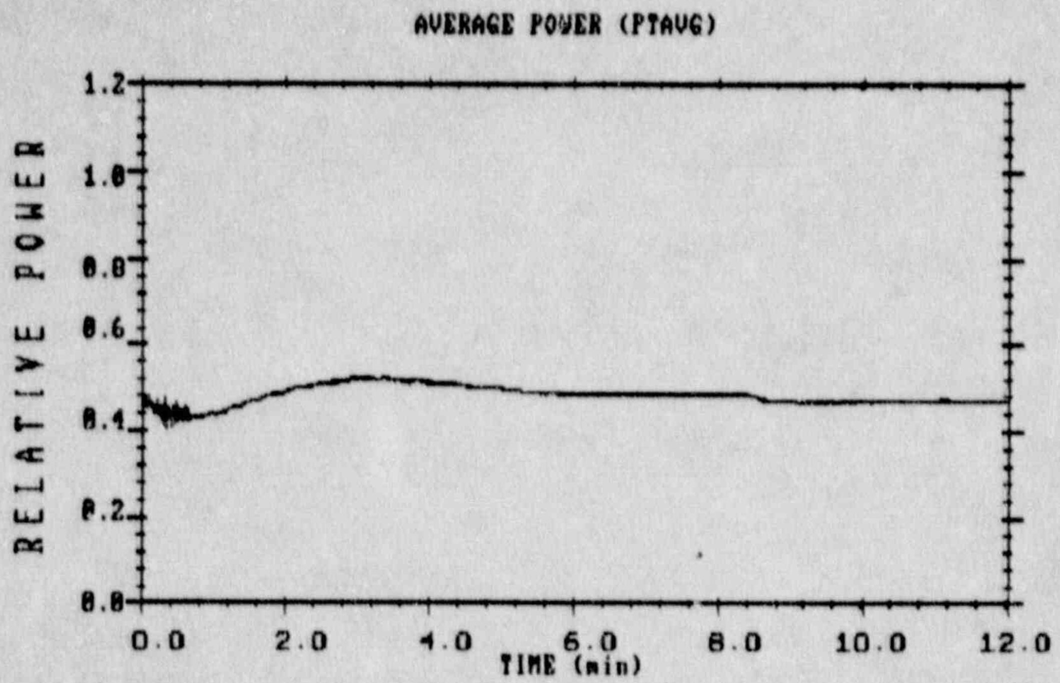

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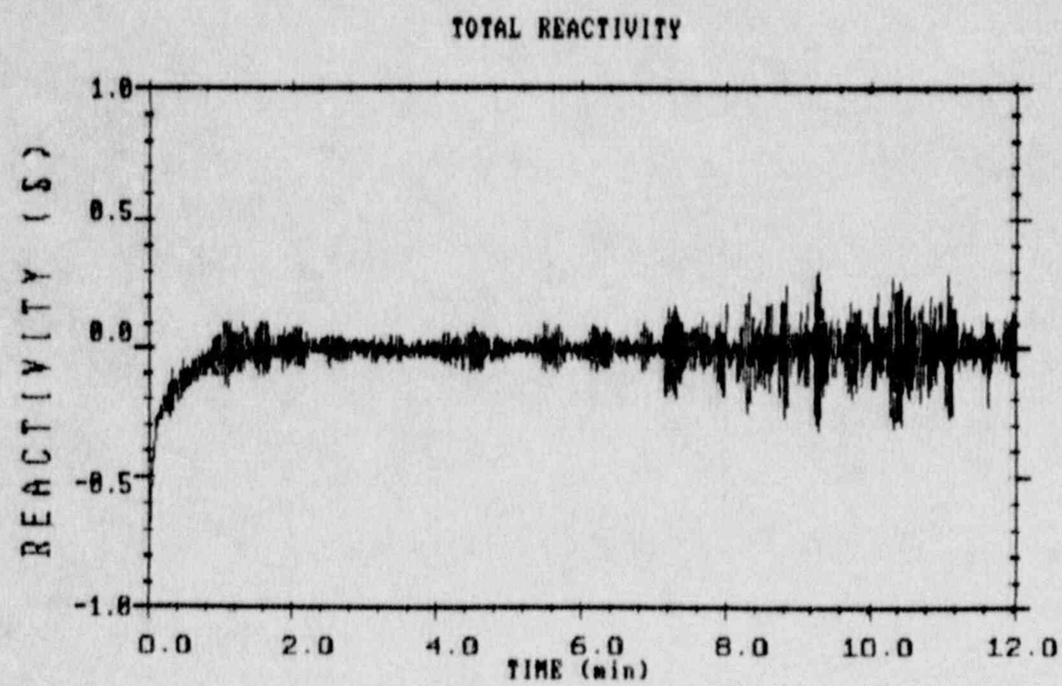

LaSalle/Plant FW Flowrate BNL Plant Analyzer 07-NOV-89 12:38

5. EPA LIMITATIONS

5.1 MODELING LIMITATIONS

- POINT KINETICS
  - AXIAL POWER SHAPE FROM TRANSIENT LASALLE DATA (STARTREC)
  - RADIAL POWER SHAPE THROUGH FIXED PEAKING FACTOR IN HOT CHANNEL AND SPATIAL WEIGHT FACTOR
- ONE-DIMENSIONAL CORE FLOW (ONLY FOR IN-PHASE OSCILLATIONS)
- NO MODEL FOR SUPERHEATED VAPOR/POST-CHF (FUEL COOLED BY FORCED CONVECTION VAPOR FLOW)
- NO TRACKING OF BOILING BOUNDARY (BOILING BOUNDARY IS 1 TO 7 CM FROM ENTRANCE FOR LASALLE CONDITIONS PRIOR TO SCRAM)
- FUEL CONDUCTION MODEL FOR THERMALLY THIN CONDUCTION REGIME

5.2 MODELING PARAMETER UNCERTAINTIES

- VOID REACTIVITY COEFFICIENT (30-50%)
- FORM LOSS COEFFICIENTS, 2-Ø FLOW ($\pm$ 30%)
- FUEL-CLAD GAP CONDUCTANCE ($\pm$ 45%)

5.3 TOTAL UNCERTAINTY (TIME DOMAIN CODE)

$\pm$ 20% FOR DECAY RATIO (FREQ. CODE)

- 20% FOR GAIN (EXPERIMENT)

6. FUTURE PLANS FOR EPA ANALYSES

- PARAMETRIC STUDIES IN SUPPORT OF TPG ON BWR STABILITY (AS SPECIFIED BY RES)
- ANALYSES REQUESTED BY NRR
- COMPLETION OF COMPUTATIONAL ERROR ANALYSIS
- COMPLETION/REVISION OF DRAFT DOCUMENTATION ON EPA ANALYSES OF BWR INSTABILITY.

SUMMARY

TRACG CAPABILITIES

- o MODELS
  - Detailed Two-Fluid Thermal Hydraulics
  - 3D Neutron Kinetics Consistent With GE Design Codes
  - Possibility Of Exploring Multi-Dimensional Effects

- o EXTENSIVE QUALIFICATION
  - Previous Thermal Hydraulic Studies
    - o Void Fraction (Interfacial Shear)
    - o Subcooled Voids
  - Kinetics Model Used For Plant Monitoring
  - Stability Specific Studies
    - o Thermal Hydraulic
    - o Plant Data