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IN THE MATTER OF:

SUBCOMMITTEE MEETING

on

FLUID DYNAMICS

Place - San Francisco, California

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Friday, 16 November 1979

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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

SUBCOMMITTEE MEETING

on

FLUID DYNAMICS

The Post Room
Fifth Floor
Barrett Motor Hotel
501 Post Street
San Francisco, California

Friday, 16 November 1979

The ACRS Subcommittee on Fluid Dynamics met, pursuant to notice, at 8:30 a.m., Dr. Milton Plesset, chairman of the subcommittee, presiding.

PRESENT:

DR. MILTON PLESSET, Chairman of the Subcommittee
MR. HAROLD ETHERINGTON, Member

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P R O C E E D I N G S

8:30 a.m.

DR. PLESSET: The meeting will come to order.

This is a meeting of the Advisory Committee on Reactor Safeguards, Subcommittee on Fluid Dynamics.

I am Milton Plesset, Subcommittee Chairman.

On my left is the other ACRS member, Harold Etherington.

We also have in attendance consultants: Dr. Bush, Ivan Catton, Zenons Zudans, and Professor Schrock is on the Bay Bridge, or somewhere, trying to get here, but he will be here shortly.

The purpose of this meeting is to develop information for consideration by the ACRS in its review of the Mark I containment long-term program.

This meeting is being conducted in accordance with the provisions of the Federal Advisory Committee Act, and the Government's Sunshine Act.

Dr. Bates is the designated Federal employee for this meeting.

A transcript of the meeting is being kept, and it is requested that each speaker first identify himself, and speak with sufficient clarity and volume to be readily heard.

Before I call on Chris Grimes for the NRC -- We don't have microphones yet, but I think if everybody is aware of this and speaks up, we will be able to hear all right.

I would like to make a few introductory remarks, and see if our consultants want to add to them:

What we would like to do is to see if we can't get some convergence of the views of the Staff and the Mark I owners group, because as you most likely know, both the Staff, the NRC Staff, and the ACRS are very anxious to resolve some of the generic items, and the Mark I containment program is one of those generic items. So we hope that we can make a big step in this direction at this meeting.

Now, we are scheduled to run until 5:00 p.m., and I suspect that we will, and I hope you make your plans accordingly.

I would like to ask the consultants if they have anything to add?

DR. RUSH: I will have something later.

DR. CATTON: I have a few questions that were left over from the last meeting.

DR. PLESSET: All right, fine.

DR. CATTON: First, there was the condensation loading of the torus. I guess what I would like to know is how important it is, because if it is important, I would like to know more about the location of the pressure transducers, and how the pressure in its mutual relationship, showing the maximum to the bottom, is arrived at.

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The distribution just didn't look correct.

The second part, looking through my notes, I notice that we didn't get an answer to the question about the relationship between the pressure loading and the time between actuations of the SRV. This wasn't fully explored.

Also, I am not sure that the question of chugging synchronization is closed.

And finally, we heard that more FSTF tests were to be requested by the Staff, and it wasn't clear at that time what those tests were, and why they were needed.

DR. PLESSET: Hopefully we will find out pretty soon.

DR. CATTON: Yes.

DR. ZUDANS: I have nothing at this time.

DR. PLESSET: Harold, any special point?

DR. ETHERINGTON: No.

DR. PLESSET: Well, I think we can go into the scheduled meeting, and I believe that Chris Grimes will make some opening remarks, and orchestrate the Staff's presentation.

MR. GRIMES: Thank you, Dr. Plesset.

My name is Chris Grimes from the Division of Operating Reactors, and I am task manager for the Mark I containment long-term program.

By way of introducing today's discussion, I would first like to give you a summary status of where the Mark I program stands:

In letters dated October 31, 1979, the Staff's acceptance criteria for the long-term program were transmitted to each of the Mark I licensees, and in order to begin the plant unique analysis for the long term program, and expedite implementation of the program.

Copies of the acceptance criteria have been transmitted to the ACRS. We also noticed the issuance of the acceptance criteria in the Federal Register, and provided copies to the interested Congressional Subcommittees.

With that action complete, the Staff and our consultants are now preparing the safety evaluation report, which is currently scheduled to be issued in December, and with some luck it appears that we might be able to meet that targeted schedule date.

Today's discussion will focus on those aspects of the acceptance criteria where there have been disagreements between the Mark I owners and the Staff, and in some cases, we are proceeding towards resolution of those differences, and to the extent it is possible, we will try and identify those for you today.

Also, some of the questions raised by Dr. Catton, I believe, I have already incorporated as part of the presentation. Hopefully, we will be able to resolve those questions.

With the exception of the SRV related aspects, Nelson Su, who is the task manager for 839, was occupied today, and couldn't come, but if it is at all possible, we will try and background whatever questions pertaining to SRV loadings that we can, but we don't have any specific presentation prepared.

We had hoped to preface the discussion about the pool swell loads with a film, which we would hope would reorient the ACRS to the phenomena involved, and until the projector arrives, we will have to postpone that.

I will now let General Electric proceed into the discussion of the pool swell loads, and begin today's discussion.

DR PLESSET: We should have this projector soon.

Larry Steiner, am I correct that you are going to speak.

MR. STEINER: I am Larry Steiner, and I work for G.E.

I would just like to briefly describe our speakers for the day, and the subjects that we will cover:

To begin with, for pool swell, Var Tashjian, from G.E. will describe pool swell, and some of the loads of interest there.

For the vent header deflector load assessment, Bill Kennedy from Acurex will describe that.

Following lunch, we also have a film of FSTF that we would like to show right after lunch. We would like to give a description of the facility.

We do have three speakers for that particular discussion: John Torbeck of G.E. will address the FSTF tests, and the test program, in general, on the data.

Randy Broman, from Bechtel, will describe fluid structure interaction, and how that was addressed in the load definition.

And Umesh Saxena, from G.E., will describe the load definition in the Mark I load definition report, the condensation oscillation definition, and how that specification was derived from the FSTF data.

We think that discussion will probably last a little more than the hour allotted, but the time allotted for the downcomer condensation load definition, we believe can be shortened considerably.

For that downcomer load definition discussion, Randy Broman from Bechtel will provide that.

With that, I will turn it over to Var Tashjian for the discussion of the pool swell loads.

(Slide)

MR. TASHJIAN: I am Var Tashjian from the General Electric Company, and I will be covering the pool swell loads at today's ACRS meeting, pool swell loads in the Mark I

Containment Program.

(Slide)

MR. TASHJIAN: The outline of my presentation today will focus on reviewing the phenomena that was presented to the ACRS on September 14th, just briefly reviewing the phenomena.

I will address the specific loads, and the structures affected by pool swell, but we will be primarily focusing on two discussion topics today, which are the three dimensional, and two dimensional torus upload multiplier, and the other item is the pool swell shape, and the associated impact timing on the Mark I header.

I will present a technical assessment on these discussion topics, and we will end with conclusions.

(Slide)

Begining with the phenomena review:

(Slide)

This is a diagram, a sketch showing the Mark I Containment Plant. It shows the location of the drywell, the main vents, and the header, which goes around, and then the downcomers. It shows the torus, the Mark I torus, and the suppression chamber.

(Slide)

This is a photograph from the G.E. Two Dimensional Test Facility. It is not a very good copy. I do have the photographs themselves, but they are the only copies that I have. You can inspect them. It might be a little bit clearer. I would like to have these back.

It shows where the header is located here, and the downcomers, kind of fogged up. It shows the LOCA bubbles, and roughly shows the shape of the pool here. That is from the two dimensional quarter scale test.

(Slide)

The next slide shows the EPRI three dimensional pool swell test. The main vent comes in here. Here is the header, and these are the downcomers.

We will be referring to these figures a little later on, when we get into the second discussion item, which is the pool swell shape.

(Slide)

This roughly gives an idea of what we mean by pool swell, here, describing the phenomena. This outlines what happens in the transit area. The DBA, the Design Basis Accident Guillotine Break for the Mark I, that means the recirculation pipe rupture. As a result of the massive temperature release from the reactor vessel, the drywell will pressurize, so the pressure and temperature will increase in the drywell.

As the pressure is increasing, the downcomer water well clears. The drywell is exposed to the wetwell.

The air is transferred from the drywell to the torus.

During that process, the bubble expands in the pool. The pool water, as it is rising into the torus, compresses the wetwell air. As the water is rising, also it is impacting on the vent header, the Mark I vent header, and eventually you have bubble breakthrough, the falling of the water back to its position, initial position.

(Slide)

The specific pool swell loads are outlined on this chart:

The torus vertical loads are meant by the uploads of the torus, and the downloads.

The torus submerged pressure is the pressure in the submerged portion of the torus.

The torus airspace pressure is the pressure inside the airspace of the torus.

Another pool swell load is the vent system impact. As the water is rising in the supression chamber it is impacting structures, so you have the vent system impact and drag.

There are also other structures above the pool surface, as well as within the pool, submerged structures, so there is impact on the structures above the pool, and drag on structures that are submerged, as well as drag on structures that are above the pool.

Then we have the vent header deflector loads, which will be discussed by Dr. Kennedy, right after my presentation.

(Slide)

As I mentioned on my first chart, the discussion topics today will concentrate on the torus vertical loads, specifically the 3-D/2-D upload multiplier, and the pool swell shape. These are the two areas that are being addressed, and there is some disagreement between the NRC Staff position, and the owners position. So, I would like to present some technical assessment that we have performed to arrive at a conservative 3-D/2-D multiplier, and the other area is the pool swell shape.

The NRC criteria stated that the vent header impact timing to be obtained from a test that would result in more severe impact on some of the structures than what is to be used, and I would like to present some technical justification of what the Mark I owner's position is on the pool swell shape as well.

(Slide)

The torus upload defined in the Mark I LDR is based on the General Electric quarter scale two dimensional tests. These are plant unique tests that were performed in the quarter scale test facility.

General Electric has performed an assessment on behalf of the Mark I owners, an assessment of what the 3-D effects are on the Mark I plants. They are based on the EPRI 1/12 scale 3-D tests.

In addition to the assessment performed here, General Electric also made some comparisons of the Livermore two dimensional and three dimensional uploads, and those will be presented today also.

(Slide)

I would like to show a typical torus vertical load history:

The download transit is what you see here, and then the upload transit is what you see here.

We will be addressing specifically the uploads.

(Slide)

My next slides shows the comparisons that we performed between the uploads derived from the quarter scale test facility, the two dimensional test facility, and the twelfth scale three dimensional EPRI test facility.

The range of the Mark I plants, for the range of Mark I drywell pressurization rates, which falls between, say, about 45 PSI per second to about 75 PSI per second. This lower curve shows the uploads, the peak upforce for unit area, from the twelfth scale 3-D EPRI test.

The curve up here shows the peak upforce obtained from the quarter scale 2-D test, and what this shows is for the range of the Mark I plants, drywell pressurization rates that the uploads given by the quarter scale test facility are conservative, and thus the load factor for three dimensional effects are not needed.

DR. CATTON: Excuse me. If I recall right from your presentation last time, the orifices on the one-twelfth scale choke, and the orifices on the one-quarter scale do not choke, so you have to scale differently for the one than you do for the other.

Is this all incorporated into the diagram that you are presenting?

MR. TASHJIAN: I am not sure.

The twelfth scale did not choke?

DR. CATTON: No, I was told last time that the one-twelfth scale -- the orifices in the one-twelfth scale choked; the orifices in the one-quarter scale do not choke.

I would think that before you could make a comparison between the EPRI 3-D one-twelfth scale, and the G.E. 2-D one-quarter scale you have to incorporate, somehow, the fact that on the one hand orifices choke, and on the other hand they did not. You can't use the same scaling.

I am asking if you did something about this in generating this graph?

MR. TASHJIAN: These uploads from the EPRI twelfth scale they were, I believe, obtained from tests that were performed with downcomer orifices, and so the choking is incorporated in the downcomer orifice tests.

So, to answer your question, the comparison does include the choking.

DR. CATTON: You didn't understand my question, but I understood your answer, so you can go ahead.

MR. TASHJIAN: On the quarter scale tests -- I am not sure. I am not sure if it is incorporated in the quarter scale test, but we will check on that.

I will get into orifice placement, and so on, in the next few slides. We can discuss those in more detail.

(Slide)

We also made some comparisons of the Livermore 3-D/2-D uploads; the uploads obtained from the Livermore three-dimensional facility were compared with the uploads obtained from the two-dimensional facility.

Three items were noticed in the Livermore test that had some influence on the differences between the 3-D and 2-D uploads, which are responsible for the reduction in the load factors for three-dimensional effects.

The major influence here is the three-dimensional structural oscillations that were observed in the Livermore three-dimensional facility, the one-fifth scale facility.

The three-dimensional, there was non-simultaneous vent clearing between the two-dimensional facility, and the three-dimensional facility, and this came from variation in initial conditions, potentially.

We also suspect that the 3-D facility was driving, pressurizing the 2-D facility, so the data was adjusted such that all the comparable data were looked at.

And the third item, of lesser importance, was the capacitance effect, and the capacitance effect being the location of the orifices in the vents. The volume that is -- the vent system volume beyond the location of he orifices were not equivalent in the 2-D facilities.

DR. CATTON: At one time you indicated that you were going to put together a little computer code to address this capacitance question.

Did you do that?

MR. TASHJIAN: Yes, I believe so.

MR. KENNEDY: Yes.

DR. CATTON: I have just not seen the results. Maybe I missed them.

MR. GRIMES: In point of clarification, we have seen an analytical model, but we have not yet received the formal documentation of the results of the model. What we have seen has convinced us that the criteria that we have are adequate, and we are still resolving the issue concerning compressibility effects, which are identified in the criteria, and we would hope that the documentation that resolves that will also incorporate results of this model that addresses the capacitance effect.

(Slide)

MR. TASHJIAN: My next slide shows the comparison of the 3-D force time histories for some repeat tests, and there is very good agreement between the two tests, but one thing is noticeable here: these oscillations. That was the first item that was pointed out in the previous chart.

These oscillations, primarily, are due to -- The next slide explains what these oscillations are.

(Slide)

The 3-D facility, more or less, was like a cantilevered beam, and the oscillations observed were doe to this cantilevered configuration. There are supports here, and here, around the sector, but one side was free.

That was one of the influences in the differences between 3-D and 2-D.

Another influence was the non-simultaneous vent clearing.

(Slide)

The next slide shows the 2-D and the 3-D facility being charged from a common drywell, and there was a suspicion that, for one thing, the vent clearing times were not simultaneous between the two facilities, and therefore, you would tend to think that the 3-D facility was driving the

2-D facility, through this common drywell, and the coupled drywell ensures common driving conditions, and it also permits 2-D/3-D facility interaction.

The control of initial conditions are extremely important, in terms of vent clearing, and so on, and the large facility will control the drywell pressure.

The small facility phenomena can be affected by these controls of the initial conditions.

DR. BUSH: Before you take that off, would you say that it is driving -- I assume that there has to be substantial attenuation.

How would you handle the attenuation effect?

Aren't you considering a feedback mechanism of some sort?

MR. TASHJIAN: I am sorry--

DR. BUSH: Well, if you assume that one is driving the other, there has to be a feedback, an interactive effect of some nature, otherwise, I don't see how you are going to have the driving, and I am just asking -- I would anticipate in a system such as this you would have substantial attenuation, and I just wondered how you handle it.

MR. TASHJIAN: Basically, the driving that I am referring to here is the fact that one of the facilities cleared earlier, then the other facility has not cleared yet, so--

DR. BUSH: So you would have rarifaction and amplification.

MR. TASHJIAN: That is right.

DR. BUSH: That is what I am thinking about. I just wondered how you handled it.

Are you postulating it, or are you trying to quantify it?

MR. TASHJIAN: My next slide quantifies these effects:

(Slide)

DR. CATTON: Can you see that effect on the drywell pressure? I mean is there a change in slope, in the pressurized rate, or something, that indicated that one had cleared before the other?

MR. TASHJIAN: There was a change in the slope, but you could also trace the waterlevel.

DR. CATTON: But the forcing is the drywell on both systems.

MR. TASHJIAN: Right.

DR. CATTON: Was there a significant change in the time rate of change of the pressure in the drywell?

MR. TASHJIAN: Yes.

DR. CATTON: I would like to see that.

MR. TASHJIAN: I don't have that slide here, but, yes, that was one of the ways that was identified the early vent clearing in one of the facilities, the change of slope in drywell pressure, which I don't have with me, but that was one of the indications.

DR. CATTON: You can't show that to us?

MR. TASHJIAN: I don't have it with me.

DR. CATTON: Okay.

MR. TASHJIAN: I don't have it with me today.

This slide shows what happens if you correct for the oscillations. The solid line shows the Livermore 3-D net torus load history, and the broken line shows the Livermore 2-D, and what you see as the broken line here is a smoothing out of the oscillations over here, and it seems like it reduces these upload differences by about half.

These oscillations are very important in this comparison.

DR. SCHROCK: Why have you shown that correction starting well into the oscillatory period? What about the first oscillation, which is the largest that is shown?

MR. TASHJIAN: This one here, I think , is a real phenomena, I believe, so it is not really a non-phenomenological oscillation. This is phenomena.

You would expect this to increase. There is no reason to believe that there should be oscillations after this first oscillation.

This is actual phenomena.

DR. SCHROCK: Yes, I understand.

(Slide)

MR. TASHJIAN: G.E. has developed a vent capacitance model, and we have run the Livermore [-D and the Livermore 3-D tests on this model to evaluate the effect of capacitance differences between the two facilities.

This shows the reduction in the uploads of about 11 per cent when the two facilities are of the same vent capacitance.

Is that correct?

DR. KENNEDY: And we also indicate that a slight difference in the calculated FL/D is there.

DR. SCHROCK: Is 11 per cent significant?

MR. TASHJIAN: It is significant enough to make a difference between a load factor or not. It is not as significant as the oscillations, or it is not as significant as the vent clearing, but it is of lesser importance than the other two items.

DR. CATTON: People who conduct small-scale experiments, and try to do a very good job, sometimes are very pleased with having accuracy within 11 per cent.

MR. TASHJIAN: This 11 per cent is really not that great of an impact on the uploads as the first two effects. Actually, the oscillations and the clearing times are responsible for the majority of the differences.

DR. ZUDANS: How did you obtain these curves? Where did this come from? Is this analysis?

MR. TASHJIAN: Yes.

Bill, maybe you can introduce the model a little bit here, the vent capacitance.

DR. KENNEDY: This was an early version of the current compressible flow model, a d the vent system, that calculated pool swell, download and upload, including the effects of mass storage in the vent system, and FL/D, due to the orifices. And running it, in this case, we were changing the -- as I recall -- the capacitance between -- the location of the capacitance between the 2-D and 3-D facility, and also changed the FL/D between the two computer runs, based on analytical estimates of the two-dimensional facility FL/D, and the three-dimensional, and the results show a small difference in the upload.

DR. CATTON: Have you had a similar kind of study of your own system, moving your orifice around and seeing what it does?

DR. KENNEDY: Yes.

We get acceptable agreement between the results of the computer code, and the change that we noted in generic sensitivity. This was a test series that was run while we systematically varied one perameter at a time, like FL/D.

DR. CATTON: An "acceptable agreement" means plus or minus 15 or 20 per cent?

DR. KENNEDY: Download was in that order. Upload impulse was in that order. Upload itself was not that good, because it was probably more like 30 or 40 per cent, due to, we think, early breakthrough by virtue of the configuration of the download.

MR. TASHJIAN: So based on the comparisons that we performed, on the assessment of the 3-D effects in the nuclear facility, and comparing those to the G.E. two-dimensional facility, the torus upload multiplier of less or equal to 1.0 is justified. And in the Livermore 3-D/2-D upload comparisons, the comparisons confirm that the upload multiplier of about 1.0, when the facility and test conditions are matching, that is another confirmation of the 3-D/2-D upload multiplier of 1.0.

(Slide)

The other topic for discussion is the pool swell shape, and I would like to bring back these two slides that I presented before.

The pool swell curvature was observed in both the G.E. 2-D and the EPRI 3-D facilities, and the shape of the pool is predominantly governed by non-uniform downcomer spacing in Mark I plants, and I will show in a slide what we mean by that, and to a lesser extent, it is affected also by the vent flow distribution. Vent flow distribution is another factor.

What we mean by sweep time here is: when does the pool surface impact the vent header. That is the vent header impact timing, and the pool surface you see here is this.

A similar curvature is seen in the quarter scale test facility. Well, the pool surface is a little -- it is a little bit later slide, but the pool surface is about there.

DR. CATTON: Is that the curvature that you feel is too great, that you just showed us?

MR. TASHJIAN: The curvature here?

DR. CATTON: Yes.

MR. TASHJIAN: The curvature that we will be talking about the sweep times is this one here, on the longitudinal sweep times.

DR. CATTON: I understand.

MR. TASHJIAN: As I mentioned, the governing, or predominant factor in the pool shape is the non-uniform downcomer spacing.

(Slide)

This is a typical sector, bay of a Mark I plant, where it shows the main vent line, and the header, the minor bend on the header, and the location of the downcomers. The spacing are such that they are not uniformly spaced. There are some dimensions that will show what these dimensions are. So, there is some variation of spacing between the downcomers and this longitudinal direction.

If the downcomers were spaced uniformly, you would expect a uniform pool rise, but due to the non-uniform spacing, you get a non-uniform pool rise.

(Slide)

The distribution of the vent flow is accomplished by placement of orifices. Another purpose of placing orifices is to simulate the flow loss distribution in the vent system.

The LDR pool shape, the sweep times that are presented in the Load Definition Report are based on the EPRI three-dimensional tests, and there is actually interpolation between the downcomer orifice tests, and the main vent orifice tests.

Just to back up a little bit here, EPRI originally performed some downcomer orifice tests, and soon afterwards performed some vent orifice tests, and at that time, the LDR definition was coming out, knowing that neither one was the actual representation of the Mark I flow distribution, more like something in between was more prototypical of the Mark I plant. And thus, what we have done is we have literally interpolated between the sweep times given by the downcomer orifice tests, and the sweep times given by the main vent orifice tests. And the conservatism in this interpolation was confirmed by later tests performed by EPRI with a split-orifice configuration.

(Slide)

Now, to give you some results of these, my next slide shows -- What you see here is the vent resistance ratio plotted against sweep time, sweep time against vent resistance ratio. Vent resistance ratio zero means the orifices of the downcomer only. Vent resistance of 1.0 means orifices in the main vents only. And so these are the two points that were done originally.

The LDR sweep times were generated based on a linear interpolation between these two, with a 50-50 split. So this is what is defined in the Mark I LDR.

Soon afterwards, EPRI SRI performed some tests that had a split-orifice configuration, and this is the data point for the split-orifice configuration, and that reveals even the conservatism in the LDR definition. Low sweep times means simultaneous impact. If the sweep time is zero, then the entire vent header system is being impacted at once, so a lower sweep time means conservative load -- the load definition for the impact on the vent header and deflectors.

So we see that the interpolation is quite a bit more conservative than given by the split orifice tests, which are prototypical of the Mark I vent flow distribution.

(Slide)

In conclusion, downcomer spacing is a predominant factor. Uniform downcomer spacing results in uniform pool swell shape. We have non-uniform downcomer spacing in Mark I plants, and thus results in non-uniform swell.

Split orifice is prototypical of the Mark I vent flow distribution, and the Mark I LDR interpolated sweep times are conservative, when compared to the split-orifice data, split-orifice tests that were performed by EPRI.

The conclusion is that the LDR interpolated sweep times are conservative.

DR. CATTON: In your model, where you looked at capacitance, did you look at a large number of orifices, as contrasted with one or two, in coming to such conclusions?

MR. TASHJIAN: A large number of orifices?

DR. CATTON: Yes.

In other words, enough so that it was almost like a continuous value of FL/D?

MR. TASHJIAN: Yes.

DR. CATTON: You did that?

MR. TASHJIAN: Yes.

DR. CATTON: And these results eventually will be available?

MR. TASHJIAN: Yes.

DR. CATTON: And you concluded that there was small change between almost continuous and the two orifices?

MR. TASHJIAN: Between -- I am sorry. The last statement I did not hear.

DR. CATTON: Split orifice means two orifices, right?

MR. TASHJIAN: Right. One in the main vent, and one in the downcomer.

DR. CATTON: How did that result compare with maybe four or five orifices?

MR. TASHJIAN: I am sorry.

DR. KENNEDY: Do you want me to try that one?

We did run such a comparison, but at the same scale, and we found that the results are equivalent. Two orifices is a pretty good representation to four or five orifices. However, we are really talking about two problems here:

One is, if you do this at the same scale as say a quarter scale, that conclusion is true. If you go now to full scale to quarter scale, you would introduce the whole problem of compressibility.

DR. CATTON: You don't have the compressibility problem at one-quarter scale?

DR. KENNEDY: No. They are essentially incompressible.

DR. CATTON: I guess I will have to wait and see your analysis.

DR. PLESSET: Do you want to clarify that?

DR. KENNEDY: You see some compressible effects, but they are truly negligible.

DR. CATTON: What ab ut at one-twelfth scale?

DR. KENNEDY: Also. Quarter scale and below.

DR. ETHERINGTON: The scale models show nice, sperical bubbles.

Have you reason to suppose that that will be true in the full scale?

DR. KENNEDY: I think that the hydrogen andrix (ph) will be very nicely spherical. I think it will be quite similar.

DR. ETHERINGTON: The bubbles won't break up?

DR. KENNEDY: I don't think so.

DR. CATTON: I am frankly surprised at your conclusions. It seems to me that if I were to orifice for one-twelfth scale, it would require significant area changes, and I can sort of visualize clear volumes within which there would be compressibility, also choking at the outlets, and to me that says that you have to consider compressibility.

DR. KENNEDY: The Mach numbers released at that steadily decrease in scale.

DR. CATTON: Compressibility has nothing to do with Mach numbers. It is just that you squeeze the gas that is in the volume.

In any event, I will be interested in seeing the results.

DR. PLESSET: Is it any more than the propagation times are smaller for smaller objects?

Is that what they are getting at?

DR. KENNEDY: As far as I can tell, it is just a matter of--

DR. PLESSET: That is not that much different.

DR. CATTON: It is just a matter of compressing the gas that is in the volume. It is not a matter of waves going back and forth.

MR. GRIMES: By way of clarification: The basic issue of compressibility that we are dealing with has to do with the fact that the orifices introduced don't allow this compression, and rarifaction waves to affect the feeding of the bubble.

The basic hydrodynamics that we have observed in all pool swell testing, not only Mark I, but Mark II, general phenomenology testing, have all shown the same general formation of the bubble, and we evolve this down to the potential affects of compression and rarifaction wave running through the vent system, and how it affects the local pressures developed in the bottom of the pool, which is really boiling it down to very fine detail, and in fact, the models do look not only at compressibility between orifices in scale, but they also have performed analysis, and we have performed analysis of full-scale prototypical systems where the orifices are not present to ...eak up the compression or rarifaction waves. And eventually that kind of report will show -- The report to be issued will show those kinds of results.

DR. PLESSET: What was your conclusion, again, about this point?

MR. GRIMES: The conclusion is: I think it is a misnomer to characterize compressibility as being negligible, or non-negligible, because we have got to think about it in terms of its net affect on the loading function.

Negligibility, in this case, means that the introduction of the orifices tends to damp out the effects of compressibility, as they would affect the pressure on the wall.

DR. PLESSET: It certainly affects the waves that are propagating back and forth.

Is that what you are saying?

MR. GRIMES: That is the point.

DR. PLESSET: Is that all right?

DR. CATTON: Well, I would agree with Chris's conclusion.

DR. PLESSET: Yes, that seems fairly--

DR. CATTON: I would go to other steps.

The compressibility tends to store up the gas behind the orifice. As soon as you start to clear the bubble, it slows down the rate of charge to the bubble, and this has nothing to do with waves, but it is a compressibility effect, and as you go down in scale, that effect goes up.

MR. GRIMES: But that relates to how well you can size the orifice to give you the charging rate that you desire for the bubble.

DR. CATTON: Sure.

DR. PLESSET: One more question.

DR. ETHERINGTON: Have the underwater baffles been removed in all of the Mark I plants?

MR. GRIMES: I would have to check that for you, but the last recollection that I have -- I don't like to rely on my memory, but -- was that they have all been removed.

DR. ETHERINGTON: This is a factor on your distribution?

MR. GRIMES: Even when the baffles existed in the Mark I plants, they were low enough that I don't think they would have been affected by the pool swell phenomena. The bubbles occur in the relative center of the pool.

DR. BUSH: I would like to ask a general question, and I will indicate that I don't expect an answer, but at least maybe I can find out someplace during the day that it is being considered:

These systems, in contrast to many of our safety systems, are dynamic. A lot of our systems, of course, are passive, and furthermore, in contrast to almost all the other safety systems of which I am aware, this is one of the very few cases where a safety system is used routinely to handle other types of loads, those, say, of the safety relief valve discharge.

We have been talking about what I call a quasi-uniform distribution, which is not the case with the safety relief valve. That is very much a function of this one's discharge, which means that you can get strongly asymmetric pressure loads, and strongly asymmetric thermal loads, and I don't think you can ignore the thermal loads.

In fact, my real point here is that we are faced with a repetitive load situation, not necessarily on a uniform chronological scale, but certainly occurring again, and again, and again. And I would strongly suspect that there is the potential, at least, for either low or medium cycle fatigue, which would lead to degradation of the pressure boundary, and my real concern is: Is there an interface, or has an interaction been established to look at what I will call the hypothetical loads, the very low-probability accident type loads, superimposed on a degarded boundary, to see what the implications are, because if you were talking of one-shot loads, such as we have been discussing here, on a pristine system, we may have one response. If we talk of going into a system after "X" years, where we have essentially degraded, or at least possibly degraded the boundary, the situation of the loads that were totally acceptable initially may not be acceptable at all.

And, in fact, we were trying to wrestle with this yesterday, and the day before: What should one be looking for in a Mark I torus, so far as an in-service inspection? And we don't have any good answers.

I guess, Chris, I don't expect an answer from the key people up here, but I would like to know really if it is being considered. I think that is the important thing.

MR. GRIMES: The only answer I can give to you at this time is that I know that that issue has arisen.

DR. BUSH: I hope it has arisen, certainly.

MR. GRIMES: I don't know to what extent we have tried to wrestle with it ourselves.

DR. BUSH: I have seen some of the liftoff values, and et cetera, and when I hear about liftoff values of that nature, then I am pretty sure that I am -- particularly if I have began to go into strongbacks, et cetera, I have the very real potential for cracking the boundaries, the wetwell boundaries.

As I say, I didn't expect an answer, but I really wanted to know if it is being considered, and how the interface was being established.

MR. GRIMES: Excuse us a moment.

MR. DEARDORFF: Art Deardorff, from Nutec, from the Mark I owners group.

I think maybe I can address that a little bit, in that for safety relief valve discharge loads, which are considered to be a normal loading on the containment, the structural acceptance criteria does require consideration of fatigue aspects for that normal operating load.

For the case of pool swell, the same structural acceptance criteria does require that you meet the same service levels for that load as you do for the normal operating loads, so there is margin.

DR. BUSH: My point is you could consider it -- for example, you could consider the repetitive pressure loads, but if you ignore the thermal loads, which are highly sensitive to the geometric aspects we find at the plant, you can be way off, your assumptions can be off by a factor of ten to a hundred. That is where my concern is.

MR. GRIMES: Dr. Bush, I would like to make sure that I understand your point clearly: Are you suggesting that we consider looking at response analysis of the containment structure for Design Basis Accident, having factored into the structural model itself certain repetitive pressure or fatigue type cyclical loadings, thermal and pressure causing some distortion of the structure, and then put the LOCA loading on top of that?

DR. BUSH: Well, distortion is not my concern so much. What I am really concerned with is the situation that after, say, 20 years of life I have imposed a very large number of thermal and pressure cycles on that system. We sometimes look at the right place, and sometimes don't. I wouldn't be at all surprised if we looked carefully at certain areas, particularly where they have added strongbacks, to find cracks. I wouldn't be surprised if some of the cracks are quite large.

Now, what happens if you have some fairly large cracks in a wetwell boundary, and now you superimpose what I will call a fairly substantial uniform load on that particular system?

I don't know that the answer is. I am just asking, that it is something that we have to look at.

They may have an excellent model, and I am not trying to denigrate it, but I have also seen cases where you look at one series of loads, and if you don't look at the superimposed loads as to the thermal, you have a totally different ballgame.

MR. GRIMES: Well, the thermal loads are incorporated in the load combinations, and we have combinations of loads for small-break accidents, and intermediate-break accidents with a safety relief valve discharge, and I think that the only thing that I would say that we have not addressed is the consideration of the potential degradation of the containment boundary, prior to a Design Basis Accident.

DR. BUSH: You realize this is very similar to the slug flow aspects that you have the real problem on cracking, in the secondary systems of the waterlines. It is very analogous.

DR. PLESSET: But you have got the question, Chris?

MR. GRIMES: We will debate it further amongst ourselves, and see if there is some way that we can address that.

DR. BUSH: I just wanted to plant the seed. I don't expect the plant to flower for some time.

DR. PLESSET: Any other questions?

If not, I think you have--

MR. GRIMES: Mr. John Ranlet, from the Brookhaven National Laboratory, will now discuss our evaluation of the net vertical pressure loads in the torus.

MR. RANLET: My name is John Ranlet, from Brookhaven National Laboratory.

The purpose of my talk today will be to discuss the net vertical pressure load data comparisons.

(Slide)

The topics I will be discussing will include a brief description of the acceptance criteria and pressure load margins.

I will then go into what was available for our comparisons.

I will then go into some detail on the G.E.-EPRI comparison, the Livermore upload comparison, and the download comparisons from both these sources.

(Slide)

The acceptance criteria which was recently published specified that the mean downward and upward net vertical pressure loads shall be derived from the quarter scale test facility, and plant-specific tests. However, based on our review of the available data base, we will require that the following margins be applied:

For the upload we impose a 21 and a half per cent margin on the mean upload. "Mean" refers to the average of the QSTF plant unique test.

Four tests were performed at each plant operating condition.

For the download we require a margin here which comes from a statistical analysis, which I will get into a little more detail in the next slide.

(Slide)

The upload margin is comprised of two parts:

15 per cent to cover uncertainty of the 3-D/2-D

comparisons, and a six and a half per cent which comes from a two sigma from a statistical analysis of the entire QSTF data base.

The download margin varies from 6.3 to 15.5 per cent on a plant unique basis, and again, it comes from a statistical analysis of the entire data base.

The 15 per cent here is the main topic of my discussion, and it is based on review of the available data base, which I will discuss right now.

DR. ZUDANS: Could you define that mean--

MR. RANLET: Excuse me?

DR. ZUDANS: How do you define the mean.

MR. RANLET: The mean was an average of the four tests performed in the QSTF tests.

DR. ZUDANS: Only comparing the maximum load, is that right?

MR. RANLET: Just the maximum loads, yes.

(Slide)

The 3-D/2-D data comparisons, which I will be talking about, were used to determine if the torus loads obtained in the 2-D QSTF plant unique tests are appropriate for a 3-D load definition.

The data base which was available for assessing the possibility of a 3-D effect on the vertical loads was the G.E. one-quarter scale 2-D tests, the EPRI -- I will refer to the EPRI tests as one-twelfth scale, to make it simple -- 3-D tests, and the Livermore one-fifth scale 2-D and 3-D tests.

The superscripts correspond to the references which are included at the end of the handouts.

DR. SCHROCK: Excuse me, this two times ten to the minus fifth is evidently a dimension quantity?

MR. RANLET: Yes, pounds force. The downmean is--

DR. SCHROCK: Pounds force.

MR. RANLEY: Pounds force.

(Slide)

This figure, like you have seen before, represents the Mark I owners' basis for asserting that the 2-D loads are appropriate for a 3-D specification.

It compares the G.E. one-quarter scale, 2-D, and the EPRI 3-D one-twelfth scale. Both of these were compared in Reference (6).

The tests were performed using the Brown's Ferry geometry at a full delta "P" with reduced submergence.

As you can see from the curve, the 2-D was higher than the 3-D during -- in the Mark I range.

However, based on our review, which I will outline in the next new graph, we have some difficulty with the comparison.

(Slide)

Based on our review of the comparison, we have concluded that it should not be used to assess a 3-D effect on pool swell uploads. The decision is based on the following considerations:

First, the Browns Ferry geometry is not prototypical of Mark I plants. The 45 degree downcomer associated with the Browns Ferry geometry causes early breakthrough.

When you have early breakthrough, you attenuate the uploads, because the wetwell airspace is not compressed sufficiently.

To illustrate what I mean by that:

(Slide)

Here are the various types of downcomer geometries.

Type I here corresponds to Nine Mile Point and Oyster Creek. Type II is the majority of the plants, which are 19 out 25 plants that have Type II geometry. Type III is Duane Arnold, and Type IV is Browns Ferry.

In the QSTF tests, it was found that this large bend here caused the early breakthrough. This did not happen in any other of the geometries.

(Slide)

The second consideration was the test conditions at which the comparisons were made. They used full delta P at a reduced submergence. The reduce submergence was, I think, three feet four inches.

When you test at these conditions, you also minimize pool swell effects.

Now, we based this on calculations we performed, plus in the Lawrence Livermore test this general trend was shown.

The flow resistance used in the EPRI test was higher-- was not matched to the resistance in the G.E. test. It was at a higher value, and the result: if you have a higher resistance, you are going to have low uploads.

The orifice location they used was a downcomer orifice, and we found from our calculations that the downcomer orifice size variation caused a distorted pool swell.

These two items will be discussed in more detail by Dr. Kosson in his review on the 3-D distribution.

DR. BUSH: When you say "flow resistance, how are you defining it?

MR. RANLET: You mean exactly how you--

DR. BUSH: This has only to do with the orifices?

MR. RANLET: Yes, the orifices.

DR. BUSH: So you are not considering head effects?

MR. RANLET: Well, the orifice was an increase in the head.

DR. BUSH: Well, I am thinking of head in the sense of level, level of head, water above the submergence plate.

MR. RANLET: Oh, I see what you are saying now.

DR. BUSH: Because that, I would think, would go the other way, so far as the uplift.

MR. RANLET: No, when you talk about the total pressure los from the drywell to the bubble, that is basically what we are talking about.

Dr. Kosson will get into that in more detail.

(Slide)

I am just throwing this up here to give you an idea of the type of different schemes that we use for the orificing.

In the EPRI test they used three different techniques:

The first was to put an orifice in this location here, which is called a vent line orifice, for obvious reasons. They have a very large volume downstream of the orifice, and it was shown in these tests that using the vent line orifice increased the load dramatically over the other type of orifice techniques.

The other technique was to split the orifice, and have one here, and several in the downcomers. Each downcomer would have an orifice placed in it, and the size will be discussed later, in a dry test.

The last technique was just to have downcomer orifices, and just place them in the downcomers, and have no orifices in the vent line.

As I mentioned, the difference in the uploads between the vent line orifice and the d~wncomer orifice was like 40 per cent. Now, this was a fairly small scale, and that is the reason why I attribute such a large effect, because you have to put a much larger resistance into the stream to try to get a close scaling of the various facilities.

DR. CATTON: You also choke, don't you, when you put that large resistance--

MR. RANLET: Yes.

DR. ETHERINGTON: These orifices were not in any of the original designs?

MR. RANLET: They were not in the prototype, no. They are just used to try to match the enthalpy flux into the bubble.

(Slide)

After we determined that we couldn't use the G.E. EPRI comparison, we turned to the confirmatory tests at the Lawrence Livermore Laboratory. These tests were performed at zero delta P with a four foot submergence, and they used the Peach Bottom geometry the Type II downcomer configuration, which I mentioned before.

The orifice locations were in the vent lines. In the 3-D sector, they had them approximately here, whereas in the 2-D sector, they were over in this area here.

(Slide)

These are the results from the Lawrence Livermore tests:

I have the upload pressure versus the drywell pressurization rate.

The open symbols are the 3-D, and the darkened symbols are the 2-D.

They used, as I mentioned before, a vent line orifice.

As you can see, the general trend over the whole pressurization range was that the 3-D was larger than the 2-D.

I have also included on here some data from single tests from G.E., and some additional tests from EPRI, which are different than the ones they used for the G.E. comparison.

DR. CATTON: When you say "pressurization rate," is this linear from the start of your test to clearing?

MR. RANLET: Yes.

In all of the trends that I have seen, and all the tests I have seen that is true.

As to your question before, where you mentioned--

DR. CATTON: That is where I was headed.

MR. RANLET: I think the only way to ascertain whether there was a difference between the 3-D and 2-D segments is to look at the pressure in the downcomers.

You could not ascertain from the Livermore tests, because they only had two pressures -- taps in the drywell, and you just couldn't tell whether one was driving the other or not.

DR. CATTON: Well, the drywell pressure is what gives you the pressurization rate, isn't it?

MR. RANLET: Yes.

DR. CATTON: And you don't want under mass flux to calculate it. You measure it.

MR. RANLET: They measure it, right.

The pressure was constant--

DR. CATTON: If it is constant over the whole test, then I don't see that there is a feedback effect.

MR. RANLET: I didn't say that there was. I don't think there was.

DR. ZUDANS: But you only have one point in the drywell, you don't know what happens in conjunction.

DR. CATTON: It depends on the location, if you look at -- That is true.

DR. ZUDANS: So you just don't know.

MR. RANLET: I would like to really draw your attention to these two tests:

This is several tests performed by EPRI at one-twelfth scale, and this is the G.E. Browns Ferry split orifice test from the zero delta P evaluations, for structural reasons.

If you will notice, the 3-D is higher than the 2-D and th's is approximately a 20 per cent difference.

Now, I understand that this is just one test, and there is some kind of randomness to it, however, the order of magnitude of the difference is basically what our criterion states.

(Slide)

In order to determine if the 3-D/2-D difference, which was shown on the previous graph was due to a truly three-dimensional effect, or a possible mismatch of the two facilities, we performed a 1-D transient pool swell analysis where we analyzed both the 3-D and 2-D tests. Indicated here are the various modes that we considered in our analysis.

The calculations show that the LL rigs were indeed mismatched to capacitance, and with a small effect due to resistance.

To comment on the 11 per cent that they mentioned before, our numbers came out a little bit lower. The effect on the peak upload pressures varied from three to nine per cent over the pressurization rate that we considered in the study, which was from 40 to 80 PSI.

Now, this, as you can see, changed the order of magnitude of the effect, but did not eliminate it.

In order to factor this into results, what we did is illustrated on the next figure:

(Slide)

What we did was we took the least-square fit of the

3-D and 2-D data, as I showed you before, and we made a ratio of that curve, and this is what we got:

(Slide)

And it varies from approximately 13 to like 25 per cent, or from 40 to 80 PSI per second.

When we took into the mismatch of the two facilities three to nine per cent over the range, we came up with this low curve here, which goes from like 10 to approximately 14, or 15 per cent.

Based on our review of how the Livermore tests and their test conditions match with the normal plant operating conditions, as well as to cover some of the uncertainties associated with analyses of these type, we feel it is appropriate to bound this low curve with a 15 per cent margin over the whole pressurization rate.

DR. BUSH: While you have that curve up, as a best estimate type of thing, or just a guesstimate, maybe, if instead of a line I make that a surface, where the coming out is the submergence, you know, you look at a four foot submergence, a three foot, or three and a half submergence, how would you say that surface would behave?

Let us say that this is four foot, as you are either reducing--

MR. RANLET: Well, the reduced submergence would cause a lower load, a lower effect, a 3-D effect.

DR. BUSH: Now, the question is: Would the surface tend to flatten out substantially as you go above four foot?

MR. RANLET: Well, I don't think it is too important, and I will show you why I don't think it is that important:

(Slide)

These are the plant operating conditions that we specified in the LDR. You have to remember the Livermore tests were performed with four foot submergence, and a zero delta P.

If you look at the cases where you have zero delta P, you will see that most of the submergences are four and a third feet, which is fairly close to four feet. However, if you look at the pressurization rates, they are in the medium range, like around 60, so based on that, we considered, when we bound the data with 15 per cent, we definitely would cover any possible effect due to variances in H and delta P.

MR. GRIMES: John, if I might, we also have the short term program twelfth scale sensitivity for submergence, quarter scale tests in the long-term program, sensitivity versus submergence, and the Livermore tests performed sensitivity on submergence, and they were all a relatively flat distribution over the range from three to five, so we would expect that the surface that you referred to, if coming out of the plane was decreasing submergence, it would fall away, and going into the plane, it would increase, but it would increase very slightly.

DR. BUSH: So it is fairly flat. That was the point I was interested in.

MR. RANLET: Yes, I am sorry.

I believe it flattens out. There is a certain maximum depth, which affects it.

DR. BUSH: So your argument is that 15 per cent essentially would bound it, what you have considered to be the total range.

MR. RANLET: Right.

To go on to download comparisons, this is the equivalent to the G.E. EPRI one-twelfth scale, and as you can see, very good agreement was obtained over the range of the pressurization rates.

Likewise -- In a likewise fashion, if you look at the Livermore tests with the same data as was included on the previous curve, you will see that there was no systematic trend. Some 2-D's are higher than 3-D's, and vice versa, and they all seem to fall pretty much in the same curve.

And as a result, we don't feel that an additional 3-D/2-D margin is required on the download.

That concludes my talk.

Are there any questions?

DR. PLESSET: I presume not, thank you.

MR. GRIMES: Now, Dr. Kosson will present our assessment of the flow distribution effects, and how they factored into our consideration of the vent header impact timing requirements.

DR. KOSSON: My name is Bob Kosson. I am from the Grumman Aerospace Corporation, working under contract to Brookhaven National Lab, and the topic that I want to specifically address here today is the pool shape effect. This is in support of Section 2.5 of the Acceptance Criteria, basically to explain why we are saying we should go with the pool shape that was developed in the vent orifice tests with the EPRI facility, rather than the LDR specification, which would like to split the difference between the vent orifice tests, and the downcomer orifice tests.

(Slide)

You have seen this figure before. Basically the EPRI model had in the vent orifice tests a vent orifice at this location.

In the downcomer tests, they placed the orifices here in the individual downcomers, and in the split orifice tests, they used both orifice locations with orifices which were simply scaled up to provide half the resistance in each.

I will refer to the downcomer pairs as downcomers (1), (2), and (3). That is a slightly different number -- They were numbered individually in the EPRI NP906. You have that reference in John Ranlet's presentation.

I might just mention, the model was a straight cylindrical model. It did not have the miter cuts.

One other thing: There is a symmetry, and when I talk about analyses, essentially, we assume that the two main vents that were used in the EPRI model were similar to symmetry about this plane, and similarly, there is symmetry about this plane for the purposes of this particular model, and that is not 100 per cent true, but it is substantially true.

(Slide)

Just as background, the scaling laws, perhaps I will just say the scaling relations that were used, if we are talking about: If I let "S" be the scale factor, the pressures go as the scale factor, the time and velocities as the square root, the enthalpy flux as the seven halfs power, and in order to accomplish this enthalpy flux, we are obliged to increase the resistance inverse with the scale factor. When we do that -- We have to know, basically, what is the proper resistance for the prototypical plant, so we know what we are increasing, and we then have to locate the orifices in such a manner that we don't change the prototypical distribution.

(Slide)

What was done in the EPRI tests, in order to establish the orifice sizes that they used, was basically to run the small scale tests, not with the model that I showed, but with a single main vent model in one-twelfth scale, and one-thirty-first scale dry tests, established from flow calibrations, steady flow calibration tests that they had Reynolds number independence, and they then established for each downcomer a curve of the mass flow rate divided by the drywell pressure in the area of the downcomer, that function versus the pressure ratio wetwell to drywell. This is in dry tests.

They then said from the scaling relations, which I have presented previously, that this function of "M" dot over PA should go as the square root of scale factor. This gave them target curves, which they tried to match, assuming that the temperatures would be the same, model and prototype, which turned out not to have been a good assumption. The prototype runs at 135 degree presumed drywell; the model tests were at 70, and this caused some differences in the resistances that they actually built into the EPRI model, that John Ranlet has alluded to earlier.

The result was that the EPRI model was run with a ratio on the order of -- equivalent full scale FL/D ratio on the order of ten to 15 per cent higher than it should have been.

In any case, they established these target curves, and then they experimentally determined orifice sizes that were required for either the vent, or the individual downcomers to match the target curves. What I mean by that is they did flow calibration tests where they measured the velocity profiles at the exit of each of the downcomers, and having established the flow rate for each of the downcomers, using this square root, they got the target curves, and then put orifices in, and tried a few orifice sizes till they got a good match with what they thought -- what they thought was a good match with the target curves.

Now, that is a difficult thing to do experimentally. The tests themselves are a little bit tedious to run, you can't be trying every very closely matched orifice sizes, and I think what you wind up with, when you put the orifices in the individual downcomers, is an approximation to your target curves, but not a perfect match.

And so, when you use downcomer orifices, you have to appreciate that what you are getting is something which deviates from the desired distribution by some tolerance associated with this crude experimental matching technique, and in fact, I think what happened in the EPRI tests was that they mismatched somewhat. They got more flow out of the downcomer three pair ratio to the downcomer one pair than they were shooting for.

All right, that is one fact. I think the ratio is about 1.33 that they wound up with, in the worst case, between the highest downcomer and lowest, and I think that they would probably have preferred a number in the ratio of 1.2 or so to 1.

(Slide)

Now, also, these calibration tests were run with no water in the wetwell. All the downcomers had the same exit pressure. The flow distribution is then that flow distribution that goes with uniform exit pressure.

The exit pressure that you actually wind up with is not uniform. You have a very significant back pressure effect from the water inertia during the bubble growth period, and that is the function of the downcomer spacing, and it is also a function of the resistance in the lines which determine the flow rate. The more flow you have coming out, the more back pressure effect you might expect, so both downcomer spacing and flow rate are factors.

It happens in this particular case that the downcomer pair No. (3), which has the lowest flow resistance in the uniform calibration tests that EPRI ran, would be expected to have the highest flow rate, also happens to have the closest spacing, and so it has the highest bubble back pressure. And what happens then is that the calibration, or the flow distribution that goes with uniform back pressure is not too typical.

Now, within the Mark I vent system, one of the peculiarities of the flow resistance in the prototypical Mark I vent system is that the losses are dominated by the "T" losses. You have the "T" losses coming from the main vent into the ring header, and you have "T" losses coming within the ring header itself, after each downcomer comes off, and within the downcomer associated with the branching.

Those are the dominant losses in the system. And those losses are very much functions of the flow distribution. So the result is that if you had the wrong distribution of flow, you may measure, in a uniform back pressure flow calibration, the wrong resistance in the individual lines, and I will show you some estimates of that kind of thing.

The analytical calculations that I will talk about will show that when the flow itself is more uniform among the individual downcomers, the flow resistance is also more uniform. Now, I am talking about changes in direction. The flow resistance is still going to wind up non-uniform, but not as badly non-uniform as in the EPRI calibration.

(Slide)

This is just a schematic to illustrate the way the analysis works, and basically you break the prototypical system up into individual resistances. In the main vent we would have an inlet loss, some duct friction, a bend loss, and then a "T" loss coming into the ring header.

Along the ring header there would be some friction, and then a downcomer coming off, the first pair. You would have "T" losses. You would have additional -- and then within a downcomer you would have friction, a bend loss, and an exit loss.

In the ring header you have some additional "T" and friction losses as you come to the subsequent downcomer pairs. And this is the same analysis that is used, I think, by all the parties.

When you get the individual resistances, we get them from idel'chik, and that is a translation of a Russian document, actually, which gives generalized losses for individual downcomers -- or for individual components, rather.

(Slide)

I am expressing them here in terms of an incompressible loss coefficient. You can do a somewhat similar calculation with compressible fanoline (phonetic), but I think it is harder to talk about.

Then these are just combining relations when you have the individual components. For series you can get a total loss coefficient, and then for a combined series parallel flow you can get an overall number for the complete vent system.

(Slide)

So we go through the analysis and we get some numbers. Now, what I am showing here are for the three downcomer pairs, the total loss coefficient, and this first column of numbers is the loss coefficients if I define them this way as a drywell to downcomer exit divided by this "Q DC, J" would be the dynamic head at the downcomer exit for the "J" downcomer, "J" being one, two, or three.

So you get this kind of variation: 6.72 to 5.07, and a ratio of about 1.33 to 1 on total loss ratio.

That is using the calibration from the EPRI tests, and saying, okay, they had a certain pressure ratio, they had certain flow rates, and so I can compute out these numbers.

Now, I go and do an analytic calculation, taking these generalized relations from idel'chik. And when I do that, I wind up with this kind of a ratio, if I assume in the idel'chik calculation that I have a particular flow split. And the flow split I am using in this middle column is the same flow split as I have here in the first column, that is the experimentally determined flow split.

I get numbers that indicate a slightly higher ratio, and I think this is an error in the use of generalized relations in the idel'chik analysis. They are intended for use everywhere. They are not particularized to the system, and that is a crudity in the analytic calculation.

When I do the same calculation, however, assuming a uniform flow among the downcomer pairs, I get a much smaller ratio: 1.19 to 1, rather than 1.46. If I say that the analytic calculation is high, just because of the relations themselves not being a very good description of the actual Mark I system, and this 1.46 number ought to be 1.33, that would be like saying the 1.19 number would come down to like 1.08.

All right, now the idel'chik does give pretty good agreement on the averages, and it gives, I think, reasonable agreement on the loss ratios, but one of the things that comes out of this clearly is that the overall loss coefficient ratio does decrease significantly if the flow is more uniform.

(Slide)

Now, the other thing in talking about flow distribution: if I come from this point downstream, you might expect that the flow distribution would depend on the ratio of losses from this point -- this is -- all the downcomers draw from a common pressure at this point, and the losses from here to the downcomer exits, that ratio of resistances, say, for this path, as opposed to this path, is what would determine the flow ratio.

So, the next slide covers that resistance itself.

(Slide)

What I am showing you now is: If I had the prototype or the vent orifice configuration, I wouldn't be affecting the resistance from the ring header to the downcomer exit, because I am not putting any orifices in there. I would get -- and this is entirely analytic, all these calcualtions -- I would get for the experimental distribution these kinds of flow resistances, or a ratio of about 1.25 to 1, if I compute a mass ratio of flows downcomer three to one. If I did the same thing for the uniform distribution, about 1.17, and what I notice is that I would have reduced the flow ratio about 6.6, or say seven percent in going from experimental distribution down to a uniform distribution. In other words, if I base my losses on an experimental distribution, or on a uniform distribution, the uniform distribution would give me such a more uniform flow loss coefficient ratio that I would then, for given back pressure conditions, get maybe seven per cent less differences in the flow.

But when you put an orifice in the downcomers, what you are doing now is you are adding, and particularly in this one-twelfth scale model test, you are adding a very large resistance in the lines. That resistance, whether it is split orifice, or downcomer orifice tends to dominate. Your "T" losses no longer make much difference, and so when you do the same calculation, you get virtually the same kind of flow ratios, and very little change.

DR. CATTON: Would you repeat that last statement again?

DR. KOSSON: I said that when you put an orifice in the downcomers, you are no longer sensitive in terms of the loss coefficient of the individual legs to the flow distribution. In other words, it now gives you virtually -- you have built in a large resistance, and that resistance tends to stay independent of the flow distribution.

DR. CATTON: Then you said something else.

Didn't you make a conclusion about the flow being independent of the orifice location?

DR. PLESSET: No, he didn't say that, not yet.

DR. KOSSON: I have been talking about going from the experimental distribution to a uniform distribution. Now, we don't know, in the wet tests, what the actual distribution is. We don't have a good computer program which tells us with coupled vent system, and bubble growth, and wetwell pressurization what the flow is at all times in the system, so we have to infer, now, what is the effect of bubble back pressure on the flow distribution.

(Slide)

So, just for illustrative purposes, I have in this a description of a Rayleigh bubble, a modified Rayleigh bubble calculation, in which we feed flow into a bubble, which is contained within a pool of finite area, and we have these kinds of relations for the bubble growth with time.

(Slide)

That enables you to compute a bubble pressure versus time, and a flow ratio, and what I am showing here is the kinds of back bubble pressure that we would get versus time, using th؛ downcomer orifices for the highest and lowest flow from the EPRI downcomer orifice flow calibration, using that for the flow rates.

Then with these ratios, computing now the ratio of flows that would come out. I haven't displayed here drywell ressure that would come up also, and follow a little higher :han this curve, but what it shows is that the ratio of flows, downcomer three to one, is actually even less than one, during, say, the first 50 miliseconds. The time range of interest here is about 100 miliseconds to impact, so that for roughly half the time, what you see is that the mass ratio really is nearly uniform, and the reason for this is the fact that this downcomer three, which has very little resistance, does have higher bubble back pressures, because of its smaller pool area, primarily, and a little bit also because of its lower resistance.

DR. PLESSET: It is not ver-- important, I believe, but where did that modified Rayleigh equation come from?

DR. KOSSON: Actually, I used some relations that were in a report developed by Valondoni for the SRV bubble calculations, and I stripped down some of the terms, because I was mostly interested in just the Rayleigh bubble, without the compressibility terms in it. So I retained the finite pool size terms, without the compressibility terms.

DR. PLESSET: Did you say Valondoni?

DR. KOSSON: Valondoni. I think that name crops up somewhere else.

DR. CATTON: This was a fully incompressible analysis then?

DR. KOSSON: Not fully, in the sense that--

DR. CATTON: The bubble is incompressible--

DR. KOSSON: It is an adiabatic bubble, no heat transfer in the bubble.

DR. CATTON: I guess I misunderstood you. I thought you said you took the compressibility effects out.

DR. KOSSON: There are some compressibility terms in Valondoni's equations that I eliminated for this purpose.

DR. PLESSET: Compressibility of the water.

DR. KOSSON: No, the compressibility terms for the air.

DR. PLESSET: For the adiabatic.

What other compressibility effect are you thinking of, or was he thinking of? It may not be fair to ask you.

DR. KOSSON: I don't think I am in a position -- I could perhaps go back over my notes, and see exactly what I left out, but I think I will garble it, if I try to tell you now.

Let me back up a minute. My purpose here is not to say, "This is the bubble pressure," not at all. All I am trying to say here is that there is a significant bubble back pressure, and this leads to a uniform flow, really, for a significant period of the time of interest. That is all I am trying to get across here, not that these numbers are correct for bubble back pressure. These may be off, but that the flow is substantially uniform for perhaps the period of interest.

DR. BUSH: How sensitive is that model to the finite versus infinite boundary situation?

DR. KOSSON: I think it is quite sensitive.

DR. BUSH: I suspect it might be.

DR. KOSSON: Yes, the "AP" term is a big factor.

When I did this, I did allow for -- there was quite a difference, more than a two-to-one variation in the pool area from the downcomer (1) and (3), because I gave downcomer (1) essentially half that miter bend.

DR. BUSH: So a quasi-infinite system would behave quite a bit differently then?

DR. KOSSON: Yes.

DR. PLESSET: I thought the NRC had some calculations of bubble growth and confined volumes.

Am I wrong in that, that Livermore was doing something of this kind?

MR. GRIMES: Yes, we have some -- The film that we hope to show is a bubble model in a finite pool.

DR. PLESSET: Analytic?

MR. GRIMES: An analytic solution, yes.

That, however, is a two-dimensional analysis, and we could not extend that particular model to do an investigation of three-dimensional bubble back pressure effect.

What we did here was -- let me start over again.

We have a number of sources for bubbles growing in finite or infinite pools, and Dr. Kosson has taken material readily available to us to investigate this particular effect, because no one of all the different sources was readily suitable to this analysis.

DR. PLESSET: This two-dimensional analysis, in what sense was it two-dimensional?

MR. GRIMES: It looked at the plane of bubble growth. It did not look at bubbles interacting, as you would get in two pairs of downcomers located next to each other. We couldn't look at how two bubbles -- the flow along the vent header.

The typical analyses that have been commonly used look at the plane of the torus, and a bubble growing in a radial dimension of the torus.

MR. CATTON: Cylindrical bubbles.

MR. GRIMES: Infinitely cylindrical bubbles, or slab bubbles of finite.

DR. BUSH: Aerial areas.

DR. PLESSET: That is the best you have.

MR. GRIMES: So far.

DR. PLESSET: I am disappointed.

MR. GRIMES: So are we.

DR. PLESSET: I wonder what this is, if this is any good.

MR. GRIMES: Like Dr. Kosson pointed out--

DR. KOSSON: I am not trying to do a good bubble calculation, please.

All I wanted to illustrate was there is a reason for perhaps using a resistance -- When you size your orifices, there is a reason for perhaps using a resistance ratio between the third and first downcomer piers that is a little bit less than what was used in the EPRI tests.

That is all I am trying to motivate there.

DR. PLESSET: Fine, I think that is reasonable.

MR. GRIMES: The only point that we feel we have to make to support the Staff's position is that there is a sufficient concern about the potential flow distribution in the Mark I vent system that would lead to a flatter pool surface than one would achieve in a load definition technique.

DR. PLESSET: Now, you are thinking of the prototype?

MR. GRIMES: Yes.

DR. PLESSET: And you are trying to address the question of how these models can help you, is my understanding, right?

MR. GRIMES: Yes.

DR. BUSH: Basically it is a sensitivity study. You are not trying to quantify. You are looking at the differences in establishing which terms, or which factors are the most significant.

MR. GRIMES: We are trying to make a decision regarding whether the load definition technique for defining header sweep time was reasonable, or close, or prototypical to the extent that we continue to pursue the three-dimensional analysis of pool swell, and proceed with implementation, or whether we would require additional conservatism in the load specification to cover an uncertainty associated with the flatter pool. And the conclusion that we reached was that we weren't sufficiently confident that the flow distribution in the EPRI tests that lead to their header sweep times were sufficiently prototypical, and so we required that they base the header sweep time on the main vent orifice tests, which caused a substantially flatter pool.

DR. PLESSET: That is reasonable, and conservative too, I take it?

MR. GRIMES: Yes, we definitely conclude it is conservative.

DR. PLESSET: Yes, I think that is clear.

DR. KOSSON: I wanted to come back to this particular viewgraph.

(Slide)

You will note that if you do analytic calculations with either the split orifice or the downcomer orifices, and compute the flow ratios, one of the things you notice is that the computed flow ratios are essentially the same in the two tests. That is, the split orifice tests simply scaled up the diameters uniformly from the downcomer orifice diameters. I think they were 20 per cent larger, or something of that sort, and the result was that the relative ratio of resistances didn't change between those two tests.

So what you would expect to get in terms of a flow distribution split, or "M" dot three over "M" dot one, is essentially the same in the two tests.

(Slide)

Now, this is a curve which is maybe a little bit different from the one that Var Tashjian presented, but the same information. This is essentially the sweep time, or impact delay time versus position, where this would be the sweep time, I guess, for here down to here, and what this compares, say, is the split orifice and the downcomer orifice are relatively on top of each other, as I think you would expect from analysis.

The vent orifice is here, and this curve also shows the LDR interpolation technique that we feel is not representative, and there was a clear difference between the vent and the split, and downcomer orifice tests.

Now, another thing you might appreciate is that these two tests, while they had very similar pool swell shapes, and sweep times, had different capacitance values.

To me it seems that capacitance is a second order kind of effect on sweep time.

The main vent orifice did have too much mass capacitance, but the fact that these two agree, and had different capacitance is to me an indication that the areas introduced in the use of this curve, by having the wrong capacitance, are not really a very strong consideration.

(Slide)

So now it is just conclusions, and these are essentially stating the reasoning again: that we had about the same distribution, both from analysis and from tests in the split orifice and downcomer orifice cases, and the indication is from both that they provide similar sweep times.

The analysis indicates that the split and downcomer orifice tests probably had an excessive flow ratio, and the effect of capacitance does not seem particularly important to sweep time.

DR. CATTON: Nos. (2) and (3) are sort of intertwined, aren't they? They would be hard to separate.

DR. KOSSON: No, what I am saying here is: analysis indicates that these tests, both split and downcomer orifice, probably had an excessive flow ratio, because they had an excessive resistance ratio.

In No. (3) I am saying the capacitance is not particularly important.

It is my feeling that the vent orifice tests provide the most prototypical load distribution. It is probably not compromised excessively by the mass capacitance effects, and therefore, we feel that should be used for sweep time.

DR. PLESSET: Thank you, Dr. Kosson.

Any other comments?

I think we will have a ten-minute break--

MR. STEINER: Just a brief summary, in case we lost our philosophy in all the technical details:

There are two main areas of difference between the NRC Staff criteria and the Mark I owners' position as we have described them: the three multipliers for the net upload, and the sweep time applied to the structures, but those are the two main differences.

DR. PLESSET: Do you feel that these are consequential?

MR. STEINER: Well, it turns out, for the upload, for example, the multiplier turns out to be 21.5 per cent. About 15 per cent, I believe, is due to the uncertainty on the 3-D effect, and about 15 per cent is applied to a relatively large number, from which another relatively large number is subtracted to get the net upload.

You multiply 15 per cent by a large number, and you get a very large difference from that then, so it is not really 15 per cent any more. The net upload is increased by substantially more than 15 per cent.

DR. ZUDANS: That is correct.

DR. PLESSET: Chris, do you want to--

MR. GRIMES: I have two different approaches to attack that argument:

One would be that we have a substantial amount of pool swell data. A substantial amount of testing has been conducted.

DR. PLESSET: Model testing.

MR. GRIMES: Model testing. And we have reached the point in time where we have tried to coalesce this information and take action, to restore the margins of safety in the plant designs. We have done our assessment on the basis of the knowledge available at hand, without consideration for its potential impact or consequences.

In considering our position, we did go back and reassess the load combinations, and tried to eliminate excessive conservatisms, while still being able to maintain margins for uncertainty.

The action that we took was to agree to reduce the service level for the DBA plus SRV load combination. I believe that is an issue that may have been addressed at the subcommittee on the Mark II review.

We dropped the service level assignment to see -- because the SRV contribution for the Design Basis Accident was substantial, was substantially more, in fact, than the consequence of the 15 per cent.

Also we incorporated in our criteria an allowable technique to reduce these margins, where demonstrated conservatisms in the tests for each plant in the configuration could be quantified. We felt that was another reasonable way to eliminate excessive conservatisms, while still being able to maintain a quantified margin of safety in the load specification.

DR. PLESSET: Let us see if Mr. Steiner wants to add anything.

MR. STEINER: Well, we agree that there may be room in some of the initial conditions, for example, to reduce the conservatisms. That is only one factor in that multiplier that is being applied.

DR. ZUDANS: I wonder if you could clarify, maybe

I misunderstood you. You said the multiplier is applied to a large number, from which another large number is subtracted.

MR. STEINER: Well, it is really the difference between two large numbers.

You have two large numbers. You subtract one large number from another large number to get the net upload--

DR. ZUDANS: And then multiply.

MR. STEINER: But the 15 per cent is multiplied, not by the difference, but by one of the large numbers.

DR. ZUDANS: No, it doesn't say so.

MR. STEINER: Well, I don't know exactly what the value is.

DR. ZUDANS: It doesn't say that.

MR. STEINER: Well, one number is the net upload, as determined from the quarter scale tests, and from that -- as adjusted to the actual plant. You subtract the weight of the water, for example.

DR. ZUDANS: And then you multiply with the 15 per cent.

MR. STEINER: No.

DR. ZUDANS: It says: upload equals upload mean plus 21 per cent times upload mean, and this is the large number?

MR. STEINER: One of the large numbers.

DR. ZUDANS: I also wanted to ask a question--

Maybe I just don't have all the details:

This second issue of time, impact. Does the load description allow the header to be impacted at different times, at different locations, as a function of this passage time?

MR. GRIMES: The issue there is that they have specified impact timing where you achieve impact at one point on the header, at one point in time, and then the load will sweep along the header, and the time that it takes to get from one point to another point in the Load Definition Report was established by the split orifice tests.

If you will recall Dr. Kosson's slide that showed you the time versus position curve. We have essentially dropped that in half by specifying that the main vent orifice tests should be used.

DR. ZUDANS: Which means--

MR. GRIMES: Which means that it is impacted twice as fast.

DR. ZUDANS: Okay, and more of the header will be exposed to the load in a shorter time.

MR. GRIMES: Well, the same amount of the header is exposed in either case, but it is a shorter time by about half.

DR. PLESSET: This is just the time that you are talking about?

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Is there a large reservation about this?

MR. STEINER: Well, it does make a difference in the structures above the pool.

Maybe some of our structural people would care to comment on the decrease in sweep time, what it means to the structure.

Well, it obviously does mean more if you decrease the amount of time over which the load is applied, and it is not just for the structure, but for other structures above the pool, which are more difficult to design.

MR. DEARDORFF: Art Deardorff, from Nutec.

This does have a significant effect on calculating the impact and drag loads for structures such as the cap locks, vent header collecters, and the overall vent system reactions, as they contribute to the overall uplift of the torus, the total load applied to the vent system that then is applied back into the ring girder through the vent header support system.

You keep adding these conservatisms on there, and finally they catch up with you.

DR. ZUDANS: I have one more question:

When the pool surface is flatter, the speed of the pool surface should be lower than when it is less flat, isn't that true? This is at the time it impacts the header.

MR. GRIMES: Well, yes, and no.

The yes aspect is that it is true if we were in fact defining the average velocity from the EPRI tests, but the average velocities derived from the plant specific tests, because it will vary with the configuration of the plant. The EPRI tests we used to establish a distribution that will be applied to that average.

The no aspect is that we were evaluating EPRI tests to determine what kind of variation there would be in the average observed in the QSTF tests, and the conclusion was that the main vent orifice tests would give us a better representation, a conservative representation of how the 2-D test should be longitudinally applied.

DR. BUSH: I think in the very early days of the Mark I, the first one, that the concept was that the SRV's would not blow into the wetwell; they blew into the drywell. Now, you mentioned the contribution of the SRV's.

Have you looked at the implications if they returned to the old system?

MR. GRIMES: Blow into the drywell?

DR. BUSH: Yes. Obviously it isn't in particular--

MR. GRIMES: We haven't considered it in the context of this program, but I do know, for example, that one plant is considering piping the safeties, which a number of plants have safety valves that still blow into the drywell. One of the plants is considering piping the safety valves into the torus, to preclude having an event of that type, and we have been, in one other subject, discussing the potential consequences of safety lifts in the drywell. It is equivalent to like a small break accident, if the valve sticks open.

DR. BUSH: But it also has some positive effects too, if it doesn't stick open.

MR. GRIMES: Well, from the standpoint of that it doesn't produce any loads in the torus is about the only positive aspect I can think of.

DR. BUSH: That could be a positive effect.

MR. GRIMES: Well, the purpose of this program is trying to establish a basis for feeling comfortable that you can blow the SRV's into the torus.

DR. BUSH: I know that. I wish you lots of luck.

DR. PLESSET: Any other comments?

Let us have our ten-minute break now.

(A short recess was taken.)

DR. PLESSET: On the record.

Mr. Steiner, I think the ball is in your court for the next item.

MR. STEINER: Bill Kennedy, from Acurex will describe the deflector load approach, primarily our concern about the method to calculate loads based on a semi-empirical approach.

DR. KENNEDY: Bill Kennedy, from the Acurex

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Corporation, representing General Electric.

(Slide)

The topic that I wish to address is the vent header deflector loads, and I wish to cover this by giving you a brief problem description, and a very brief description of the present load prediction metodology, a comparison with the quarter scale tests results, and then how the NRC modifications to this method affects the load situation.

(Slide)

The deflector is a piece of structural steel pipe, pretty heavy wall, like Schedule 160, with further deflector structures welded to it, to give it greater breadth, located between the pool surface and the header, for the purpose of splitting the rising surface of the water, and preventing high velocity impact on the header.

There are four types that are under consideration: a straight pipe, a pipe with equal leg angles welded to each side, a pipe with tees welded to it, and a wedge-shaped type, which was used on Duane Arnold.

(Slide)

This is a film tracing from a quarter scale movie that shows a typical performance of the deflector. These are times from the start of the event in miliseconds, and these are surface locations of the water surface, and of the bubble, and the effect, I think you can see, is to split the -- there

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is really nothing but froth, or air in a region roughly defined thusly, protecting this part of the header from serious impact.

DR. ZUDANS: Where are the loads from the deflector transferred to?

DR. KENNEDY: The deflector loads are taken out in the vertical ring header support columns, every 20 feet or so, and then down to the ring girder.

DR. ZUDANS: They are not transferred to the header itself?

DR. KENNEDY: No.

(Slide)

The method that we derived to predict the loads on the header are twofold:

One, we could make direct use of the quarter scale test data, appropriately scaled. We would like to do this in all cases, but the method of measuring this did not get in place in the quarter scale test program until about two-thirds of the way through the program. As a consequence, only about half the plants have measured deflector loads. The other half, we must depend on some quasi-empirical, or analytical method to predict their loads, but for those plants that we do have measurements, we would elect to use the measured loads.

The analysis for the remaining plants would consist of two parts: a flow field prediction, which would calculate the acceleration, the velocity, and displacement history of the water surface in the region of the deflector, and a drag measurement based on this flow field.

The flow field is calculated based on a simple one-dimensional slab bubble model.

(Slide)

When I say "calculated," it is really a combination of a measurement from the quarter scale, and a calculation procedure. The measurement involves measuring the terminal speed of the water, from movie data, and adjusting the affect of mass in this slab bubble model, to give agreement with the terminal speed, as measured in the movies.

The reason for using any model at all is because of the difficulty in getting acceleration and velocities early in time from the movie data.

When you differentiate the displacement curves twice, small data errors can introduce large anomalies in the acceleration curve. So we used this simple model to give us a physically correct early-time acceleration history, but again, we adjusted such that it agrees with the quarter scale movie data terminal speed.

(Slide)

Here is an example. This is the effective mass of the slab bubble. It has been adjusted so that we get the correct terminal speed, but it gives us a realistic and smooth acceleration time history, early in time, when it is difficult to deduce that from the movie data.

(Slide)

The second part of the analysis involves taking that flow field, and calculating a load, and the load is assumed to consist of impact, which is a term we use to mean the exchange of momentum from the uniformly rising pool, when it impacts the structure; the structure itself, the deflector, will slow down the water adjacent to the deflector, and that momentum will be delivered to the deflector.

If the pool continues to accelerate, there will be acceleration drag. As you immerse it, it will start to float, and ultimately there will be something that we would call "a steady drag," although it is not very influential in the load definition.

The impact and the steady drag would be predicted by a classical drag coefficient equation that would say that the forces, the drag coefficient, which is a function of immersion depth times a maximum deflector projected area, and a local dynamic pressure, and the acceleration and buoyancy would be given by an equation which would relate the force to the local acceleration times the hydrodynamic mass and the displaced mass, and the buoyancy would be the displaced mass times the acceleration of gravity.

That is the essence of the method, without the numerical considerations, really, given yet. We will get to those in a minute.

The method of measurement of the loads in the quarter scale is as follows:

The deflector-- a scale model deflector was put in all tests where it will be used in a real plant, and the method that we used to measure the loads was on two of the four standard tests that were run. The deflector was attached to the facility itself. The load path of the drag was into the deflector, and into the facility, not into the vent headers.

On the other two tests of identical test conditions, the load path was directly into the vent header. By subtracting the measured load from these two tests from the tests where the load path did not go through the header, the net load delivered to the deflector could be deduced.

(Slide)

Here is a table showing what measurements we did acquire, and the range of plant parameters that they represent, and the remaining plants for which data is not available, and the range of parameters that they represent.

These are test parameters that we think influence the loads on the deflector:

One of the most important is the clearance from -- the initial clearance from the water surface to the deflector, and of the measurements that we have made, we range from the deflector resting right on the water surface, to a full scale stand-off distance of 21 inches. In other remaining plants, where the analysis technique must be used, zero to 14 inches.

The width as measured was 25 to 30, the remaining plants 20 to 26.

The pressurization rates measured 46 to 74, to be analyzed, 54 to 74.

And the submergence.

The point of the slide is that we think we have covered in the measurements the range of the important parameters that govern the load. We don't have a plant that is way out of the range of existing measurements.

(Slide)

Here are typical results from three configurations, using the existing method. Here is quarter scale measurement, the circles. This is acceleration component, impact and steady drag, and the summation, being this bounding curve.

This was for a configuration where the deflector sat right on the water, so there would be no initial impact spike, there would be just acceleration and steady drag.

(Slide)

The same configuration with an intermediate water stand-off distance yielded this type of agreement between calculation procedure and measurement.

This is the initial impact spike that now occurs because the water can accelerate before hitting the deflector.

(Slide)

The third example, with a further stand-off distance is shown here.

We have, all told, I think, twelve configurations that were measured.

(Slide)

Six plants with some variation in each plant configuration of something, like the water clearance, I think, or perhaps -- I think that was the major variation within a given plant configuration.

In all cases, as currently structured, deals something like either no margin to a factor of two over-prediction on the peak load, with like an average over-prediction of 33 per cent.

(Slide)

We thought that was a comfortable position, but Professor Sonin says, "Yes, but..." and he was right.

What he said was that you have constructed a drag coefficient versus immersion depth curve, which when combined with your assumed velocity field gives you a reasonable prediction, but he said, "I think that you have over-predicted the velocity field, and underestimated the drag coefficient," and he worried that maybe some range of test parameters would make this potential mismatch non-conservative. And he cited a reference from Von Karman at I think 29 on the impact of sea plane floats, and Von Karman said that the drag coefficient on a wedge-shaped body ought to achieve a value of pi, at full immersion.

And we have found that a drag coefficient looking more like one, in full immersion would agree with the data. So now we have the difficulty of -- with our conservative velocity field, and the NRC suggested drag coefficient, a substantial over-prediction of the measurement. I think you see the potential for a negative factor of three, two and a half. And we ran some examples, and indeed that is the situation at the moment.

First of all, the NRC published the criteria, and stated that the loads from the quarter scale could be used, but that they would put in analytically the initial impact spike, and we see no objection to that. It will have a minor increase in structure response, but I don't think it will make a major effect on the loads.

(Slide)

In the loads that are based on the analysis, their conclusion was that they felt the velocity field prediction was indeed conservative, but that the drag coefficient wasn't, and they published their own drag coefficients.

If we apply those -- If we apply our drag coefficient in combination with our velocity predictions, we get typically, as you saw, something like 20 to 30 per cent over-prediction on the loads. If we apply the drag coefficient which achieves a value of pi in full immersion, we would predict something anywhere from two and a half to three times the measured load, and that can be a substantial penalty on the design.

So the resolution that I would propose at this point, and we have had some discussions -- I don't have the resolution ready yet, but it is in work -- and that is to allow the drag of the deflector itself to influence the acceleration of the water mass. In other words, as the drag of the body is being felt by this rising mass of water, it will indeed locally slow down this slug of water, and we did not account for that in the analysis. I have done some preliminary calculations, and I feel confident that if we include that term in the overall momentum equation for the water rise, we can get the Von Karman drag coefficient, and our velocity field to once again agree with the data.

DR. CATTON: That would mean that you would have to solve the full velocity field, wouldn't you? You would have as boundary conditions your measured surface velocities, and the bubble growth. You would have to solve the intermediate part.

DR. KENNEDY: I feel we could still do it in a one-dimensional sense, where we define a certain mass of water that is locally involved with the impact of the deflector, and state that it is being accelerated by the net pressure forces acting on it, including the force of the drag of the deflector, which we have not put into our--

DR. CATTON: So you are arguing that you would slow down that whole mass?

DR. KENNEDY: The mass locally adjacent to the deflector, and there is good evidence that this happens in the tests. We wouldn't slow anything down on here, obviously, but --

(Slide)

You will notice that at around 390 to 400 miliseconds the water surface is achieving a steady speed, where at the same time: 370, 380, 390, 400, the bubble top indicates that the water in this region is being seriously retarded in acceleration, and I am confident that it is because of the drag imposed by the deflector.

In our analysis, we are assuming that the water continues to accelerate as if it were not impeded by the deflector.

DR. CATTON: It seems to me that if you want to do something rational, you are going to have to solve, at least, that problem two-dimensionally as an intrinsic flow problem. If you don't--

DR. KENNEDY: That is possible, but--

DR. CATTON: If you don't, it is all just argument, most of it.

DR. KENNEDY: I would approach it from a momentum integral standpoint, where the sum bounding volume empirically adjusted, by our quarter scale data base, would be used -- We write the momentum integral on this mass, including the drag term of the deflector, and we have done some initial calculations of the mass, roughly defined by the boundaries of the bubble and of the surface, that do indeed give pretty good agreement.

DR. CATTON: What happens when you change that volume?

DR. KENNEDY: Clearly, the answer changes, so it is a coefficient that has to come out--

DR. CATTON: So it is a highly empirical method of correlating your data, is what you are telling me.

DR. KENNEDY: It is an empirical method.

DR. SCHROCK: Could I ask about this " C_D of (Y)"?

Could you describe what that looks like? Could you give me a little clearer picture of what you mean there?

Does "C_D " continue to increase, or change--

DR. KENNEDY: This is "C_D" as a function of one, and it is -- At impact it achieves an initial value which is derived from the DSI cylindrical impact test data, and then falls, according to a fit from that data, and this is matched to the Von Karman analysis, which achieves the value of pi, I think, at full immersion.

DR. SCHROCK: What does full immersion mean? Is that the deflector?

DR. KENNEDY: That is when the undeflected water surface has risen to the top.

DR. SCHROCK: So, you are taking it to be constant, after it is fully submerged, by that definition?

DR. KENNEDY: And then decays down to, ultimately, a value with a ventilated wake. Ultimately it has to achieve -- if this is a steady flow past a wedge, with a ventilated wake, it should have a drag coefficient of approximately .7.

So the history of the drag coefficient looks like this, according to the NRC criteria, and the one that is in the current LDR methodology looks like this.

So that is where we stand at the moment. It is important to us to come to some resolution, because a factor of two and a half to three is a noticeable penalty on the structure.

DR. PLESSET: It seems to me that this is a synthetic approach that may have unfortunate results.

I don't think the drag curve looks like that. Do you really, the one you drew me, that you showed us a little while ago?

DR. KENNEDY: The one that goes up to pi?

DR. PLESSET: The one that goes up, and then down, and then up again.

Do you believe that?

DR. KENNEDY: What I believe -- Let me put it on the table. I think the real drag coefficient -- Professor Sonin will no doubt comment on this.

DR. PLESSET: Maybe we can wait.

We don't need to press you. We will ask him.

DR. KENNEDY: I think I would like to state where I think it is, though. I am pretty sure that this is correct and conservative, because we have a set of fairly well-run tests from DSI of cylinders being impacted into flat pools, and the loads were measured by pressure transducers.

DR. PLESSET: What was the time response like?

DR. KENNEDY: Their natural frequencies were probably 50 kiliHertz or something.

They ran quite a few tests, and got consistent results, which, for a cylinder, shows a very sharp initial impact spike, and then something looking like an expedential decay.

Then Von Karman's analysis was for a wedge with not this blunt leading edge, but with a sharp wedge leading edge, based on the hydrodynamic mass as a function of immersion, and it indeed shows something starting from zero and going to pi, and then it would impulsively drop to separated wedge value at full immersion.

I think what is happening, what really happens, is this part of the curve--

DR. PLESSET: You got those two shifted in time, right? Those two drag--

DR. SONIN: Maybe I can explain that later.

DR PLESSET: All right. We will let him finish.

DR. KENNEDY: I think what is really happening is that this part of the curve is correct. Momentum is being exchanged between the drag object and the water, and that some of the velocity field has already been established by this blunt object, such that this peak probably isn't achieved in a blunt wedge.

I would guess this is correct, and something in here, greater than what we initially assumed, but less than pi is probably what happens, is my guess.

DR. PLESSET: Okay, thank you.

DR. CATTON: Could you put back on this diagram?

DR. KENNEDY: Certainly.

(Slide) [The pool and bubble profiles, slide No. 4]

DR. CATTON: You have your bubble surface velocities from this diagram.

DR. KENNEDY: That is right.

DR. CATTON: Right below the header.

What happens if you just use those?

DR. KENNEDY: That is in the method at the moment, and that is an alternate. We have a function that we attempted to multiply the free surface here by some -- what we called a "turndown function," or "desolaration function," that was deduced from the bubble top speed here. That is an alternate procedure that might yield the same results, although we were trying to be conservative initially in use of that. We might go back and get less conservative--

DR. CATTON: Where is the peak on that drag curve, with respect to those times you have listed on this figure?

DR. KENNEDY: It varies, of course, from one example to another.

DR. CATTON: Could you try?

DR. KENNEDY: Sure.

I think this represents this case, if I am not mistaken.

DR. CATTON: What I was trying to do was get an idea as to when the peak drag coefficient occurs, that you have in your diagram, relative to the picture.

DR. KENNEDY: Well, I think in all cases that the peak drag coefficient will occur at full immersion, which will be something in the order of -- oh, I don't know -- 330 to 340 miliseconds.

DR. CATTON: So you are using the velocity at 330 miliseconds.

DR. KENNEDY: That is correct, inferred from an unimpeded acceleration.

DR. SCHROCK: You don't take the peak of that velocity, like the 340, that is not used, but it is the one that is way off out of this picture?

DR. KENNEDY: What was done was that we -- You see, the deflector kind of screws up the observations in the region of where you would like the measurements, so we went off 18 inches off centerline, to make our measurements, and found a correlation between the velocities at 18 inches off centerline, and centerline velocities, for those cases where there were not a deflector.

So in the body of the analysis, we would take the data, 18 inches off centerline, and apply a ratio: one over .9, or 1.11 factor, to account for pool curvature.

DR. SCHROCK: The reason I was puzzled earlier is because I think this analogy to the aircraft landing is not really valid. Once the thing is totally submerged, the flow field continues to be substantially influenced by the other structural things present, and the way the bubbles are growing, and how they interfere with each other.

And so, from the instant that it becomes fully submerged, from that point onward, I don't have a very clear picture of the rationale then for the drag coefficient, I guess. From what I have heard, it doesn't sound to me as though it is going to be very meaningful, because it is based on a velocity which is very vaguely defined, and it is the velocity squared.

DR. CATTON: And 340 is about the time when it is fully submerged, isn't it?

DR. KENNEDY: Approximately, yes, 330 to 340.

DR. BUSH: If you did away with the deflector, the initial response of the object would be quite a bit different -- well, not than the actual field, itself. The moving field, I think it would lag a lot, and then it would start to lead, but I am not sure what would happen in the leading aspect, once you started to accelerate. I am talking now of the structural response. I don't care about the water response, because, after all, the thing that we are concerned with ultimately is what happens to the structure, because this is a very short pulse.

DR. KENNEDY: It is really not. Correct me if I am wrong, but this is almost load following for the structure, is it not?

DR. BUSH: In the first tenth, or two tenths of a second?

DR, KENNEDY: I am informed by the structural analyst that this shape, the typical deflectors that are going in there are almost load following. Now, they won't follow this initial impact spike.

DR. BUSH: We are not talking about the deflectors. I am talking about throwing the deflectors away, and looking at the response of the whole system.

DR. KENNEDY: I am sorry. "Throwing the deflector away"?

DR. BUSH: Yes, in other words, looking at the response of the headers, et cetera, without the deflectors in there. I am just saying I think that their inertial response would lag, and because the inertial effects would lag substantially, then I don't know what happens after it does accelerate.

DR. KENNEDY: If you leave the deflector out of the problem -- That is how we started all the testing.

DR. BUSH: That is right.

DR. KENNEDY: Obviously the impact on the header is later in time than this, at a significantly higher velocity, and we have got a very high peak pressures.

DR. BUSH: Against the header?

DR. KENNEDY: Against the header.

DR. BUSH: I agree on that.

All I am saying is that you now have an inertial effect to the header itself, and I am wondering what the system response is, as contrasted with the load against the header.

DR. KENNEDY: Well, this protects the header from any impact, or a significant portion.

DR. BUSH: I grant that, but I think the magnitudes of the loads are a lot less than they are in slugging, for example.

MR. GRIMES: Professor Sonin will present the Staff's criteria for the deflector loads.

MR. SONIN: My name is Sonin, and I am from M.I.T., and I am a consultant to the NRC.

Bill Kennedy has gone over much of this, so I will try to make this brief.

(Slide)

The first slide shows the various types of vent header deflectors that are being considered by the owners.

Type (1), the pure cylinder, is in fact not being contemplated, if I understand it correctly, so we don't have to spend too much time on this.

MR. DEARDORFF: That is not a true statement.

MR. SONIN: That is not a true statement. That was made quite some time ago, but in any case, all the deflectors have basically two components:

One is the cylindrical part, which may or may not be there at the front, or totally, and the other is the 45 degree dead rise angle wedge, which is not there in Type (1), but is present in all the others, to some extent or other.

Now let me summarize the situation: There are two alternative ways that the NRC has accepted for the load specification on these devices. One -- Well, here is

Alternative (A):

(Slide)

Alternative (A) was proposed by the owners as being a load obtained from plant-specific quarter scale test facility tests, which simulate, automatically, both the pool swell process and the deflector impact directly.

In addition to that there are adjustments for 3-D pool swell effects, and the timing, which you have heard earlier.

Now, the NRC has accepted this, provided that the empirical impact spike for the initial cylindrical portion, if it is there, on the deflector, is put in, because the instrumentation for the QSTF load deflector loads was not always rapid enough to pick that up, so if that is put back in, it is all right.

We also require them to interpret the 3-D pool swell effects conservatively, as required by the NRC, and as you heard earlier this morning.

In addition, we have asked them to put in the effect of the inertia, due to the added mass of water during impact, and that inertia can be evaluated for simplicity as a fixed inertia, which is taken from the initial impulse associated with the impact.

Now, this, I think, is the simpler part of the specification.

(Slide)

Before I go on to that, let me show you the impulse due to initial impact on a cylinder that were required to be put in. This is derived from the EPRI report, from empirical data, and it says that at the point of initial impact you have essentially an effective force, which is equal to seven dynamic heads averaged over the diameter of the cylinder, and very rapidly, as the water advances over the cylinder, "d" is the cylinder diameter, so this parameter on the bottom would be one, if you fully immersed the cylinder.

Very rapidly that decays down to what amounts to a drag value, which is put back later. This is just initial impact that is in there.

(Slide)

Now, Alternative (B)--

DR. CATTON: So they have subtracted out the drag?

MR. SONIN: I beg your pardon?

DR. CATTON: They have subtracted a drag component from this particular -- or you have from this--

MR. SONIN: This is something that we have asked them to add to the empirical data. The empirical data is accurate for the slower times, whcih determine the drag automatically, but their instrumentation did not pick up this very rapid initial spike. So we just asked them to put this back in, so they would be sure to have in the specifications.

Alternative (B), and that is the one that there was some discussion of earlier, the owners postulate -- I have simplified this somewhat, but they postulate that the drag can be expressed in terms of one component, which is an impact transient and steady drag, okay, and another component which is essentially an acceleration and buoyancy drag.

What they do is they deduce the impact transient, the steady drag curve correlation, from available data, which is for constant velocity impact of an infinite, or semi-infinite flat pool on the deflector, and they deduce the other contribution due to acceleration drag from correlations which are available for uniformly accelerating flow, in fact from uniformly accelerating fully submerged flow.

Finally, in order to evaluate the magnitudes of these correlation formulas, they need the pool swell velocity and acceleration, and essentially they take that from plant-specific quarter scale test facility tests, without deflectors. Now, I say "essentially" because there is actually -- what they actually do is they use this model for an equivalent one-dimensional pool swell, and then adjust that to the empirical data, so as to match the empirical data, and derive those quantities from that model, which you heard about just earlier.

Now, the NRC feels that this approach, although as several people pointed out earlier this morning, this approach, although it is not accurate in this very complicated pool swell situation, is acceptable, provided all the ingredients are done conservatively. And to insure that all the ingredients are put in conservatively, we have made some changes in the way that we would like to see this applied.

First of all, instead of step No. (2)(a), the NRC differs from the owners in its steady drag for cylinders, and also it differs from the owners in its specifications of this step, of the impact transient on wedges. And I will go into both of these steps in a moment.

In addition, we also require, as before, that the added mass of the water be accounted for when these loads are applied to the structure, and the structural calculations carried out.

(Slide)

Now, first of all, regarding the impact transient and steady drag on cylinders, the owners specify something which has the initial impact -- This is a dimensionless force on the ordinate, and a dimensionless time on the axis. The NRC requires that there be an initial impact transient spike like this, derived from the EPRI data, followed by a steady drag value.

We differ somewhat from the owners' specification.

The owners specify the steady drag value as 1.2 up to about half immersion, and then it dropped to .5 after full immersion, for a cylinder now, and their argument is that the EPRI data gives you this 1.2 roughly, the EPRI data extended to dimensionless time of about .4_, at most, and they argue that the value of .5 should apply to a fully ventilated wake, and I take that from data on cavitating flows, with zero cavitation number, arguing that there is a direct analogy between a fully ventilated wake, and a cavitating flow with zero cavitation number.

Now, we interpret the EPRI data differently. The EPRI data for cylinder impact shows to us that there does not seem to be -- the data levels off as far as one can see, at least one cannot be sure that it does not level off. It levels off at values, which depend on the conditions of operation. In fact, one can argue that they should depend on the Froud numbers associated with the impact, and we do not see the value of .5, which is derived from zero number cavitation flows, necessarily applies, because zero number cavitation flows are analogous to infinite Froud number impacts, and we do not neccessarily have an infinite Froud number in the practical instances.

So, what we do is: the NRC derives this final study, the drag value from the EPRI data, by assuming that -- I mean, one can justify this, that if you have super-critical

Reynolds numbers during the impact, then in the final studies, the drag should be a function of the Froud number which is the remaining dimensionless parameter in the problem.

(Slide)

These are data points that scatter somewhat, because of the difficulties in determining this number exactly from the EPRI data, and we have drawn a best-fit curve through that, assuming that the drag coefficient is a function of Froud number.

We have leveled it off at this value, because we have looked, for example, at cavitating flows, which have some analogy to the ventilated wake problem, and in that you never see a drag coefficient which is higher than 1.4. So we have taken that as an upper limit. Also, you end up with the drag coefficient of .5, at zero cavitation number flows, which should be analogous, roughly to the case when you have infinite Froud number impact, and so we end up there.

This is essentially the best-fit curve, not a totally conservative one, but our assumption here is that the data scattered here is not intrinsic, there is nothing stochastic in this problem. It is just a matter of the nature of the measurements.

So this defines NRC specifications for cylinders.

For wedges, let us take a pure wedge first.

(Slide)

For pure wedges, one can see from dimensional grounds that a dimensionless force during the immersion process, after measuring "t" equals zero, from the time of impact, is some function of the angle times the dimensionless time, like this "h" being the height of the wedge.

This is a fairly straightforward argument, using dimensional analysis.

The function of deadrise angle beta, the coefficient here -- this is during the immersion process, and before the surface has reached the top part of the wedge. This function of beta was derived first by Von Karman in 1929, and he got this simple form based on a rather clever and simple argument for the process.

Now, Von Karman's argument was approximate, and had certain limitation. Wagner, a few years later did some more careful computations for this impact transient, and he obtained the limiting solutions for zero and 90 degree angles beta, and also for one point in between, and suggested this empirical formula, "empirical" now not meaning experimental, but emprical based on his computations, for all angles beta.

This is not an analytical expression here, but it correlates with the three points, the three angles beta that he made the computations for.

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The next slide shows the difference between

Von Karman and Wagner's computations for this coefficient alpha, between the dimensionless force and time, and it shows that at 45 degree angles, deadrise angles, for the wedge, the two are exactly the same, and we are of course concerned with 45 degree angles in these applications. And that the value of the coef icient alpha is pi for 45 degree angles, based on either one of those formulations.

Now, the questior is: What is correct? There are differences between Wagner and the simpler analysis, physical analysis, of Von Karman, and there is data which supports Wagner's correlation.

(Slide)

First of all, Mayo, in 1945, and I am referring to NACA Tech Note 1008, in this instance used essentially a Wagner type analysis, which was somewhat modified to include planing impact. In other words, you have an angle like this in addition to a straight horizontal wedge. He was doing this analysis for seaplane floats, and he compared his planing impact data with experiment, and he showed that if you multiply all of his numbers by .82 he essentially correlated with the experiments. In other words, the experiment was slightly below the data, or the analysis, based on Wagner's method.

Monaghan, in 1949, and here I am referring to Royal Aircraft Establishment Tech Note Aero 1989, that is not a date, that is the number, she did essentially a similar argument, or a similar analysis, and showed that it agreed with experiments up to about 40 degrees.

In 1950, Pierson published some careful theoretical performed calculations, which repeated Wagner's calculations for more angles, and he said that his calculations -- If you multiplied Wagner by 1.08, the resulting curve bounded all of his calculations for various angles beta.

So the question is: Which of these do you take, .82, Mayo's experiment said that Wagner was higher, and Pierson said that Wagner was slightly lower than his analysis.

Later more experiments have been done, for example, by Chuang, in the David Taylor Model Base, in Report 2268, in 1966, and in the Naval Ship Research and Development Center Report 3248, in 1970, which again showed that Wagner's type of theory was really pretty good, compared with experiments.

So we have some confidence that Wagner's theory applies, and hence, we define a dimensionless -- for a pure wedge, an impact transient of this sort, where this initial line goes up at a slope corresponding to the Wagner's theory, and as it happens, also Von Karman's theory for the 45 degrees.

(Slide)

Now, we know that this applies before the surface reached the top level of the wedge. We do extrapolate right up to the top level, and then -- I mean, eventually, it has to go -- It has to fall to the steady drag value corresponding to a ventilated wake for a wedge, which is about less than .7 slightly. So we know it has to go to this line. We have simply, rather arbitrarily let the line fall from a value of 3.14 to the .7, at a point about 1.5 dimensionless times.

DR. PLESSET: I think this is all very good. I think this is a very instructive thing, but as far as .8 or 1.0-something, I think that is relatively unimportant, because a slight variation in the angle of incidence would make a bigger effect than these other things.

Have you considered that?

MR. SONIN: The angle of incidence?

DR. PLESSET: You are taking the water as being the incidence normally on the wedge.

Is that correct?

MR. SONIN: Yes.

DR. PLESSET: If you had a slight deviation from the normal--

MR. SONIN: You mean if the wedge were misplaced?

DR. PLESSET: Well, the water is not coming up as a plane normal to the wedge. There is just no reason for it to do, is there?

MR. SONIN: Well, the wedge is at the center of the axis of symmetry of the torus.

DR. PLESSET: But that doesn't mean the water is going to be symmetrically incident.

DR. ZUDANS: The water is not intelligent enough to consider that.

DR. PLESSET: I don't question your result, but I think that to talk about 3.14159, or something, is maybe gilding the lily. I would be satisfied with 3.0.

MR. SONIN: Actually, so would I.

The reason I brought up the 1.08, and .82, I abandonned that, and essentially said 1.0, right? But I would take one rather than .5.

DR. PLESSET: Oh, yes.

MR. SONIN: And I will come back to that later, because the owners specification had it down here at about 1.4, and not at three.

DR. ZUDANS: I have a question:

On this Wagner's and Von Karman's curve, how would that merge with a plate, say, zero degree?

DR. PLESSET: That theory doesn't include that. That goes into compressibility effects.

DR. ZUDANS: Up to what point is that curve valid?

MR. SONIN: Chuang has gone into that in the references that I mentioned, and he says that Wagner's theory is quite accurate in angles above -- I forget whether it is 12 or 15 degrees. I really forget the exact number, but it is way below 45 degrees.

I am talking about the deadrise angle from the horizontal.

DR. ZUDANS: Yes, I understand.

MR. SONIN: Now, even at, say, seven degrees, if you look at his numbers, he is not that far off. It is only when you get to a few degrees that the air effect that Dr. Plesset mentioned really becomes important.

DR. ZUDANS: If that is the case, what happens if the water chooses to incline to one of the surfaces, reduces this 45 degrees, say, to 30 degrees and you have drag coefficients that are much higher?

MR. SONIN: Well, I guess my judgment would be that it is unlikely, given the symmetrical placement of the vent header in the torus, that there be such a difference. I mean, these curves that I have here show, for example, what happens to that coefficient, as a function of angle. There are two sides. You increase the force on one, and you decrease it on the other, when you incline the wedge.

DR. CATTON: So you rotate it.

DR. PLESSET: I think it is kind of reasonable what he says.

DR. ZUDANS: In other words, what you are saying really is that while you increase the one side, you reduce the other, and the total net force might not be greatly different.

Is that it?

DR. SONIN: That, plus the assumption, just based on a judgment of looking at these things, that it is unlikely that there is a large angular effect.

DR. ZUDANS: In this diagram, where would be the point for a plate?

MR. SONIN: For a plate? Infinity.

DR. ZUDANS: Than that is theoretical.

DR. PLESSET: It doesn't go to infinity.

MR. SONIN: The impulse is finite, of course. This is this.

Shall I proceed?

DR. PLESSET: Please.

(Slide)

All right, so we go -- This is the case when you have a different type deflector, with a cylinder followed by the wedge shape, and here we have again used judgment to draw the curve.

The initial spike is obtained from the cylinder correlation from EPRI, and that is there, because that spike is over before the water has passed the cylindrical portion. So the water doesn't feel the wedge during the initial spike.

Then we have simply said that what follows is the impulse, or at least the transient for the wedge evaluated as if the wedge had started below the cylinder, at the point of its projection at the sides.

DR. SCHROCK: Why is that 2.9 and not seven?

MR. SONIN: Because the non-dimensional factor here is the width of the whole device, and not the diameter.

DR. SCHROCK: Okay.

MR. SONIN: So, in other words, it is the same. The 1.6 here is an error. We have actually 1.5. I don't know how that crept into the diagram. 1.5 is what it is.

There is some rationale for choosing this drop-off to .7. What we have said is that the total impulses associated with this transient should not exceed significantly the impulses associated with a flat beam of the same width, because that has about the maximum impulse that you can get. And so this is conservative, because it does have about the same impulse as the flat beam of the same width.

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We go on to another type of deflector. This follows the same principle here. It is just pure geometry about how we scale -- This is for the deflector Type (2), which is slightly different. Here we assume that the transient is -- The peak occurs when the water passes the mid point, and again the numbers are straighforward, based on the same kind of ideas.

DR. ZUDANS: An interesting excercise on this figure: Suppose we begin to shorten the dimension "w" and make the angles steeper and steeper.

MR. SONIN: Yes.

DR. ZUDANS: According to what you have proposed, you still have to maintain 3.14 as the peak point.

MR. SONIN: No, this is only from 45 degree wedges.

DR. ZUDANS: You would go down--

DR. PLESSET: You would go to zero.

DR. ZUDANS: Then you really are at the diameter of the cylinder. You should get back into your 1.2 flat curve. In other words, you could draw a set of curves here within this spike, the second spike, with ever-reduced peaks.

MR. SONIN: I am afraid I am not quite following what you are saying.

DR. ZUDANS: I am talking about changing -- Say, supposing now we would change the angle as we go, just a mental excercise.

MR. SONIN: Yes.

DR. ZUDANS: Whether or not it is a physical process that makes it into a cylinder, whether the mathematic successions you could make--

MR. SONIN: Well, you always have to be tangent to the cylinder here.

DR. ZUDANS: Right, but as I reduce the dimension, I would wrap around this straight portion of the surface, and your second spike would go down gradually, by a Von Karman's prediction, for instance, and at some point, where the dynamic -- you should see the difference.

MR. SONIN: This specification would not reduce to the cylinder in that excercise, because we have not included here the steady drag for the cylinder. The cylinder steady drag falttens out, okay? And that starts at some level when the water is about, you know, like up here, let us say .2 or so of the diameter, then you are into the steady drag for the cylinder, somewhere over here.

So we haven't included that.

DR. ZUDANS: In practice actually, when you talk about a gut feel, this wouldn't be very different from a cylinder in reality. In other words, theoretically there is a difference: you have to go very high with that peak point for that theory, but in practice, whether it is prefectly round, or not perfectly round may not make such a big difference.

DR. PLESSET: You wouldn't go up, necessarily, if it were a real true cylinder. He could do that analysis just as well, but he has limited himself to a wedge always 45 degrees.

MR. SONIN: That is right.

DR. PLESSET: And all of these curves relate to a little different geometry or net dimension of a 45 degree wedge.

DR. ZUDANS: I do understand, of course, what you

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are showing, I am just wondering whether it is reasonable to expect that you have that peak on this configuration in reality.

MR. SONIN: Let me put it this way: This is not a general curve. You would not reduce to the cylinder when you do what you said. It is drawn, based on the judgment of what goes on physically as the flow goes around these various parts, for this particular geometry.

DR. CATTON: Isn't what you are asking: Doesn't the cylindrical bottom change that ultimate drag coefficient?

MR. SONIN: A cylindrical--

DR. CATTON: The 3.14 comes from a wedge with a sharp edge impacting on the flow, and here you have a blunt surface that is impacting on the surface. I would think that that would put it somewhere between the cylinder and a wedge.

MR. SONIN: I would think you are absolutely right, if that is the question, and I do feel that taking 3.14 is a conservative way of approaching it.

DR. CATTON: It might be quite conservative.

MR. SONIN: It might be, but we simply have no data to indicate what the real value is.

DR. CATTON: When you look at that cross-section, gee, that looks like a cylinder, pretty damn close.

MR. SONIN: This?

DR. CATTON: Yes.

If I had to bet, I would bet that it would be a lot closer to the cylinder than to the wedge.

MR. SONIN: And I would be with you that somewhere in between -- I wouldn't base licensing on that.

DR. CATTON: Maybe I misunderstood the hook-up, but it looked to me like it made a factor of three on the load.

DR. PLESSET: No.

DR. CATTON: Didn't it go from 26 to 61.

DR. KENNEDY: For Type III that is correct.

DR. CATTON: For this type?

DR. KENNEDY: It is a little different.

This type peaks at 50 per cent submergence, and the example that I used peaked at 83 per cent submergence.

DR. CATTON: So there is no big difference here.

DR. KENNEDY: This will make about a factor of three difference.

DR. PLESSET: I don't think it would be an enormous difference between taking into account the cylindrical, do you?

MR. SONIN: It is a judgment.

DR. PLESSET: You could do it, the same analysis, if you wanted to.

MR. SONIN: If you do Von Karman, you could do it, and you would get exactly the same value, you see, but that is the nature of Von Karman's analysis, and that is why it isn't quite right.

DR. PLESSET: It is pretty good, though.

DR. ZUDANS: Could you use the Von Karman's argument and technique and do it for curved surfaces. rather than--

MR. SONIN: Yes, but Von Karman isn't right.

Von Karman will -- You see, if you look at Von Karman's analysis, and you can do all these shapes with Von Karman, quite simply, in fact, I have done them, most of them, but what Von Karman shows, for example, is that if you apply his analysis, the total impulse, you get for a given width of body is independent of the angle, if it is a wedge, of the wedge. And you get the same impulse for a cylinder, as well as for a wedge, or a flat plate, if you were to apply it -- I mean in the limit of a flat plate, which is not right.

The actual and total impulse is higher the flatter, or the blunter the body, and that can be a significant difference. You are talking about 50 per cent easily, so you can do that exercise, but it is not going to give you the really correct answer.

DR. BUSH: Well, there is one big problem, so long as they are mathematical exercises, that is one thing, but when you take those and begin to add the conservatisms, and then convert it into a modification, it may go one way or the other. We have trapped ourselves in the seismic area ver badly in that respect.

DR. PLESSET: Well, that is one thing that the Staff has to justify when they do these things, pile the conservatisms up, like a Tower of Babel, or something analogous, right?

MR. GRIMES: That is correct.

DR. BUSH: That is what comes out, a Tower of Babel, on the conversions.

Well, that is the real concern I would have, and that is if you begin to change the structures.

DR. PLESSET: Right, I think this is very reasonable.

DR. BUSH: I think this is a highly conservative approach, but how conservative, I confess, I don't know.

DR. PLESSET: But the analysis here is reasonable?

DR. BUSH: I don't argue that.

MR. SONIN: Well, we feel that this is conservative, but a large measure of the conservativeness comes, probably, from the way that the velocity and acceleration are imposed on the formulas, rather than from the formulas themselves.

And, as I said, the Wagner formula for initial impact, that means the slope of the dimensionless force versus dimensionless time curve, has found verification in experiment, and here, for example, is the G.E. test data, which shows -- This is for the Duane Arnold plant, which has a single row of downcomers coming from the vent header.

(Slide)

They have put in a pure wedge in the QSTF, and measured the impact transient for the pure wedge in the QSTF for the Duane Arnold situation, and this shows a comparison of the measurement, and our specification.

Now, this measurement, as you can see, has this initial slope, which is off by almost a factor of two. Now, we don't think that the actual drag coefficient curve is off by a factor of two, based on what we have seen in other data.

Also, the measurement shows quite an absurd result, which would say that even though the velocity was shown to be a reasonably constant, the pool surface velocity, during this process, in the absence of the vent deflector. With the vent deflector, the drag goes to zero, very quickly, after the water passes the deflector.

Now, my interpretation of this is that the wedge does, in fact, affect the water flow over it significantly, and the error, or the difference is there because of that, rather than because of the specification, or the drag force coefficient.

MR. GRIMES: The point that we would like to make regarding this specification is that the basis for the Staff position was that we felt that the correction for excessive conservatism in the load definition, as posed by the Mark I owners, was being taken in the wrong place, and that the drag coefficient shouldn't be adjusted, because you can't find the velocity properly. You should define the drag coefficient correctly, and then correct the velocity, and the first method of defining velocity Dr. Kennedy presented was a technique that was not proposed in the LDR. It is a technique that they are currently pursuing, and that they hope will provide a better definition of the loads.

It has become quite apparent, since we have issued the criteria, at least since August 2nd, when we issued them for comment, that we were going to have to establish, in some detail, the method to define the velocity for these loads, and come to some agreement on how that can be done.

Now, what they have proposed sounds like it might be a reasonable way to resolve this issue, and when they can develop a method for defining velocity, we will settle on something that we can agree to.

MR. HANAUER: Steve Hanauer, H-a-n-a-u-e-r.

I would like to emphasize this point:

The reason this comes so far off from the measured is because the velocity being applied to the drag coefficient is so far from the actual velocity of the fluid, which is causing the force. The measurement of the unperturbed or free field velocity and the application of a drag coefficient simply doesn't predict at all the forces, because the velocities are so wrong.

And so the difficulty is not in the drag coefficient, as several people have said, but in the fact that the velocity was not at all representative of the actual situation.

DR. CATTON: Why does it go to zero?

MR. SONIN: I think I can take a crack at that.

It is conjecture, of course, because I haven't looked at the details of this. I haven't seen the films, but my feeling is that it goes to zero, because first of all, the slug of water that rises and hits the deflector has a finite thickness, and that thickness is not that enormous, compared to the width of the device, and so what happens is that it goes over -- the slug impacts the deflector, and locally that slows down the water, for one thing, and the water curves over it, and I don't know exactly why it goes to zero, but I can certainly see why there is a turn-down here, because of the interaction, or the feedback of the deflector onto the water surfaces, the water surface velocity, itself.

DR. CATTON: The movie should reflect that.

MR. SONIN: The movies would probably give quite a lot of information of what would actually go on.

MR. DEARDORRF: They do.

I think for this particular configuration of

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Duane Arnold with the single downcomer that that deflector is probably in the bubble, by the time you get-- to submergences.

DR. PLESSET: To get back to Steve's point:

You are saying -- Let me see if I understand it -- that they are overestimating the appropriate velocity significantly.

MR. HANAUER: Enormously.

DR. PLESSET: Oh, well, I said "significantly," we will make it "enormously."

MR. GRIMES: By a factor of three.

DR. PLESSET: That is a big difference.

MR. HANAUER: Well, if you will think back to the owners' presentation, as to how he estimated the velocity, he went 18 inches off to the side, and estimated it from the movies, in a region not at all affected by the deflector, or very little affected by the deflector.

MR. GRIMES: Using a "typical turndown function," which is a point that we pursued during our refute that was based on pool swell in a field, without a deflector, where you couldn't see the affects of the deflector on the local velocity.

DR. CATTON: Normally a drag coefficient is defined in terms of a free-stream velocity, and that is the velocity as unaffected by the object.

MR. SONIN: But there is a feedback practice, when there is this finite slug, there is a feedback.

DR. CATTON: That is the point. I think the finite slug aspect is where it is at, not where they picked the velocity, so much.

MR. GRIMES: Our point was: the method used to predict the velocity be applied with the drag coefficient.

DR. CATTON: You can't characterize the velocity, I think, as the finite thickness slug.

DR. ZUDANS: Maybe you should not calculate that drag in terms of velocity.

DR. PLESSET: It generally turns out to be pretty handy.

DR. ZUDANS: You can't use the velocity for the drag calculation, in this case, because you have no information of the two-dimensional, or three-dimensional--

MR. SONIN: Well, all right. Our position here is, or the philosophy is that if they use the method that they propose for the velocity, then they are certainly conservative, because all these effects pull the velocity and acceleration down, and so it is all right if they do that.

If they want to modify that method, which as we heard from Bill Kenedy today as a possibility, then we look carefully to make sure that the net result is not non-conservative.

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DR. ZUDANS: I think your point is well taken, if they do this all conservatively, but if you are looking from a scientific point over here, tested on a physical concept, is this a proper place to use this drag description method.

MR. SONIN: We would prefer, by far, that they go to purely empirical methods--

DR. ZUDANS: Right.

MR. SONIN: --but since there is a large number of plants in which deflectors have not been tested in the QSTF directly, it is an option that they want to preserve.

MR. GRIMES: A point that I should make to the ACRS, and also make to the owners group, at the same time, is that we have two specifications in the criteria -- We realize that there is a cost benefit associated with designing a modification based on an analytical technique that has a significant amount of conservatism, as opposed to going back and performing additional tests in QSTF to directly measure the forces in the deflector. We specified the two criteria, and left the cost-benefit aspect to the utilities to decide.

There is a cost benefit schedule implication here that we are up against as well.

I failed to mention the time aspect, which reminded several different people.

We are also trying to push a schedule to resolve this issue, and certainly with the number of plants involved, it would not be practical for all of them to go back and repeat the tests.

DR. BUSH: But, Chris, I have seen cases of empirical analyses data that the usual end product is that after all of it is done, you say, "Now, we will apply a nice conservative factor two to these values," and therefore, the value of the empirical studies disappears.

MR. GRIMES: In our acceptance criteria, we did not apply any factors of two.

DR. BUSH: I am not talking about you, I am talking about several other times that it has been done.

MR. GRIMES: We are continually reminded to try and avoid that approach.

DR. BUSH: I think that is desirable.

DR. PLESSET: I want to thank Professor Sonin.

Were you really finished?

MR. SONIN: Yes.

DR. PLESSET: I think, at this point, that this should have been a problem of the '70's, not of the '80's, right?

MR. GRIMES: Right.

DR. PLESSET: I think everybody would be happy with that.

Any other questions?

We will be leaving this topic, and coming to another important topic, presumably after lunch.

Any other questions?

Let us have an hour recess for lunch then, and we will come back and continue.

(Whereupon, at 12:15 p.m., the meeting was recessed to return at 1:15 p.m.)

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AFTERNOON SESSION

1:15 p.m.

MR. PLESSET: We will reconvene.

Chris, will you start us off?

MR. GRIMES: Yes.

We have two films. The first is a computer simulation. This was developed by Livermore to look at fluid structure interaction effects. It shows not only the hydrodynamic processes, but some of the structural responses as well.

The first few segments of the film show some validation runs that I will explain as the film is going on, and the last segment of the film shows a simulation of a response in a Mark I torus.

MR. PLESSET: Before you start it, maybe we can try to close that curtain.

(The film is started.)

MR. GRIMES: The first problem was a cylinder oscillating back and forth in a pool of water, and the computer simulation shows the velocity vectors of the fluid in reaching of the cylinder, and how the pool in the surrounding volume is also affected by the motion of the cylinder.

This was done for a variety of cylindrical speeds to check the algorithms that were put into the code.

This is at a faster speed; the same problem.

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DR. BUSH: This is infinite boundary set-up?

MR. GRIMES: No, finite annulus liquid.

DR. BUSH: Finite annulus.

MR. GRIMES: This is a simulation of some fluid structure interaction tests that were conducted at M.I.T. in a small cylindrical tank, with a flexible bottom. It was a single vent in a pool of water, and you can see -- If you look at the bottom of the tank, you can see the simulated flexible plate, and you will notice that as the bubble grows the plate starts to flex.

MR. CATTON: That is a strange bubble.

MR. GRIMES: That is a function of the stability of the surface of the bubble as it is growing: it slows in one area, and then it can't catch up.

Mr. Landgrum can address the specifics.

MR. PLESSET: It is a Mark I.

MR. GRIMES: This is a Mark I simulation.

They didn't put a header in there, simply modelled the two downcomers of the pair.

MR. SONIN: It is a 2-D simulation.

MR. GRIMES: It is a 2-D simulation. It shows the double-bubble growth. The bubbles have the same general kind of motion that have been observed in the test, although it is not exactly the same, but in UCLA's experiments, I think they are referred to as "strawberry bubbles," and that is that type of shape that was experienced here.

You will also notice the motion of the torus at the bottom.

DR. BUSH: What do they call this? I missed the word?

MR. GRIMES: PEL-IC.

DR. BUSH: It looked like: PELE-IC.

MR. GRIMES: As I recall this is a derivative of the solar--

MR. LANDGRUM: It uses a form of the solar algorithm that was originated in Las Alamos.

MR. STEINER: We are going to show a film of the FSTF at this point.

MR. GRIMES: While Mr. Bates is setting the films up, the next film was provided by General Electric. It is actually sort of a summary film of the FSTF tests, and it was produced for the benefit of the utility management so they could get an overview of the testing program, and they have been kind enough to loan it to us, for the purpose of this meeting, to sort of bring you back into the Mark I condensation, and that there have been a number of meetings recently about the Mark II condensation, so we will show that film to introduce General Electric's discussion of the FSTF results.

(The film is shown.)

MR. STEINER: One of the NRC criteria requests additional FSTF tests. We feel that the current data base and the current CO load specifications are adequate as they exist.

As I mentioned before, we are going to have three separate presentations right now: John Torbeck will lead off with a description of the facility, very much like you just saw.

Randy Broman will then discuss fluid structure and reaction as what was accounted for, and finally Umesh Saxena will describe the load specification and how it was derived from the data.

John?

MR. TORBECK: Let me show my first slide.

(Slide)

MR. TORBECK: As Larry said, what I am going to do is very briefly talk about the test objectives, because those were pretty well described in the movie, and briefly go over the test description, so it may be a bit redundant with what you just saw in the movie.

I will talk about the test matrix, and then go into a little more detail on some typical test results for condensation oscillation and chugging, showing hydrodynamic and structural responses from the facility.

(Slide)

MR. TORBECK: I will go quickly over this, because it was stated in the movie: The key objective was to get hydrodynamic loads resulting from steam condensation, using a representative structural model of a full-scale Mark I containment.

What we did was we selected a 22-1/2 degree sector of a typical Mark I, in terms of its structural characteristics, and the Monticello plant that we used as our basis, we thought, would lead to a somewhat conservative condensation load, because of its configuration.

We then scaled the drywell, the vents, and the flash boiler to be ab le to fully simulate the blowdown, and the structural response was matched directly to that of Monticello.

We had a large volume of hydrodynamic and structural instrumentation, which I will get into a little more detail on, and the high speed data acquisition system, which was capable of recording data at the rate of up to 256,000 samples a second.

(Slide)

MR. TORBECK: Just for reference purposes, we have got a typical Mark I containment here, which was also shown in the movie.

What we chose to simulate in the test program was a bay like this bay in Monticello, for a couple of reasons:

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One was that we felt that with the concentration of eight downcomers in this bay, as opposed to four here, we would get a conservative characterization of the hydrodynamic response, and also by looking at this sector here, we could do a good job of modelling the structural response.

The test facility simulated these vent pipes by having two vent pipes come from the drywell into each side of the 22-1/2° sector facility.

I am going to go into a little bit more detail on some of the test results later, and this is the kind of data I am going to be talking about. pool wall pressures that are down here on the bottom of the torus. I will show some pressure readings here in the downcomer, in the vent pipe, or in the -- I will refer to this as the ring header. They are actually in this location here. And then also some in the vent pipe, and some in the simulated drywell.

(Slide)

MR. TORBECK: I don't think there is too much reason to dwell on this one. It was described fairly well in the movie. Again, you can see the two vent pipes here which had representative path lengths, and flow cross-sectional areas, so that we would get representative velocities through this part of the vent, as well as in through the downcomers here.

(Slide)

7 MR. TORBECK: The test matrix consisted of ten tests, and these are shown in the order that we performed them.

The first reference test here was a small steam break with a 70° pool temperature, and a 3'4" submergence.

Then we increased the diameter of the blowdown nozzle and did a second test with steam with all other conditions the same.

Then we changed the blowdown configuration to test a small liquid break, went back to the small steam break configuration, primarily to get chugging data, and increased the freespace pressure; ran again with the same blowdown configuration, with an increased pool temperature.

Then we decreased the submergence and raised the pool temperature. The next test was done with an increased submergence. This was increased to four and a half feet, at this point. That one was run again with a 70° temperature.

This test was conducted with the vacuum breaker, the prototypical vacuum breaker that we had at the facility. We blocked it off, so that we could measure the effects of decreased air content. And then we ran a large steam break with pool conditions the same as they were for the base condition here, and a large liquid break.

MR. CATTON: Is there any reason that the only test that you increased the pool temperature for was the small

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steam break?

MR. TORBECK: The--

MR. CATTON: I believe that is test five and six.

MR. TORBECK: Actually, these two.

Our expectation was that chugging was going to be the process that was most strongly affected by pool temperature, and since these were tests that we expected to get a large amount of chugging with, the small steam breaks, we increased the temperature for those conditions.

MR. CATTON: But at the tail end of M7 and M8, don't you get down to a low mass flow, with a hotter pool?

MR. TORBECK: Yes.

MR. CATTON: But if you started initially with a higher temperature, you would get....

Go ahead.

MR. TORBECK: There is -- that is true that we have a high temperature, and a low mass flux at the end of these tests. I think when I get into the details of the chugging that we observed, you will see that what we actually found out was that if we got the pool temperature high, at the time when the mass flux was low, we wouldn't get chugging, and I will show that in a map of the test conditions.

(Slide)

MR. TORBECK: We had 256 channels of data recording capability. This was a digital data acquisition system.

And we had the capability to sample each channel at up to a thousand samples per second.

We then had the instrumentation broken up into several different kinds of, types of instrumentation, to measure the torus shell response in terms of strain, displacement, and acceleration at various occasions on the shell. We also measured the strains in the torus support column, bending moments in the downcomers, the strains at the attachment of the downcomers to the ring headers, torus wall pressures at about two dozen locations.

The pressures in the vent header or in the ring header and in the vents going back towards the drywell, the downcomer pressures, the drywell pressures, the downcomer and ring header level probes, we have capacitants type probes or conductivity type probes located in the downcomers and also in the bottom of the ring header.

We had therma couples throughout the pool to measure the pool temperature, and we had instrumentation to measure the vent flows, and also the blowdown flow rate.

(Slide)

MR. TORBECK: I won't go into the details of this chart because it just summarizes the instrumentation in terms of the different types of instrumentation we had and where it was located. The total here comes up with 427 channels.

What we did was we had that many measurement locations on the facility, and we selected from those 427 channels 256 channels to read during the test.

(Slide)

MR. TORBECK: I will briefly go over the condensation oscillation test results, focusing on results during the large liquid break which is the one that resulted in the largest amplitude wall pressures.

The way we established the condensation oscillation regime in terms of analyzing the data was we looked at the wall pressure traces as we were going through the blowdown, and when we started getting wall pressures which had harmonic kinds of characteristics and some substantial oscillation, that is what we chose as the initiation of the CO., and we continued to look at that data until the water re-entered the downcomers as indicated by the output from conductivity probes.

(Slide)

MR. TORBECK: I will walk through very quickly a typical wall pressure output. This particular location is on the bottom dead center of the wet well about a quarter of the way from one end. There was not very much variation during the CO. period, the pressure amplitude on the bottom of the wet well in the axial direction.

DR. CATTON: Did you have a pressure transducer located immediately below one of the vents?

MR. TORBECK: Yes. I don't have a trace from that, but in the CO. regime there was a pretty clean spacial distribution of the pressure amplitude, the variation around the torus going from the bottom up to the water level had a pretty linear characteristic with depth, and along the axial length there was essentially no variation in amplitude.

DR. CATTON: So the fact that the one that was right underneath it was closer to the vent exit. It did not measure a higher pressure? Is that what you are telling me?

MR. TORBECK: That is true. Well, not a higher pressure than what was observed at the bottom. I have got to qualify that just one bit more, and that is that we did see, and I will show you some more detail of these traces, some higher frequency perturbations on the base signal which we are characterizing as being the result of the structural vibration.

Randy Broman will talk in more detail about how we are correcting for that, and there was some spatial variation resulting from structural vibration modes, but not from the fact that it was closer to the end of the downcomer.

DR. ETHERINGTON: What is the time scale?

MR. TORBECK: This is supposedly proprietary information.

DR. ETHERINGTON: Excuse me.

DR. CATTON: I guess I would just like to register surprise that the pressure transducer that was closer to the action didn't measure a higher pressure.

MR. TORBECK: The pressure transducer -- the pressure transducer in the vent system near the source, that was the highest amplitude pressure that we observed. I will go into the details about that.

DR. STEINER: One comment. These figures that John is showing, they are proprietary, but they are directly from the FSTF report which has the scales and everything.

DR. ETHERINGTON: Fine. Thank you.

MR. TORBECK: We have included the figure numbers and the report number so you can find it easily.

And I have included the normalization factor here or something to kind of help you in terms of reference purposes in terms of how this amplitude compares with the amplitudes that I will show later on other curves, and how this amplitude during CO. compares with the amplitude during chugging.

What we are doing is starting off, and this is early on in the test, and we are going on in time. You can see the pressure amplitude building up, continuing to build up.

(Slide)

MR. TORBECK: And going to fairly large amplitude, staying very harmonic, a very harmonic kind of signal with a relatively constant amplitude over about an 8 second period, and then we start decaying.

(Slide)

MR. TORBECK: This was for the large liquid break. I think I neglected to say that.

And then we have gotten into very low amplitude values here, and this particular test ran out of -- actually, the flow rate decayed very rapidly back about half way through the last slide. This particular test didn't chug, so we don't have anything that looks like chugging.

(Slide)

MR. TORBECK: This is a summary slide of how the amplitude of the wall pressures without an FSI correction varied as a function of the energy flow rate out through the end of the downcomers.

This average amplitude value here is a spatial average, and it is the average of the zero-to-peak pressure values over a period of about a second.

As I will show you later, we have got the data, it is around 7 Hertz, so it is about 7 to 8 cycles that are making up these data points, and, as I said, it is a spatial average considering the net vertical load, basically, divided by the cross-sectional area of the torus.

What you can see here, there is a reasonable linear correlation of the data with the energy flow rate. We did some other things with plotting it as a function of steam mass flux and total mass flux, and it didn't correlate as well as it does on energy rate. I am not proposing this as being the magic correlation, but it just seemed to work reasonably well on this data set.

The important thing to note here is these data points. This is from the early part of M-8, and then it went up, the large liquid break, and then the amplitudes did something like this and came down as the flow rate decreased to this one here, and as Umesh Saxena will tell you later, we have taken data from these time periods from M-8 and some from this vicinity, I believe, for M-7 for development of the load definition.

(Slide)

MR. TORBECK: This is looking at the dynamic part of the pressure oscillation signal or pressure signal at the bottom of one of the downcomers. Actually it is about three feet up from the end of the downcomer, near the knee of the downcomer showing the pressure oscillation as a function of time.

What it will show is that --

(Slide)

MR. TORBECK: This has a fairly clean frequency content with the dominant frequency, I think, that is around 6 or 7 Hertz, and I will show this better on the next slide, and another one in the range of about 8, and then another one at about 12. I will show how that frequency content varies with time.

(Slide)

MR. TORBECK: These are the dominant frequencies, the ones that are solid here,and the time period we just looked at was this one here, with the dominant frequency -- I said that wrong. It is about 5 Hertz. The next two peaks are about 8, and around 12.

These open circles here are from the ps- -- other frequencies from the PSD curves which had a peak on the PSD curve of at least 1/10th of the amplitude for the dominant frequency.

You will see there is not too much variation in the dominant frequency as a function of time as the -- at this point when the -- we ran out of liquid in the steam vessel and the flow rate dropped very rapidly, then the frequency began to shift up a little bit. This, as the amplitude is dropping very rapidly also.

We are getting, I think, into a mode here where the frequency is more controlled by the vent acoustics instead of what is going on right at the end of the vents in the condensation process.

(Slide)

MR. TORBECK: I will quickly walk through the pressures in other parts of the facility at the same time interval we just looked at, about 31 seconds into the blowdown.

This is going up the downcomer, further away from the condensation source, into the vent pipe, and you can see, we have a very clean harmonic signal here.

(Slide)

MR. TORBECK: As the PSD shows, there is just essentially a single frequency in the data at that location.

(Slide)

MR. TORBECK: The next slide is a similar kind of trace. To trace -- the amplitude here is a bit lower if you compare with the "X" that I have shown on the ordinate there. This is going back up about half way up in the vent pipe towards the drywell.

(Slide)

MR. TORBECK: And this is the pressure oscillations in the drywell. It looks like a pretty noisy signal, but that is down in the range where we are getting a bit of instrumentation noise into the signal. As you can see, the amplitude is much lower here because the scale here is about a tenth lower than it was on the other plots.

(Slide)

MR. TORBECK: Now, if you go out onto the wetwell shell at that same time interval, this is looking at a different location than the one that we looked at through the full blowdown. This is at bottom dead center, right at the bottom of the torus, and mid-way axially through the facility.

You can see there is a bit more frequency content in this signal than what we observed in the vent pipe, and also the amplitudes are a bit higher than what was in the vent signal, and that is a little bit hard to take if there is not something else going on, and what we have concluded is that the thing is introducing, that is introducing these additional spikes, is the structural response.

(Slide)

MR. TORBECK: This is showing the PSD that indicates that we have got some new frequencies introduced here. If you will look at the readings on the shell, and from some of the work that Randy Broman has done, these frequencies correspond to structural vibration modes of the shell itself.

(Slide)

MR. TORBECK: This was some other data that we looked at. There is a lot more that is presented in the report, and some of them that are better indicates of the fluid structure interaction than this particular one.

What we are showing here is the shell displacement as a function of time; the acceleration at that same location, and the pressures at the same location. Again, bottom dead center. And if you will look closely at these, you will see some periods in which we are getting some large amplitude spikes which correspond to some fairly large amplitude inward accleration of the shell, and looking over a range of readings, different locations in the same vicinity, you can make a pretty good supporting story for the idea that the fluid structure interaction is, indeed, affecting these pressures that we are measuring on the shell.

What I am trying to say is that looking at this location by itself is not really adequate because if the shell is coming in some other location four or five feet away, it is actually going to increase the pressure at the location, the reference location.

DR. CATTON: You are arguing that the fluid structural interaction was the cause of the high peaks, was that correct?

MR. TORBECK: Yes.

DR. CATTON: Here, when I look at the displacements and the pressures, it seems to me that they are in phase, and if they are in phase, I would think that that would decrease the measured pressure.

MR. TORBECK: The dominant frequency here, that is really true. But some of the higher frequencies here, we are getting inward acceleration, well, inward acceleration which corresponds pretty closely to times of the spikes, and you have to look at more than one location. One location by itself is really not a very good characterization of this. I think if you want to get into the details of it, it would be wise to read this section of the FSTF report because it has a lot more information.

DR. CATTON: So what you are saying is that quite frequently the pressure and displacement are out-of-phase?

MR. TORBECK: Yes.

DR. CATTON: Even though this diagram that you have shows them in-phase?

I look at the highest peak and I follow the line down.

MR. TORBECK: Pardon me?

DR. CATTON: I look at the bottom and I see a high pressure peak, and I look at the top and I see a high outward displacement.

MR. TORBECK: But this corresponds to an inward acceleration at that time. Okay. It is outward displacement.

DR. DEARDORFF: You have to look at the fact that when the displacement is outward, you have got a high radial inward acceleration, and that is when the high pressure peak occurs. You have got to displace the spring outward so it accelerates the shell inward at the same time that you observe the pressure spike.

DR. CATTON: Okay.

MR. TORBECK: And looking at one location is really not enough. You have got to look at several locations.

(Slide)

MR. TORBECK: I am going to briefly review the chugging results that we got from the facility. I am not talking about this nearly as much as the CO. because the chugging results were quite a bit lower in terms of amplitude, and also in terms of the structural response that we observed, as I will point out at the end of the presentation.

(Slide)

MR. TORBECK: The way that we established when chugs were occurring was to look at lateral acceleration measurements near the bottom of the downcomers. We had accelerometers on the bottom of each downcomer and level probes also near the bottom of the downcomers, and when we got a coincident wetting of the level probes, and an increase in the acceleration or high acceleration value, a pressure that was on the order of like about 5 G's, then we would identify this as being a chug in that particular downcomer, and we could identify chugs in each of the downcomers that way.

(Slide)

MR. TORBECK: As I said, we got a bit less chugging than what we were expecting to get when we launched the program. We actually had chugging on just four of the tests, and I will talk in my next slide a little bit about why we think we got less than we expected.

The four tests that we got chugging on were all with the small steam break.

First of all, the nominal or the base test; then one in which we increased the freespace pressure; one in which we increased the submergence; and one in which we blocked off the vacuum breaker.

This shows the time interval over which we got chugs.

This is just the difference in those two numbers, and the approximate number of downcomer chugs. These are the total of all of the downcomers, all of the chugs in all 8 downcomers.

(Slide)

MR. TORBECK: This is a plot of the average mass flux in the downcomers, average steam mass flux in the downcomers as a function of the pool temperature at the bottom of the downcomers.

What we did was take the average of all of the therma couples that were in the vicinity of the bottom of the downcomers to obtain the temperature values, and then plotted as the mass flux was decaying during the tests, how the temperature at the bottom of the downcomers was increasing, and then identified the chugging regimes.

You can see also noted on here how the air content in the vents decayed. This is representative for all of the tests in which we had the small steam break, these four tests right here.

What you can see from this is all of our data seems to, in terms of the chugging, the range in which we observed chugging to occur, can be bounded by what we have suggested here as a chugging boundary. This seemed to be somewhat supportive of our hypothesis that if you got the temperature high enough locally, the chugging would really not occur, and all of these conditions here, condensation oscillation type of conditions.

(Slide)

MR. TORBECK: Here are a couple of typical chugging traces, two different locations one at bottom dead center near the end wall, another one at bottom dead center in the middle axially of the pool.

This is for the test with the deep submergence, and these identify the times at which we got the level probes re-wetting and the acceleration spike on the ends of the downcomers.

You can see actually in this particular regime here the largest amplitude pressure oscillation during chugging occur before the chug itself, and it is, again, a very oscillatory kind of signal. We think it is strongly coupled to the acoustic frequency of the vents.

(Slide)

MR. TORBECK: This is a very cryptic summary of all of the structural information that we got through these tests, and it identifies the maximum stress values, dynamic stress values measured during the condensation oscillation conditions of M-8, and the chugging conditions of M-1 which were the dominant ones for all of the tests.

From this you can see that generally during chugging these stresses are much lower than they were during CO., and also generally the stress values are pretty low compared to allowable values.

The one location that is different than that is the untied downcomer and the stresses in the attachment region of the downcomer. I believe it has been verified that all of the Mark I plans actually have tied downcomers. We had left ours free in the facility to get better lateral load information in terms of trying to define the applied loads on the end of the downcomers.

Any questions?

DR. BUSH: On this particular slide I visualize that the torus, which because of dimensions and thickness is a pretty flexible structure, I would anticipate in a full torus that under loads that are not actually symmetric in time, at any given time, they may average out as such, that I would periodically go through what I call an elliptic mode and then a recovery mode on the whole torus, just like taking a donought and pulling it this way.

At the same time, I would expect in a cross-section to have the same thing. In other words, it would tend to go elliptic, and the two of them would probably either compliment or reinforce one another.

Now, for the life of me, I can't see how a 22-1/2 degree segment, because of the stiffness aspects can simulate that, so I don't know how you can extrapolate how the real torus would be here.

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MR. TORBECK: You mean in terms of some kind of mode for the whole --

DR. BUSH: Not only this way, but if I take the whole thing, you know, it will tend to move this way, and then at the same time it may negate itself, but a lot of times it will tend to amplify. So I don't think I could extrapolate from what stresses I see here the probable stresses in a full torus. Maybe you can do it, but I don't see how.

MR. TORBECK: I am not a structural expert. I am not really capable of explaining.

DR. BUSH: Do you understand the point I am making?

MR. TORBECK: I understand the point you are making.

MR. BROMAN: I am Randy Broman from Bechtel.

To the extent that the condensation oscillation load is symmetric or is uniform about the major axis of the donought, as you say, the torus will not go elliptical in the manner in which you are suggesting.

DR. BUSH: But I don't think you can prove this that way. I think that it may average out, but, in any event, if I look at it as a time function, I would be very surprised if it were actually symmetric at any given time. It might average out as such, but I would be hard put to believe that it would do it otherwise.

MR. TORBECK: I can speak in terms of the forcing function, I guess. I would expect at least the fundamental or the dominant -- the frequency corresponding to the dominant forcing function or the distribution of the largest amplitude pressure oscillation would be symmetric as Randy was saying.

DR. BUSH: Well, that is very much dependent on the vent loading and unloading.

MR. BROMAN: That is correct. If you get the same pressure in each of the vents, you have, in essence, a symmetric loading, uniform about the large circumference of the torus.

DR. BUSH: The same pressure which is very sensitive to path dependency.

DR. ZUDANS: As a dynamic thing, it is not going to be symmetric.

DR. BUSH: I don't believe that under any circumstances it will average out to be symmetric. But I think if I look at it as a time function at any given time I freeze it, I will be very surprised if it is actually symmetric.

DR. ZUDANS: And maybe this test allows us to determine what the point loads are on the surface, but certainly it doesn't tell you much about the information on stress state in the torus itself.

DR. BUSH: You may have lots of margin. I am not arguing that, but I am just saying that I would have a hard time figuring out how to extrapolate because I think the stiffness in the 22-1/2 segment is much different.

MR. TORBECK: If you are talking in terms of the feedback of the structure --

DR. BUSH: That is what I am talking about.

MR. TORBECK: There were major efforts to make the structural response modes simulated even in terms of the gross -- I am not sure what the right mode is, the one where the whole torus would stretch in and out, that was simulated in the structural modelling of the facility.

DR. ZUDANS: Except it was simulated only for one mode, uniform stretching. I remember the supports were calculated that way.

MR. TORBECK: That is true.

DR. ZUDANS: So what Dr. Bush is talking about doesn't have much to do with uniform stretching. You just make it round or make it oval.

DR. BUSH: I have a feeling as though it should have a lot of margin. Whereas I wouldn't believe in numbers, I strongly suspect that the -- you know, the types of loads it would be having, that some of them, in fact, will tend to cancel.

DR. PLESSET: I don't think there are any further questions. You can proceed.

DR. ZUDANS: I have one question.

Do you have some kind of a strain measuring device for buttress supports where the second section was attached to the ends to measure the ends load?

MR. TORBECK: No, we did not instrument that. We didn't think that was a representative -- well, it was not geared towards being anything representative of Mark I, so we didn't measure stresses there.

DR. ZUDANS: I didn't really mean to stress the end plates. I meant what did the outside space see in terms of loading that would show up as a reaction to the outside? I am not concerned about stresses on those end buttresses, but what kind of forces were transferred from this 22-1/2 degree segment into the support system.

MR. TORBECK: No, we didn't measure that.

DR. ZUDANS: We were given some information as to symmetry or no symmetry because one end was 90 degrees and the other end was 22-1/2. The added wall reactions you could see whether its movement, at least force-wise, whether it was symmetric or not.

DR. PLESSET: I think we had better go on.

MR. TORBECK: There are some things that we can do in terms of looking at the bending moments in the support columns in the north-south direction, and those were generally very small along the axis of the torus; those were very small.

DR. ZUDANS: I agree that you do have some instruments you may have to find out more.

(Slide)

MR. BROMAN: Okay. My name is Randy Broman. I am with Bechtel Power Corporation here in San Francisco. I will have to apologize here in starting. I have a case of laryngitis today, and if I sound funny, I am going to try to do the best I can. If people can't hear me, please speak up.

Bechtel was involved in the Mark I program in performing a number of tasks in the structural design and analysis area, and we were involved in the work that is being described here on the full-scale test facility. We were involved in work relating to the testing and evaluation of fluid structure interaction effects in the test data -- or in the testing, and, specially, the direction of our work was to develop or to incorporate, to provide a means of incorporating in the load definition a removal of fluid structure interaction effects which were found to have influenced the measure test data in the facility.

I will start out and give a little background as to how we got into this area, and then describe the analysis that we did for the fluid structure interaction.

Our work at Bechtel started with a generic structural analysis of a typical 56 PSI torus for condensation oscillation. This was in 1977, prior to the FSTF testing.

At that time there was a limited amount of test data available from the 4T facility, and that test data was used to develop an earlier preliminary load definition for condensation oscillation, and there was a desire to use that data and apply it to the loading information in analysis of a typical torus to determine what kind of response would be predicted.

It was recognized when this was done that the analysis was preliminary in that the FSTF testing was to follow. So it was simply an initial exercise to determine where our starting point was. We did that analysis. Subsequently in 1978, we began to get test data from the full-scale test facility, the condensation oscillation test data.

The data that we were getting from the facility, from the full-scale test facility, included data both on loading data, pressure data in the torus, and also structural response data, and John Torbeck has described, of course, the kind of data that we were getting.

Given that we had the loading data and the structural response data, what we did was we took some of the measured loading data and compared it against the analysis that we had previously done to determine whether the analysis that we had done, coupled with the new loading information we had, would tend to predict the kind of structural response that we were actually measuring in the FSTF facility.

When we did that exercise in 1978 during the course of the testing, the answer came back, no, we were not getting good correlation. In other words, the structural analysis was not predicting the structural response in the facility very well.

At that time we began evaluation really of the test data itself and the structural analysis techniques in an attempt to determine what the reason for the poor correlation was, and during the course of that evaluation, and looking at the pressure data and the structural response data, it was felt that the reason for the poor correlation lay in the area of fluid structure interaction.

At that point we began development of structural models of the FSTF, and one of the exercises that we did at that time is we took the measured pressure data from FSTF and applied it to a model of the FSTF dry structure, and that was the first time we got a reasonable correlation between the analysis and the test.

Now, the pressure data that we were applying, of course, incorporated any changes or variations in pressure associated with fluid structure interaction. Therefore, given that the FSI effect is incorporated in the load if it is applied to the dry structure, it should give a correct answer, and it did.

Starting in approximately September 1978, we began structural analysis of FSTF considering fluid structure interaction where we modelled both the structure and the FSTF structure and the contained fluid.

At that time we had three objectives. One was to extract rigid wall pressures from the test data, and I am going to go into the reasoning behind use of the rigid wall pressures in a moment. But the idea here is that it is the rigid wall pressure that is appropriate for use in a load definition for the condensation loading on other Mark I plans.

The second objective that we had in doing the work was to develop an analytical technique which would predict the test results for structural response, and the reasoning here is that after the FSTF testing is over, people are going to take the load definition and they are going to apply it in plant unique analysis for other plants. So, therefore, we wished to develop an analytical technique that would give us a correct or reasonable answer in having the FSTF data provided a good basis of correlation for our structural models.

The final reason or final objective that we had here was to assess structural response for our LDR load definitions, load definition report, which has been submitted to the NRC.

The idea here is that the LDR load definition is going to represent consideration of data from various test conditions, time periods, and the tests, what we wanted to do was to apply the LDR load definitions to our FSTF structural models and see whether the responses that we would predict on that basis would be reasonable in comparison with test data for structural response.

(Slide)

MR. BROMAN: This slide illustrates, I think, the basic concept or theory behind the analysis that we did.

At the top of the slide there is an equation for pressure. What this equation says is the total pressure is made up of a pressure due to source where the source would be the source at the vents in the facility plus a term which I have given as mass of water times the acceleration. That is the fluid structure interaction pressure. So the total pressure that one would measure at the surface of the shell on the inside of the shell would be made up of these two components.

Now, the FSI portion, the mass of water times the structure acceleration is facility unique. In other words, the acceleration of the structure is going to reflect the facility unique structural characteristics of the FSTF.

On the other hand, the pressue due to source is considered to be portable in terms of load definition. In other words, with regard to pressure due to source, the FSTF has been designed to represent a conservative case for definition of source pressure.

Now the concept in terms of our subsequent analysis is that one can take in the actual facility -- we had the situation here on the left where we have source pressure which is applied in the facility which is a flexible wall facility, and what we will get, the source pressure, will result in a pressure at the wall. That wall pressure will include the source term plus the FSI term and we will get an associated response due to the source in that manner.

It can be demonstrated that the actual situation of the source in the flexible wall system, the analysis for that situation can be broken down into two parts where if the source can be inferred from the test data, that source can be used to define a rigid wall pressure, and that is what is indicated here in the center diagram.

Once the rigid wall pressure has been defined based on the source, that rigid wall pressure can then be applied in an analysis of the facility in an analysis of the flexible structure, and one will obtain in that way the same answer as if the analysis was done directly in coupled fashion.

Now, the significance of this is that if the source can be inferred, the source can be used to define the rigid wall pressure. At this point we are generic.

The incorporation of the fluid structure interaction part or the part due to the flexible structure can then be done on a plant unique basis where the rigid wall pressure is applied in a plant unique analysis, and the plant unique fluid structure interaction effects are accounted for in the plant unique analysis.

DR. PLESSET: You say this could be demonstrated rigorously?

MR. BROMAN: It has been demonstrated.

DR. PLESSET: No limitations on the demonstration?

MR. BROMAN: As far as I am aware, no. This can be demonstrated.

DR. PLESSET: Yes, Mr. Sonin.

MR. SONIN: I might shed some light on this because we working on this at M.I.T. and one can show precisely what presumptions are on which this scheme is based on, and they are reasonable assumptions in this practical application. We have also done experiments that confirm the scheme under simulated pools and other conditions, so there is reason to believe that there is some -- that this scheme will hold.

DR. ZUDANS: Provided there is no feedback from torus deflections to the source function.

MR. SONIN: That is one of the major assumptions, that the torus deflections are small enough.

DR. PLESSET: It could be very well a significant coupling.

DR. BUSH: Tin canning, to me, would seem to be a real possibility here with the DOT ratios that you have.

MR. SONIN: The basic assumption is that the torus deflections are so small that the corresponding feedback to the bubble is not significant.

DR. ZUDANS: That must have been added to the linearity of the entire problem.

DR. SCHROCK: Is the acceleration a vector quantity in this?

MR. BROMAN: Let me just respond again to the first question. I might answer the question in a little bit different way and say that using the computer technique that I am going to be describing, we have, in fact, made this comparison. In other words, we have done the analysis from source, and then we have done the analysis in two steps as it is shown on the righthand side, and we have gotten identical answers.

DR. ZUDANS: I don't think that is a convincing argument because you have developed source on the basis of known results. All you did, you used the source to repeat the results, so that is not a justification. But I don't think we are taking exception to the principle. It is okay, as far as I am concerned.

DR. BUSH: When you convert from the generic to the plant specific, what is the major or what are the major factors that influence the D/T ratios, or the stiffening or what?

MR. BROMAN: In the plant unique structural characterisitcs?

DR. BUSH: Yes.

MR. BROMAN: Yes, exactly as you said.

DR. BUSH: Those are the critical factors?

MR. BROMAN: The D/T ratios is very important.

DR. BUSH: And any stiffening, I assume, would be very important because it would essentially be a pseudo D/T change if you stay on the structure.

MR. BROMAN: Ring girder stiffness and column stiffness, things like that?

DR. BUSH: Yes.

MR. BROMAN: There are plant unique structural characteristics in terms of the shell itself, the ring girder, and the columns. There is no question they are plant unique.

(Slide)

MR. BROMAN: Okay.

This slide described, really, the overall procedure for development of the load definition and also provides a lead-in and a description of the work that we did with respect to fluid structure interaction.

First of all, in terms of the overall procedure, I think certainly a basic statement to make here is that the basis for the load definition -- the load definition is the test data from the FSTF, and I might say that later on I am going to be talking about test data for a particular test and time period which we used to verify the procedure. But in terms of development of the load definition, test data from the various tests and time periods was taken into account and Umesh Saxena is going to be talking about that part of the work.

The second point I make here is that the loading we are talking about here is a periodic loading, and the amplitude of the loading is relatively constant. That is significant in terms of the procedure that was used to define the loads, and the procedure that will be used to do the analysis as follows.

Taking the data from the test the way the work was done, the pressure loading data was broken down in terms, or represented in terms of a four-year series, a co-signed series. The analysis was done for each term in the co-sign series, and then the analysis results for each term in the series were summed to get the total solution. So what we are doing is taking the periodic loading, representing it by a series, doing the analysis term by term, and then summing.

(Slide)

MR. BROMAN: I have a slide here which shows the test data itself simply to demonstrate that we do have a periodic type of loading. What the slide shows here is it shows the total vertical force on the torus for a particular time during the testing. This happens to be time in test M-8 that we used to verify the model. I am going to talk about that in a minute. And what this particular plot represents is that pressure from the individual gauges on the torus was integrated to get total vertical force on the torus.

When we developed our FSI correction curve, which I am going to show in a minute, this total vertical force was the parameter that was used for the correlation. So I am simply showing here that this is a periodic load.

DR. ZUDANS: I have a question to the previous slide. Let me just repeat and see whether I understood you correctly.

When you did two sets of analysis, one you did assign periodic single frequency source and fixed the walls and computered the rigid wall loads. The other one you had fluid in there and flexible walls and you had two sets of dynamic responses, periodic responses. And then you computed that correction factor that you call -- does it mean that what you plan to do is to just factor the measured pressures on the surface by this factor, and as far as their spatial distribution and face distribution to leave it the same as it was measured in FSTF?

MR. BROMAN: No. Everything that you said is what we did, I guess, up to the last point, and that is that the pressure distribution for the rigid wall case is different from that from the flexible wall case.

DR. ZUDANS: Spatially?

MR. BROMAN: Spatially, that is correct.

DR. ZUDANS: And what do you apply this factor to?

MR. BROMAN: To total integrated vertical load, and that is a significant point. What we are correcting is the integrated pressure or the total vertical load.

DR. ZUDANS: So you do not attempt to produce correct spatial distribution of response functions within the shell? The only thing you correct is the net vertical load?

MR. BROMAN: That is correct.

DR. ZUDANS: What do you then know about the state of stress in the torus itself?

MR. BROMAN: I am afraid I don't understand the question, but maybe if I go through the sequence --

DR. ZUDANS: Why don't you proceed. Maybe I am jumping ahead.

(Slide)

MR. BROMAN: I guess actually you have started to get into the analysis process itself. As I said, the correction that we are making is on a total vertical load. What I might say here is that in the analysis what we found is that for the rigid wall loading a pressure distribution in the torus very closely approximated a hydrostatic pressure distribution. In other words, if you apply the source pressures in the model, the fluid model, with rigid wall pressures and predict the rigid wall pressure, what you have is something very close to a hydrostatic distribution.

Now, in the flexible wall case, that is not true, and the reason is that the flexible wall pressures will vary based on, let's say, mode shape effects in the torus. In other words, the flexible wall pressures were being affected by shell accelerations, and the shell accelerations, in turn, are being affected by mode shape or deflection patterns for the torus. So therefore the flexible wall pressure has a different shape.

Now the reason that we chose to use integrated pressure for the correction is that we felt basically that that is a way of averaging out the error in the procedure. We could have done the correlation based on any individual or single pressure gauge in the torus, but our feeling was that if you worked based on a single pressure gauge, you are subject to any error in measurement of that particular gauge, and also any error in prediction of the structural response or the pressure at that local point. By working on integrated pressure, that is a manner of averaging out the errors or limiting the error in the procedure.

The manner in which the work was done or the FSI correction curve was done is indicated here under the second bullet. We developed the finite element model of the FSTF and contained fluid. We used the NASTRAN computer program for this work.

As has been indicated already, we did two sets of analyses and we applied unit sources on the downcomers, and we repeated each analysis twice, one for the flexible structure and one for the fluid with rigid wall. We ran the analyses at increments over the range of frequencies that were determined to be significant from the test data.

When we do the two series of analyses, what we get out of the analyses is wall pressure, rigid wall pressure and flexible wall pressure. When we had done the analysis in each case, we integrated the wall pressures to get net vertical load. The net vertical load is the quantity that we compared to develop our ratio of rigid to flexible pressure.

DR. ZUDANS: Now, were these analyses then essentially quasi-static periodic load and you really didn't do a dynamic analysis? They are not transient. They are stationary steady state solution?

MR. BROMAN: Steady state solutions, yes.

DR. ZUDANS: So they are static solutions in this case. Essentially static frequency comes in as a factor?

MR. BROMAN: I am not sure I would call it static, but, yes, they are steady state solutions.

DR. ZUDANS: Okay.

MR. BROMAN: Again, recall back to what I said before. What we are doing is we are taking the test data which is a periodic loading of fairly constant amplitude, and we are breaking it down into its frequency subcomponents, and then we are doing the analysis for each component, each frequency component in the loading, simply combining results.

(Slide)

MR. BROMAN: On this slide I have put a description of the analytical model. I might say in your handout there is a picture of the analytical model. The model was developed using NASTRAN. We modelled one-half of the FSTF facility simply assuming symmetry about the mid-plane of the facility. There are about 500 elements and modes in the model. The shell is modelled primarily using quadrilateral shell elements, and then we also represented the structural details of the facility also with columns, the ring girder, and so on.

The model also includes the FSTF of the end caps in the facility. It includes the restraint rods that go back to abuttrance and, in fact, we even calculated the stiffness for the abuttrance themselves although they are so rigid that their flexibility didn't really influence the solution.

The model also includes the fluid in the facility. We used the technique to represent the fluid which has come to be referred to as the consistent mass matrix method. That is a representation or a solution technique which considers the fluid to be imcompressible. So it is an incompressible fluid solution.

The way the model is configured, it allows the load to be applied at the source which is what we did to develop the correction curve, and the load can also be applied at the wall or on the wall of the model. This allows us to do the comparison between the source solution and the rigid wall solution, and also it allows to check out the load definition which is a rigid wall load definition.

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(Slide)

MR. BROMAN: As I indicated, there is a picture of the model in your handout. I am not sure there is much more that I can say about it. For the most part, it is a standard finite element model with the inclusion of the fluid in the torus.

(Slide)

MR. BROMAN: This slide indicates what we did to verify the model. Basically we had three methods for verification of the model.

The first level of verification is pretty much standard procedure for verification of a finite element model that consisted of static load cases, things like applying uniform internal pressure and checking the calculated show of stresses against what they should be from a hand solution, checking the weight of the model to make sure it weighed what it should, and so on.

The second level of verification was comparison against shake test results. There was a shake test done in this facility using an eccentric mass shaker, and the response was measured for the applied vibratory load, and we compared -- basically what we compared from that is the shake test defined for us or indicated to us what were the frequencies of high response in the actual test facility. We were able to compare that against the responses that we were getting from our analytical model.

The third method of verification was use of the actual test data from the condensation oscillation testing. The data that we used was from test M-8, the period from 24 to 25 seconds, and the idea here and the manner in which we did the verification was we used the FSI correction curve that we developed to correct the test data for that period.

So we took and we measured flexible wall pressures. We developed total vertical load by integration. We applied our FSI correction curve. We developed the rigid wall pressure, then we applied the rigid wall pressure to them in an analysis using the model.

The check on the model is to answer the question on this basis: Does the model predict the test data for structural response in that time period? And I have some results of that comparison here.

DR. ZUDANS: At this point you can answer my previous question: How did you apply your factor to generate the rigid wall pressure? To what did you apply to the measured pressures on the test?

MR. BROMAN: Yes. Let me describe the process.

We have measured flexible wall pressure data, a number of different gauges on the shell. We take the pressure data from those gauges, and we integrate all the gauges simply multiplying the gauge times tributory area type calculation.

We integrate the pressure over the surface of the shell to get net vertical load as a function of time, flexible wall.

Now we take that net vertical load as a function of time and we correct that using the curve to get rigid wall total vertical load as a function of time.

The rigid wall total vertical load as a function of time is what we apply in the analysis.

DR. ZUDANS: Yes, but what do you apply to this surface of the shell?

MR. BROMAN: What we found in the analysis, in the source analysis, when we made the source analysis for the fluid with the rigid wall, what we found was that the pressure distribution generated by the source, the rigid wall pressure distribution, closely approximated a hydrostatic distribution about the circumference and a uniform distribution along the length of the facility.

Now, given that we have a hydrostatic uniform distribution, we can take that distribution and calculate the total vertical load associated with it. It has a total vertical load, so what we do when we know what the total rigid wall load is supposed to be based on our FSI correction, we then take that total vertical load and break it down and apply it as a hydrostatic uniform distribution.

DR. ZUDANS: And where does your correction come in terms of different frequencies that are generated?

MR. BROMAN: The correction is done at each frequenc . In other words, as I say, we took total vertical load, and we broke it down in a frequency by frequency basis so that the calculation that I am talking about is done one frequency at a time.

DR. ZUDANS: Okay. So that is all right.

So what you are doing really is a quasi-static calculation where you didn't have to integrate all the period or you could simply have a static solution with the frequency affecting your stiffness matrixes.

MR. BROMAN: Yes.

DR. ZUDANS: Then you turn around and each of these pressures now are multiplied with a multiplication factor that you completed on the basis of net vertical load, and then you turn around and adapt all these contributions, add up all these contributions, and then you get the final solution?

MR. BROMAN: Yes, that is correct.

DR. ZUDANS: Okay.

DR. BUSH: It seems to me that any weaknesses which are in the assumption going from the rigid wall -- or from the flexible wall to the rigid wall generic case are going to be mirrored when you go back towards the flexible wall.

DR. ZUDANS: Of course. No way of considering the fact that the source load may be influenced by structure. But, however, you can't get everything.

MR. BROMAN: Let me say something about the source load. Let me say a little bit about source load being influenced by structure.

I recognize that certainly it is an assumption in the technique, and I might say that for the condensation oscillation loading typically we were getting dominant load frequencies in, I guess if you consider all the tests, let's the 4 to 7 -- 4 to 8 Hertz range, that is your dominant loading frequency. The structure does not have very significant dynamic amplification or resonance at those frequencies, so the structure, this structure, would not tend to influence the source very much because its frequencies are higher than the dominant frequency in the loading.

DR. ZUDANS: Okay.

(Slide)

MR. BROMAN: This curve simply shows the result of the analysis. What it shows is the amplification curve. It is a plot of amplification factor as a function of frequency. At the lefthand side of the plot, of course, the curve is asyndetonic to 1.0. It simply says for zero frequency load there is no amplification which should be the case.

Another important characteristic of this curve is that it shows peak amplification, in other words, maximum difference between flexible and rigid wall pressure, at about 16-1/2 Hertz, in the range of 16 to 17 Hertz. This happens to be the frequencies of storing dynamic response for this shell, for this test facility.

I might say this is also significant in the sense that when the shake test -- this is the analysis result. When the shake test was done on the facility, the shake test showed the same type of behavior which is at the maximum amplification or maximum dynamic response in the shake test tended to occur in the 16 to 17 Hertz range.

DR. ZUDANS: Here to generate this curve that you just showed you performed calculations essentially at one-thirteenth--

MR. BROMAN: Yes, that is correct. This curve represents a repetition of the calculation at 1 Hertz intervals across the frequency range that is shown here.

DR. ZUDANS: Once with rigid walls and once with flexible walls?

MR. BROMAN: Yes. That is what the curve is. It is calculation of the ratio successively, and this curve represents the ratio of total vertical loads, flexible versus rigid.

DR. ZUDANS: Just in curiosity, when you did the dynamic analysis, did you really analyze it as a dynamic problem for these frequencies?

MR. BROMAN: You mean as a time history? Did we do

49 a time history analysis?

DR. ZUDANS: Right.

MR. BROMAN: No, not for condensation oscillation.

DR. ZUDANS: When you did these, when you generated this curve, did you do a time history analysis for one side?

MR. BROMAN: No. This is based on frequency response analysis.

Another result of the analysis that I would just like to mention briefly, what this curve shows is a typical result from the verification run. As I indicated earlier, we made a verification run where we took our rigid wall loading definition based on this test, M-8, 24 to 25 seconds, and we applied it to the model. This is one of the results of that calculation, and the point I want to make here goes as follows:

This happens to be axial membrane stress at bottom mid-span of the torus, and the way we have plotted the result here is we have plotted the stress on a cumulative basis. You remember, as I indicated before, we are doing this analysis on a frequency by frequency basis. In other words, we do the analysis at each frequency increment and then we sum results to get the total.

Now, the way I have done this is I have plotted cumulative response versus frequency as how the response builds up as I add more and more terms in my solution.

The point I want to make is that as we approach a value of about 30 Hertz, the additional contribution to response is really negligible on this plot.

Just to go into that a little more down in here, we have significant loading. This was really the dominant frequency of the loading in this particular time period. In here, in this range here, we have a combination both of loading and also of some structural response. If you look at your amplification curve, there is some amplification here. So there is both some load and some amplification.

As we get up here, what we feel based on calculations and studies we have made, there is additional build-up as you go through here, the bounce frequency of the torus considering just the total mass versus stiffness of the columns lies in the 25 Hertz range. Beyond this there is simply not very much in the source to contribute to the total response.

(Slide)

MR. BROMAN: The last slide here is simply, in a sense, the results of -- actually it presents results for a couple of different cases.

First of all, it presents results for this verification run that I described.

What is indicated here is key structural response quantities from the FSTF at bottom dead center mid-bay of the torus, bottom of the shell, lowest point in the shell. We have got three quantities here: axial membrane stress, hoop membrane stress and radial deflection.

The other quantities that we have put on here are the two column forces, the inner and outer columns in the facility.

On the lefthand column on this chart I have indicated the actual test data for this time period, test M-8, 24 to 25 seconds.

In the next two columns I have indicated results of the analysis that I described, the verification run. Okay, the first column here is algebraic sum. The algebraic sum considers the specific phase relationships or signs on the individual frequency components in the solution. In other words, when we do the co-sign series fit to the test data, the terms come out and they have signs, plus or minus signs, reflecting their phasing in the loading.

In the first column here we have considered the signs. So this represents the true verification of the analytical model considering the signs, and I think you can see that the correlation between the analysis and test data was reasonably good.

In the next column here I have made the same comparison only neglecting the signs of the frequency components, and I think the reason for doing that was simply the thought that perhaps the signs might change with time or something like that, and the idea here is to see how much does that change the predicted result, and you can see, as one might expect, it tends to increase the predicted response. In other words, some of the components in the load were out of phase, and when we do this we have lost the phasing. It is a conservative representation.

On the next three columns, this represents analysis of the LDR load definitions. Umesh Saxena is going to follow me. He is going to describe how the LDR load definition was developed, but the point is during the development of the LDR load definition, number one, there was consideration of tests in time periods other than that which we considered in our verification run. In other words, they considered all of the tests and time periods.

And, second of all, to some extent there was a bounding of the data in the load definition. If you look at the results in these three columns, you can see what the comparison between an analysis for the FSTF, the LDR load definition versus the test data, is.

Now, I might say go back here to this column and say this test data for structural response represents a particular test and time period. It does represent, let's say, nearly the worst time period in terms of structural response. I won't say absolutely the worst time period, but it was certainly close. This is within a few percent of the worst responses measured for any test.

I think you can see if you look at this that when you do the analysis for the LDR load definition and compare with the test data, certainly some conservatism has been introduced.

I guess another point to make is that the worst case happened to be represented by LDR load case no. 2. I will mention what the difference between the three cases in a second. In any case, this was the worst case.

I have put on the righthand side here simply the ratio of the calculated response to the FSTF test data, and you can see that based on the LDR load definition and the analytical technique that we have, we certainly are conservative with respect to the test data.

Going back here, these three cases do represent three different tests and time periods, and they were felt to be governing cases for definition of loads and the LDR. They are characterized by a somewhat different frequency content in the three different time periods. Umesh Saxena is going to go into that in more detail.

I think that that basically concludes the analysis that was done for FSI.

DR. PLESSET: Thank you.

Maybe we should go directly to the last part of this.

MR. SAXENA: I am Umesh Saxena. I work for General Electric. I will be describing the C and O definition which was developed for the torus shell. This load specification is developed for the hypothetical loss of coolant accident.

As John Torbeck discussed this loading is caused by the periodic pressure oscillations on the torus shell. In developing this load definition, FSTF test data was used and the load definition was developed in a very conservative manner.

(Slide)

MR. SAXENA: The items I would like to cover in my presentation will be the objective and the load definition, the approach which is followed in coming up with the load definition, and the FSTF test data, what are the key features, and the data application which includes data base selection which found the data base for the load definition, the data reduction/analysis which was performed to come up with the load definition, and finally the load definition, and last, the summarization.

(Slide)

MR. SAXENA: We will start with the objective to develop the condensation oscillation load definition for torus shell from the FSTF test data.

Briefly, the test which were involved in this load definition, we examined the entire FSTF CO. test data. From this examination the maximum pressure amplitude data segments were selected as data base.

Wall pressures which were taken from 24 sensors on the surface were spatially averaged, leading to the average vertical pressure loading on the torus shell.

Then from the data base segment selected,the PSD analyses were performed to see the frequency curve.

An FSTF FSI effects were accounted for, as Mr. Broman said, to develop an FSI curve. We used this curve to account for the FSTF FSI effects.

Finally, a load definition, rigid wall pressures as a function of frequency were specified as load definition.

(Slide)

MR. SAXENA: I would like to show you some key features. FSTF facility provided the test data, the Mark I full-scale test data, and also we have the test of one bay of the Mark I torus.

We also have the test data for both liquid and steam break test.

We also noted from the test data the load magnitude of break size and type dependent, and we also noted the highest pressure amplitude for observing the large liquid break.

Just for history purposes, this is a time history which was measured at the single location of transducer 3181 which was right at the bottom side. You can see that the pressure magnitude initially increases, then eventually starts going down, and somewhere in that time duration we have got the maximum sighting. This is from the test number M-7, the large steam break.

A similar time history from the same pressure location is for test M-8 which is the large liquid break.

Once again we see that this pressure amplitude initially increases and goes to a peak value, then it starts decreasing with the time.

Under the data application the first part was data base selection. We again looked very carefully at all the FSTF data, and we selected a segment which produced the maximum CO. loading. Based on this criteria we picked up the two runs, the M-7 and M-8 which were large steam and large liquid break, and from these two test runs we picked up three data segments of maximum pressure amplitude as a data base for the final load definition.

(Slide)

MR. SAXENA: The data segment which was selected from these two test runs, M-7 and M-8, are these: M-8 we picked up the 4 second duration data, and that characterized the maximum power at between 4 and 5 Hertz.

The second segment from the same run for the duration of 24 to 28 seconds characterized the maximum power at the frequency between 5 and 6 Hertz.

The M-7 run for this duration characterized the datum with the maximum power at frequency of 6 or 7 Hertz.

So this way by taking three data segments, we bounded the frequency variation.

(Slide)

MR. SAXENA: This is the time history, and this time history is different than the previous one. This is the time history of the average vertical pressure loading of the torus.

(Slide)

MR. SAXENA: And you can see from the PSD curve the dominant or the maximum power is around 4 and 5, as I said already. Again, this is a time history for data segment number 2.

(Slide)

MR. SAXENA: And this is the PSD information for the segment number 2. You can see the dominant frequency for the maximum power is a different dominant frequency.

(Slide)

MR. SAXENA: And finally for the third segment is the time history, and this is a PSD run which shows the maximum power of the dominant frequency between 6 and 7.

(Slide)

MR. SAXENA: So if you put all these together, these three together, I think the shift is quite obvious.

(Last six slides placed on top of one another.)

MR. SAXENA: As to the data application, therefore, for this data analysis for each of the selected data segments, we especially integrated the major wall measures to provide an average vertical type load, and I would like to, just for clarity (slide) show you how we integrated those pressures.

So we can assume that we have got individual transducers and we took this pressure, come up with FI to get the F total. You get -- this was integrated over this total number of sensors, and this average pressure was obtained by dividing the F total by the A total. The A total is the summation of this segment here.

(Slide)

MR. SAXENA: Coming back to the data reduction analysis after obtaining the average pressure time history, so we got so-called integrated vertical pressure time history which was done for me.

This obtained time history represents the overall loading on the torus shell.

Then from the data segments power spectral density calculated and PSD of each one segment was generated. So if we go back to the three data segments, 4 seconds each, that means we have got four 1 second PSD's for each data segment, and PSD values were averaged over the 4 seconds period for each data segment, and by doing that we came up with the amplitude versus the frequency values which were compiled.

Next, in order to make a generic load definition, we have to account for the FSI -- for the FSTF FSI contribution.

And then we explain the procedure developed and came up with this so-called FSI curve. So FSI factor as a function of frequency obtained from that analysis, and then the factor at each frequency was applied to this amplitude frequency values to come up with so-called rigid wall pressures.

(Slide)

MR. SAXENA: And once again, since we are dealing with the average wall pressures, then we came up by using this relationship, a so-called base line rigid wall pressure, as a maximum pressure which would be seen at the bottom of the torus, and this was obtained by using P(b), the base line pressure times K with P bar, and K is the factor which was calculated to show the highest distribution of the pressure on the torus shell.

And as part of the load definition, torus loading defined as a rigid wall pressure versus frequency. Let me clarify here that the rigid wall for that part of the base line magnitude, the three alternate frequency spectra, 4 to 16 Hertz, is specified which are the same as three cases which Randy Broman mentioned in his presentation.

This alternate spectra --

DR. ZUDANS: At this point I don't get -- on the first line this is a continuation of my previous question, the first item, the pressure is essentially hydrostatic except its amplitude is increased as a function of frequency.

MR. SAXENA: Let me clarify here, the first line, the torus loading defined as rigid wall pressure versus frequency --

DR. ZUDANS: And that quantity, rigid wall pressure, that is the hydrostatic pressure?

MR. SAXENA: Previously we were dealing with the average wall pressure, and this average wall pressure --

(Slide)

MR. SAXENA: Let me make it very clear, this is the average type of distribution.

DR. ZUDANS: On a projected diameter?

MR. SAXENA: That is right.

So this is what we call my rigid or base line rigid wall pressure.

DR. ZUDANS: Where is this line applied? Do you wrap it around the circumference?

MR. SAXENA: Yes. We provide over a specified attenuation from base line.

DR. ZUDANS: So you are saying if you took a vertical line from a surface support to the bottom, you would have a later distribution?

MR. SAXENA: Yes. It will be shown in my next slide.

DR. ZUDANS: So it is hydrostatic?

MR. SAXENA: It comes up very close to being a hydrostatic distribution.

DR. ZUDANS: Now I finally understand.

Go ahead.

MR. SAXENA: So the load definition looks like this.

(Slide)

MR. SAXENA: In which you have the amplitude versus frequency which goes from the range of 0 to 50 Hertz, and here we have got three ordinate spectra, number 1, and number 2, and number 3. For the load definition, the load definitely consists of the frequency from 0 to 5, as defined here, 15, 16 to 50 plus one of these spectra at a time will be placed into this open box, and is again, this is the base line rigid wall pressure.

(Slide)

MR. SAXENA: Now here we specify how we define the pressure distribution along the wall which is more or less hydrostatic distribution.

(Slide)

MR. SAXENA: Realizing that we had some differences between the FSTF vent area pool area ratio, so we developed so-called multiplication factor which can be used for different plants depending upon what is the value of this pool-to-vent area ratio.

(Slide)

MR. SAXENA: So in John Torbeck's presentation you saw already the average amplitude correlation with the energy rate. So here is what we did.

We did some statistical analysis and calculated what the 99 percent confidence interval which will show the bonding value for this curt ...n in the data.

Right over here, this is the predictive maximum enthalpy rate for CO for a typical plant. So if you look over here, on the top of my head for this kind of energy rate, the maximum pressure value, the 95 percent confidence interval is about 7 point some PSI.

(Slide)

MR. SAXENA: Finally, we can summarize the definition, the load definition in this manner: Full-scale test data employed, data segments of maximum pressure amplitude formed the data base.

No credit for amplitude and frequency variation with time (observed during the test) was taken because we used the 4 second maximum pressure ampu-segment(phonetic) and applied it over the entire CO duration.

FSTF FSI effects accounted for so we can make it a sort of generic general loading.

Finally, CO load definition conservatively formulated.

DR. ZUDANS: Now, this is the load that will be used for all plants?

MR. SAXENA: Yes. This is the rigid wall load.

DR. ZUDANS: Now, the FSI factor was developed from

FSTF facilities?

MR. SAXENA: That's right.

DR. ZUDANS: And that is very facility dependent. So how can you justify using FSI factor from FSTF to generate the loads on other facilities where the frequency contents will be different?

MR. BROMAN: Okay. The specific objective of the FSI work that I described was to develop a rigid wall load which would remove the facility unique FSI effects.

DR. ZUDANS: But it didn't because you analyzed, you performed harmonic analysis of FSTF facility to generate for each frequency what you call FSI factors, and this factor is the one that is used to define the dynamic load as a function of frequency contents for other facilities, and clearly, this carries with itself the natural frequencies of the FSTF and not of the other facilities.

MR. BROMAN: Well, okay.

First of all, let me finish the first statement that I was going to make. A specific objective of the analysis done on FSTF was to develop a rigid wall load which would not have in it facility unique FSI effects.

The second part of the story or the second thing that it is necessary to do is when the plant unique analysis is done or when the analysis of each plant is done, the FSI characteristics unique to that facility must be incorporated in the analysis of that facility, and that is exactly what will be done. In other words, the analysis of each Mark I plant will consider FSI for that plant, and the analytical technique.

DR. ZUDANS: Okay. That means that you have to do what you did for FSTF for every facility?

MR. BROMAN: You would have to do a couple fluid structure analysis, yes.

DR. ZUDANS: So you did not eliminate that need. Why don't you just go directly to the source and use the source function to get the response?

MR. BROMAN: Okay. You could, in fact, do the analysis from the source, and as indicated by, I think it was the second side that I showed, you should get the same answer, whether you go from source or from rigid wall. The reason to use rigid wall loading was simply for convenience. Some of the computer programs being used by participants in the program work better if you use rigid wall loading. It is simply a convenience.

If you are using NASTRAN which is what we did for the FSTF work, it would make no difference.

DR. ZUDANS: All right.

DR. PLESSET: I think we will take a break now.

(A 10 minute break was taken.)

DR. PLESSET: On the record.

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It turns out that we have to stop the meeting at 5:30 and I would like to have both Chris Grimes and Mr. Steiner to know that. So let's go on.

MR. GRIMES: I would like to preface Professor Brennen's presentation by saying that we are very fortunate. It appears that the remainder of the afternoon will go much faster than we had originally envisioned and hopefully we will be able to recoup our losses and achieve our schedule by the end.

DR. PLESSET: Well we don't want to cut anything off, but we do have a deadline for other reasons, so why don't you go ahead.

MR. BRENNEN: In the interest of time, Mr. Chairman, I will skip a great many of the viewgraphs in the handout.

DR. PLESSET: But not the essential ones.

MR. BRENNEN: I hope not, and I will move directly into those areas where there is some difference of opinion between the Staff and the Mark I owners group.

And let me just say, by way of prefacing my remarks, that because of the processes that we are dealing with are unsteady, turbulent, two-phase flows, and none of us have any reliable or proven engineering methods for dealing with these flows, that the load definition necessarily relies on full-scale measurements, and that is the reason that the full-scale test facility blowdowns were conducted.

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The only slight problem with that is that they are entirely prototypical, and I want to emphasize that again, except for FSI effects, and as it has already been discussed, we therefore require an FSI model to extract the FSTF FSI effects, and then further application of the same kind of technique to insert the FSI effects into the plant's unique analysis.

(Slide)

MR. BRENNEN: The only questions, therefore, which might remain concern the FSI effects, and the extent of the data base that is available from the point of view of determining uncertainties in the load definitions.

I won't say anything more about the FSI effects. I think, in gener l, our response has been that this is a reasonable first order approach to the structure interaction problem. But there are still some uncertainties associated with it, but that that can be taken care of by sufficient margin in defining the actual loads themselves.

(Slide)

MR. BRENNEN: So I am going to move and talk entirely from now on about the adequacy of the data base in terms of generating a load definition. This is the FSTF test matrix, and I just want to point out again that the data bases for condensation oscillations is primarily M-8. I should perhaps add M-7 in there too, though it is only recently, just a few moments ago, that I realized how M-7 was actually factored into the problem. The chugging data base is really much more extensive, in a sense, because it is taken as the worst events occurring in four different blowdowns.

We don't really have too much argument with the chugging data base. The points of contention, really, revolve around the M-8, the condensation oscillation data base.

(Slide)

MR. BRENNEN: Let me give you a little bit of our present thinking on condensation oscillations and on the FSTF results as we see them.

This graph, and I hope it is clear to you, is a plot of the peak-to-peak pressure amplitude, the bottom center pressure this is, and it is a measure, therefore, of the total load.

Since the distribution is always the same, it is plotted against the total mass flux in the vents, and when one inspects this one sees that M-8 has by far the largest steam flow rate because it is a liquid blowdown, and that with time during M-8 the condensation oscillations first grow and then they decay.

Now most of the tests show the same pattern and growth in the amplitude followed by a decay.

M-7 which is also factored into the condensation oscillation data base is down here. I have only shown two others, M-2 and M-5. The main point of this figure is to point out that by far the largest amplitudes occurred during M-8. Therefore, the condensation oscillation data base is basically constructed from M-8, and that that occurred at the largest flow rates, and that none of the other tests approached either those amplitudes or that flow rate.

I might just also say a word about what we feel determines this history of the fluctu tions as a function of time.

It is only supposition, but model tests have indicated that the amplitude is a function of the air content and the steam flow rate, and the steam flow, the steam flow rate, and possibly the pool temperature. Now, the pool temperature influence is not quite so clear for CO but we feel at the present time that this initial increase is due to a decline in the air content, and that that finally reaches a peak, and that the subsequent decline in CO is a result of a decreasing steam flow rate. That, of course, is purely speculative.

Let me just mention what kind of magnitudes M-8 actually achieves. It achieves DBA levels. It is really the only test that achieves DBA levels.

(Slide)

MR. BRENNEN: This graph is a graph of the mass flow rate against time with some typical DBA's on there, and also

M-8. M-7 does not approach a DBA. Therefore, we really only have one blowdown which approaches a DBA blowdown.

You might also ask whether this other data that might be factored in in trying to determine an uncertainty in the CO, the fact of the matter is that virtually all of the previous tests that have been performed, R. Vicken and GC 14, do not approach the steam flow rate, the total flow rate of M-8.

And our concern is that knowing that these processes have a stochastic nature, we are concerned that we do not really have any handle on the uncertainty and the magnitude of the CO at these large flow rates. We really only have one blowdown on which to base any judgment. The feeling at the present time is that is not sufficient.

We do, however, recognize, and I must stress this, that M-8 is prototypical of a DBA, and therefore the load definition, as presently constituted, is conservative relative to M-8, and, therefore, is a reasonable load definition as it stands.

However, we cannot justify for ourselves that it is necessarily conservative because we don't have, I believe, any way of evaluating the uncertainty in the CO magnitudes at that large flow rate.

DR. ZUDANS: I have a question.

Are you just as uncertain about the mean value or mostly about the oscillations about the mean?

MR. BRENNEN: I am concerned with the following, and let me go back to the graph that I had for a moment here. I am concerned with the following:

I have one curve, if you like, for a high flow rate. I do not know if I repeated the test whether it might be like that or it might not be like this. This may be the very worst case. If we repeated the test, perhaps it would be lower. Perhaps it would be higher. I don't know with one value, with one test on which to base my judgment. I have no sure way of knowing whether that is high relative to the ultimate mean that one might observe or low.

I think that is the principal point that concerns me with respect to the load definition, and I think I will just leave it at that if you wish.

MR. GRIMES: Let me try to summarize the Staff's position regarding condensation oscillations, and that is that based on the review that we have done of the FSTF results, we feel that M-8, because of its prototypicalness and because of the nature in which the loads have been derived from the test data, inherently includes some conservatisms relative to M-8 that we feel could probably counterbalance any uncertainty associated with CO load magnitudes, and that all that is left, therefore, is to demonstrate that. So we will proceed with implementation to the program using the present load defintion techniques and then confirm that the uncertainty in the load magnitudes is within the bounds of the inherent conservatisms at a later time.

DR. PLESSET: How will you do that?

MR. GRIMES: By requiring additional FSTF tests to establish an uncertainty.

DR. PLESSET: I guess that is the crux of the matter. You have said it.

MR. GRIMES: Thank you for bringing it out.

DR. BUSH: I thought Tepco was planning some tests?

DR. PLESSET: Those are Mark II.

This is kind of emphasizing, I guess, what Professor Brennen called the stochastic nature. I don't like that word. It is not very stochastic in some sense. It is not statistics in the sense --

MR. BRENNEN: I will withdraw that word.

DR. ZUDANS: I guess the only reason that you need it is to be able to set some kind of a confidence, but is it really that empty of required information on the other tests? To me it looks like very deterministic.

MR. BRENNEN: Very deterministic?

DR. ZUDANS: The results are very deterministic. I am just wondering --

MR. BRENNEN: I don't have any way of quantifying either the determinism or the uncertainty at the present time.

DR. ZUDANS: What I am wondering is if you ran 10 more tests that you would really depart that much from M-8.

MR. BRENNEN: I would be surprised if you would also. I would be surprised, but that is a judgment based on a gut feeling rather than any available data.

DR. ZUDANS: I think that in addition to that, here you have something that you have no place else, a full-scale test and a full blowdown, and you don't want to believe it. How can you believe all the other things that you are accepting without such cases?

MR. BRENNEN: I did not say that I didn't want to believe it. I am quite prepared to believe it.

DR. ZUDANS: But the point is, my feeling is this additional test is not necessary, but that is just a personal feeling. It is not the position of the Committee. I think that I believe this test more than anything else I have seen.

MR. BRENNEN: So do I. It is simply a matter that I cannot prove it to be, the load definition to be conservative unless I know what kind of uncertainty there is in that result.

MR. GRIMES: Let me suggest, Mr. Chairman, we have taken the position that we do not feel that the Staff should be in an untenable position of using a gut feeling to establish uncertainty levels in the condensation process, and, therefore, we have required that additional testing, and that is our position at this time. And if the ACRS feels that you could provide us, in any way, guidance on how we could proceed in this matter, we would be happy to accept it.

But on the present time, based on our assessment of the data, and especially the way that we tried to factor in all of the existing knowledge that there is regarding condensation phenomena, we would feel that we should proceed with additional testing and confirm that what we are doing is reasonable and prudent.

DR. BUSH: First of all, you put hypothetical bounds around this hypothetical action. Now you strip out the conservatisms. In other words, what are the implications of this with regard to structural response which, after all, is the ultimate factor with which you are concerned? Let's increase it by 20 percent.

MR. GRIMES: Well, it would not be like 20 percent. We would be talking about increasing it like orders of magnitude -- I'm sorry.

DR. BUSH: Not orders of magnitude.

MR. GRIMES: Factors of two or three.

DR. BUSH: If you want to talk about factors of two, I might buy it, but I wouldn't buy orders of magnitude. You don't have the energy source to get orders of magnitude.

MR. BRENNEN: If you look at the data that is available at the lower flow rates, that can vary by as much as a factor of two or three in magnitude.

DR. CATTON: For a repeated run?

MR. BRENNEN: For apparently data that should be consistent, yes.

DR. ZUDANS: But those loads are small to begin with. In other words, you may have a lot more relative loads as compared to the information that you have in the larger load test. I don't know.

DR. CATTON: What, specifically, would be the test that you are requiring?

MR. GRIMES: We are talking about an additional two tests that, as I understand it, there is a contingency for two beyond that. We are looking at like two more tests to establish some kind of repeatability.

DR. CATTON: There would be two more M-8's?

MR. GRIMES: Right.

DR. BUSH: What would you do if they were both much lower?

MR. GRIMES: If they are both much lower?

DR. BUSH: That is right. It is not an impossibility. What would you do?

MR. GRIMES: I would be satisfied. Much lower?

DR. BUSH: What would you do? I am sure you would redesign the first M-8, wouldn't you?

MR. GRIMES: Probably get the proposal from the Mark I owners group to lower the loads.

DR. CATTON: And a discussion as to why this one was not typical.

MR. BROMAN: I would like to make a point with respect to the comment "why not add 20 percent." I would like to reiterate the point made on my slide that when we do the analysis for the LDR load definition, we have a margin on calculated structural response of at least 100 percent. Personally, I think that is plenty.

DR. PLESSET: Yes.

MR. LOGUE: I am Bob Logue. I am the owner's group chairman. I would like to also say here at this point in time that we are quite concerned of the fact that there is this added test being asked for. It is not an inexpensive test. It is not going to be done in a month or two. It would be taking almost 9 months or a year to really complete this test and get the results.

Now, if those results turn out that there is some difference, there may be some questions asked about why they are different, and then we will be asked to do some more tests, and I can see this as a completely open book.

The NRC letter did not say two additional tests. It says "until they are satisfied." And we are very much concerned about this to the point where I have told Mr. Grimes that when we do respond to his request, that we may very well ask to speak to their management to express our concern about the openness of this.

Meanwhile, we are proceeding with modifications based upon the LDR load definition, and if it turns out that we have half the load, we will have spent twice the money for this particular aspect.

DR. PLESSET: Well, I think that to respond to your concern, that this is an indefinite process. I think that you are being a little bit pessimistic. I would expect that you are being -- I think that the Staff would hopefully be reasonable. They are not going to continue to want this FSTF run indefinitely. I'm sure they don't want that. You would agree with that statement, wouldn't you?

MR. GRIMES: Yes, very much so.

DR. PLESSET: You will try to be reasonable?

MR. GRIMES: As a matter of fact, if we had felt that there was anything, any way to get out of having to review more FSTF data, we would have done so.

DR. PLESSET: They don't relish it either, I'm sure, right?

MR. GRIMES: That is correct.

DR. PLESSET: So there is that point.

DR. ETHERINGTON: How bad were the conclusions of the short-term program?

MR. GRIMES: How bad are the conclusions for the short-term program?

DR. ETHERINGTON: Well, have you found that they were very badly in error in the long-term program?

MR. GRIMES: Not that bad. We have continued to reassess our minimum factors of safety, looking at the weaknesses in the structure, and we feel that the conclusions of the short-term program still remain valid today; that we have sufficient margin of safety to assure continued operation. But we want to restore the margins of safety back to the code levels, and that is the whole purpose behind the program.

And the recent issue on downcomer loads was one area where we felt that the conclusions in the short-term program might have been violated, and we took action to try and correct the issue.

DR. ETHERINGTON: I felt they got an astonishing amount of material out of a very simply series of tests in the short-term program.

MR. GRIMES: Relatively speaking, that is true. We could have probably put uncertainty bounds about the 12-scale data and used it in the long-term, but I think we are working to a much higher level of detail now than we had then, and part of the resolution of this issue has been a much better understanding of the phenomena which did not come out of the 12-scale tests.

DR. PLESSET: You were on the program to make a summary.

MR. STEINER: We will have a summary by Bob Logue

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at the end of the program.

DR. PLESSET: I think that maybe we might do that before we have our general discussion. Is that agreeable with you?

MR. GRIMES: You want to skip the downcomer load discussion?

DR. PLESSET: Well, we had better go through that then. You have that? I'm sorry.

MR. STEINER: Ours is relatively short, I believe.

DR. PLESSET: But he has another presentation.

MR. GRIMES: I can try to make mine shorter.

DR. PLESSET: Well, why don't you go ahead.

MR. STEINER: Well, as Chris indicated awhile back, he did express a concern about some of the downcomer stresses measured in FSTF, and as a result of that concern, they have requested that we redefine our downcomer load approach. We have done that, and have gone through and identified a revised approach. We have informed the NRC of that. We don't have results as of yet. We hope to talk to the NRC in early December. Randy Broman of Bechtel will present the approach that we have outlined to NRC.

Randy.

DR. PLESSET: Why don't you go ahead.

MR. BROMAN: Okay.

As Larry indicated, the work that I am going to be talking about now is work that is underway to basically postulate and confirm a load definition for downcomer loads during condensation oscillation.

What I am going to describe here is the approach to the load definition. The work is underway. It is not completed yet.

Basically, the approach is -- this is another correlation technique. In some sense, the overall method might be thought to be similar to what I described previously for the torus. In this case, the idea here is that we are going to postulate a load definition for the downcomers during condensation oscillation. We are going to do an analysis for the postulated load definition, and then we are going to compare the results of the analysis in terms of structural response with measured structural responses from the test.

The idea here is if we can develop a load definition which when used in analysis will predict the test, we, therefore, have a good load definition, and we can use that load definition to analyze individual plants.

What the load definition is basically is an oscillating pressure in the downcomers. I will show you a little bit about that later.

The idea here is that as we have oscillating pressure in the downcomers, since the downcomer is open on the bottom, there is a net vertical thrust associated with that pressure in the downcomer, unbalanced thrust. The downcomers open at the bottom, and the thrust is exerted vertically on the downcomer above the vertical leg of the downcomer.

The way we are going to do the analysis for the postulated load definition is using another finite element model, a NASTRAN finite element model. I will show that in a minute. We do have a couple of means by which we can verify this analytical model.

Number one, there was some static testing done in the facility where they took jacks and they jacked between downcomers. So from the jacking tests what we can get is load versus deflection on the downcomers.

We can analytically represent that test. We can apply a load statically to the analytical model, and we can look at the deflection we would predict using the model. If we can predict the results of the jacking test, that is a static verification.

Similarly, there is to be what is called the downcomer snap test. This has been requested by the NCR Staff. What is to be done here is the downcomer will be deflected in the facility and then there is a means by which -- basically a cable system will be used to deflect the downcomer, and then the cable will simply be released, and that will, following release of the cable, the cable is initially under tension. When the cable is released, the downcomer will oscillate dynamically.

When it oscillates dynamically after the snap test, a frequency will come out of that snap test. We can compare that frequency with the frequency we would predict in the analysis. The frequency we are talking about here is a swing frequency for the downcomer swinging back and forth. So we can also compare that against analysis.

Given that, we have an analytical model which is verified statically and dynamically and we can use it for the purpose that I mentioned, which is the correlation to verify the load definition.

DR. BUSH: This would be done with the fluid end so you look at the dampening effects of the fluid on this?

MR. BROMAN: Yes, that is correct. It will.

The computer model is also developed using NASTRAN. If you want to look ahead, there is a picture of the model in your handout. The model represents the FSTF header from the column supports to the mid-bay point. Again, the FSTF is symmetric above mid-plane, so it is necessary for us to model only one-half of the header.

(Slide)

MR. BROMAN: The header is represented as a shell. We use shell elements in the finite element model.

For the analysis that we are doing, it appears that the governing case for definition of the loads is again this test, M-8, as has been mentioned a number of times earlier. We are going to simulate in our analytical model the configuration of the downcomers in the test M-8. What I mean by that is that in various tests there were different tying schemes used between downcomers. In some cases there was no tie between downcomers, and in other cases there was a prototypical tension tie, and in other cases there was a modified tie which is called a tension compression tie.

In the analysis we will simulate the M-8 configuration which has downcomers numbers 5 and 6 untied, and 7 and 8 tied.

We will consider an effective mass of water with the downcomer.

One of the things that we plan to do is that in our verification run against the pluck test, we are going to review our assumption with regard to effective mass. If we get a bad prediction on frequency, we will assume that this is the reason the effective mass is the thing which would cause a poor correlation against the snap test, and we would adjust an effective mass accordingly.

DR. ZUDANS: I have a question.

That means that you are not modelling fluid in your NASTRAN model this time? You are just adding the effective fluid mass?

MR. BROMAN: That is correct.

DR. ZUDANS: You realize that the effective fluid mass changes the frequency and the result will be only valid for a single frequency?

MR. BROMAN: Yes. This is not a sophisticated -- as sophisticated a technique for representing the fluid.

On the other hand, in all honesty, my belief is that it is not necessary to have a very refined model of the fluid in this case to get good correlation. If I am wrong, we will find out in the correlation, and we will fix it.

DR. PLESSET: They don't have a large frequency range, so that they should be in pretty good shape.

DR. ZUDANS: This is why I stayed quiet because they have a dominant frequency. So that's okay. Really, you care only about one frequency.

MR. BROMAN: I think that more than that. In all honesty, I think that the dynamic amplification, in this case, is not very great, and if the dynamic amplification is not great, the effective mass is not very important. The test 8 in itself suggests that.

I have indicated on the bottom of this slide what the postulated load definition is. This load definition is based on the pressure taps in the header from the test M-8. It is stated from 25 to 30 seconds, basically.

First of all, we have about a 1-1/2 PSI static pressure differential inside of the header versus outside. The inside of the header has higher pressure.

Second of all, in the header we have about plus or minus ?-1/2 PSI with a frequency of about 5-1/2 Hertz in this test.

And then finally, in the downcomer itself, based on the pressure measurements, it appears that there is about a plus or minus 5 PSI, also with 5-1/2 Hertz.

(Slide)

MR. BROMAN: This is the analytical model, and unless there are any questions, I don't think I will go over that in any more detail. I have explained it.

(Slide)

MR. BROMAN: This slide shows the procedure itself. I think I have really explained most of this already. As I say, we have a static verification, a dynamic verification. Then we have static pressure runs. The static pressure in the downcomer and header corresponds to the differential pressure case, what I call the two-to-one pressure case here. It is actually twice as much pressure in the downcomer as in the header. The reason for running that case is subsequently when we run the dynamic analysis for similar condition, it will tell us what our dynamic amplification was.

In the dynamic analysis we will apply the load definition that I just suggested. We will do harmonic analysis of frequency response analysis, and we will do a correlation against the test data.

The primary means of correlation is strains measured in the header shell, primarily in the vent-to-ring header intersection. We have strain data from gauges in the header and that intersection area, and the idea is that the load definition and the analysis technique must explain those strains. That is basically the closure.

I indicated the closure on the bottom of the postulated load and the analysis explains the strains. In effect, we are done, with one exception. I have indicated on the bottom line,given that we have a good solution for this test in time period, it is necessary beyond that to look at other tests in time periods. This would be in similar fashion to what Umesh Saxena described for the torus load definition. I might say this particular test, M-8, in this time period presents at least nearly the highest loads, if not the highest. In any case, to close the task, we must look at the other tests and time periods.

One other line I have indicated to you is to look at phasing. If we did not get good correlation between the postulated load definition explaining the measured strains, and we were unable to explain it in terms of an inaccuracy in the model itself, one of the things that we would want to look at is the possibility that there could be phasing between pressures in adjacent downcomers.

Review of the data itself suggests that that effect is not very important, so that we do not believe that we will go to this step, but if, as I say, we do not get good correlation, that is one of the things that we are going to have to look at.

DR. ZUDANS: Just one question to your dynamic analysis: Let's say you do harmonic analysis with such-and-such frequency. Is this in the form of pressure applied internally?

MR. BROMAN: Yes.

DR. ZUDANS: And uniformly around the circumference?

MR. BROMAN: Yes.

DR. ZUDANS: If so, how are you going to get effects of bending at all?

MR. BROMAN: I'm sorry. Bending? Where?

DR. ZUDANS: Well, you are talking about swing frequency. If you apply uniform pressure, it is not going to swing very much.

MR. BROMAN: No. The thing that would cause it to swing, if it does, which personally I don't think it does, but the thing that would cause it to swing is if you have pressures out-of-phase, let's say you have more pressure on this side than this side at an instant of time, what you have is an unbalanced torque about the header here which would cause it to swing.

DR. BUSH: Non-uniform strains?

DR. ZUDANS: In some presentation this morning one of the tests reported where these downcomers were not strained you had 46,000 PSI stress due to bending, and that means that there is a lateral load on the downcomer.

MR. BROMAN: Now what would happen, the load definition we are talking about is a load that is applied vertically upward, pressure applied upward, resulting in a net vertical thrust upward in the downcomer. If you untie these two downcomers, you take that tie out and you apply a thrust up that way, you will get lots of stress up here. There is no question about it.

In other words, there is a net bending about this axis here.

DR. ZUDANS: I understand that. That is primitive, but are you talking about condensation oscillations? The condensation is of some bubbles that are outside the downcomer. They can induce pressure on one side of the downcomer and not on the other, so you could have a direct radial load.

MR. BROMAN: You are saying a pressure imbalance within the downcomer?

DR. ZUDANS: Right, on the outside surface.

MR. BROMAN: Okay.

My belief is we will find out when I make the analysis, but my belief is that the condensation takes place within the downcomer here.

DR. ZUDANS: That's chugging. That is not condensation.

MR. BROMAN: No condensation oscillation also.

DR. CATTON: We were given some definitions this morning. Are you going to change them?

MR. BROMAN: No.

DR. CATTON: It's okay if you do.

MR. TORBECK: John Torbeck of General Electric.

Our expectation is that there would not be much pressure variation around the downcomer as a result of CO.

DR. ZUDANS: And do you think that you will get all the bending load just because it is offset?

MR. TORBECK: Yes. It is strictly a result of the drop in the interim pressure relative to the external pressure on the freespace surrounding the downcomer.

DR. CATTON: Is this a synchronist process?

MR. BROMAN: Let me make one more comment, and then I will try to answer that question.

You can verify, if you just take the kind of pressures that we are talking about here, 5 PSI, and take that pressure and consider that it is unbalanced, in other words, that it is applied upward here and it is unbalanced, it is not balanced going downwards, so you have a net vertical thrust. If you calculate the thrust load associated with that

5 PSI and apply it to the downcomer, you will get substantial local stresses here. That is a very significant thrust load. You will get significant local stresses.

DR. ZUDANS: I would agree to that, but if you applied the same load and tie down your downcomers, the resulting stress would not be dramatically different, and yet in your test results --

MR. BROMAN: Will not?

DR. ZUDANS: Be dramatically different.

MR. BROMAN: Oh, yes it will.

DR. ZUDANS: Well, I don't know.

Now your first result in a factor of three different with tied and not tied down.

MR. BROMAN: They will be dramatically different because when you tie the downcomers and you apply the thrust, what you are doing is the vertical thrust is creating a moment about this point this way. The tie will then create a reaction in this way here which will counterbalance the moment about this point up here.

DR. ZUDANS: But the other leg is not tied anyplace.

MR. BROMAN: Which other leg?

DR. ZUDANS: There are two things that are connected freely. It is not like you tie them down to some rigid surface. It is not the same effect.

Why don't you run it and we will see what you get.

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DR. CATTON: Unless the bubble oscillation process is synchronized exactly, then they would cancel.

DR. ZUDANS: That is correct.

MR. BROMAN: That is correct. If they synchronize, they will cancel. That's right.

DR. BUSH: I wouldn't want to bet on them being synchronized.

DR. BUSH: But he is betting on that.

MR. BROMAN: Pardon?

DR. BUSH: I disagree with that.

MR. BROMAN: I know I am betting they are synchronized.

DR. CATTON: People were indicating that they didn't believe that synchronization occurred, and now we hear you telling us from GE that you believe in synchronization. That is rather interesting.

MR. TORBECK: During the CO period, if you compare the pressure signals inside of the individual bends at the low frequencies up to like 10 Hertz, they are in-phase, and that is where the majority of the pressure occurs. It is in the range of 0 to 10.

DR. ZUDANS: Are they in-phase without exception?

MR. TORBECK: Yes, without exception in that frequency range.

DR. CATTON: We will have to remember that.

DR. PLESSET: Chris, are you ready?

MR. GRIMES: Yes. I am only going to be 15 minutes.

I would like to present the Staff's criteria for the downcomer loads. We used to call them downcomer lateral loads, but because of the issue that Randy just discussed regarding whether or not it is a lateral loading component or a vertical thrust load, most of the documentation on it was changed just to refer to them as downcomer condensation loads until it is settled.

For untied downcomers we have specified a loading function on the basis that without the tie, regardless of whether it is a vertical thrust load or a lateral load, you can express them as equivalent provided that you have assessed the data correctly.

You will notice that if the load is a vertical loading function on the condensation oscillation regime, it can be defined with a lateral component, and therefore we have proceeded with the acceptance criteria for the untied downcomers by specifying a requirement for the dynamic load factor scaling. That was proposed by the Mark I owners to be based on a plec test or a snap test, as Randy suggested, that would establish the natural frequency in damping that was occurring in FSTF to get a more reasonable scale factor to the plant specific downcomers, and that would predicated on a 5.5 Hertz driving function which is the natural swinging

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mode or is in the range of the natural swinging mode of the downcomers, and the specific plant unique analysis would have to assume that they are in resonance.

The tied downcomers, however, because of the concept of two equal thrust loads causing a wishbone mode that does not occur near the resonant condition, that is, the wishbone mode is up near 17 or 19 Hertz where the forcing function is at 5.5 Hertz, we felt that we did not have a sufficient amount of information that specifically or especially with regard to the question about how well the pressures inside the downcomers are phased to establish a load definition in the critera. So we simply stated that the loads would have to be developed, and we are pursuing that issue with the Mark I owners right now to try and resolve that aspect of it.

(Slide)

MR. GRIMES: I will put this slide up again just to show you that if you go back through the lateral load definition technique derived from the strain measurements in FSTF, a lateral load equivalent function is derived in the form of a histogram, number or cycles at particular amplitudes, and that is applied at specific locations on the downcomer. For CO the load always tended to be in the plane of the downcomers. For the chugging, the load was random and occurred with a specific stochastic nature or randomness around the exit of the downcomers.

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(Slide)

MR. GRIMES: Therefore, for our chugging load assessment the same general technique is used. In this particular case there isn't as much of a propensity towards dynamic amplification because it occurs not as a sinesoidal function but like a triangular pulse. So there isn't as much of a dynamic amplification when you approach resonant conditions.

So our criteria concentrate more in terms of how the result in static equivalent loads are to be applied. The Mark I owners proposed that the upper 95 percent confidence limit result in static equivalent loads be used, and we felt that it would be prudent to use the maximum observed which was like, I believe, 10 to 15 percent --

MR. SONIN: Fifteen percent?

MR. GRIMES: Fifty percent higher than the upper 95 percent confidence limit. But that would be used for a determination of the ultimate strength of the downcomer for that single load.

Then for a fatigue loading consideration we felt that in that particular instance a statistical type of approach should be appropriate, and therefore we specified that a 95 percent non-excedence probability for a single loading function should be used.

In terms of the directionality of the loading

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function for numbers of downcomers that experience a load in a single direction, that would cause loads on the vent header and the vent header supports, the Mark I owners proposed that a number of 10^{-2} should be used, and that was derived (slide) from this type of analysis. This is from the downcomer load assessment report. It shows the magnitude of the force on each downcomer as a function of probability of exceeding that force at least once per loca.

The Mark I owner specified 10^{-2}, and in order to approach this we took an approach of saying, well, the probability of this force being exceeded, coupled with the probability of a design basis accident should be like on the order of 10^{-7} which would put us down in the 10^{-2}, 10^{-3} range, but because these functions are so steep we said, well, we aren't even going to mess with that. We will go down to 10^{-4} because it doesn't increase the load that much, and it provides us with a nice conservative assessment technique.

DR. ETHERINGTON: You have got 95 percent NEP and 10^{-4} NEP.

MR. GRIMES: They are two different assessments. One is a fatigue loading for a single downcomer. The other is a multiple loading on a number of downcomers.

DR. ETHERINGTON: But is that an exceedence probability or a non-exceedence probability, 10^{-4}?

MR. GRIMES: 10^{-4} non-exceedence probability.

DR. PLESSET: Non-exceedence, yes.

DR. ZUDANS: Doesn't this mean almost certain exceedence?

DR. ETHERINGTON: That means a certain exceedence.

MR. GRIMES: I'm sorry. It is an exceedence probability. I apologize.

DR. PLESSET: We got that straight anyway.

DR. ZUDANS: Now, what are these numbers?

MR. GRIMES: This figure shows the load that a number of downcomers would experience in the same direction. For example, on this curve five downcomers would experience a load of approximately 2 KIPS.

I am going to have to phrase so that I don't get into the same problem I just got into.

There is a 10^{-3} probability that five downcomers will experience a load in excess of approximately 2 KIPS.

DR. BUSH: That is a phi sigma value. All you have to do is think of a different model and it will range anything from a factor of 100 to a factor of 1000 swing. But let's not argue that when you get into these values.

DR. PLESSET: Okay.

MR. GRIMES: That was the approach that we took in the acceptance criteria, and we proceeded to issue those criteria, and we will continue a resolution of the CO downcomer load.

DR. PLESSET: Thank you, Chris.

I believe we have one more presentation.

MR. STEINER: We have a couple of things to say.

MR. SOBON: My name is Bert Sobon, and I am with General Electric. I would like to make some subjective comments about the earlier discussion that we had on FSTF tests, the extra tests.

I guess I heard -- and I don't want to quote out of context -- but I heard a comment that the Staff was trying very hard to find a way not to have to do these additional tests, and I can think of some subjective ways that that might be done, and I don't know that the Staff has considered them, but I would just like to throw them out as some reaction to what was said.

As an example, when we do the test and analysis for predicting containment response we ignore the heat sinks in the drywell. During the initial phase of the drywell, that heat sink would absorb some of the energy and the mass flux of the vent system should be reduced for a period of time. It may be short, as short as five seconds, say, but it would be short, but it would reduce for a period of time the mass flux.

Also, we --

DR. PLESSET: Do you have an estimate of how much, Bert?

MR. SOBON: I don't have a real good idea. I am just saying that there are things out there that can be thought about, and I will try to tie it altogether when I finish that would add to some of the reasoning that might be found to accept the load as it is for safety's sake without further confirmation.

The second item is that when we do the test we try and configure the test such as we quickly purge all the air over, and in doing so we increase the amplitude. In other words, any air content in the steam condensation phase would tend to reduce the amplitudes, the oscillations. So there are two effects there that we have purposely forced this facility to do, and in each case it would tend to either reduce the mass flux or the amplitude, perhaps both.

Now, to get this high mass flux, you have to have the biggest break which also requires a guillotine rupture and all of these sorts of things that go into the standard break analysis. In doing that, therefore, you have the high energy or high mass flux through the vent system for the shortest period of time. In other words, the bigger the break the shorter the time the high mass flux would exist.

So if you couple that with the mitigating effects of condensation on the walls and equipment, and the air content that is there due to the rather complex configuration within the drywell, prolonging the air carry-over, you have a good chance of not having what might be called the empirical basis that you see in these tests that cause some concern, and the need maybe to satisfy the uncertainty.

I think Umesh Saxena in his presentation showed also that if you look at the trends from the data that we do have, that there is no real indication that you should have a departure.

Then the final point is that if you look at the load definition, the value that we used for design is a summation of the amplitudes in each of these frequencies, and it comes up to about three times what we have observed in the tests, even in the high mass flux.

So all these things coupled together, I think, could form a basis, subjective, granted, but it could form a basis for not having to do additional tests.

DR. PLESSET: Thank you.

MR. STEINER: We have one other summary from one of the owners chairman from the owners' standpoint.

MR. LOGUE: Bob Logue, Philadelphia Electric, Mark I owners chairman.

I have sat here all day today rather quietly except for one outburst, and I would like to relate to you our perspective of where we are, and also to update you as to the status of where we are.

In the past year or so there have been several of what I think I would call real key issues that have been resolved. The LDR was issued in December 1978. That was revised with Part B earlier this year, and since it was sent in in March we have had roughly 10, perhaps 15 working group meetings with the NRC trying to resolve differences.

Their report, the NRC acceptance report, was to be issued in May, and for reasons out of our control and mine too, that was issued in draft in August and was issued timely October 31st of this year.

Meanwhile, of course, we have not been unmindful of the fact that there was a date of 1980 mentioned in the SCR report for the short-term program. So there have been some plans that have already gone ahead to make some modifications. Others have -- in design modification hardware is being purchased for installation at some time in the future.

(Slide)

MR. LOGUE: I would like to point out that we are not talking about inexpensive fixes, and these are some of the items that are being installed: T-quenchers, vent deflectors, torus saddles which cost on the order of $1.5 million per saddle -- I'm sorry, unit, to reinforce columns, anchor bolts, downcomer reduction, and, of course, drywell, wetwell, delta P.

Now, the impact of all of this, and especially the NRC acceptance criteria, and you have heard that we don't agree 100 percent with some of their ideas, will extend the program into 1980, especially the FSTF re-test. The LDR will have to be revised to reflect NRC acceptance criteria, and we are going to have to, I'm sure, meet with the NRC to try and resolve these,although we have over the past several months tried to meet with them. They have not been unreasonable, nor do I think that we have. We just don't see eye to eye yet.

As a result of all of this, the AE's who have been going ahead with their structural evaluations may have to go back and re-do them which will, of course, impact upon schedule.

We are proceeding at some risk because if we do things and install modifications and then it does turn out that we have to go back in again, that will require modification on a modification. We are trying not to spend money that way.

Earlier this year all owners responded to the NRC with a schedule for completion of modifications. Now, I said before there was a 1980 date. The NRC was aware before they issued that that especially those companies where they had more than one unit per plant would have a most difficult time meeting 1980, and some of these do go beyond 1980. Most of them though are being done in the 1980, 1981 timeframe. There are a few that go up to 1982 and 1983. This is our schedule and work is proceeding to meet the schedule in all cases that I can think of.

(Slide)

MR. LOGUE: I guess in summary the owners are proceeding, but apparently these loads are becoming more complex than we thought that they would be, and as a result of this there is some delay in schedule.

As we proceed to resolve our differences with the NRC, there will be some interaction that may result in either more tests or more structural evaluation, or something, but this, I guess, is the way that it is. We are proceeding at risk. These fixtures are not inexpensive in our case, and in our own plants we are spending about $30 million on this, and we don't like to spend our customers' money any more than anyone else does.

We believe that the LDR basically does give solutions which we consider are practical, and, frankly, I think some of the questions I have heard today may be looking for the small dot on top of the "i," and I would like to get across to you, we've got to stop asking questions and get on with getting the results done. If we were to wait, as we would normally, we wouldn't build a plant until the SER was issued to the ACRS letter for the NRC, had said "go," "construct." We are proceeding based upon our best judgment as to what we know about loads, and there is some risk there.

I just want to assure you though that we want to maintain the open channels of communication with the NRC, and I am sure that we will, and with the SERS.

We can wrap this up. It has been four years now, and, frankly, I think we are all getting tired of hearing Mark I. I know I am.

DR. PLESSET: Well, thank you, Mr. Logue.

You said a few words that I like to hear. You said the Staff was fairly reasonable, and I think they are. You said that you are reasonable, and I think that that is true too.

DR. ZUDANS: But you don't see eye to eye.

DR. PLESSET: That's true, but let me assure you that this subcommittee would also like to hear the end of Mark I. We would welcome that. It seems to me that we are getting close.

MR. LOGUE: I hope we are.

DR. PLESSET: I hope that we are. I think it is clear what the request of the NRC is, and I think that there is some reasoning for it.

The ACRS, this subcommittee is in a very good position because what it says doesn't carry any weight at all. It is the full committee who has to make meaningful statements, and they might ask us what we thought, if it gets to that. So before we adjourn, I might just go around the table and see if there is anything that any of the people we have here would like to

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add.

Harold, would you like to say anything?

DR. ETHERINGTON: No.

DR. PLESSET: He is a very wise man.

DR. ETHERINGTON: I think it has been a very informative program here today.

DR. PLESSET: Very informative. I think it has clarified things considerably for me, and certainly exposed what the NRC wants and what the owners group doesn't want. So that part I have learned a great deal.

Virgil?

DR. SCHROCK: Well, I agree with what you just said. I haven't been involved in this for some time so I am glad to hear an updating of it. It sounds quite reasonable.

DR. PLESSET: Well, we welcome your addition to this, and I think you may hear more.

Zenons, do you want to say anything?

DR. ZUDANS: No, I don't want to add anything.

DR. PLESSET: Spence?

DR. BUSH: I would like to make a plea. I had a very frustrating evening yesterday trying to delouse all of the signal on all of the acronyms and symbols that we used. I ran across about 150 of them of which almost a third weren't identified, and that can be extremely frustrating, plus the fact that in several instances the same symbol meant three different things.

MR. GRIMES: We may issue that material as a separate new reg.

DR. BUSH: One other comment only, and that is that I think that with regard to this business of DBA loads, that serious consideration should be given to the relative probabilities of break size. That had been the consideration because my personal opinion backed up by some statistics would be that the DBA break has about two to three orders of magnitude lower probability than does, say, the intermediate break, and I think that is a factor that people must consider in the overall analysis of that situation.

DR. PLESSET: That has been brought to the forefront very strongly by recent events. That's true.

I don't think I can add anything that is wiser than what we have just heard, so I will thank you all, and I would appreciate it if the consultants would send me, via Andy Bates, comments after they have had a chance to reflect on the meeting.

If any of you would like to get some other report material or other transcripts or this transcript, if you let Dr. Bates know, he will do his best to provide it.

DR. CATTON: I would like a copy of the transcript.

DR. PLESSET: That was a prompt request. I think they would all like that.

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Let's adjourn this meeting. Thank you all.

(Whereupon, at 4:45 p.m., the meeting on the above matter was adjourned.)

TASHJIAN

MARK I CONTAINMENT PROGRAM

POOL SWELL LOADS

ACRS MEETING

SAN FRANCISCO, CA.

NOVEMBER 16, 1979

VAR S. TASHJIAN

GENERAL ELECTRIC CO.

VST - 1
11/16/79

OUTLINE FOR POOL SWELL LOADS

- PHENOMENA REVIEW
- SPECIFIC LOADS/STRUCTURES AFFECTED
- DISCUSSION TOPICS
  - 3D/2D TORUS UPLOAD MULTIPLIER
  - POOL SWELL SHAPE/IMPACT TIMING
- TECHNICAL ASSESSMENT
- CONCLUSIONS

POOL SWELL

PHENOMENA REVIEW

VST - 3
11/16/79

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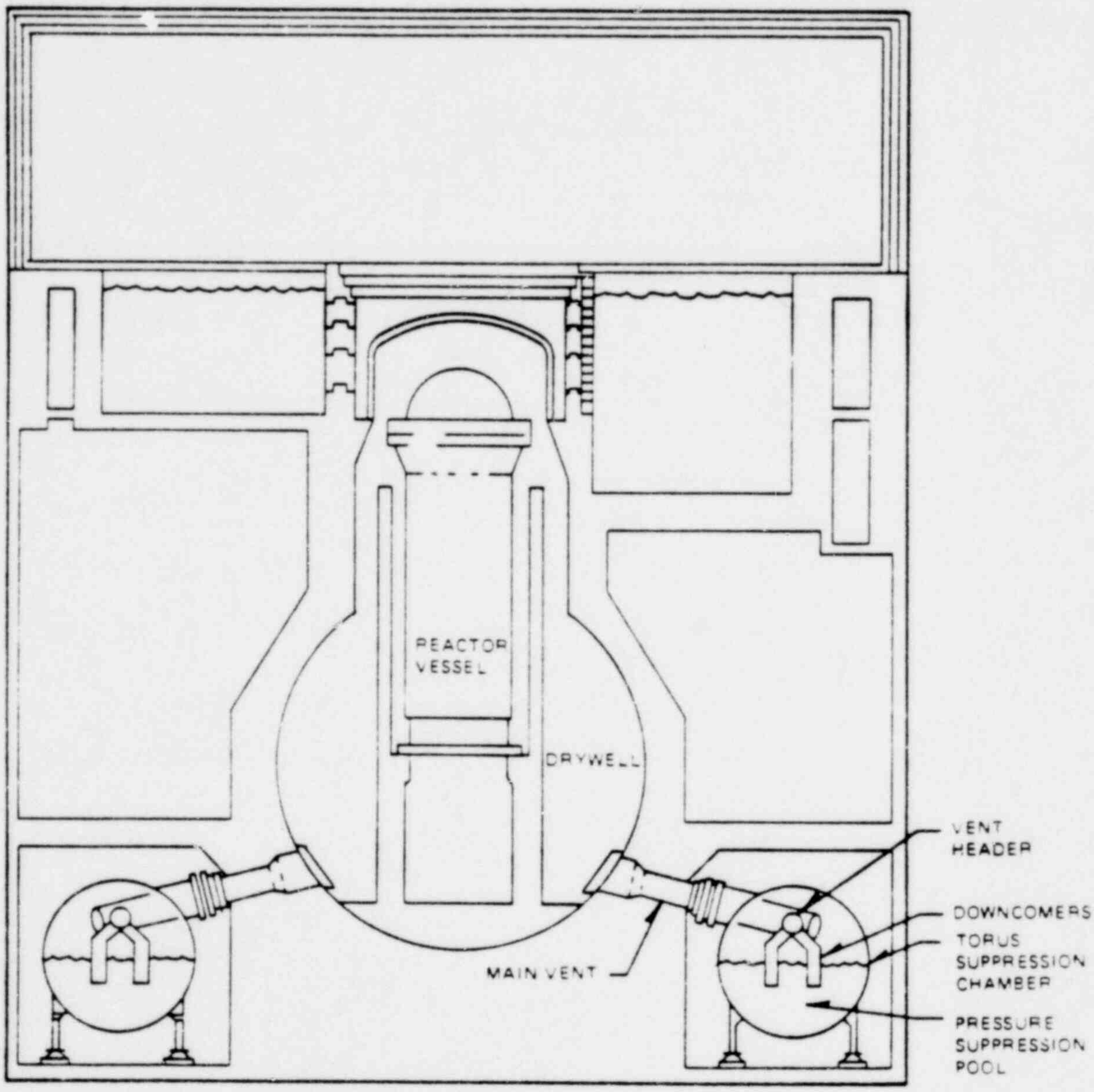

TYPICAL MARK I CONTAINMENT

VST - 4
11/16/79

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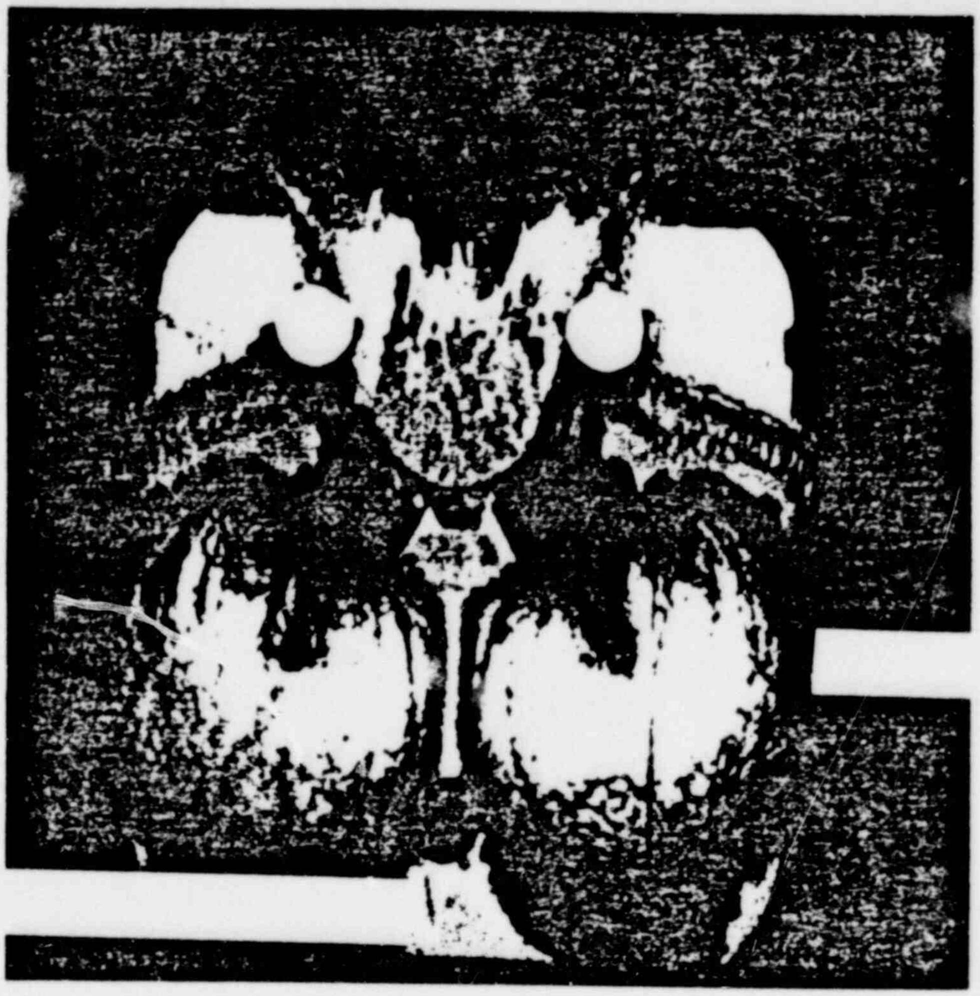

TYPICAL MARK I POOL SWELL
(GE TWO-DIMENSIONAL TEST FACILITY)

VST - 5
11/16/79

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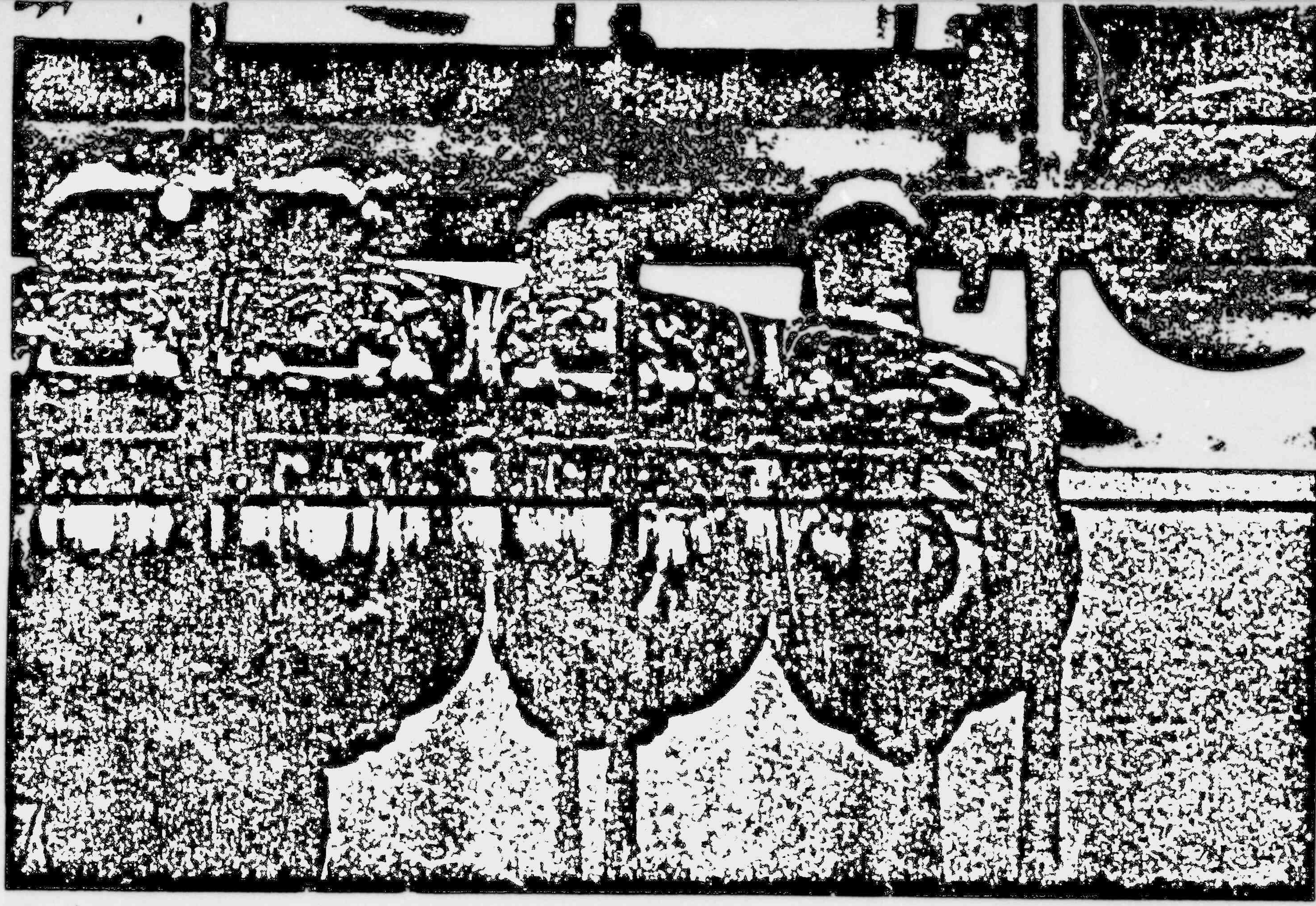

TYPICAL MARK I POOL SWELL

(EPRI/SRI THREE-DIMENSIONAL FACILITY)

VST - 6
11/16/79

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POOL SWELL - DYNAMIC EFFECTS OF
DRYWELL AND VENT SYSTEM AIR FORCED IN TO WETWELL

- DBA GUILLOTINE BREAK

- DRYWELL PRESSURE AND TEMPERATURE INCREASE

- DOWNCOMER WATER CLEARS; DRYWELL AIR IS EXPOSED TO WETWELL

- BUBBLE EXPANSION IN WETWELL

- POOL WATER COMPRESSES WETWELL AIR

- POOL WATER IMPACT ON VENT HEADER

- POOL BUBBLE BREAKTHROUGH

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POOL SWELL

SPECIFIC LOADS/STRUCTURES AFFECTED

- TORUS VERTICAL LOADS
- TORUS SUBMERGED PRESSURE
- TORUS AIRSPACE PRESSURE
- VENT SYSTEM IMPACT & DRAG
- IMPACT & DRAG ON OTHER STRUCTURES
- VENT HEADER DEFLECTOR LOADS

POOL SWELL

DISCUSSION TOPICS

- TORUS VERTICAL LOADS
  - 3D/2D UPLOAD MULTIPLIER
- POOL SWELL SHAPE
  - VENT HEADER IMPACT TIMING

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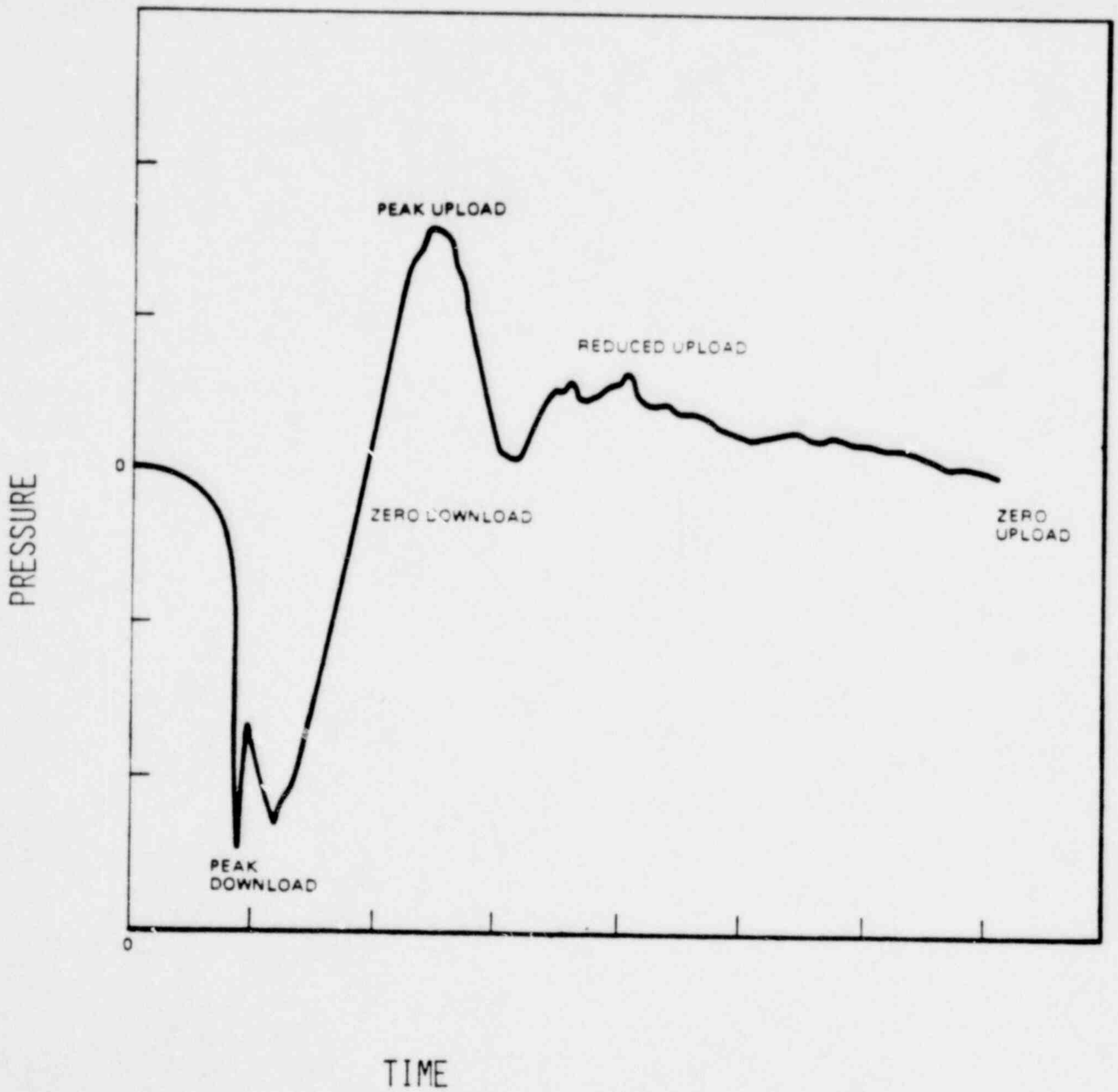
POOL SWELL

TORUS UPLOAD

- BASED ON GE 1/4 SCALE 2-D TESTS
- ASSESSSSMENT OF 3-D EFFECTS BASED ON EPRI 1/12 SCALE 3-D TESTS
- COMPARISONS OF LLL 2-D AND 3-D UPLOADS



TYPICAL MARK I NET TORUS VERTICAL LOAD HISTORY

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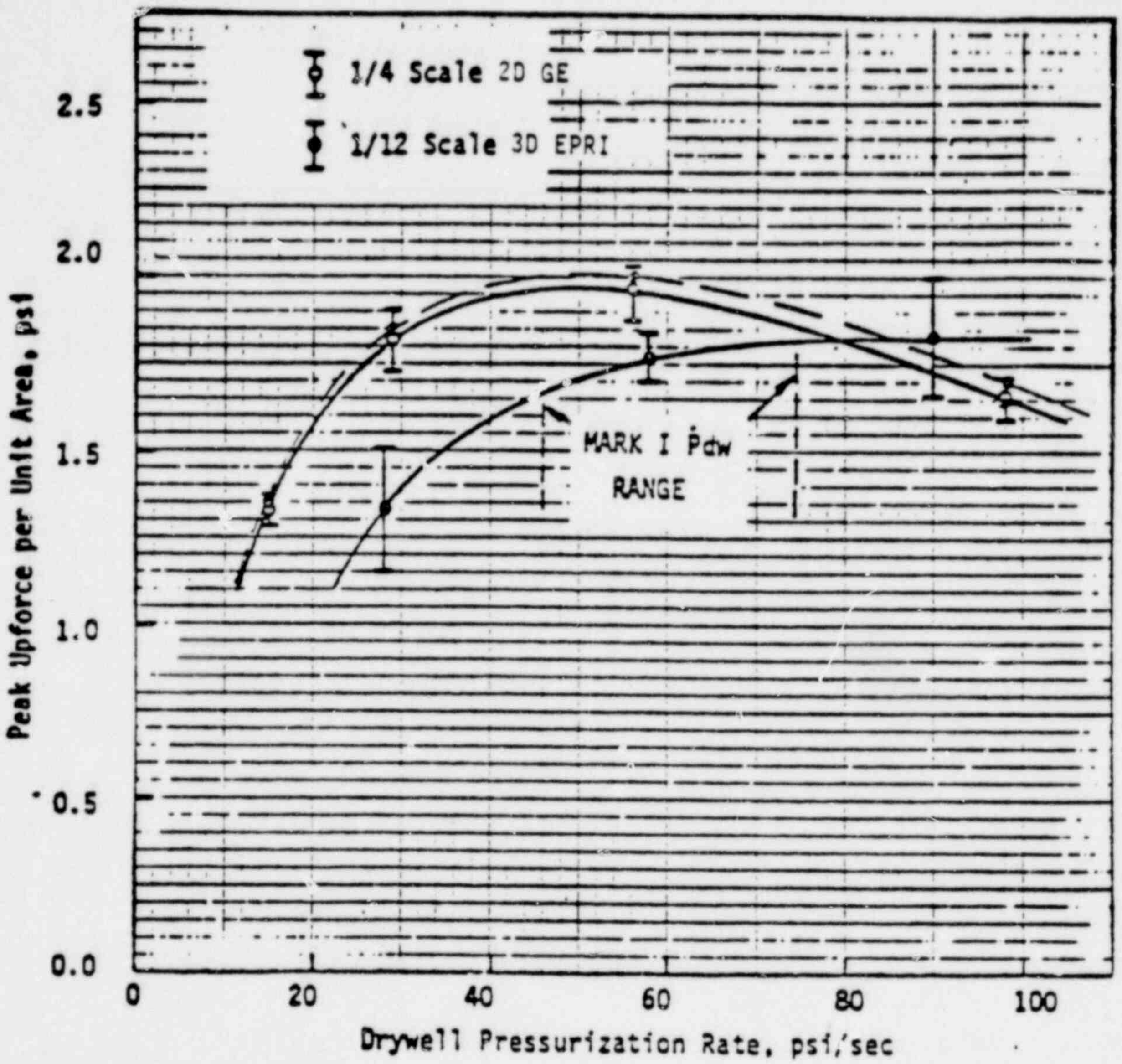
11/16/79



COMPARISON OF PEAK UPFORCES BETWEEN
1/4 SCALE AND 1/12 SCALE

VST - 12
11/16/79

LIVERMORE 3D/2D UPLOAD COMPARISONS

- INFLUENCE OF 3-D STRUCTURAL OSCILLATIONS
- 3D/2D NON-SIMULTANEOUS VENT CLEARING
  - VARIATION IN INITIAL CONDITIONS
  - 3D DROVE 2D
  - ONLY COMPARABLE DATA MEANINGFUL
- CAPACITANCE AND FL/D DIFFERENCES

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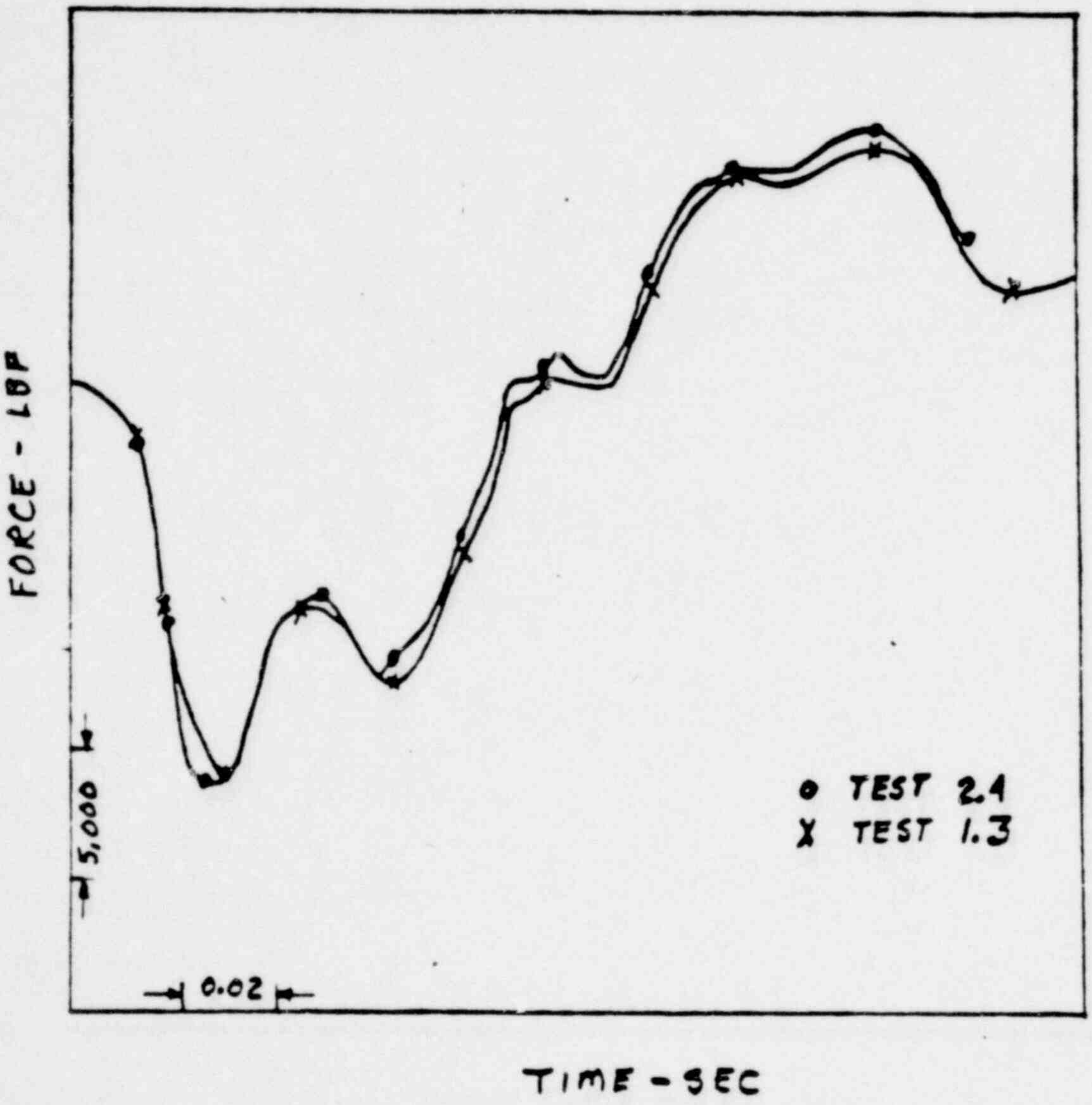
11/16/79

COMPARISON OF 3D FORCE TIME HISTORIES FOR TWO REPEAT TESTS (1.3 & 2.4)



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11/16/79

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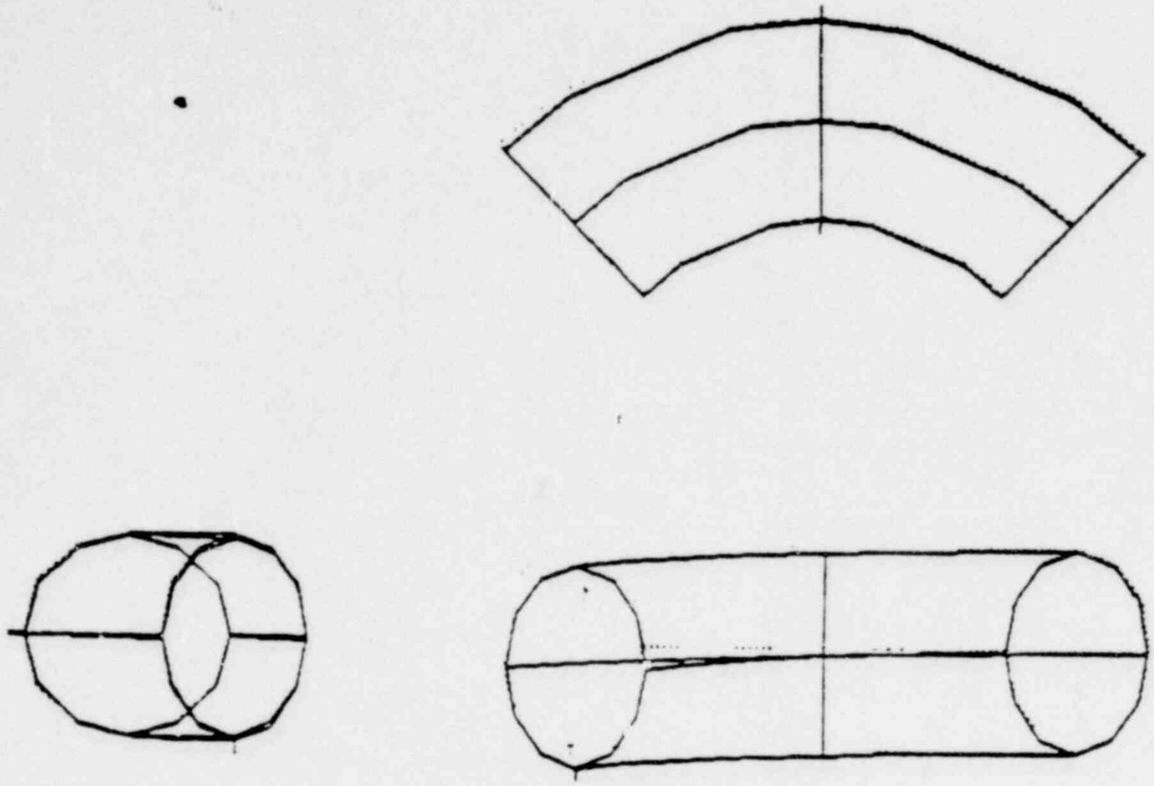
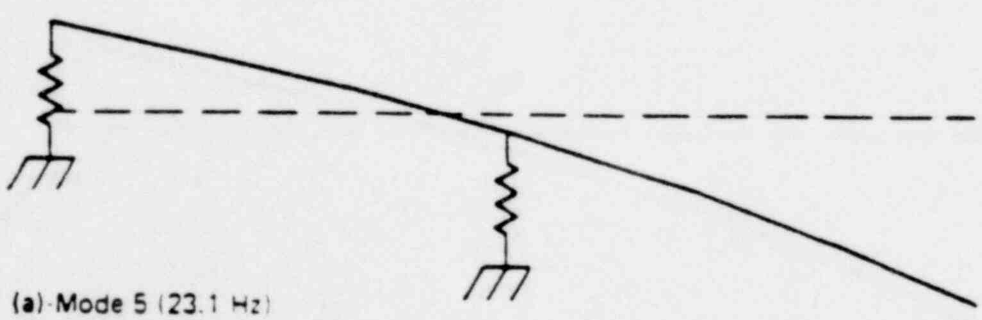


Fig. 55 Experimentally-determined mode 5-25.9 Hz torus vibration

FUNDAMENTAL VERTICAL MODE OF VIBRATION
BASED ON HAMMER BLOW TEST

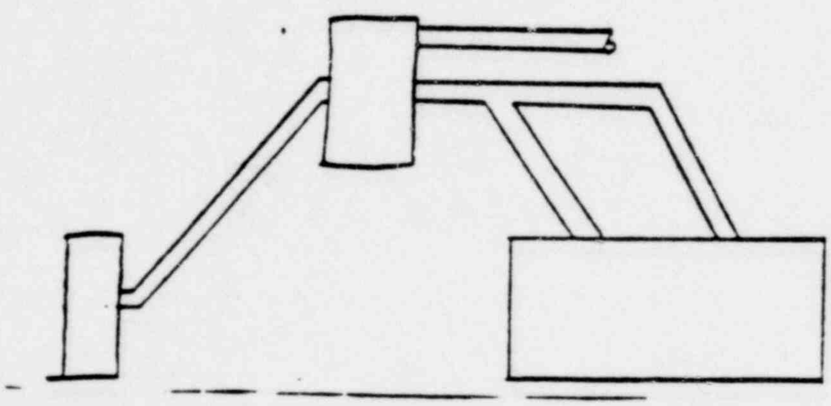


(a) Mode 5 (23.1 Hz)

FUNDAMENTAL VERTICAL MODE OF VIBRATION
BASED ON ANALYTICAL MODEL

LIVERMORE 2D-3D FACILITY INTERACTION

- COUPLED DRYWELL ENSURES COMMON DRIVING CONDITIONS
- COUPLED DRYWELL PERMITS 2D-3D FACILITY INTERACTION



- CONTROL OF INITIAL CONDITIONS EXTREMELY IMPORTANT
  - LARGE FACILITY WILL CONTROL DRYWELL PRESSURE
  - SMALL FACILITY PHENOMENA CAN BE AFFECTED

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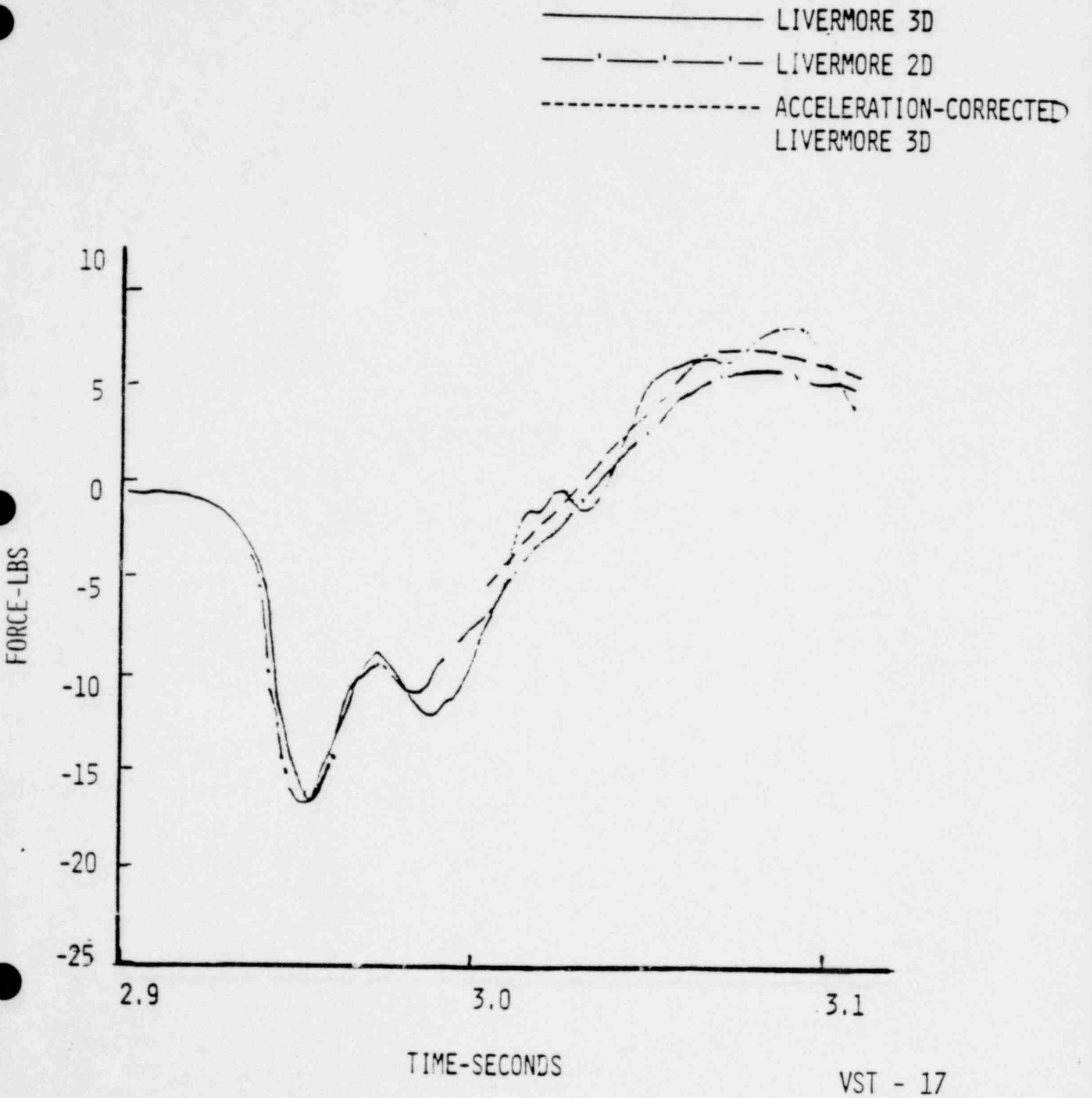
EFFECT OF STRUCTURAL OSCILLATION ON LIVERMORE 3D/2D UPLOAD RATIO



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11/16/79

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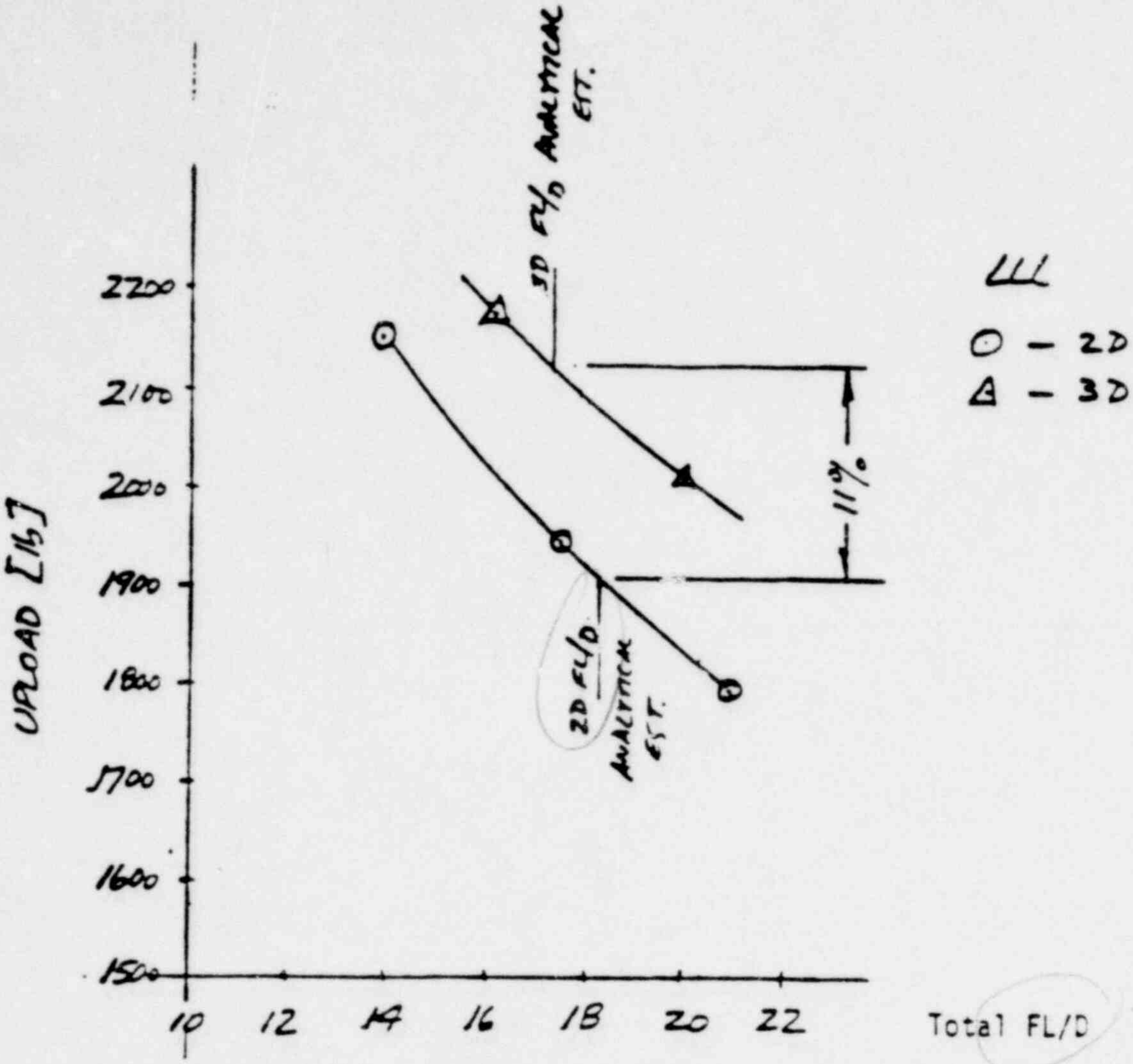


Results of Vent Capacitance Model Simulation of LLL 3D and 2D Test

TORUS UPLOAD

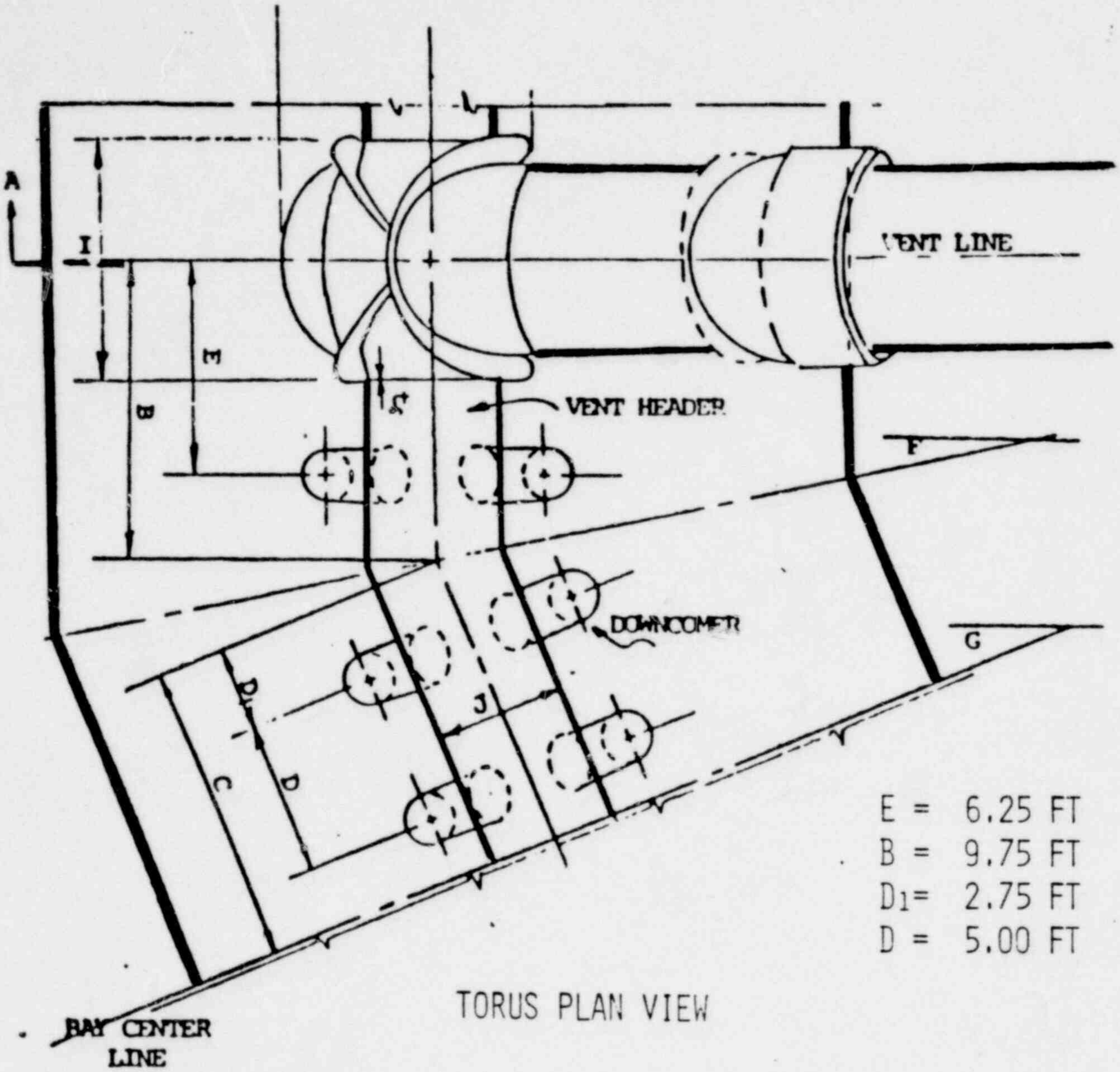
CONSLUSIONS

- EPRI (3D)/GE (2D) TEST DATA COMPARISONS SHOW 3D/2D UPLOAD MULTIPLIER $\leq$ 1.0

- LLL 3D/2D UPLOAD COMPARISONS CONFIRM 3D/2D UPLOAD MULTIPLIER $\approx$ 1.0 WHEN FACILITY & TEST CONDITIONS ARE MATCHING

POOL SWELL SHAPE

- POOL SWELL CURVATURE OBSERVED IN BOTH GE (2D) AND EPRI (3D) FACILITIES
- GOVERNED PREDOMINANTLY BY NON-UNIFORM DOWNCOMER SPACING IN MARK I PLANTS
- VENT FLOW DISTRIBUTION ANOTHER FACTOR



E = 6.25 FT
B = 9.75 FT
D_1= 2.75 FT
D = 5.00 FT

TORUS PLAN VIEW

TYPICAL MARK I NON-UNIFORM DOWNCOMER SPACING

VST - 21
11/16/79

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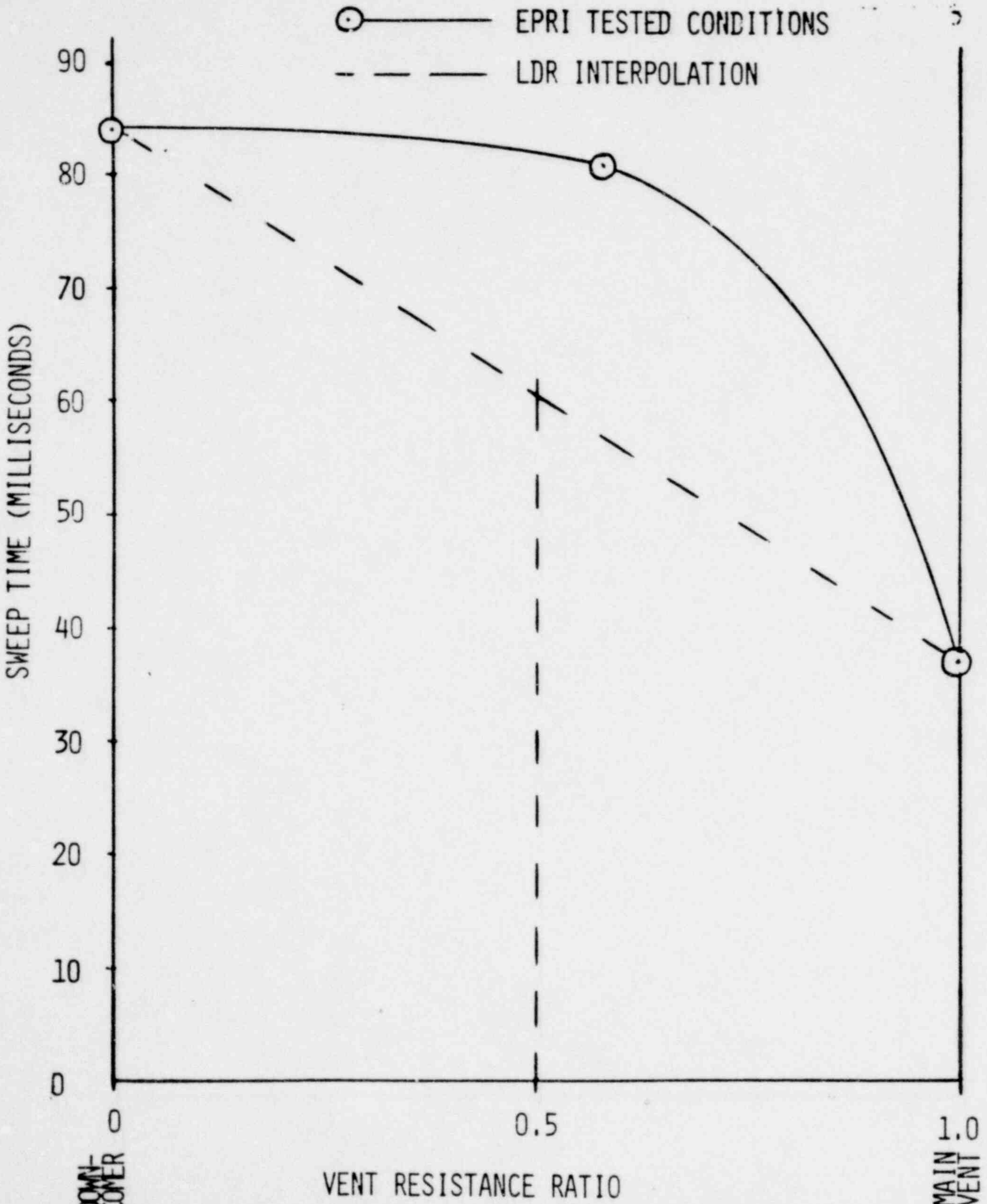
POOL SWELL

VENT FLOW DISTRIBUTION

- ACCOMPLISHED BY PLACEMENT OF ORIFICES
- LDR POOL SHAPE BASED ON
  - EPRI 3D TESTS
  - INTERPOLATION BETWEEN DOWNCOMER & MAIN VENT ORIFICE TESTS
- CONSERVATISM CONFIRMED BY EPRI SPLIT ORIFICE TESTS

VST - 22
11/16/79

EFFECT OF VENT RESISTANCE DISTRIBUTION ON IMPACT SWEEP TIME



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VST - 23
11/16/79

POOL SHAPE

CONCLUSION

- DOWNCOMER SPACING PREDOMINANT FACTOR
- SPLIT ORIFICE PROTOTYPICAL OF MARK I VENT FLOW DISTRIBUTION
- MARK I LDR INTERPOLATED SWEEP TIMES ARE CONSERVATIVE

RANLET

MARK I CONTAINMENT PROGRAM

NET VERTICAL PRESSURE LOAD

DATA COMPARISONS

ACRS FLUID DYNAMICS SUBCOMMITTEE

SAN FRANCISCO, CA

NOVEMBER 16, 1979

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OUTLINE OF PRESENTATION

- ACCEPTANCE CRITERIA AND PRESSURE LOAD MARGINS
- AVAILABLE DATA BASE
- GE-EPRI UPLOAD COMPARISON
- LLL UPLOAD COMPARISON
- DOWNLOAD COMPARISONS

ACCEPTANCE CRITERIA

- THE MEAN DOWNWARD AND UPWARD NET VERTICAL PRESSURE LOADS SHALL BE DERIVED FROM THE QUARTER SCALE TEST FACILITY (QSTF) PLANT-SPECIFIC TESTS (NEDE-21944-P)

- BASED ON OUR REVIEW OF THE AVAILABLE DATA BASE WE WILL REQUIRE THE FOLLOWING MARGINS:

$$UP = UP_{MEAN} + 0.215\ (UP_{MEAN})$$

$$DOWN = DOWN_{MEAN} + 2 \times 10^{-5}\ (DOWN_{MEAN})^2$$

WHERE "MEAN" REFERS TO THE AVERAGE OF THE QSTF PLANT-UNIQUE TEST RESULTS (LBF).

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PRESSURE LOAD MARGINS

- **UPLOAD MARGIN**

$0.215\,(UP_{MEAN})$ →
- 15% to Cover Uncertainty of 3D/2D Comparisons
- 6.5% (2σ from Statistical Analysis of Entire QSTF Data Base)

- **DOWNLOAD MARGIN**

$2 \times 10^{-5}\,(DOWN_{MEAN})^2$ → 6.3 to 15.5% (2σ from Statistical Analysis of Entire QSTF Data Base)

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3D/2D DATA COMPARISONS

OBJECTIVE: To Determine if the torus Loads Obtained in the 2-D QSTF Plant Unique Test are Appropriate for a 3-D Load Definition.

The Data Base Available for Assessing the Possibility of a 3-D Effect on Pool Swell Vertical Loads Consists of:

1) GE, 1/4-Scale, 2-D Tests[1,6]

2) EPRI, 1/11.7-Scale, 3-D Tests[2,3]

3) LLL, 1/5-Scale, 2-D & 3-D Tests[4,5]

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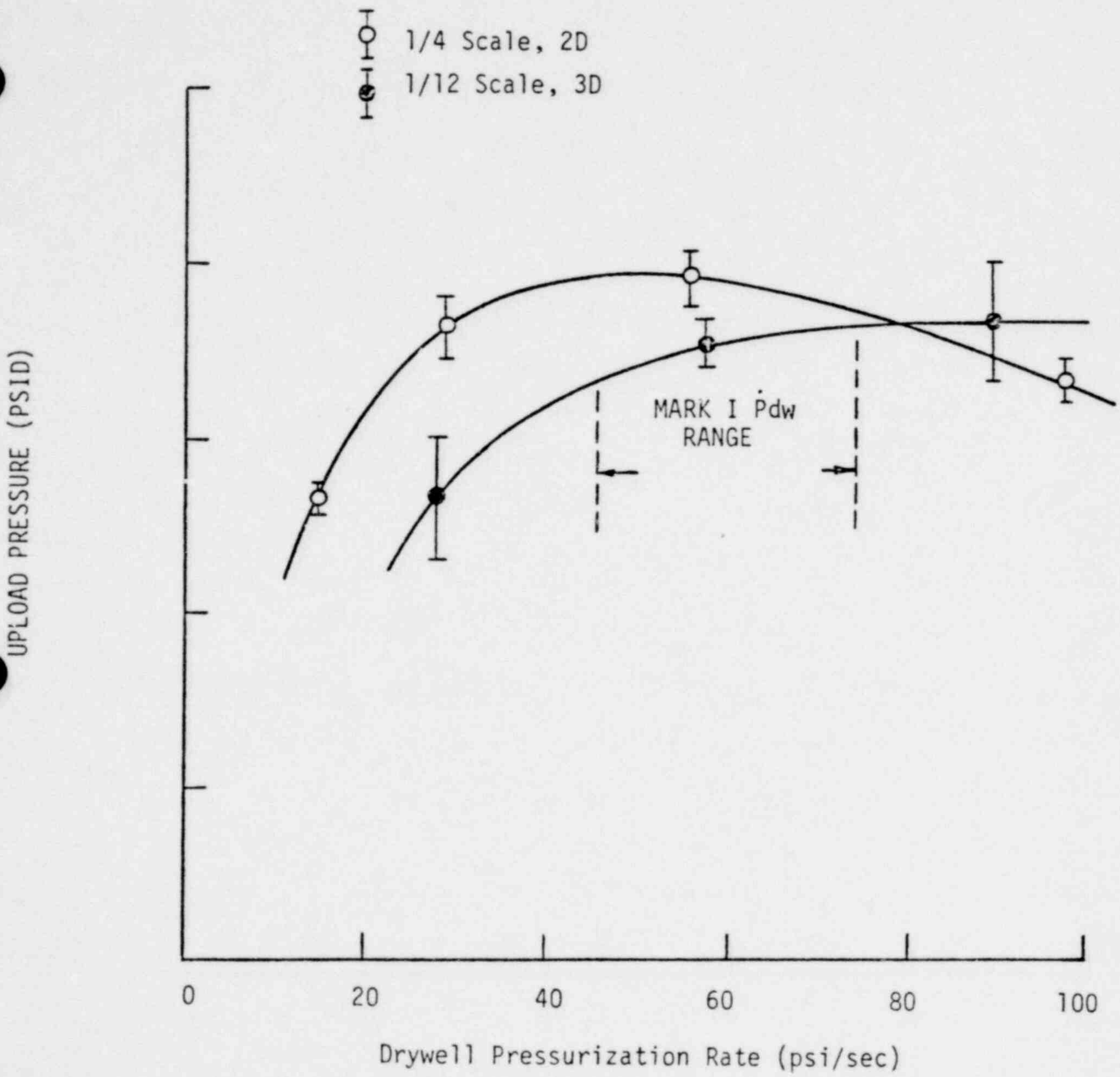

Figure 2. Full-Scale Equivalent Upload Pressure as a Function of Drywell Pressurization Rate (full ΔP, 40" Downcomer Submergence)

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GE-EPRI UPLOAD COMPARISON[6]

BASED ON OUR REVIEW OF THE GE-EPRI COMPARISON, WE HAVE CONCLUDED THAT IT SHOULD NOT BE USED TO ASSESS THE POSSIBILITY OF A 3-D EFFECT ON POOL SWELL UPLOADS. THE DECISION IS BASED ON THE FOLLOWING CONSIDERATIONS:

- PLANT GEOMETRY - BROWNS FERRY GEOMETRY IS NOT PROTOTYPICAL OF MARK I PLANTS. THE 45° DOWNCOMER CONFIGURATION CAUSES EARLY BREAKTHROUGH.
- TEST CONDITIONS - FULL ΔP AND REDUCED SUBMERGENCE MINIMIZE POOL SWELL EFFECTS.
- FLOW RESISTANCE - EPRI TESTS WERE CONDUCTED AT HIGHER VALUES OF FLOW RESISTANCE.
- ORIFICE LOCATION - DOWNCOMER ORIFICE SIZE VARIATION CAUSED A DISTORTED POOL SWELL.

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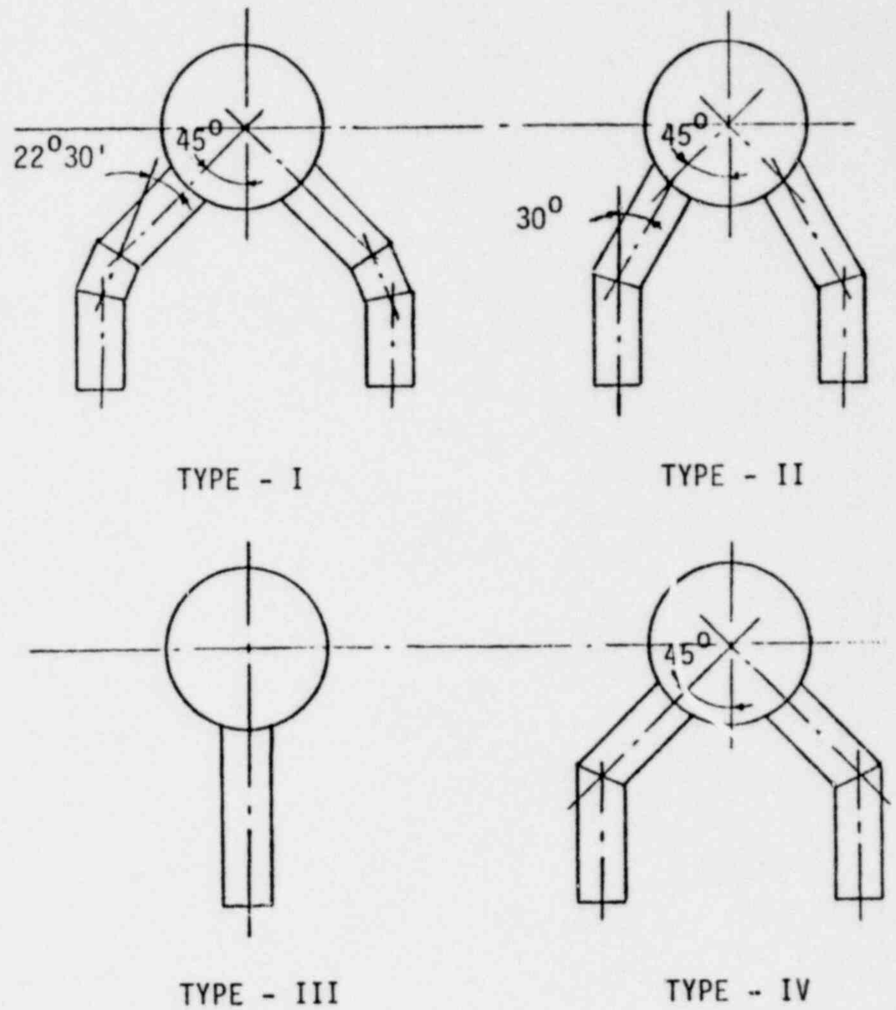
DOWNCOMER TYPES

Plant	Type	Number of Downcomers
Browns Ferry 1, 2, 3	IV	96
Brunswick 1 & 2	II	96
Cooper Station	II	80
Dresden 2 & 3	II	96
Duane Arnold	III	48
Fermi 2	II	80
Fitzpatrick	II	96
Hatch 1 & 2	II	80
Hope Creek 1 & 2	II	80
Millstone	II	96
Monticello	II	96
Nine Mile Point 1	I	120
Oyster Creek 1	I	120
Peach Bottom 2 & 3	II	96
Pilgrim	II	96
Quad Cities 1 & 2	II	96
Vermont Yankee	II	96



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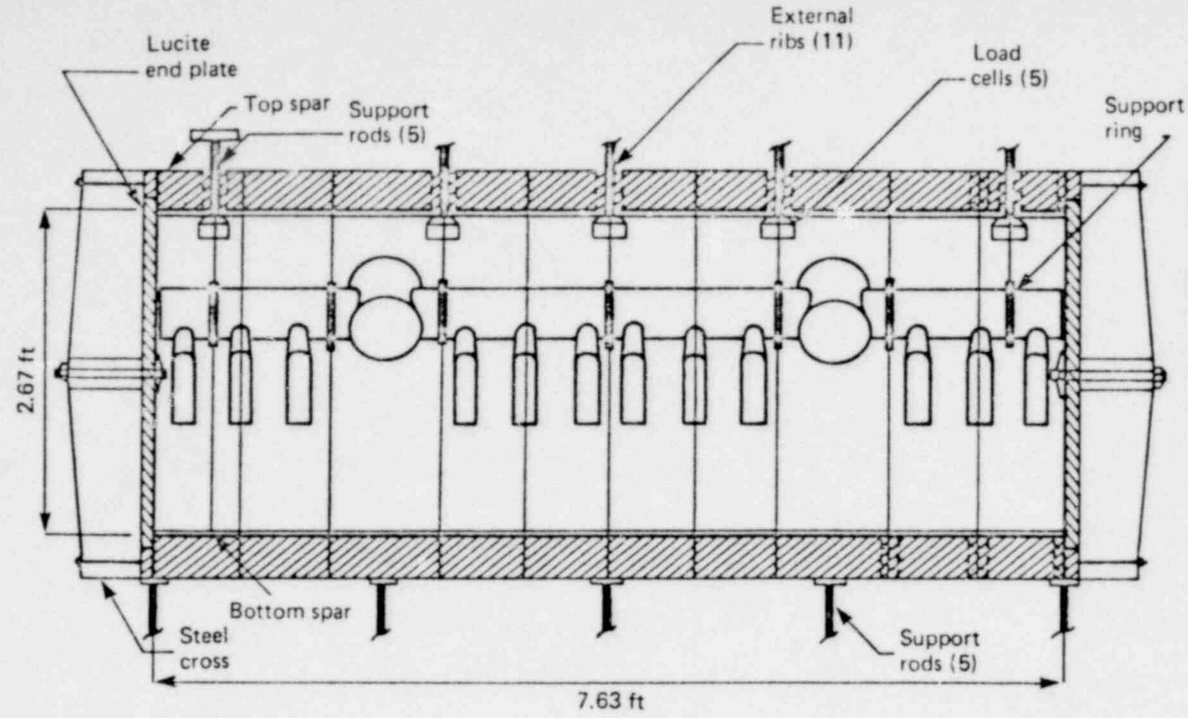
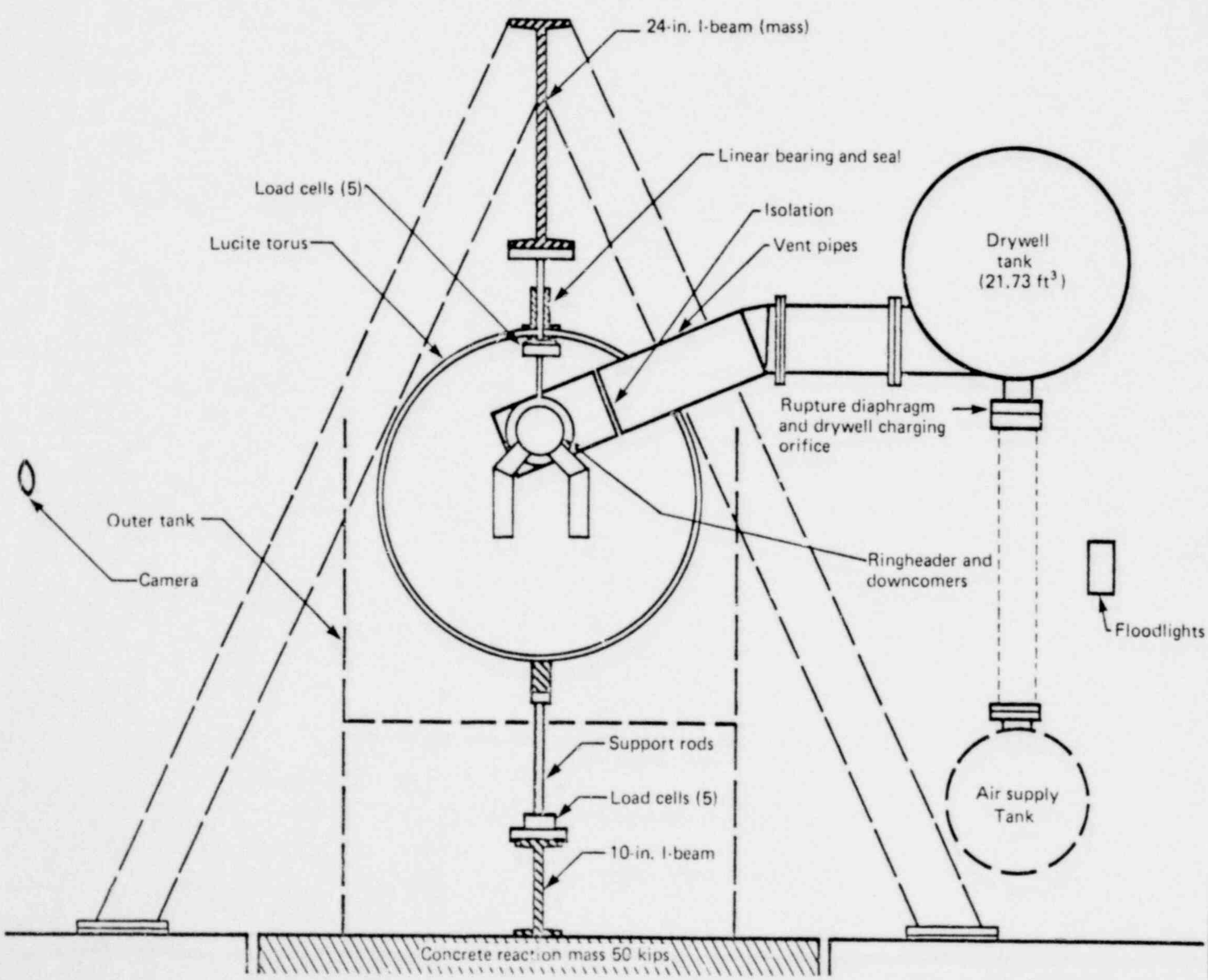
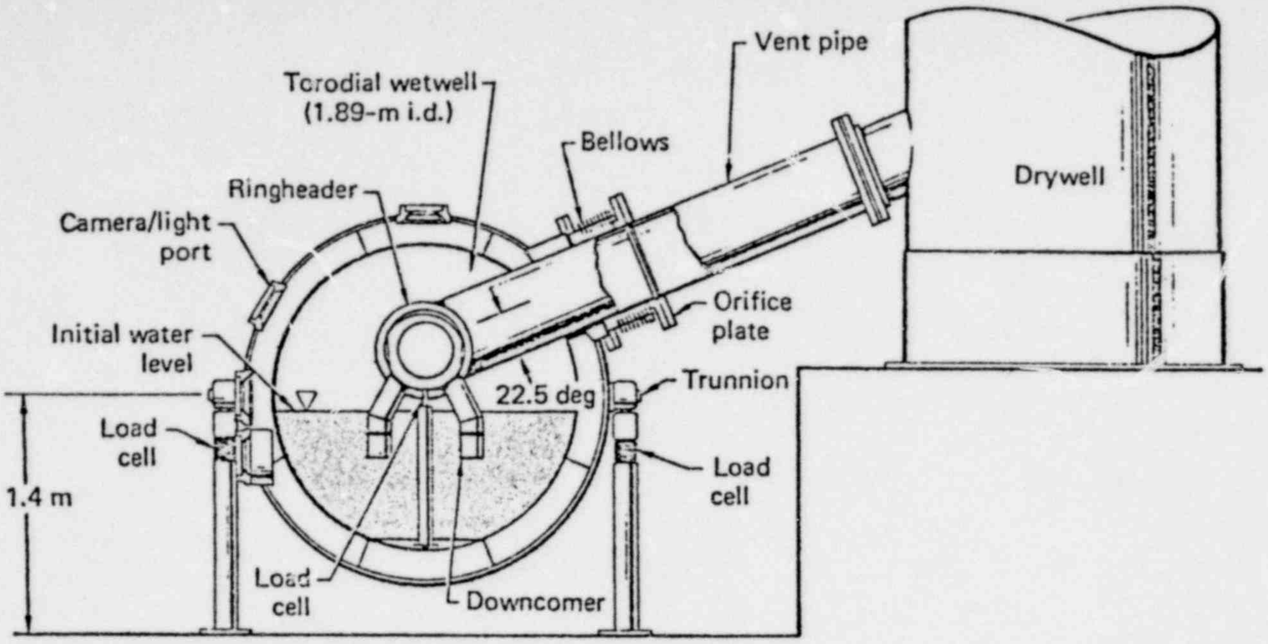
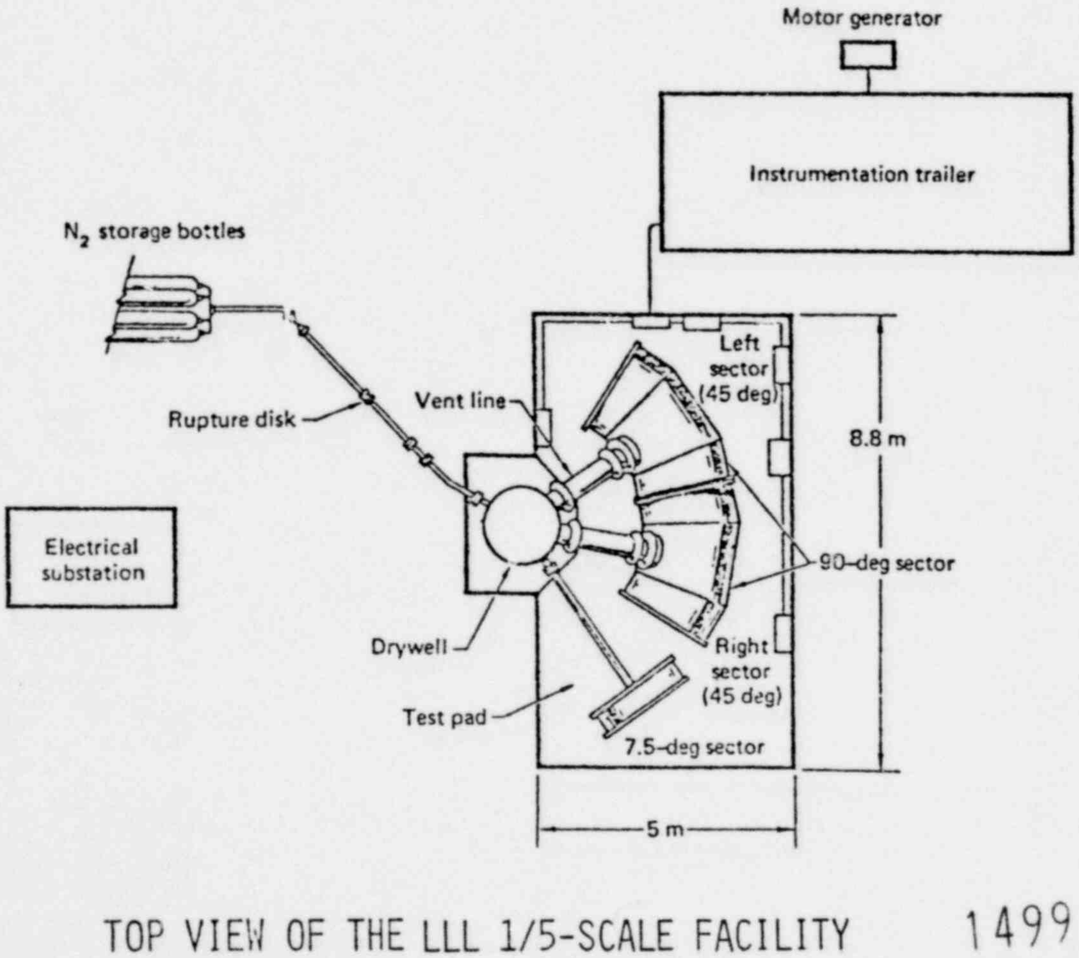

Lucite
end plate
Top spar
Support
rods (5)
External
ribs (11)
Load
cells (5)
Support
ring
2.67 ft
Bottom spar
Steel
cross
Support
rods (5)
7.63 ft


24-in. I-beam (mass)
Linear bearing and seal
Load cells (5)
Isolation
Lucite torus
Vent pipes
Drywell
tank
(21.73 ft³)
Rupture diaphragm
and drywell charging
orifice
Outer tank
Camera
Ringheader and
downcomers
Floodlights
Support rods
Load cells (5)
Air supply
Tank
10-in. I-beam
Concrete reaction mass 50 kips

1499 300 33



CROSS-SECTION OF THE LLL 1/5-SCALE PRESSURE SUPPRESSION EXPERIMENTAL APPARATUS



TOP VIEW OF THE LLL 1/5-SCALE FACILITY

1499 301

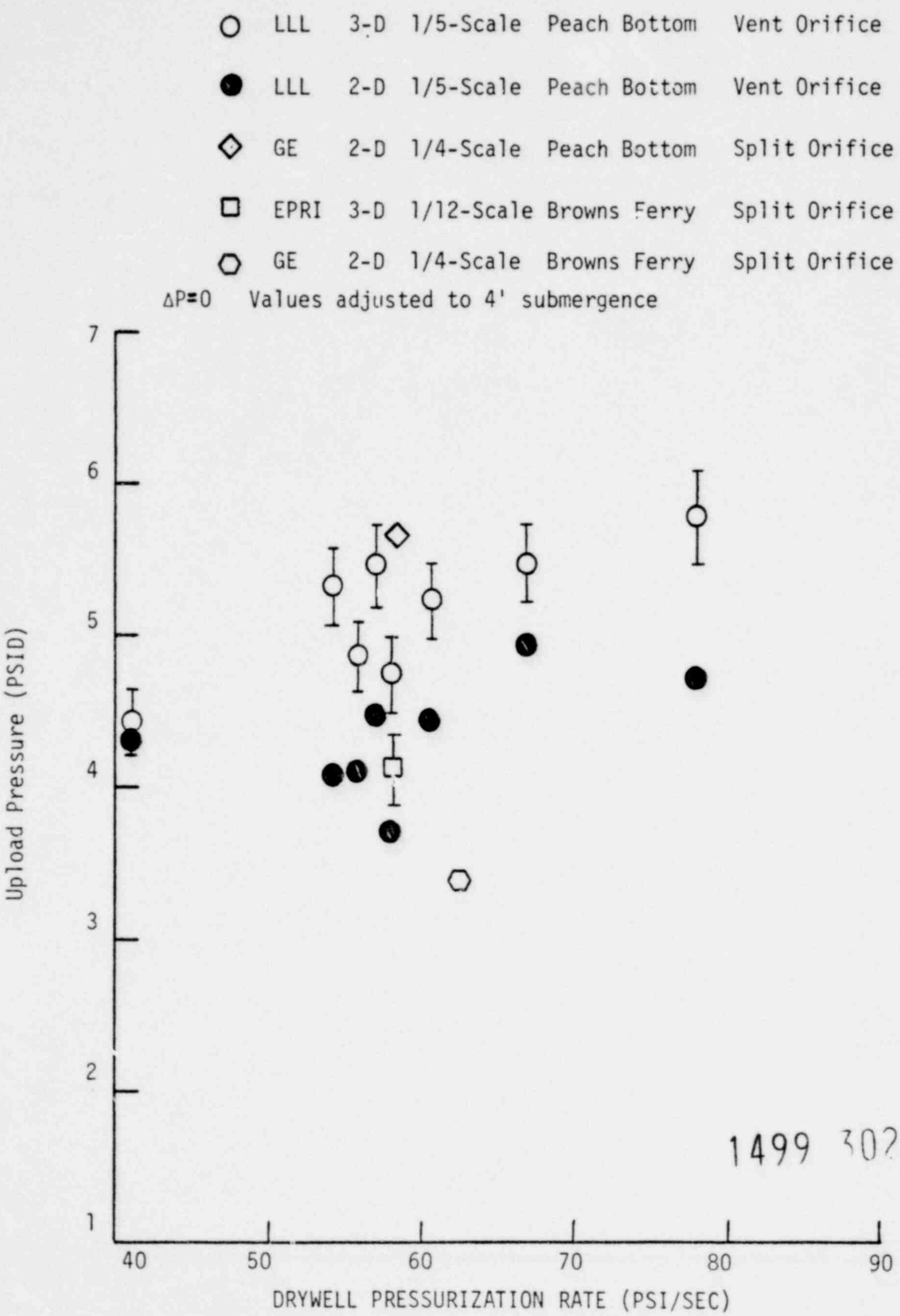


FIGURE 1. Full-Scale Equivalent Upload Pressure as a Function of Drywell Pressurization Rate (Zero ΔP, 4 ft. Submergence)

1499 302

ANALYSIS OF LLL TEST RESULTS

PURPOSE: TO DETERMINE IF THE EXPERIMENTAL TREND AS INDICATED BY THE DATA WAS DUE TO A 3-D EFFECT ON POOL SWELL OR POSSIBLY A MIS-MATCH OF THE 3-D AND 2-D SECTORS.

METHOD: A ONE-DIMENSIONAL TRANSIENT POOL SWELL ANALYSIS WAS PERFORMED FOR BOTH THE LLL 2-D AND 3-D SECTORS. THE SYSTEM, AS MODELED, CONSISTED OF DRYWELL, VENT LINE VOLUMES UPSTREAM AND DOWNSTREAM OF THE ORIFICE, HEADER VOLUME, DOWNCOMER VOLUME, BUBBLE VOLUME, LIQUID SLUG AND WETWELL AIR-SPACE VOLUME.

RESULTS: THE CALCULATIONS HAVE SHOWN THAT THE LLL RIGS WERE INDEED MIS-MATCHED DUE TO DIFFERENCES IN CAPACITANCE (VOLUME) AND RESISTANCE. THE EFFECT ON PEAK UPLOAD PRESSURES VARIED FROM 3-9% OVER THE RANGE OF PRESSURIZATION RATES CONSIDERED IN THE STUDY.

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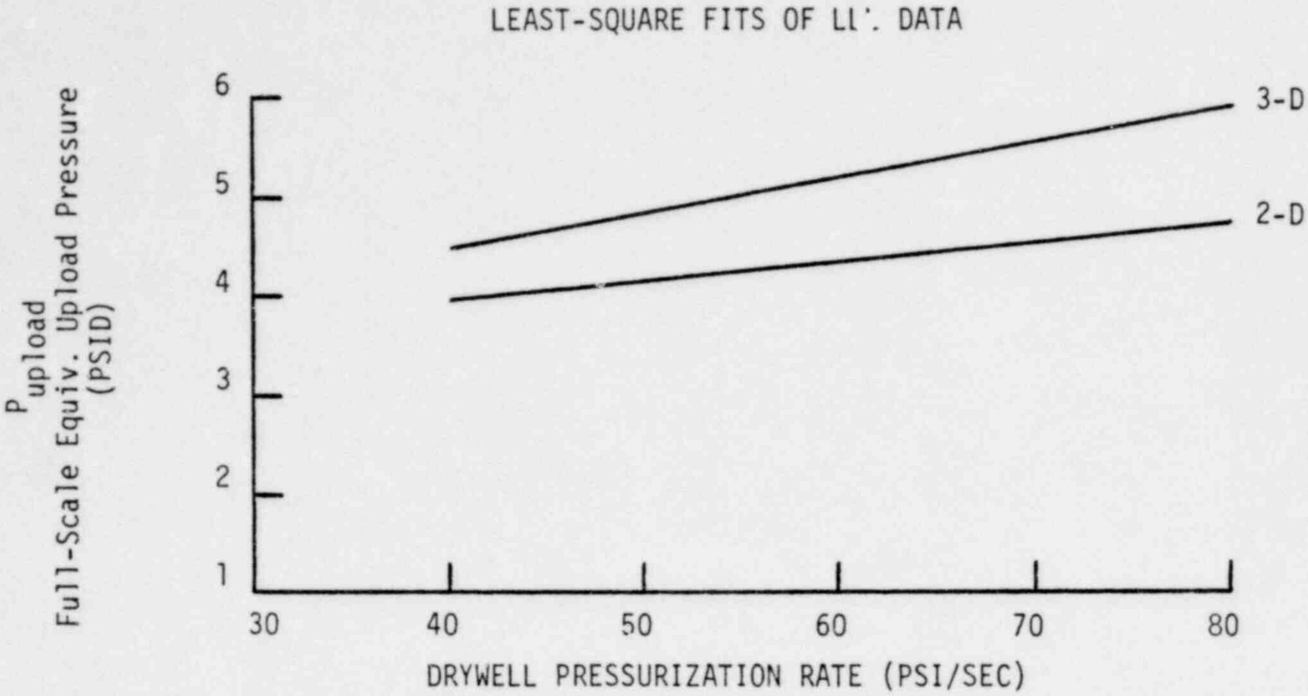

FIGURE 3A. Full-Scale Equivalent Upload Pressure as a Function of Drywell Pressurization Rate (LLL Data)

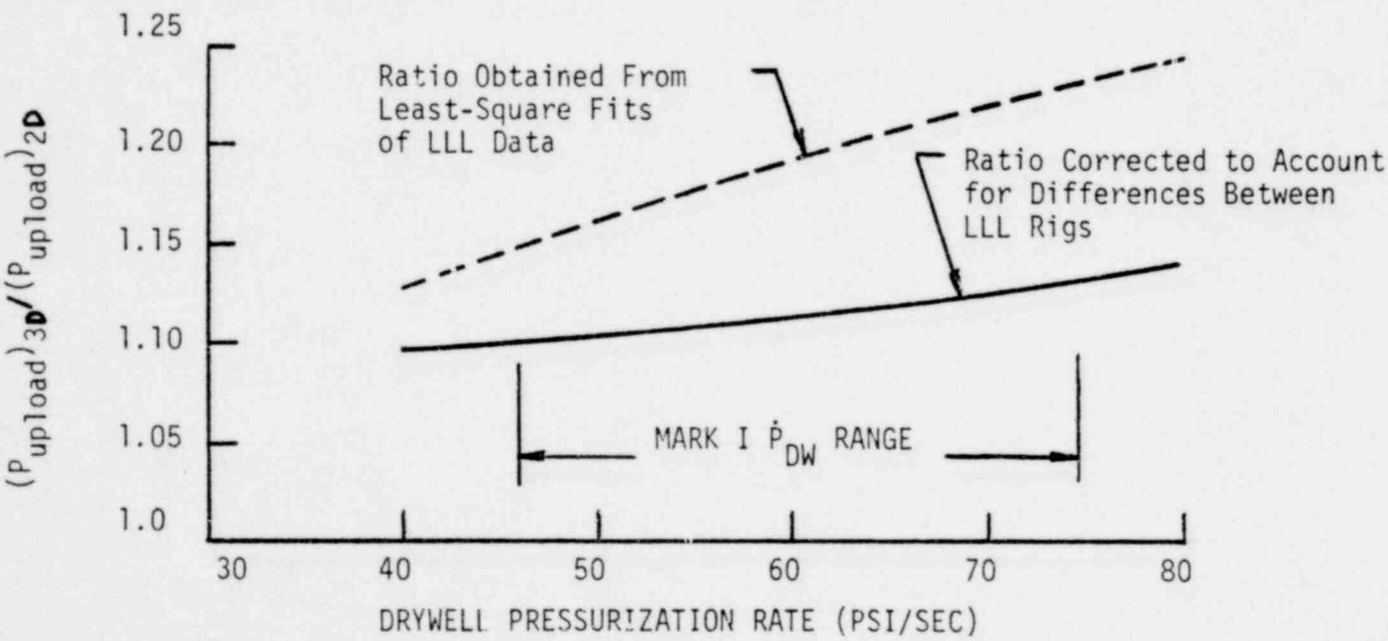

FIGURE 3B. Ratio of 3D/2D Full-Scale Equivalent Pressures Obtained From LLL Data

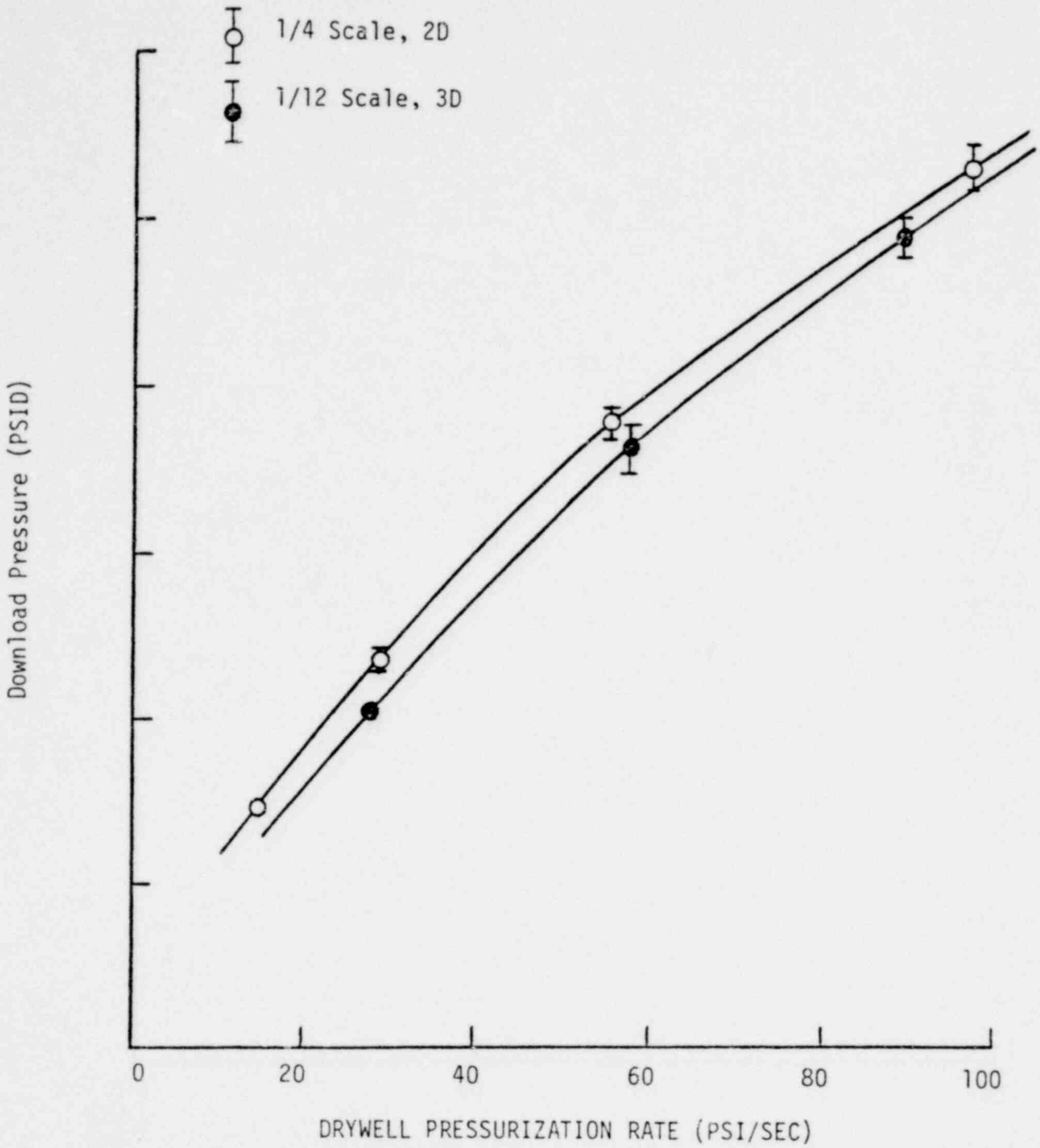

FIGURE 5. Full-Scale Equivalent Download Pressure as a Function of Drywell Pressurization Rate (full ΔP, 40" Downcomer Submergence)

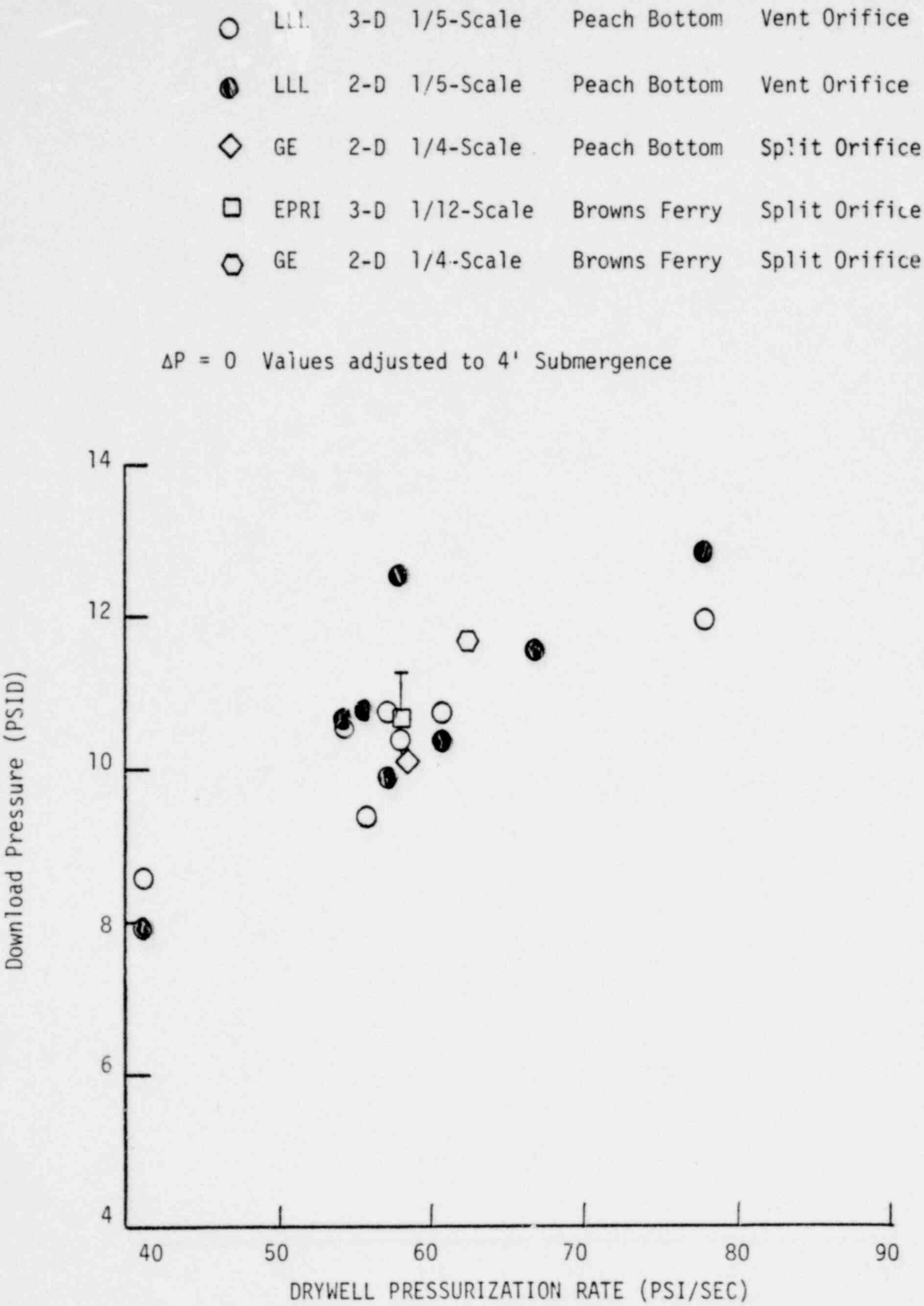

FIGURE 4. Full-Scale Equivalent Download Pressure as a Function of Drywell Pressurization Rate (Zero ΔP, 4 ft. Submergence)

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REFERENCES

1. "Mark I Quarter Scale Plant Unique Tests, Task 5.5.3, Series 2," NEDE 21944-P, January 1979.

2. "Three-Dimensional Pool Swell Modeling of a Mark I Suppression System," EPRI-NP-906, October 1978.

3. Summary of EPRI Split-Orifice Test Results, April 1979.

4. "Final Air Test Reports for the 1/5-Scale Mark I Boiling Water Reactor Pressure Suppression Experiment," UCRL-52371, October 1977.

5. McCauley, E. W., et al., "Best Estimate Analysis of the Hydrodynamic Vertical Load Function," UCRL report to be published, March 1979.

6. "Mark I Containment Program, Comparison of GE and EPRI Torus Load Test Results, Task Number 5.10," NEDE-21973-P, February 1979.

7. "Mark I Containment Evaluation Short Term Program - Final Report, Addendum 2," GE NEDC-20989-P, June 1976.

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KOSSON

POOL SWELL FLOW DISTRIBUTION EFFECTS

OBJECTIVE:

IN SUPPORT OF SECTION 2.5 OF THE ACCEPTANCE CRITERIA, SHOW WHY THE SWEEP TIME FOR RING HEADER IMPACT SHOULD BE BASED ON 3D MODEL TEST DATA USING ORIFICES ONLY IN THE MAIN VENT LINE.

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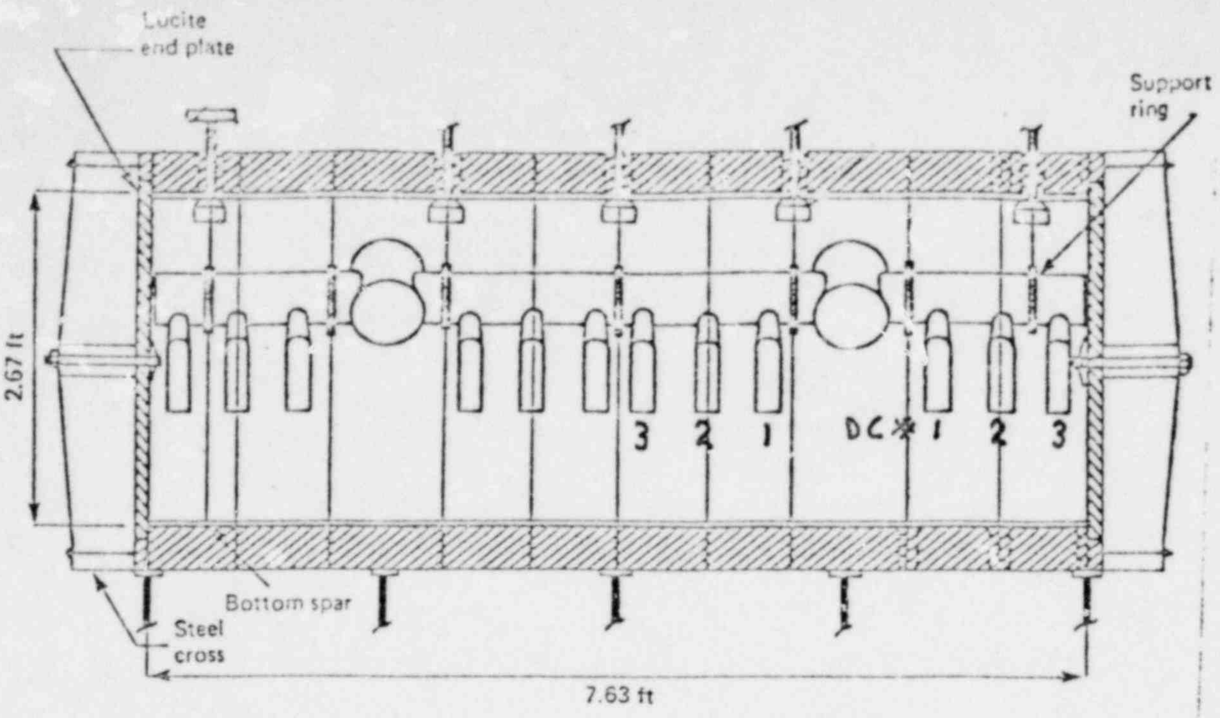
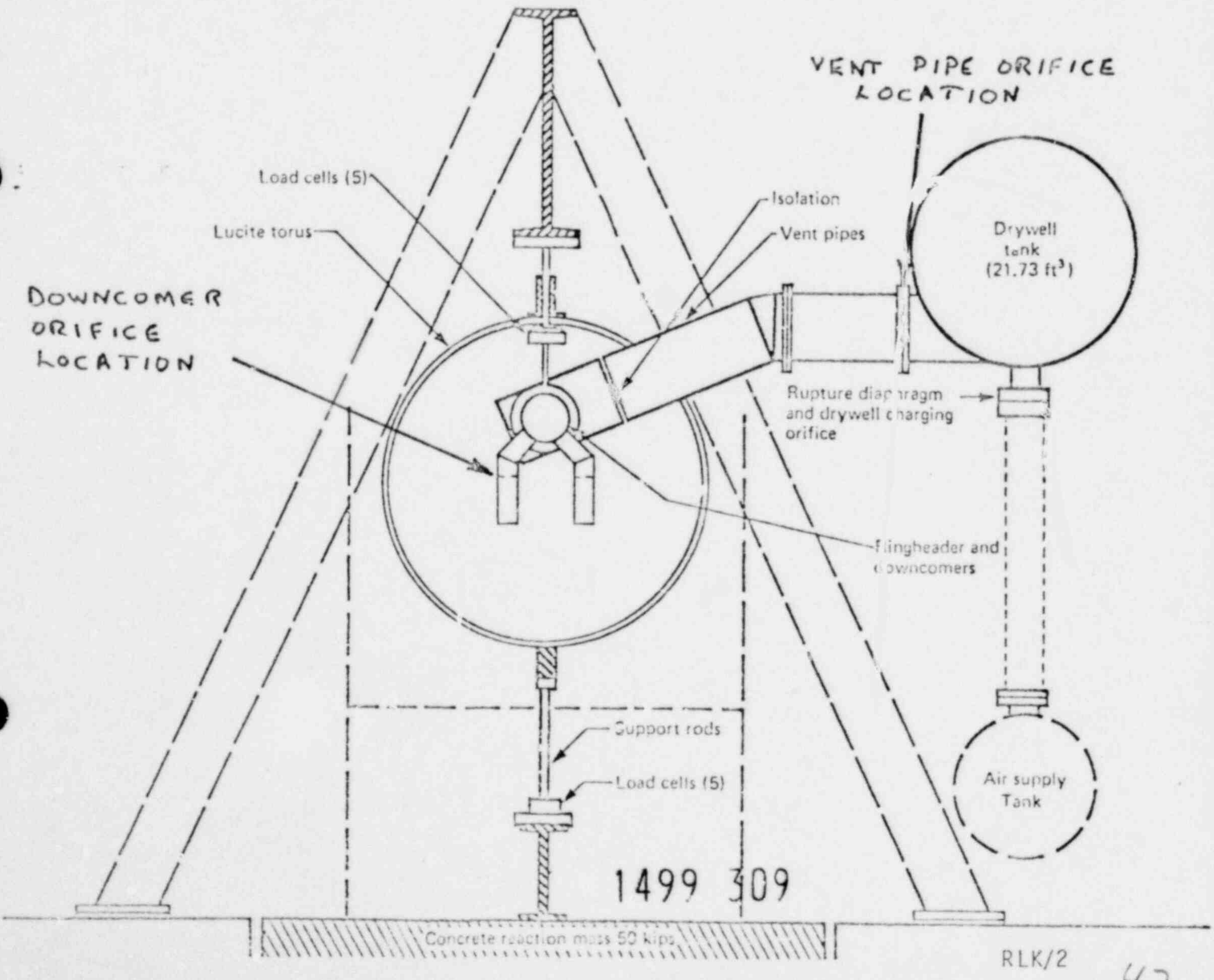


EPRI- SRI 1/11.7 SCALE MODEL



1499 309

RLK/2
11/16/79 42

3)

SCALING LAWS

LET $S = L_M/L_P$

THEN $P_M/P_P = S$

$T_M/T_P = S^{1/2}$

$V_M/V_P = S^{1/2}$

$(\dot{M}H)_M/(\dot{M}H)_P = S^{7/2}$

THESE REQUIRE $(fL/D)_M/(fL/D)_P = 1/S$

PROBLEMS

1) MUST KNOW $(fL/D)_P$ TO SIZE ORIFICES

2) MUST LOCATE ORIFICES TO PROVIDE CORRECT FLOW DISTRIBUTION

1499 310

RLK/3

11/16/79

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43

EPRI-SRI ORIFICE SIZING

1) Used steady state flow calibration tests with "dry" 1/11.7 and 1/31 scale models and no orifices

2) Established $\left(\frac{\dot{M}}{P_{DW} A_{DC}} \right)$ vs. $\frac{P_{WW,DRY}}{P_{DW}}$ for each downcomer

3) Established "target" curves of

$$\left(\frac{\dot{M}}{P_{DW} A_{DC}} \right)_M / \left(\frac{\dot{M}}{P_{DW} A_{DC}} \right)_P = \sqrt{S}$$

assuming $T_M = T_P$

4) Experimentally determined orifice sizes required for vent or individual downcomers

Note: It is difficult experimentally to obtain the "exact" orifice size. From curve fits presented for downcomer orifices in NP-906 the ratio of highest to lowest downcomer flow rates seemed excessive (approximately 1.33 to 1).

1499 311

RLK/4

11/16/79

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UNCERTAINTIES ASSOCIATED WITH DOWNCOMER ORIFICES

1) FLOW CALIBRATIONS WERE DONE "DRY", WITH UNIFORM EXIT PRESSURE AT ALL DOWNCOMERS. DURING EARLY BUBBLE GROWTH, BUBBLE PRESSURE CAN VARY FROM ONE DOWNCOMER TO THE NEXT.

2) DOWNCOMER PAIR #3, WHICH HAS THE LOWEST FLOW RESISTANCE, HAS THE SMALLEST POOL AREA AND THE HIGHEST BUBBLE PRESSURE DURING EARLY BUBBLE GROWTH.

3) "T" LOSSES WITHIN VENT SYSTEM VARY WITH FLOW SPLIT AMONG DOWNCOMER PAIRS.

4) ANALYTICAL CALCULATIONS INDICATE MORE UNIFORM FLOW RESISTANCE WHEN INDIVIDUAL DOWNCOMER FLOWS (DUE TO DIFFERENCES IN BUBBLE PRESSURE) ARE MORE UNIFORM.

RLK/5

11/16/79

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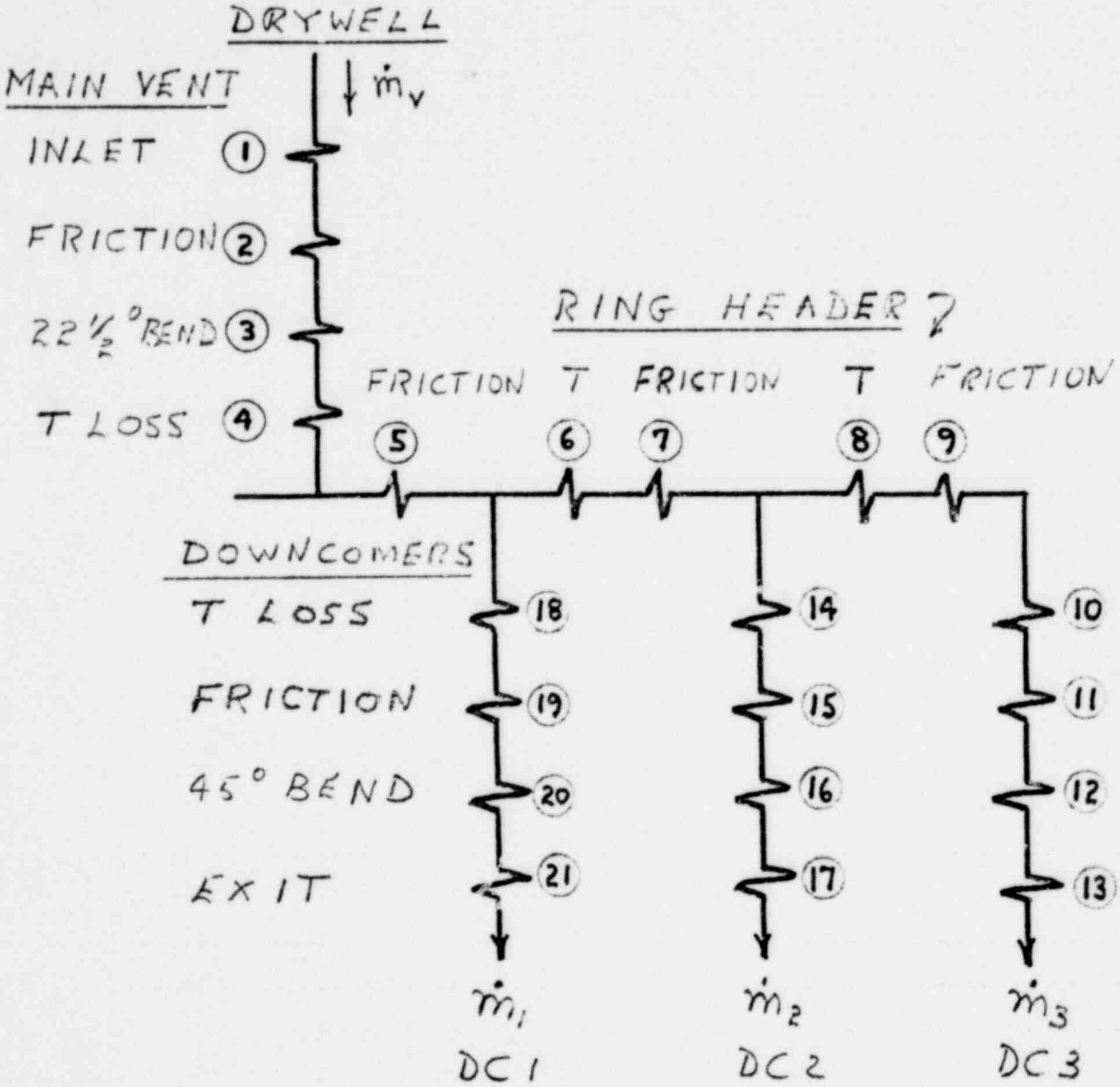
SCHEMATIC FOR FLOW LOSS CALCULATIONS

EPRI-SRI MODEL (NO ORIFICES)
BROWNS FERRY



RLK/6

11/16/79

1499 313

46

VENT SYSTEM LOSS COEFFICIENT

SINGLE COMPONENT:

$$\Delta P_i = K_i q_i = \frac{K_i \dot{m}_i^2}{2 g_c \rho A_i^2}$$

K_i VALUES FROM IDEL'CHIK

SERIES FLOW PATH, INLET TO DC EXIT

$$\Delta P_{T,J} = K_{T,J} \, q_{DC,J}$$

$$K_{T,J} = \left(\frac{\dot{m}_v}{\dot{m}_{DC,J}}\right)^2 \sum_i K_i \left(\frac{A_{DC}}{A_i}\right)^2 \left(\frac{\dot{m}_i}{\dot{m}_v}\right)^2$$

COMPLETE VENT SYSTEM

$$\Delta P_T = \bar{K} \bar{q} = \frac{\bar{K} \dot{m}_T^2}{2 g_c \rho (N_{DC} A_{DC})^2}$$

$$\frac{1}{\bar{K}} = \left[\frac{1}{N_{DC}} \sum_J \frac{1}{\sqrt{K_{T,J}}}\right]^2$$

1499 314

RLK/7

11/16/79

COMPARISON OF FLOW COEFFICIENTS
EPRI-SRI MODEL, NO ORIFICES

$$K_{T,J} \equiv (P_{DW} - P_{B,J})/Q_{DC,J}$$

FLOW PATH	MEASURED EXPERIMENTAL (DRY TEST)	IDEL'CHIK CALCULATION EXPERIMENTAL DIST	IDEL'CHIK CALCULATION UNIFORM DIST
$K_{T,1}$	6.72	6.99	6.07
$K_{T,2}$	5.53	5.39	5.45
$K_{T,3}$	5.07	4.80	5.10
AVERAGE $\bar{K}$	5.71	5.62	5.52
$K_{T,1}/K_{T,3}$	1.33	1.46	1.19

ADJUSTED VALUE FOR UNIFORM DIST $K_{DC1}/K_{DC3} = \frac{1.33}{1.46} \times 1.19 = 1.08$

NOTE: 1) IDEL'CHIK $\bar{K}$ VALUE CLOSE TO EXPERIMENTAL.

2) IDEL'CHIK OVERESTIMATES $K_{T,1}/K_{T,3}$ RATIO

3) $K_{T,1}/K_{T,3}$ DECREASES AS FLOW BECOMES MORE UNIFORM

RLK/8

11/16/79

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COMPARISON OF LOSS COEFFICIENTS (RH TO DC_{EXIT})

AND MASS FLOW RATIOS (EQUAL ΔP)

	EXPERIMENTAL DIST.			UNIFORM DIST.			%Δ
	$K_{T,1}$	$K_{T,3}$	$\dot{M}_3/\dot{M}_1$	$K_{T,1}$	$K_{T,3}$	$\dot{M}_3/\dot{M}_1$	$\dot{M}_3/\dot{M}_1$
PROTOTYPE MODEL-VENT ORIF	4.13	2.51	1.25*	3.64	2.51	1.17*	-6.6%
MODEL-SPLIT ORIF	34.3	25.4	1.16	33.8	25.5	1.15	-0.8
MODEL-DC ORIF	76.0	56.8	1.16	75.5	56.9	1.15	-0.3

*THESE VALUES ARE INDIVIDUALLY HIGH DUE TO USE OF IDEL'CHIK GENERAL RELATIONS - % CHANGE IS BELIEVED TO BE REAL, HOWEVER.

NOTE: FOR SPLIT AND/OR DC ORIFICE CASES, FLOW DISTRIBUTION HAS LITTLE EFFECT ON K_T VALUES.

1499 316

RLK/9

11/16/79

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BUBBLE PRESSURE CALCULATIONS

FROM EPRI FLOW CALIBRATION WITH DOWNCOMER ORIFICES

$$\frac{\dot m}{P_{DW} A_{DC}} = \frac{1}{144}\left[C_1 - \frac{C_2}{(C_3 - P_B/P_{DW})^{C_4}}\right]$$

FOR ADIABATIC FLOW INTO BUBBLE

$$\dot P_B = \frac{(\gamma-1)\,\dot m\, h_o}{V_B} - 3\gamma P_B \frac{\dot R}{R}$$

MODIFIED RAYLEIGH BUBBLE EQUATION (FINITE POOL)

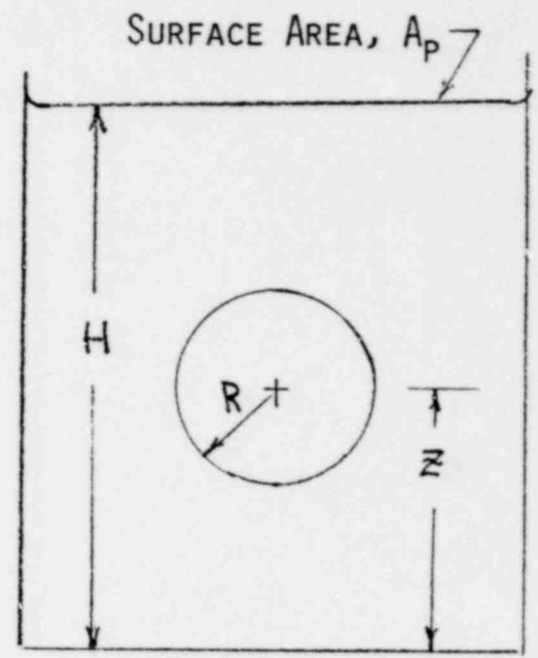
$$\ddot R = \left[\frac{g_c(P_B - P_\infty)}{\rho_l} - \dot R^2\left(\frac{3}{2} + f_1\right)\right]\frac{1}{f}$$

WHERE

$$f_1 = \frac{R}{z} - \frac{R}{(H-z)} + \frac{8R}{\sqrt{A_p}}$$

AND

$$f = \frac{R}{2}\left(2 + f_1\right)$$



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RLK/10

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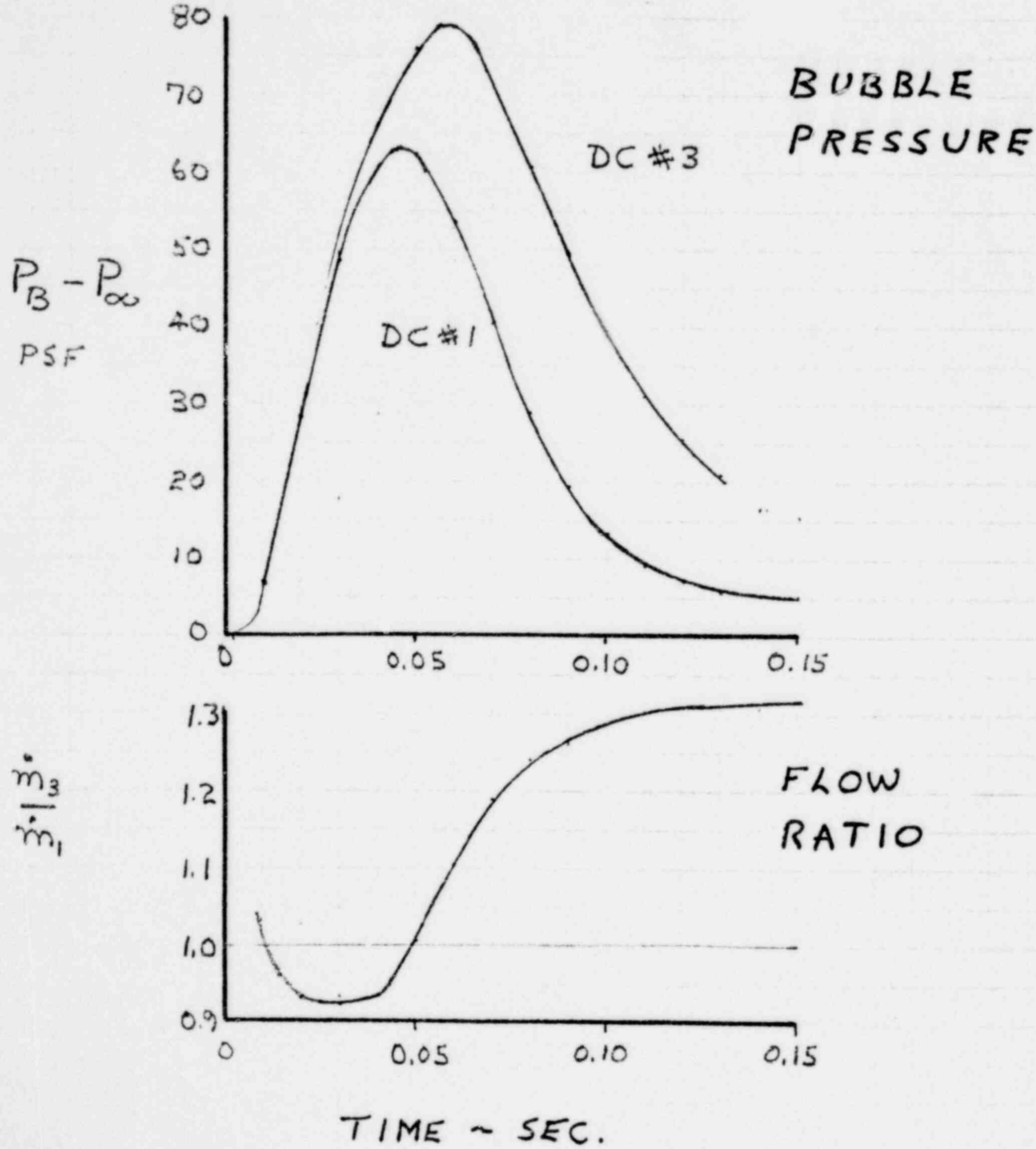
MODIFIED RAYLEIGH BUBBLE CALCS



1499 318

RLK/11
11/16/79 51

HIGH HEADER WATER LEVEL GAP ZERO ΔP

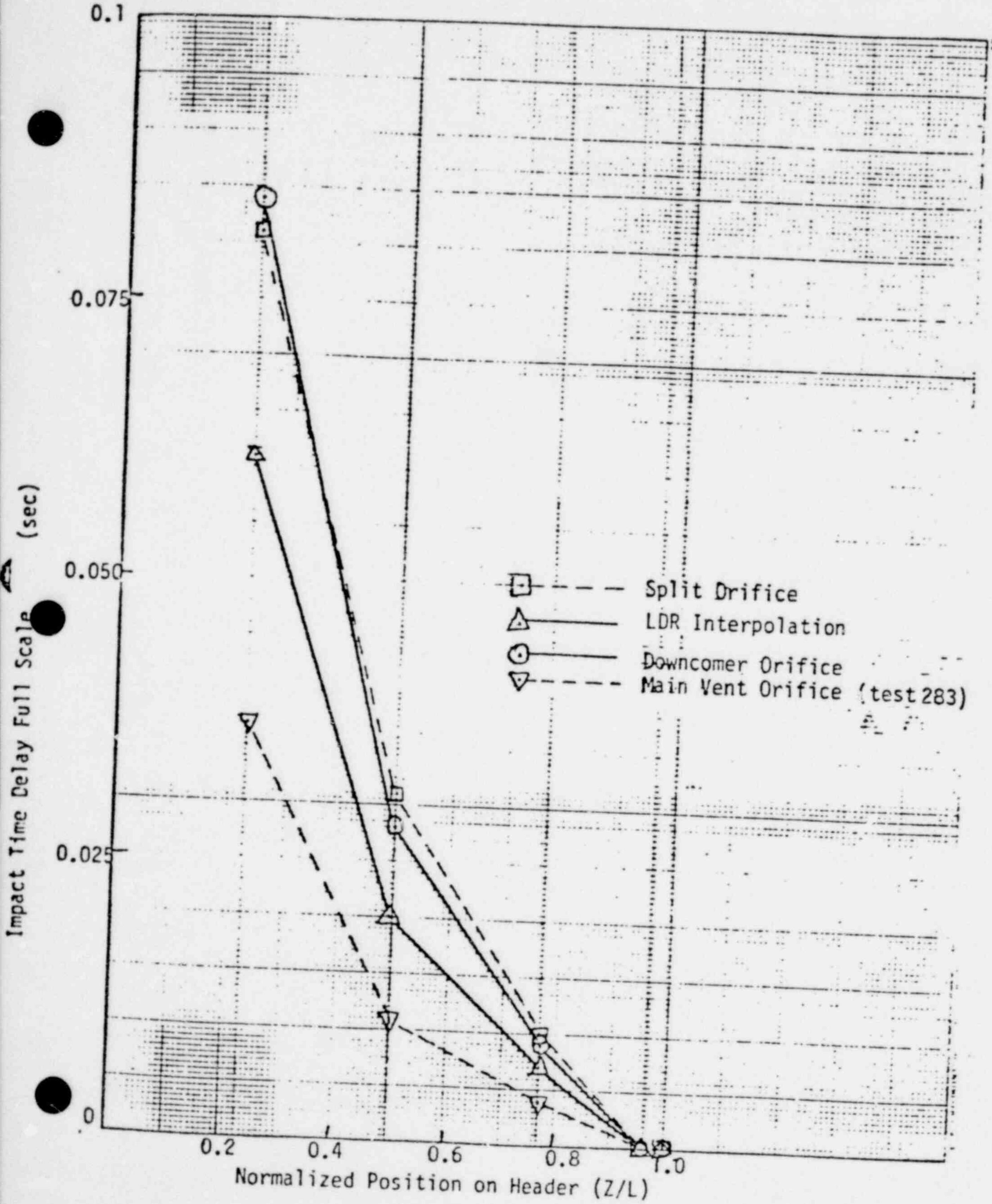


Figure 21-1

1499 319

RLK/12
11/16/79 52

CONCLUSIONS

1) ANALYSIS AND TEST INDICATE THAT THE SPLIT ORIFICE AND DOWNCOMER ORIFICE PROVIDE THE SAME FLOW DISTRIBUTION AND SWEEP TIME.

2) ANALYSIS INDICATES THE SPLIT AND DOWNCOMER ORIFICE TESTS PROBABLY HAD AN EXCESSIVE FLOW RATIO ($\dot{M}_3/\dot{M}_1$).

3) THE SPLIT AND DOWNCOMER ORIFICE CONFIGURATIONS HAD DIFFERENT MASS CAPACITANCE EFFECTS BUT THIS DID NOT SEEM TO AFFECT SWEEP TIME.

4) THE VENT ORIFICE TESTS PROVIDE THE MOST CORRECT (PROTOTYPICAL) FLOW DISTRIBUTION. MASS CAPACITANCE, WHILE EXCESSIVE, MAY NOT HAVE AFFECTED SWEEP TIME.

5) THE VENT ORIFICE TESTS APPEAR TO PROVIDE THE BEST ESTIMATE OF RING HEADER SWEEP TIME AND, FOR CONSERVATISM, SHOULD BE APPLIED IN LOAD CALCULATIONS.

RLK/13

11/16/79

1499 321

MARK I CONTAINMENT PROGRAM
VENT HEADER DEFLECTOR LOAD DEFINITION

ACRS MEETING

SAN FRANCISCO, CALIFORNIA

NOVEMBER 16, 1979

ACUREX Corporation

KENNEDY

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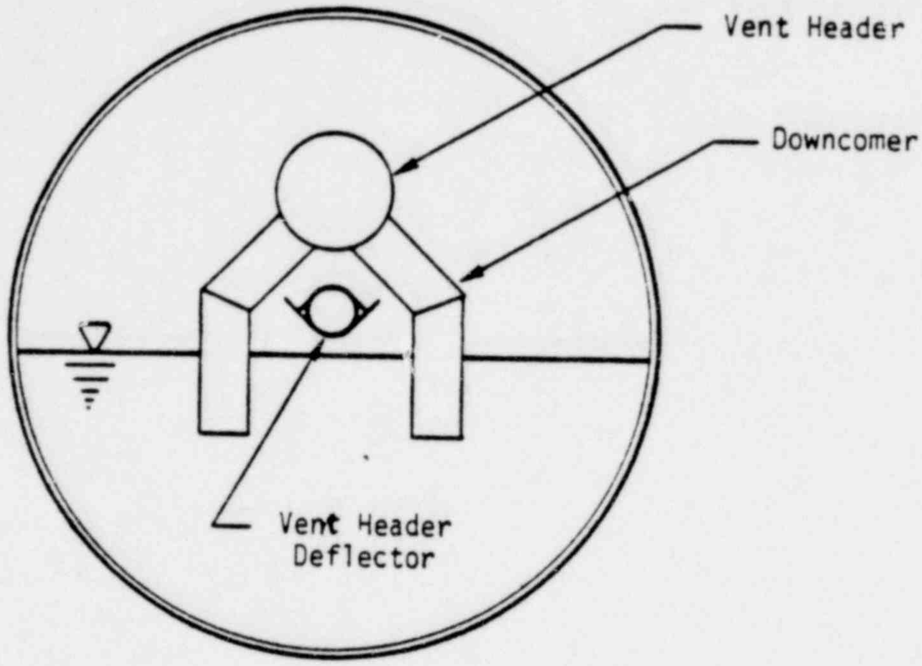
OUTLINE

VENT HEADER DEFLECTOR LOAD DEFINITION

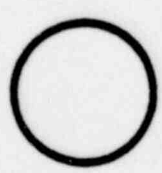
1499 322

- PROBLEM DESCRIPTION
- PRESENT LOAD PREDICTION METHOD
- COMPARISON OF PREDICTION TO QUARTER SCALE TEST FACILITY (QSTF) DATA
- NRC MODIFICATIONS TO METHOD AND EFFECT ON LOADS

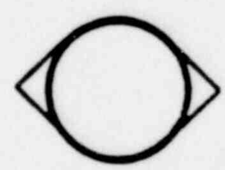
1499 123



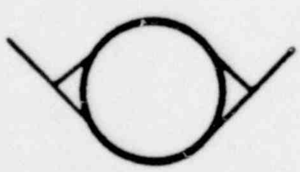
Typical Vent Header Deflector



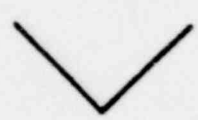
a) Pipe (Type 1)



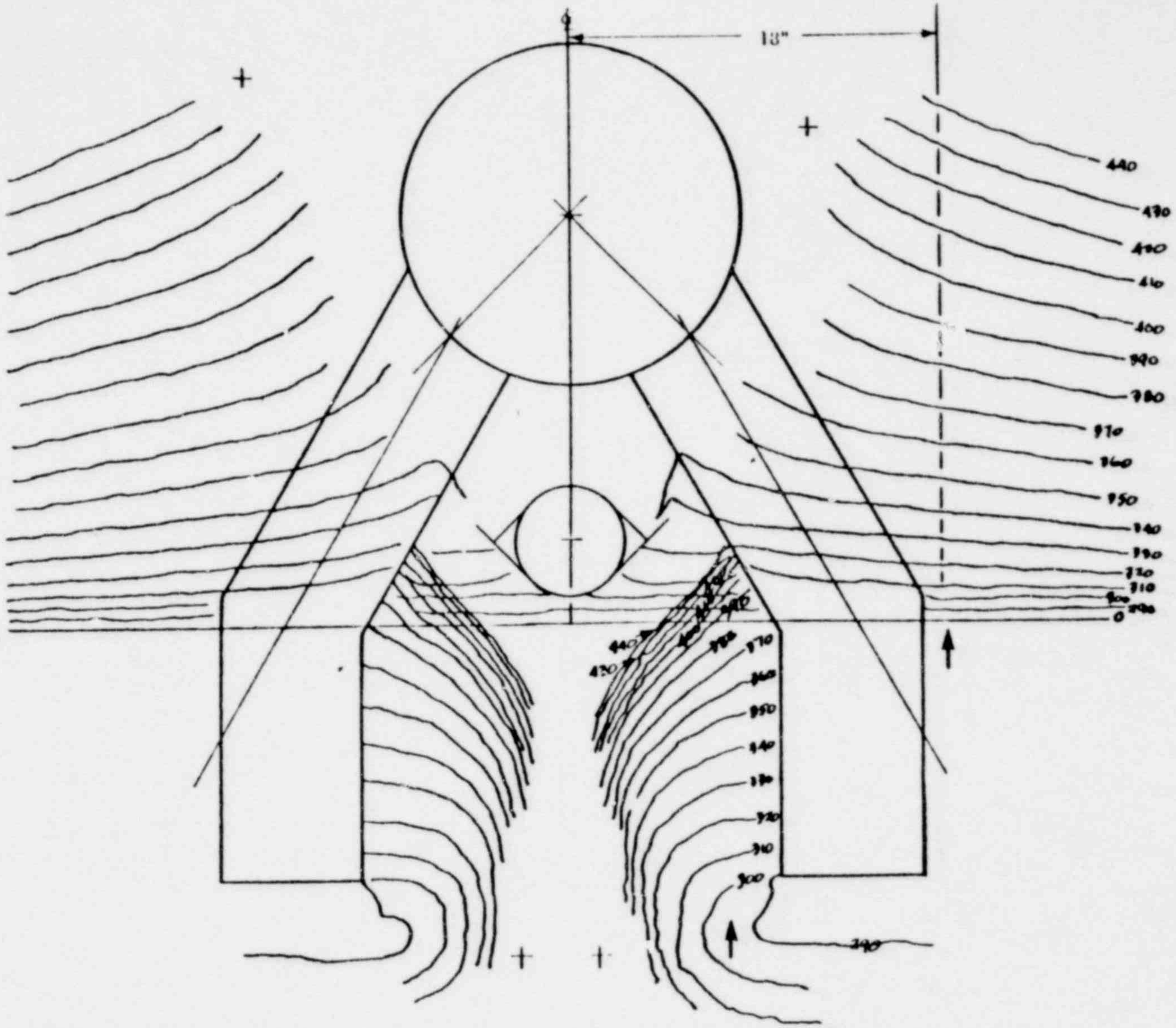
b) Pipe with Angles
(Type 2)



c) Pipe with Tees
(Type 3)



d) Wedge (Type 4)



Pool and Bubble Profiles from High-Speed Motion Pictures

LDR

DEFLECTOR

LOAD PREDICTION METHODOLOGY

1499 325

A) USE OF QSTF DATA

B) ANALYSIS

1) FLOW FIELD PREDICTION

- WATER SURFACE VELOCITY HISTORY CALCULATED BASED ON ONE - DIMENSIONAL POOL SWELL MODEL

- EFFECTIVE MASS OF ONE - DIMENSIONAL MODEL ADJUSTED TO YIELD "CORRECT" TERMINAL VELOCITY FROM QUARTER SCALE MOVIE DATA

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LOAD PREDICTION METHODOLOGY

1499 326

II) LOAD PREDICTION

- LOAD CONSISTS OF IMPACT, ACCELERATION DRAG, BOUYANCY AND "STEADY" DRAG
- IMPACT AND STEADY DRAG CALCULATED BY:

$$D_1 = C_D\ (Y)\ A\ q$$

WHERE $C_D\ (Y)$ = IMPACT & "STEADY" DRAG COEFFICIENT AS A FUNCTION OF DEFLECTOR IMMERSION DEPTH, Y.

A = DEFLECTOR PROJECTED AREA

q = DYNAMIC PRESSURE OF WATER SURFACE = $\frac{1}{2}\rho V^2$

- ACCELERATION DRAG & BOUYANCY CALCULATED BY:

$$D_2 = (M_H(Y) + M_D\ (Y))\dot{V} + M_D\ (Y) g$$

WHERE $M_H(Y)$ = HYDRODYNAMIC MASS OF DEFLECTOR AS A FUNCTION OF Y

$M_D(Y)$ = DISPLACED WATER MASS OF DEFLECTOR AS A FUNCTION OF Y

$\dot{V}$ = ACCELERATION OF WATER SURFACE

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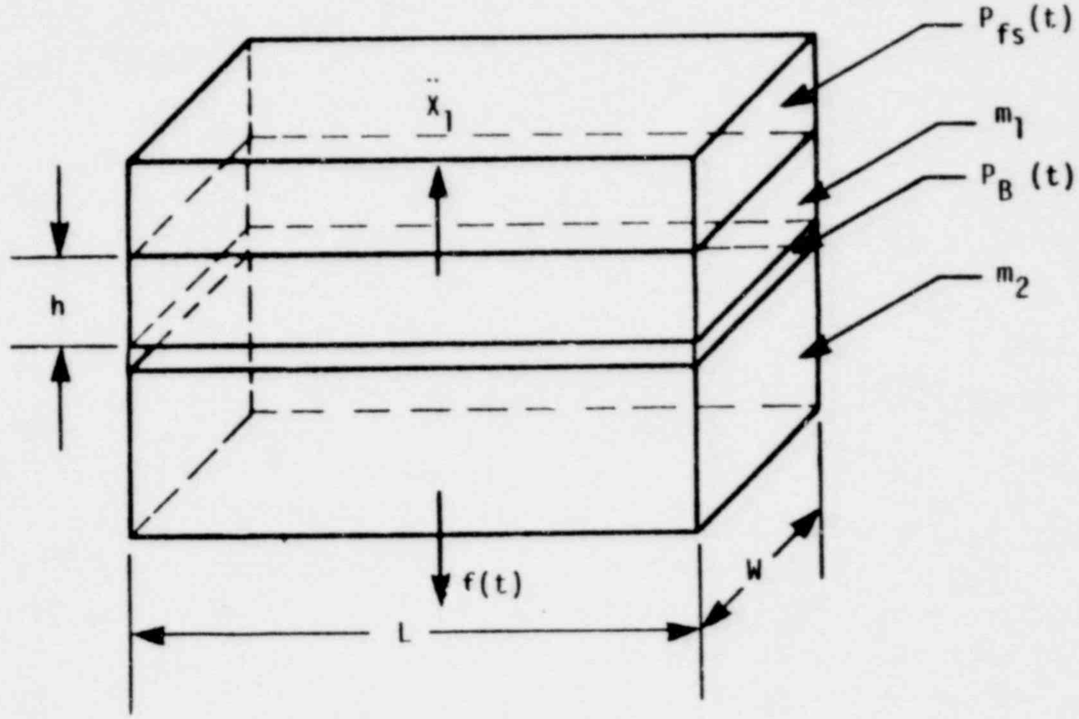
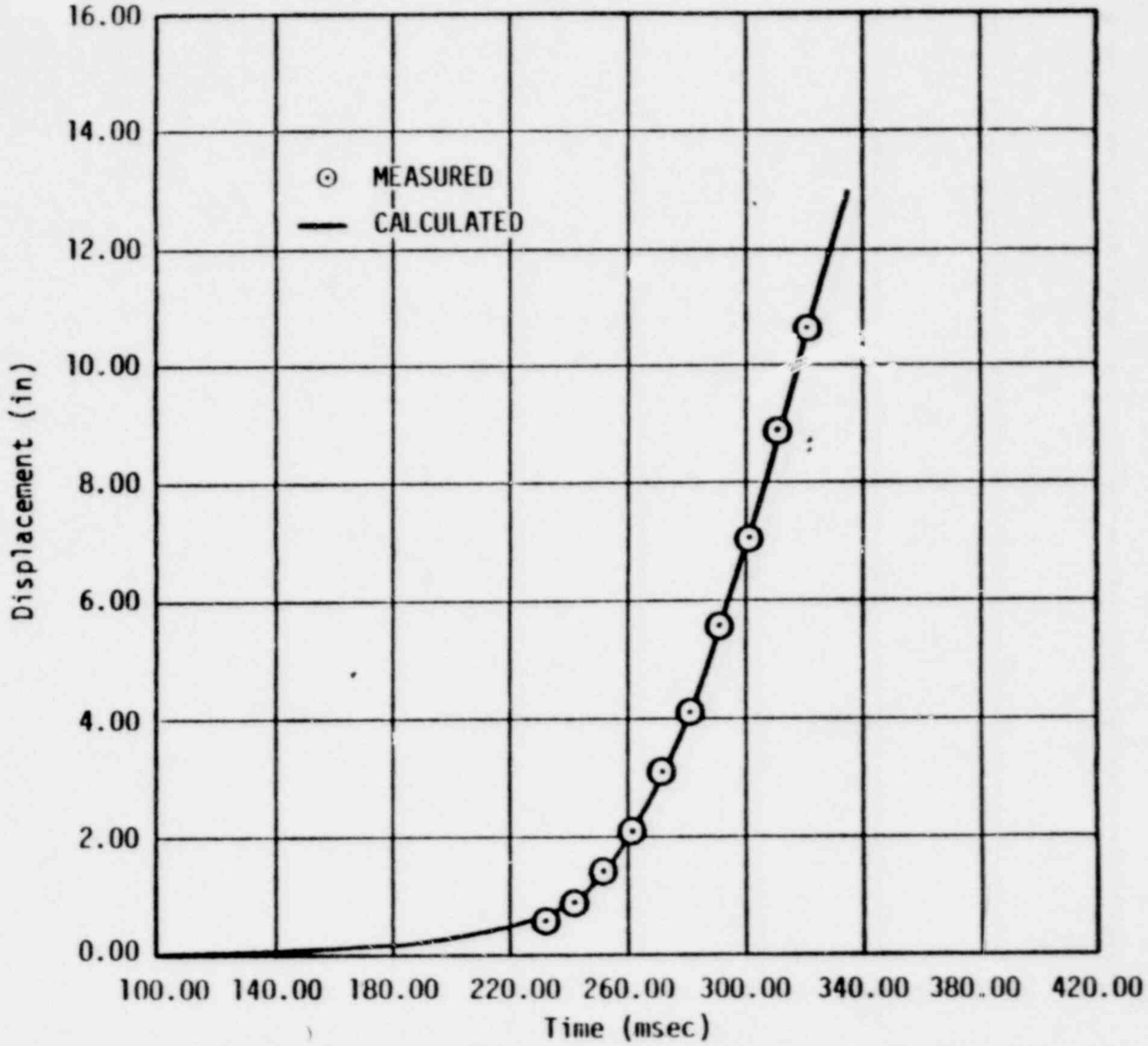


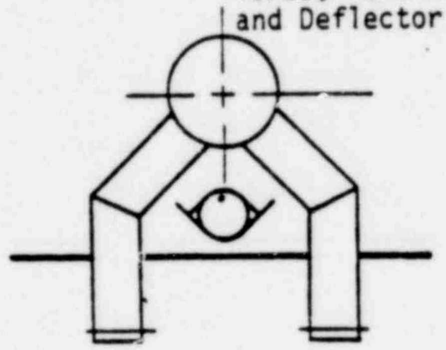
One-Dimensional Pool Swell Model

1499 327

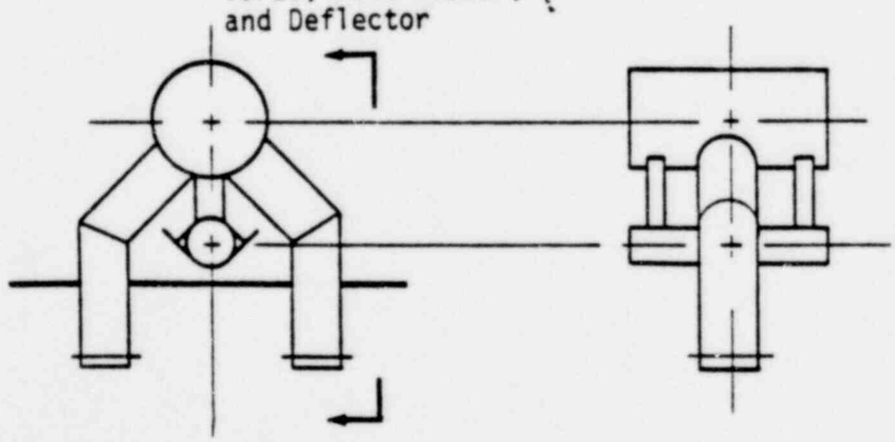


Calculated and Measured Pool Surface Displacement
for a Typical QSTF Test Run

1499 328



Torus Mounted Vent Deflector



Deflector Mounted to Vent Header

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1499 330

RANGE OF PARAMETERS INFLUENCING DEFLECTOR LOADS (FULL SCALE VALUES)

	DEFLECTOR LOADS MEASURED IN QSTF (6 PLANTS - 12 CONFIGURATIONS)	REMAINING PLANTS FOR WHICH DATA IS NOT AVAILABLE (7 PLANTS)
1) CLEARANCE (IN) (DISTANCE FROM BOTTOM OF DEFLECTOR TO WATER SURFACE)	0 - 21.05	0 - 14.29
2) DEFLECTOR WIDTH (IN)	25.3 - 30.0	20.0 - 26.0
3) $\dot{P}$ (PSI/SEC)	46.1 - 74.0	54.4 - 74.7
4) DOWNCOMER SUBMERGENCE (FT)	3.0 - 4.25	3.33- 4.4

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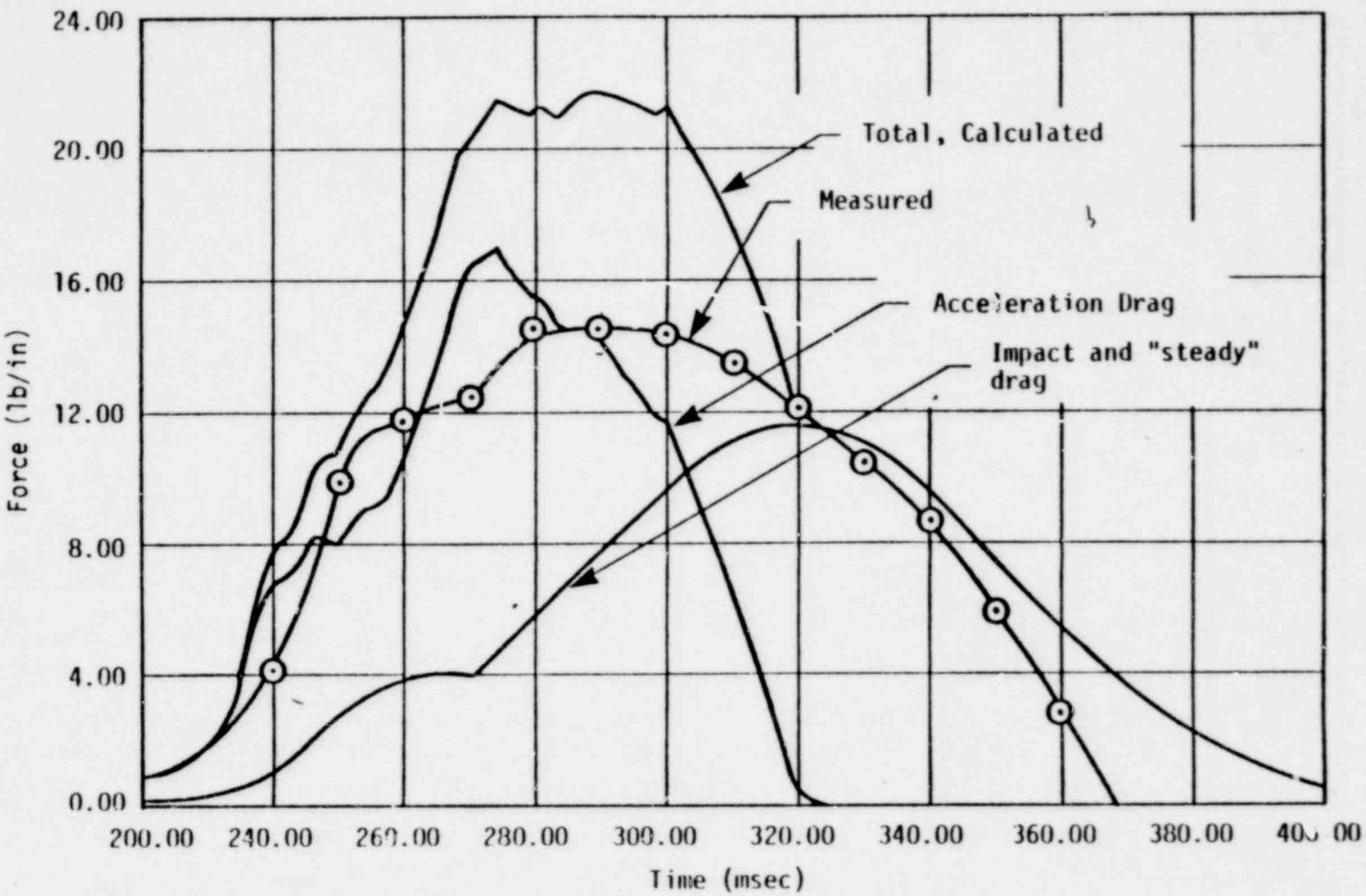
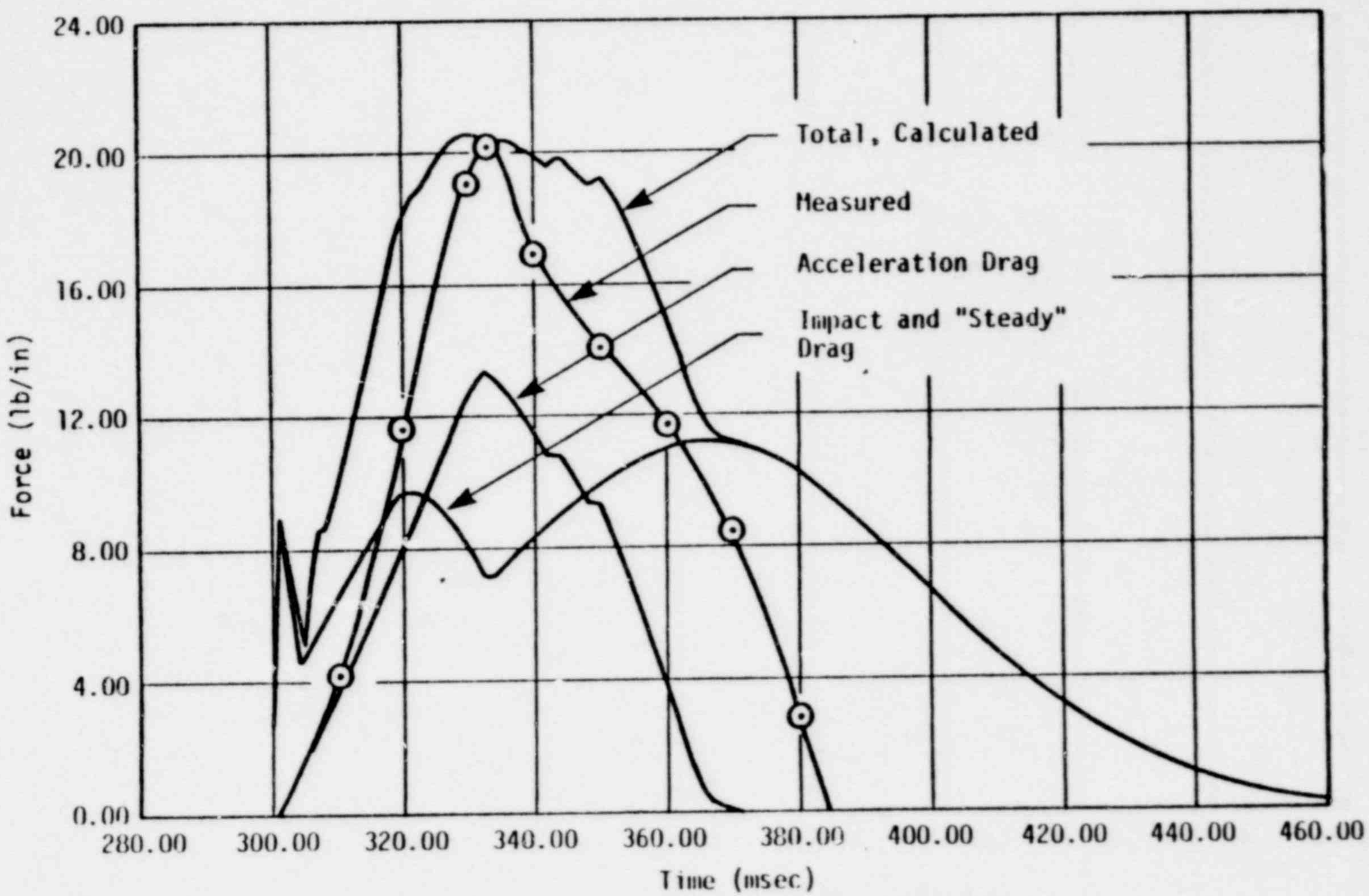


Measured and Calculated QSTF Vent Deflector Loads, Case 1

1499 331

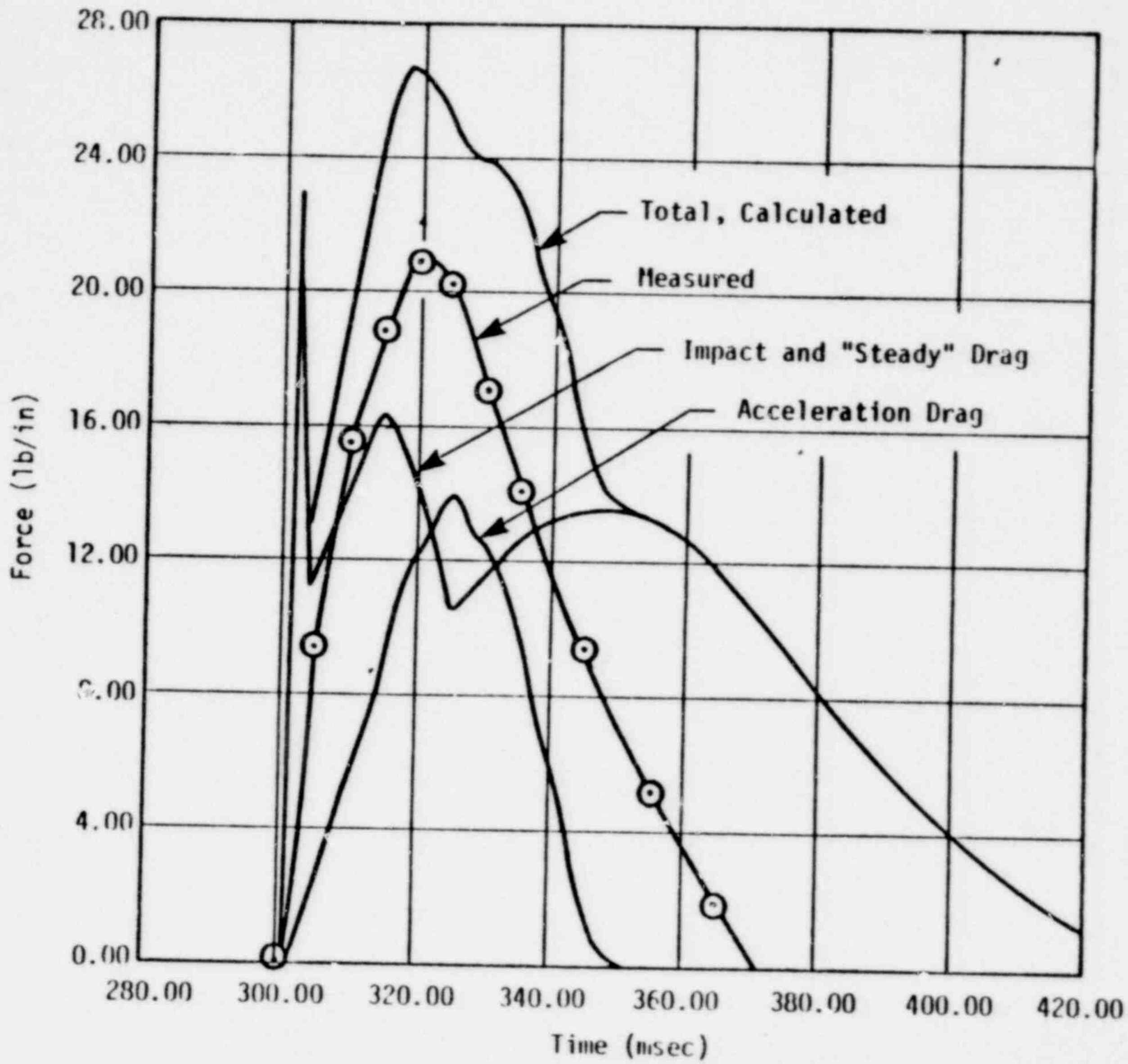
64



Measured and Calculated QSTF Vent Deflector Loads, Case 2

1499 037

65



Measured and Calculated QSTF Vent Deflector Loads, Case 3

1499 333

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COMPARISON OF CALCULATED AND MEASURED PEAK DEFLECTOR LOADS

PLANT	TEST	DEFLECTOR TYPE	CALCULATED / MEASURED	CLEARANCE/WATER SURFACE TO DEFLECTOR (INCHES)
A	5	PIPE W/Ts	1.50	0.0
	17A	PIPE W/Ts	1.00	1.635
	21	PIPE W/Ts	1.28	3.585
B	8	PIPE W/ANGLES	1.10	5.645
	12	PIPE W/ANGLES	1.08	5.645
C	8A	PIPE W/Ts	1.31	0.54
	10	PIPE W/Ts	1.09	0.54
	13	PIPE W/Ts	1.00	3.83
D	6B	PIPE W/ANGLES	1.93	0.575
E	10	PIPE W/ANGLES	1.50	1.13
	15	PIPE W/ANGLES	1.60	1.13
F	10	PIPE W/ANGLES	1.54	1.15
		AVE	1.33	

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SIGNIFICANCE OF NRC CRITERIA ON LOADS

A. LOADS BASED ON QSTF DATA

- INCLUSION OF ANALYTIC INITIAL IMPACT SPIKE - MINOR INCREASE IN STRUCTURE RESPONSE

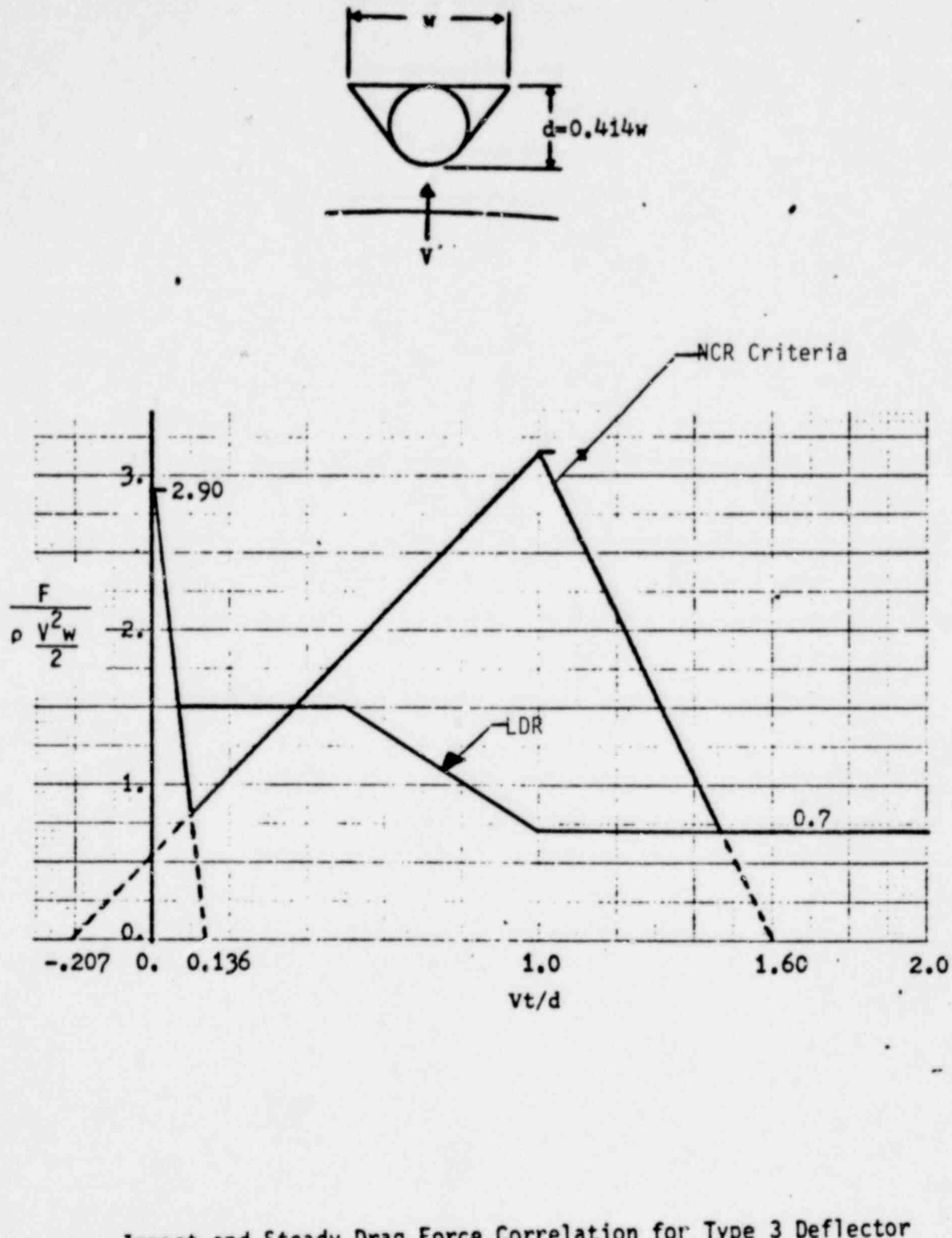
1499 335

B. LOADS BASED ON ANALYSIS

- NRC CRITERIA IN MOST CASES WILL CAUSE A LARGE INCREASE IN LOADS

EXAMPLE -

MEASURED PEAK LOAD (QSTF VALUES)	21 LB/IN
LDR PREDICTED PEAK LOAD	26 LB/IN
NRC CRITERIA	61 LB/IN



Impact and Steady Drag Force Correlation for Type 3 Deflector

69

MARK I VENT HEADER DEFLECTOR LOAD DEFINITION

(NRC)

for

A.C.R.S. Fluid Dynamics Subcommittee
San Francisco, 16 November 1979.

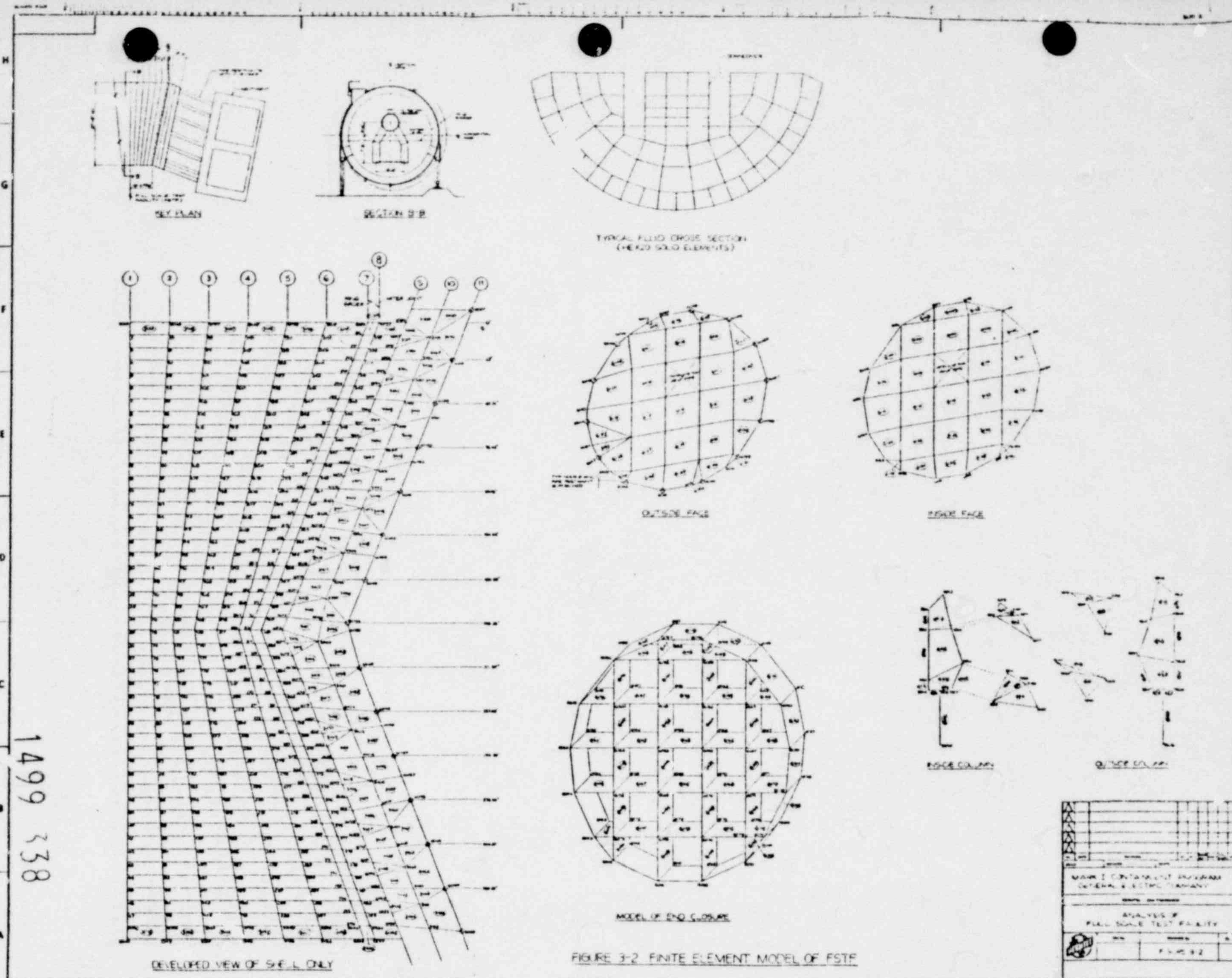


FIGURE 3-2 FINITE ELEMENT MODEL OF FSTF

1499 338

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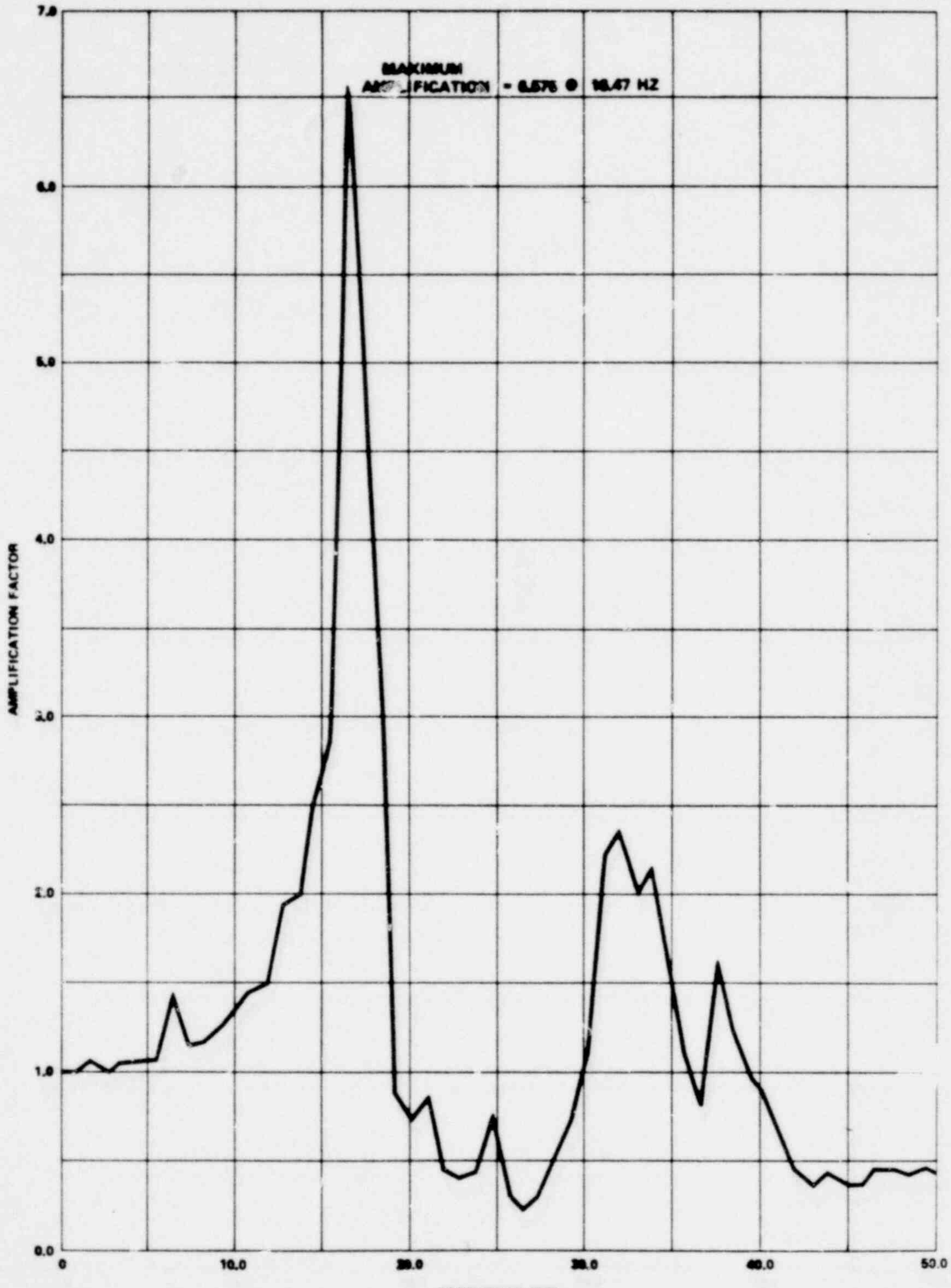
VERIFICATION OF ANALYTICAL MODEL

- STATIC CHECK CASES

- COMPARISON AGAINST SHAKE TEST RESULTS

- ABILITY TO PREDICT FSTF STRUCTURAL RESPONSE TO CONDENSATION OSCILLATION LOADING
  - Data From Test M-8, Period From 24 To 25 Seconds, Used For Verification
  - Convert Measured Flexible Wall Pressures To Rigid Wall Pressures Using FSI Correction Curve
  - Dynamic Structural Analysis Based On Rigid Wall Loading
  - Compare Predicted Structural Response Quantities With Measured Data



RESULTS OF ANALYSIS

AMPLIFICATION FACTOR FOR TOTAL VERTICAL FORCE VS FREQUENCY

("RIGID" FORCE = "FLEXIBLE" FORCE / AMPLIFICATION FACTOR)

1499 340

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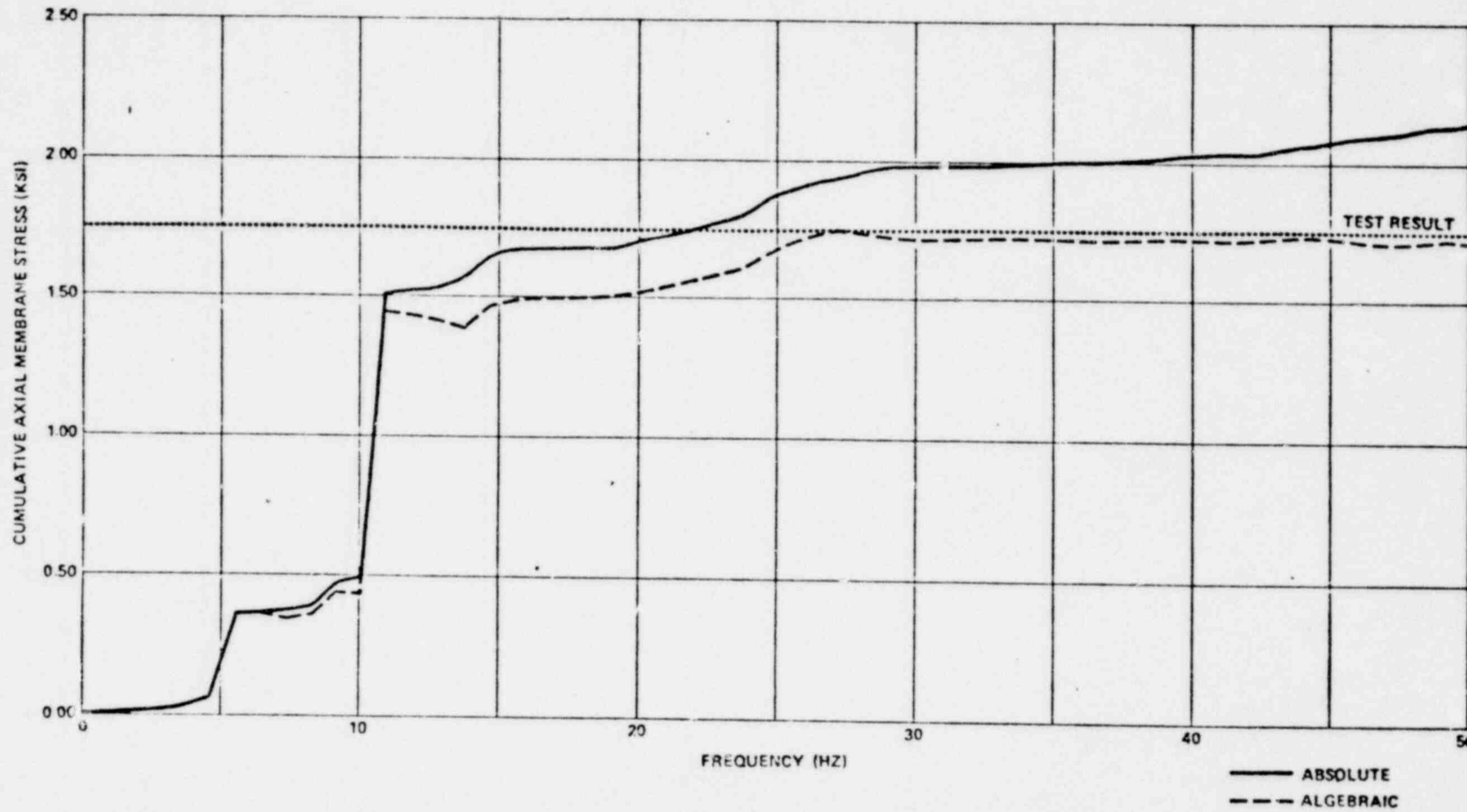
RESULTS OF VERIFICATION RUNS

CUMULATIVE AXIAL MEMBRANE STRESS AT BOTTOM MID-SPAN (SOURCE ANALYSIS)



1499 341

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CONDENSATION OSCILLATION LOADING

FSTF RESPONSE

QUANTITY		TEST DATA (1)	FSTF ANALYSIS (2)		LDR LOADS (3)			MARGIN
			ALGEBRAIC	ABSOLUTE	CASE 1	CASE 2	CASE 3	LDR CASE 2/TEST DATA
BOTTOM DEAD CENTER	AXIAL MEMBRANE (KSI)	1.94	1.80	2.22	3.55	4.57	2.20	2.36
BOTTOM DEAD CENTER	HOOP MEMBRANE (KSI)	2.06	1.80	2.35	3.90	4.60	2.45	2.23
BOTTOM DEAD CENTER	RADIAL DEFLECTION (INCHES)	0.086	0.101	0.129	.230	.275	.141	3.20
INNER COLUMN AXIAL FORCE (KIPS)		93.3	101	136	254	290	172	3.11
OUTER COLUMN AXIAL FORCE (KIPS)		111.5	116	152	278	320	186	2.87

NOTES:
(1) DATA FOR TEST M-8 TIME PERIOD 24.8 TO 25.9 SECONDS
(2) LOAD APPLIED AT MULTIPLES OF .91 HZ. FREQUENCIES 0-30 HZ CONSIDERED.
(3) LOAD APPLIED AT STRUCTURE NATURAL FREQUENCIES
FREQUENCIES 0-30 HZ CONSIDERED. ABSOLUTE SUM.

1499 342

SON/N

MARK I VENT HEADER DEFLECTOR LOAD DEFINITION

(NRC)

for

A.C.R.S. Fluid Dynamics Subcommittee

San Francisco, 16 November 1979.

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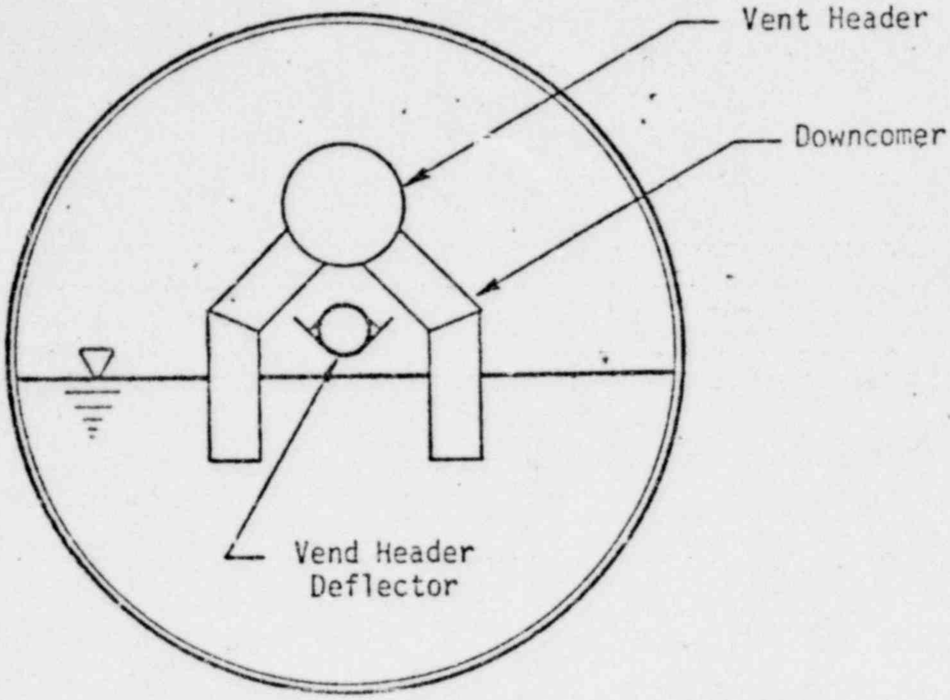


Figure 1-1. Typical Vent Header Deflector

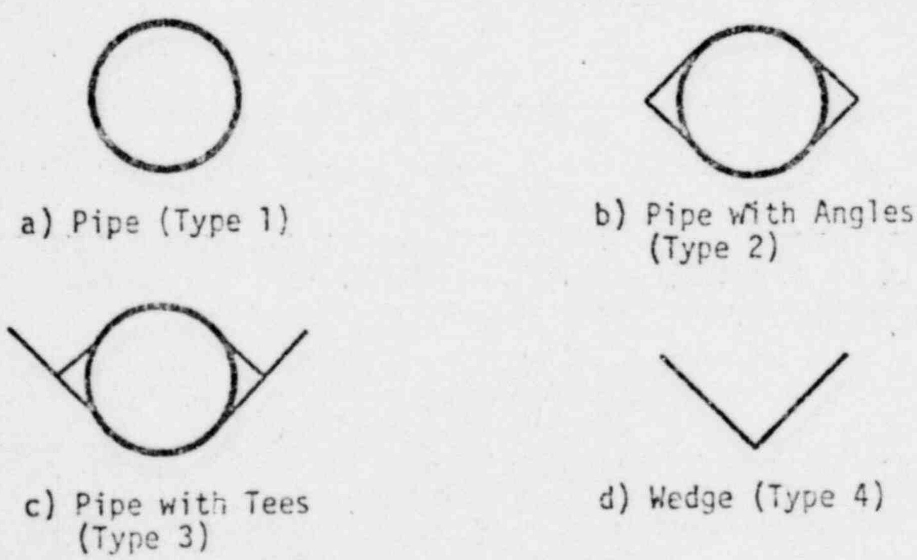


Figure 1-2. Deflector Configuration and Types

ALTERNATIVE A

OWNERS:

LOAD OBTAINED FROM PLANT-SPECIFIC QSTF TESTS, SIMULATING BOTH POOL SWELL & DEFLECTOR IMPACT DIRECTLY

+ ADJUSTMENTS FOR 3D POOL SWELL EFFECTS.

NRC : OK IF

1. ADD EMPIRICAL IMPACT SPIKE FOR CYLINDERS (FROM SRI/EPRI DATA)

2. INTERPRET 3D POOL SWELL EFFECTS CONSERVATIVELY, AS REQUIRED BY NRC.

3. WHEN LOAD IS APPLIED IN A DYNAMIC CALCULATION FOR A MARK I DEFLECTOR, THE INERTIA DUE TO THE ADDED MASS OF THE WATER SHOULD BE ACCOUNTED FOR ($M_H = I/Vw$)

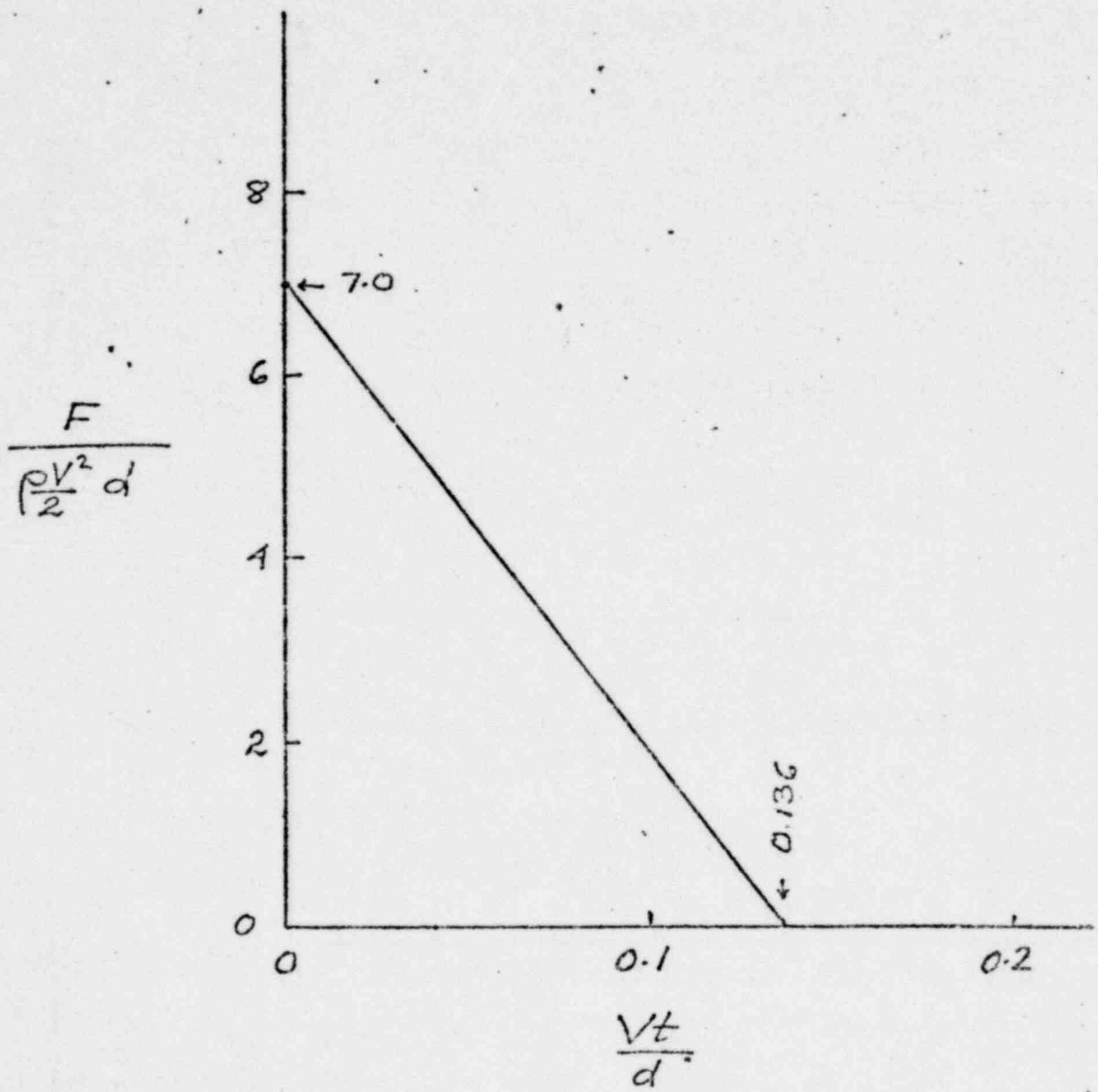


Figure 1 : Impact force transient for addition to the empirical data for Types 1 to 3 deflectors.

ALTERNATIVE B.

OWNERS:

(1) POSTULATE THAT

$$F(t) = F_1[t, V(t)] + F_2[a(t)]$$

impact transient & steady drag — acceleration drag

(2) DEDUCE

(a) $F_1(t, V)$ from available data on constant-V impact on flat pool

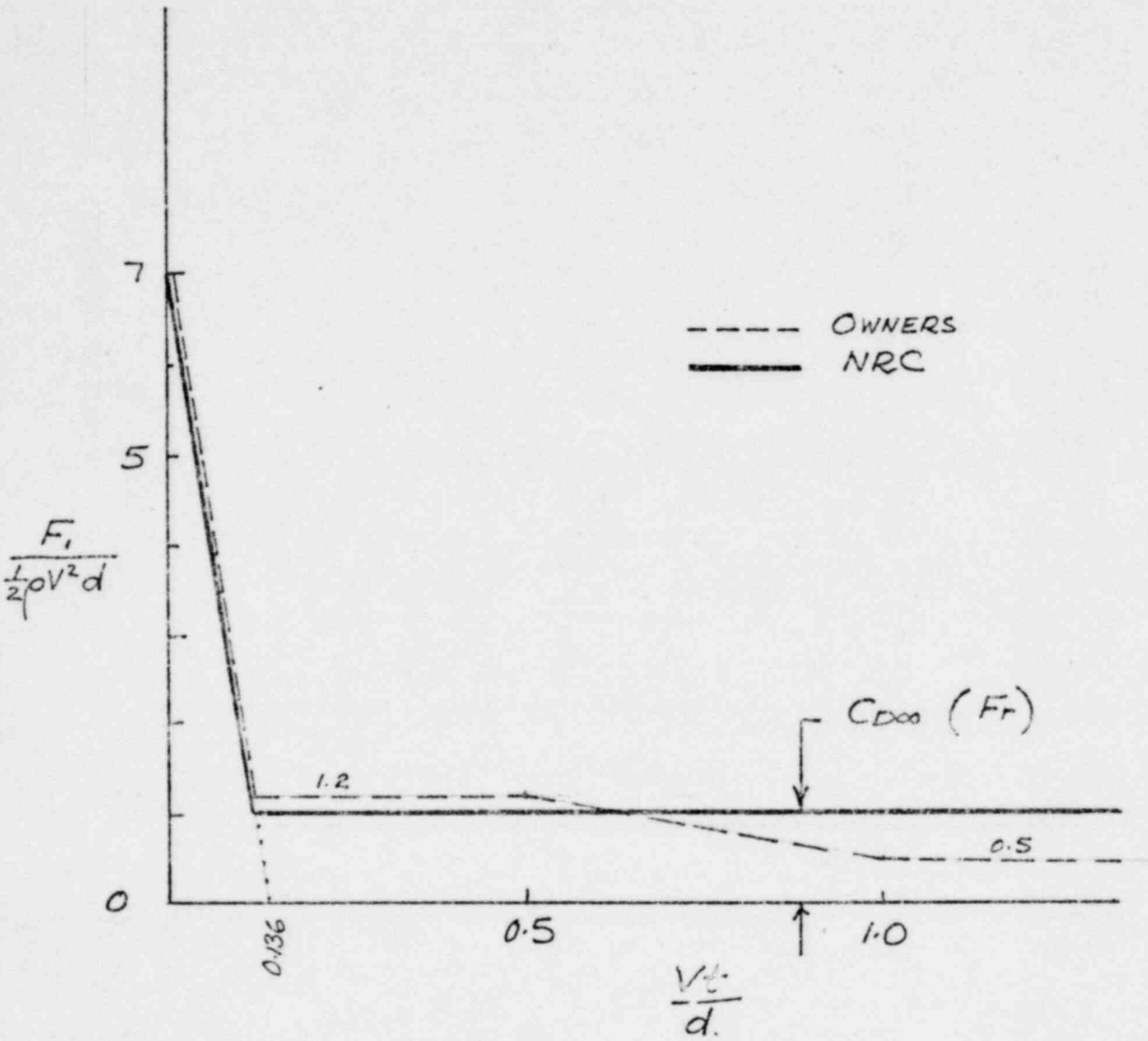
(b) $F_2(a)$ from available correlations for uniform incident flow

(3) OBTAIN $V(t)$ and $a(t)$ from plant-specific QSTF tests without deflectors.

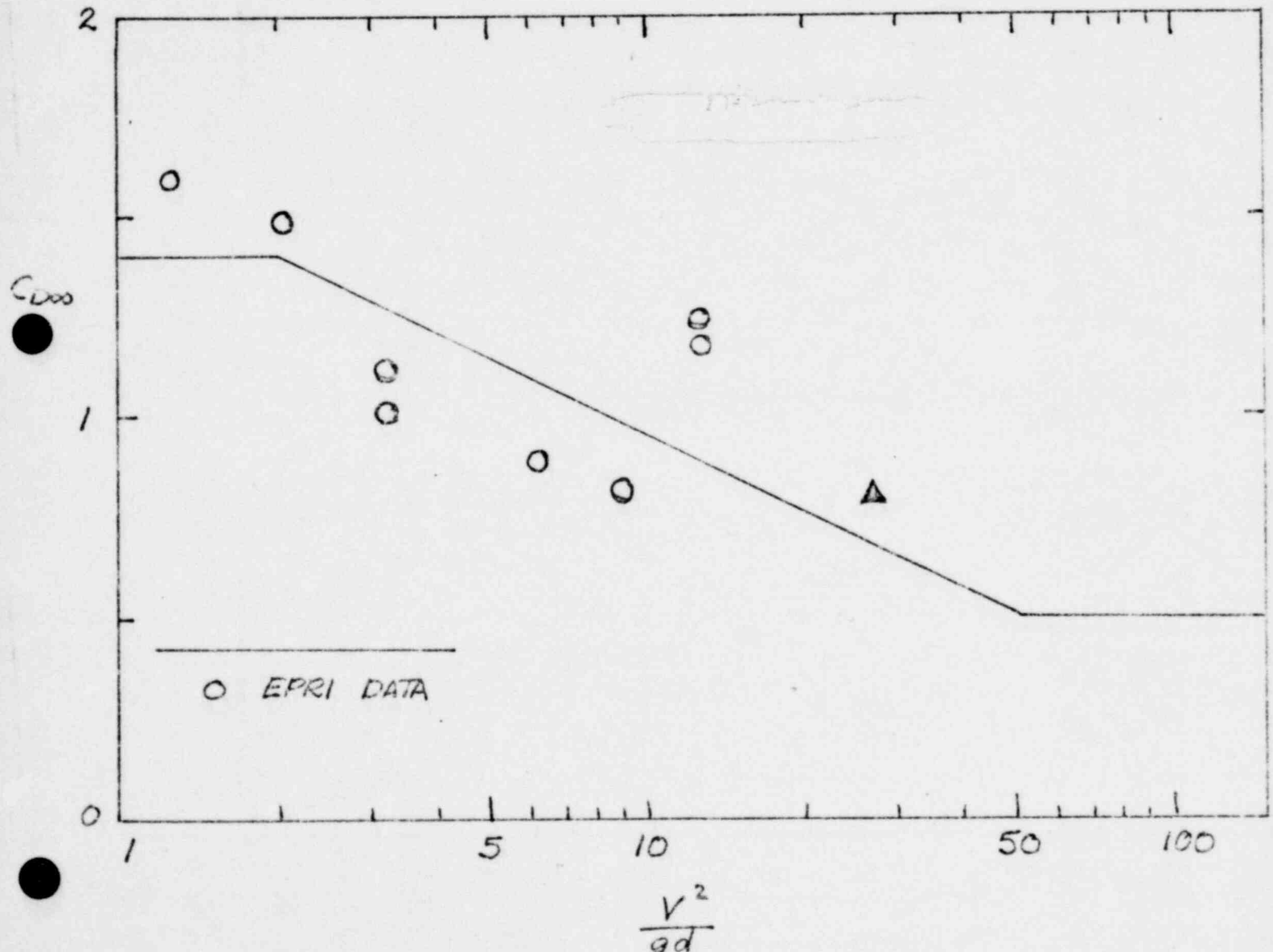
NRC:

Finds the general methodology acceptable, but differs from the owners in its application of step (2a).: the owners' have a nonconservative steady drag for cylinders, and have not accounted properly for the impact transient on wedges.

In addition, NRC requires that when the load is applied, the added mass of the water must be accounted for.

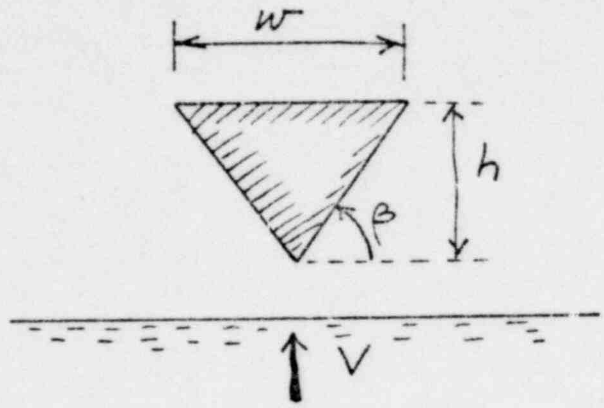


IMPACT TRANSIENT / STEADY DRAG FOR CYLINDER



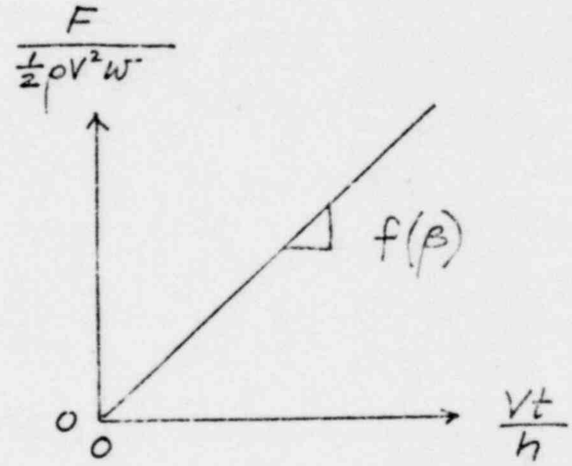
"Steady" drag (with ventilated wake) from EPRI data.

IMPACT TRANSIENT FOR WEDGE



On dimensional grounds,

$$\frac{F(t)}{\frac{1}{2}\rho V^2 w} = f(\beta)\cdot\frac{Vt}{h}$$



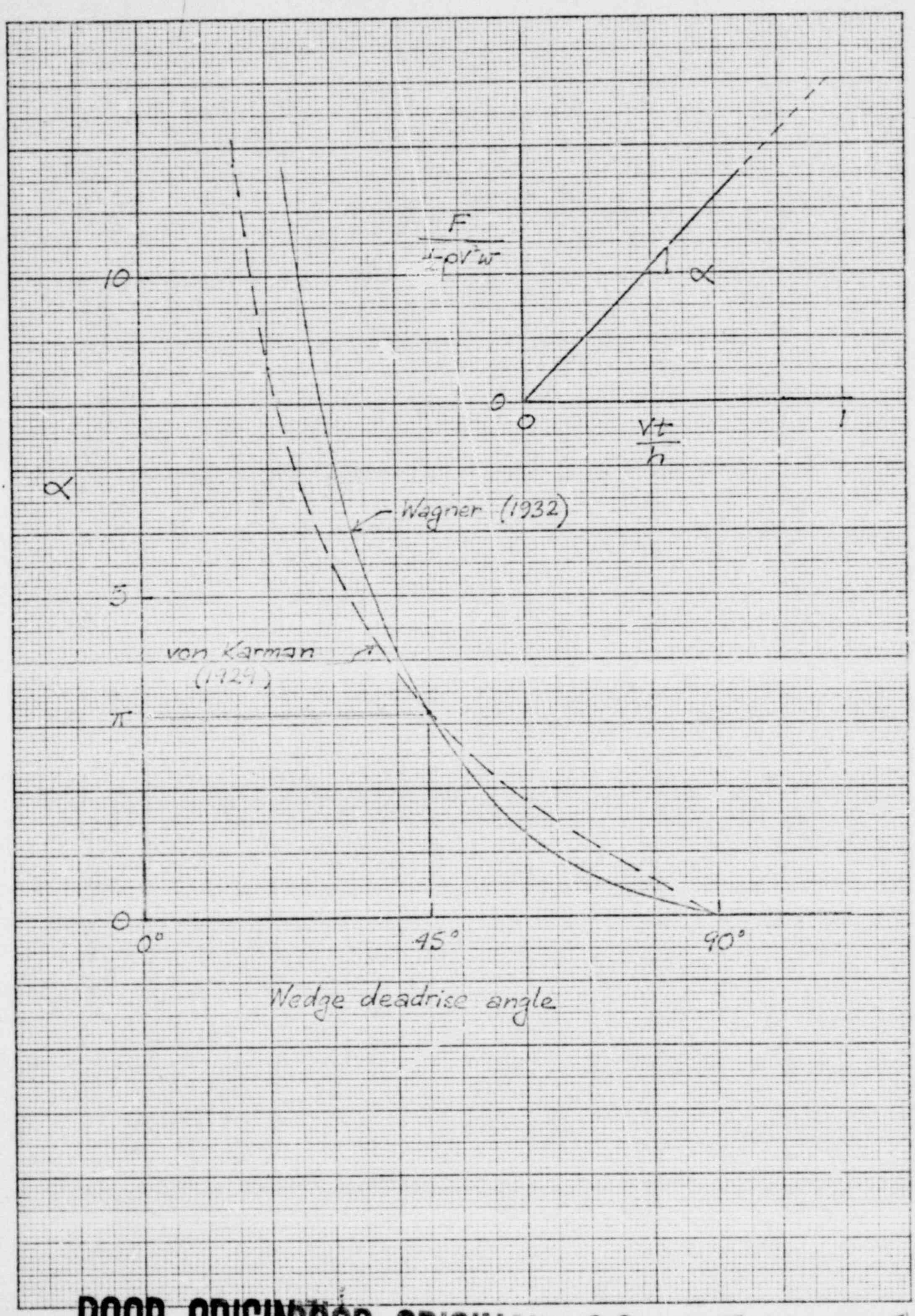
Theory.

Von Karman (1929)

$$f(\beta) = \pi \cot\beta$$

Wagner (1932)

$$f(\beta) = \pi\left(\frac{\pi}{2\beta} - 1\right)^2 \tan\beta$$



F / ½ρV²w
10
α
0
0
Vt / h
1
α
Wagner (1932)
5
von Karman (1929)
π
0
0°
45°
90°
Wedge deadrise angle

 1499 351

Mayo (1945). : analysis for planing impact.
0.82 × Wagner ≃ expt.

Monaghan (1949): analysis of planing impact.
agrees with experiment ($\beta \leq 40°$)

Pierson (1950) : analysis for vertical impact;
bounded for all β by
1.08 × Wagner.

Chuang (1966, 1970): experiments for max.
pressure. ~ Wagner.

etc.

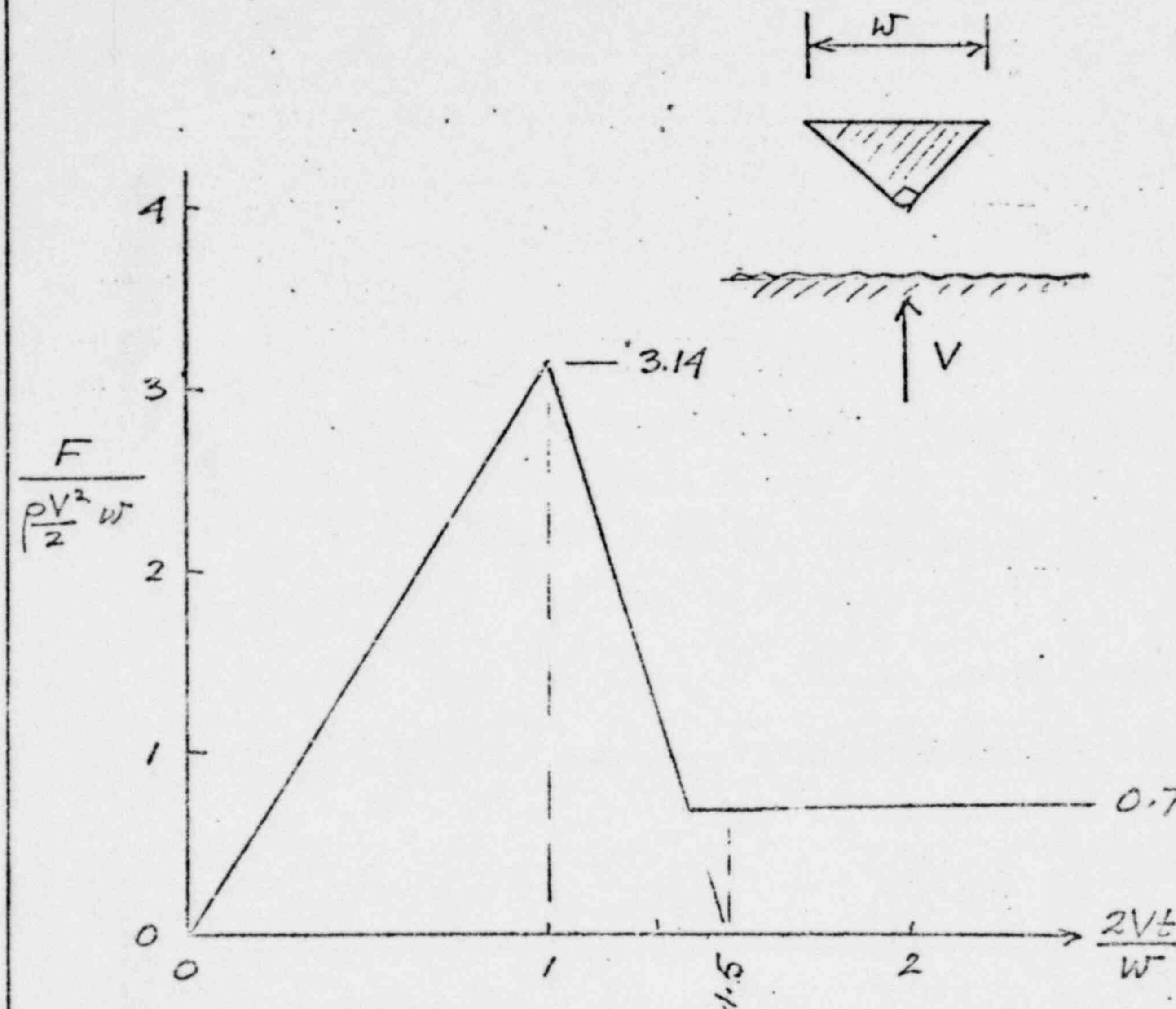


Figure 5: Impact / steady drag force correlation for type 4 deflector.

1499 353

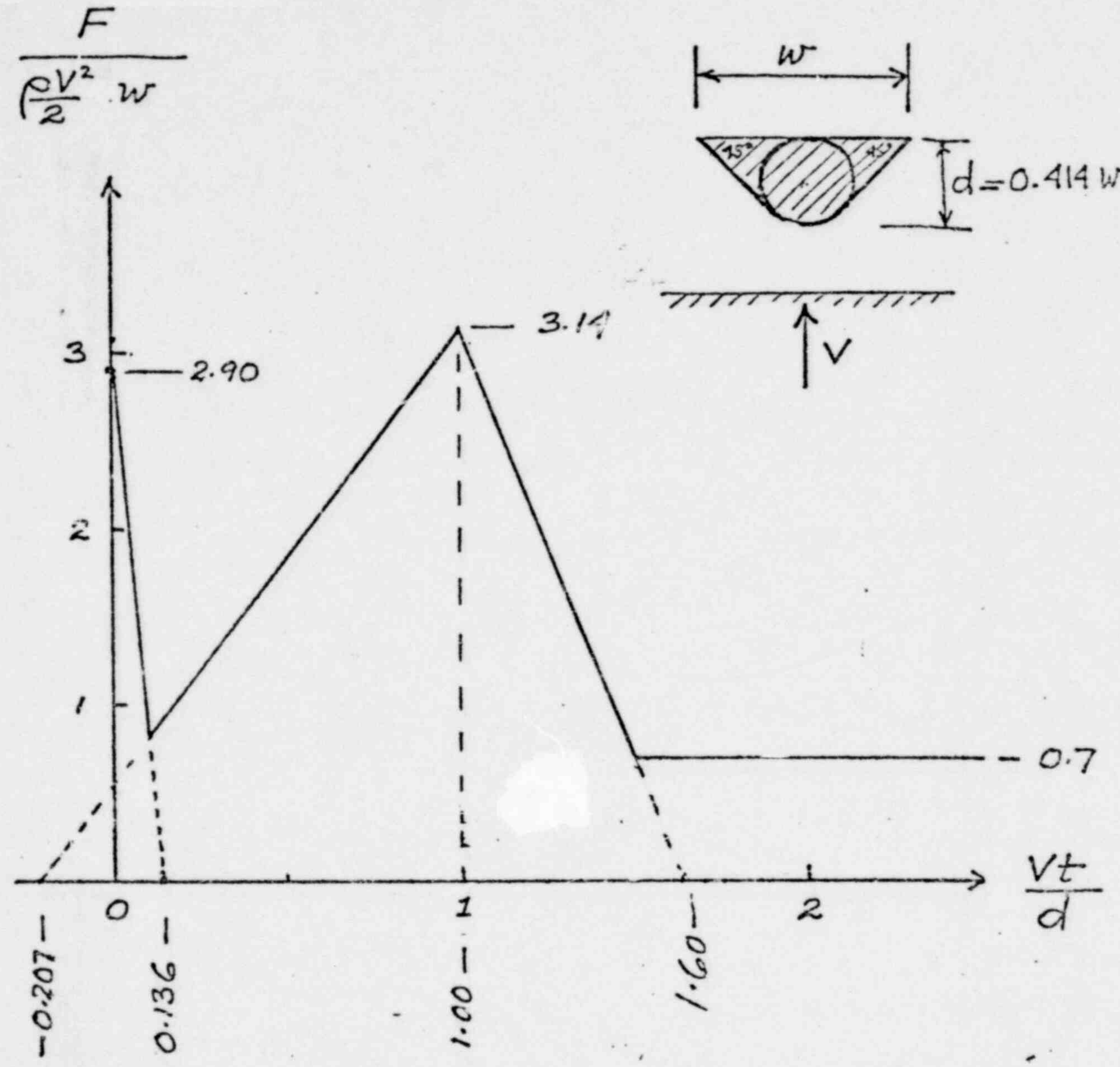


Figure 4: Impact/steady drag force correlation for Type 3 deflector

1499 354

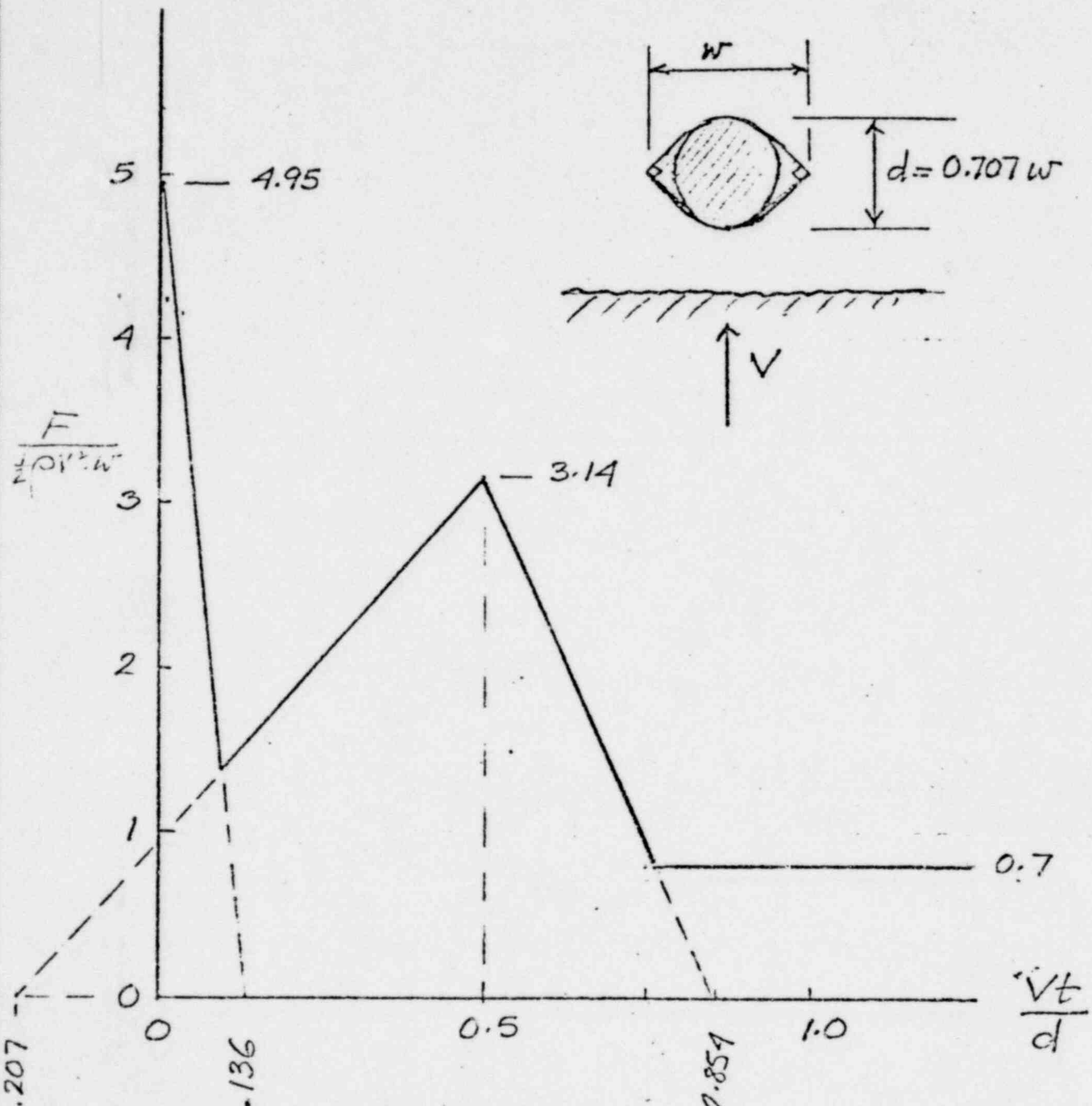
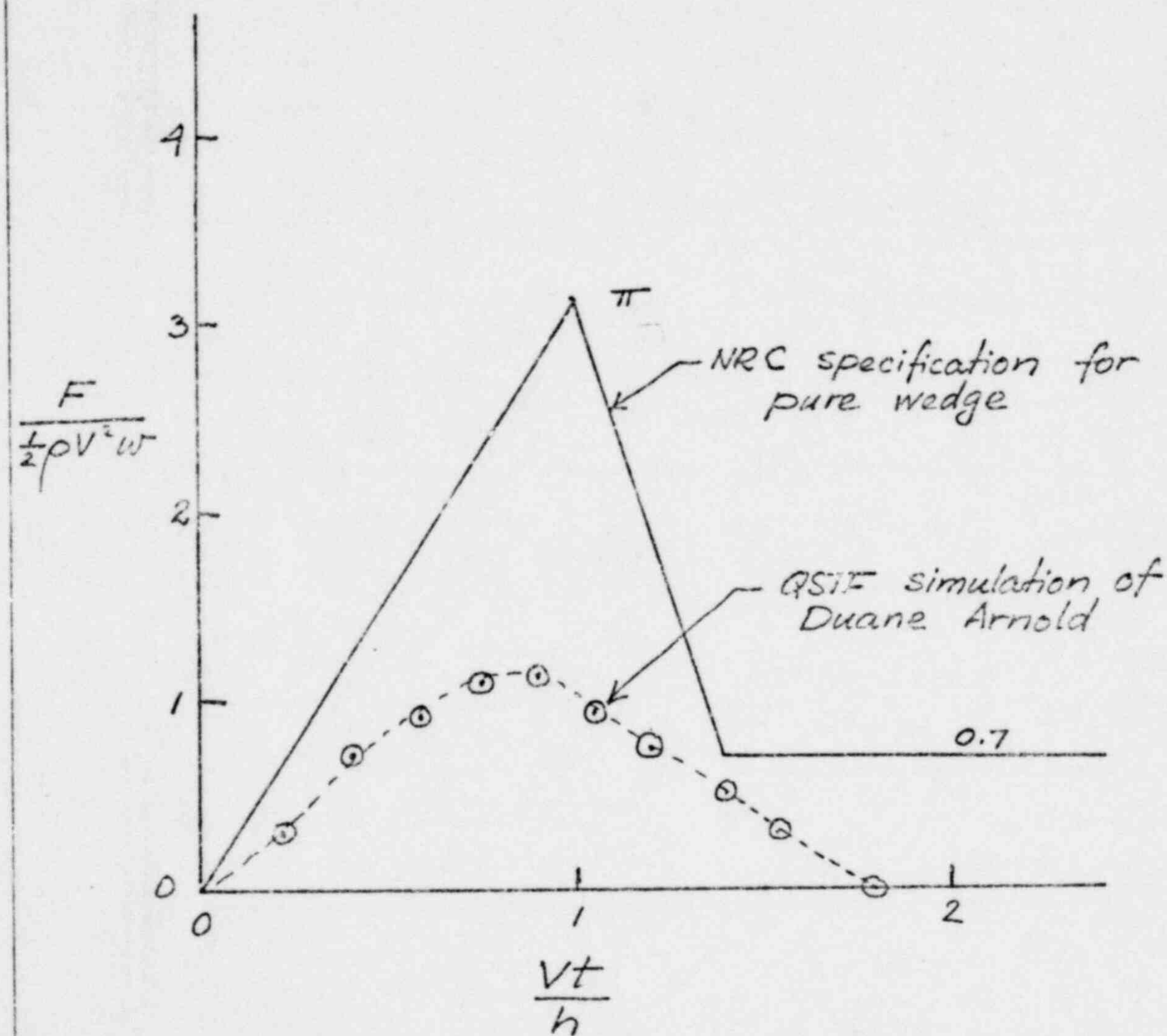


Figure 3: Impact/steady drag force correlation for Type 2 deflector.

1499 355



Impact transient for 45° half-angle wedge in uniform flow, compared with data from Mark I QSTF.

1499 356

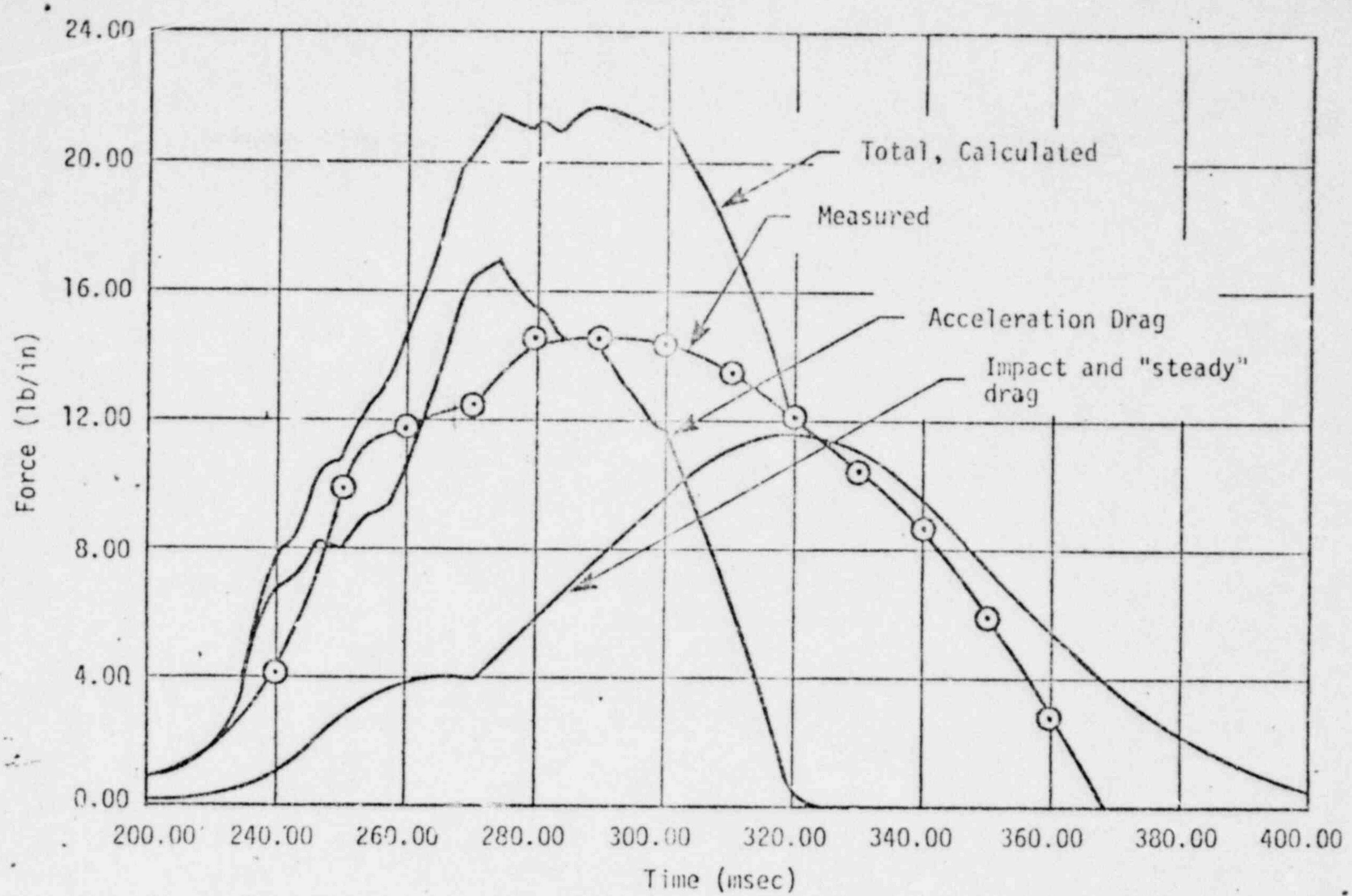


Figure 4-3. Measured and Calculated QSTF Vent Deflector Loads, Case 1

1499 357

84

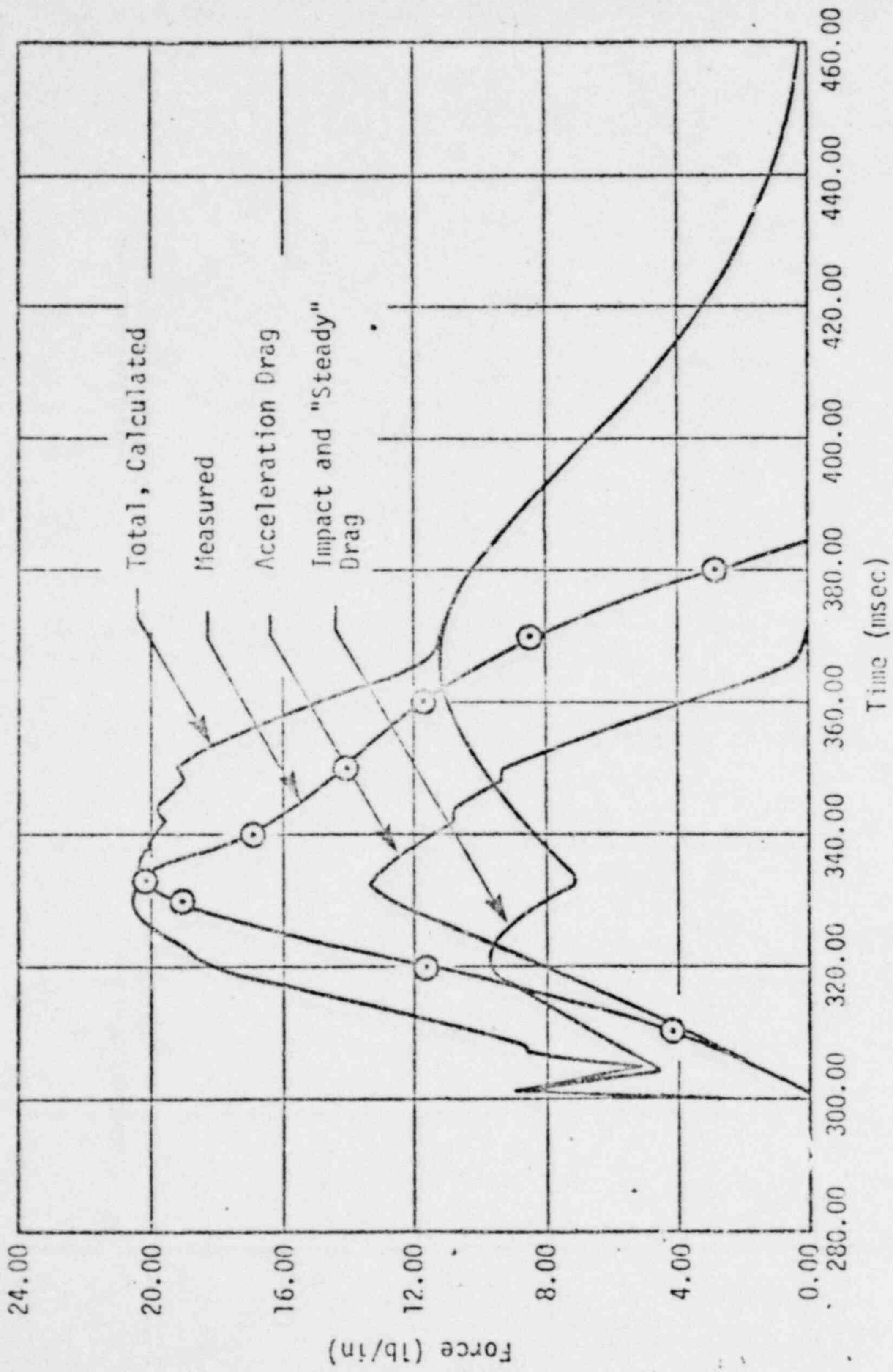


Figure 4-4. Measured and Calculated QSTF Vent Deflector Loads, Case 2

1499 358

85

Brafman

ANALYSIS OF FULL SCALE TEST FACILITY
FOR
CONDENSATION OSCILLATION LOADING

BACKGROUND

- Structural Analysis Of Typical 56 psi Torus For Condensation Oscillation Completed July 1977
- FSTF Testing For Condensation Oscillation Completed August 1978
- Extrapolation of Analysis Results, Based On Wall Pressures Measured In Testing, Yields Predicted Structural Response Inconsistent With Test
- Investigation Suggests Fluid - Structure Interaction (FSI) Affects Measured Wall Pressures
- In September 1978, Start Structural Analysis Of FSTF Considering Fluid-Structure Interaction

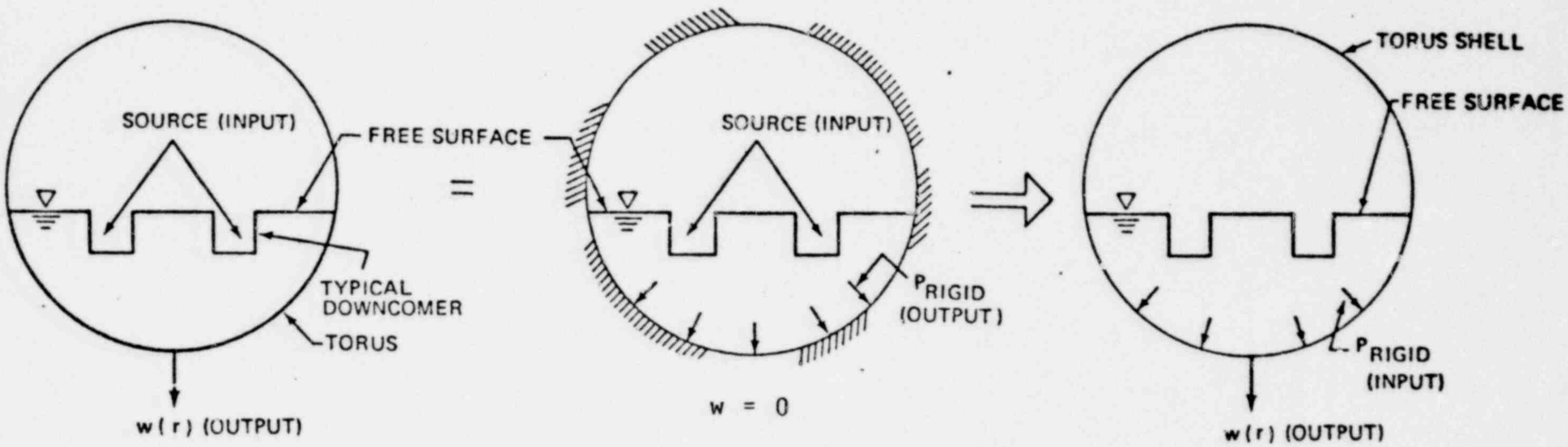
Objectives Are:

- Extract "Rigid Wall" Pressures From Test Data
- Develop Analytical Technique Which Will Predict Test Results For Structural Response
- Assess Structural Response Based On LDR Load Definition

1499 359

86

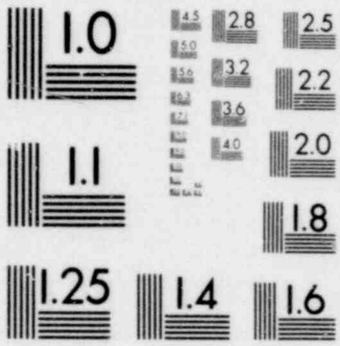
$$\{P_T\} = \{P_S\} + \{M_W\}\{\ddot{X}\}$$



{ FLEXIBLE TORUS — FORCING SOURCE APPLIED AT THE DOWNCOMER } = { RIGID TORUS — FORCING SOURCE APPLIED AT THE DOWNCOMER } ⇒ { FLEXIBLE TORUS — RIGID WALL PRESSURE INPUT AT THE WETTED SURFACE OF THE SHELL }

1499 360

IMAGE EVALUATION TEST TARGET (MT-3)



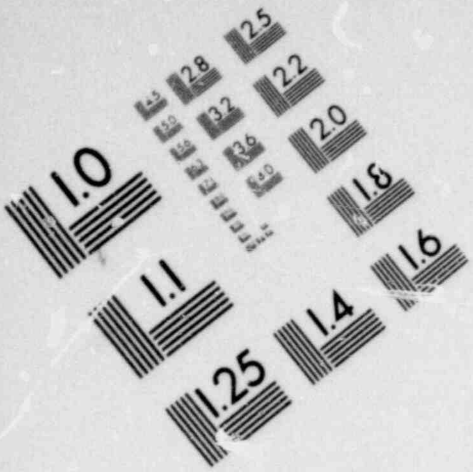
6″

IMAGE EVALUATION TEST TARGET (MT-3)

6″

IMAGE EVALUATION
TEST TARGET (MT-3)

6″



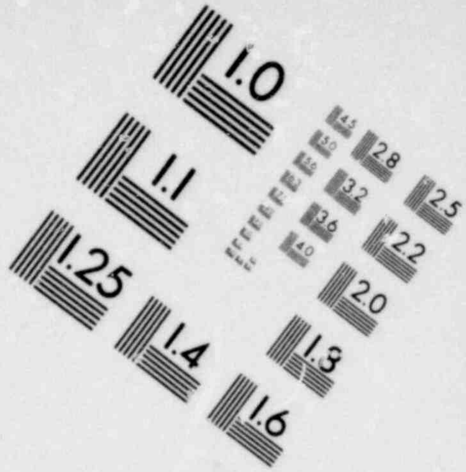
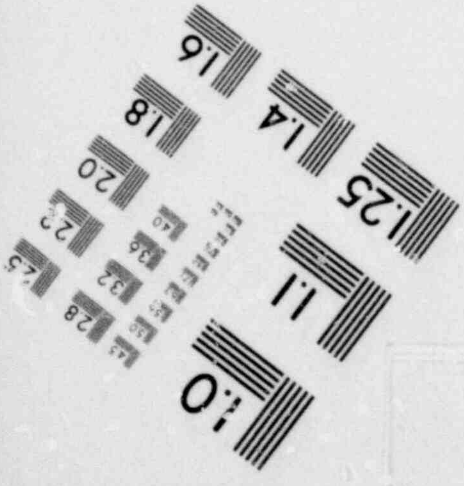
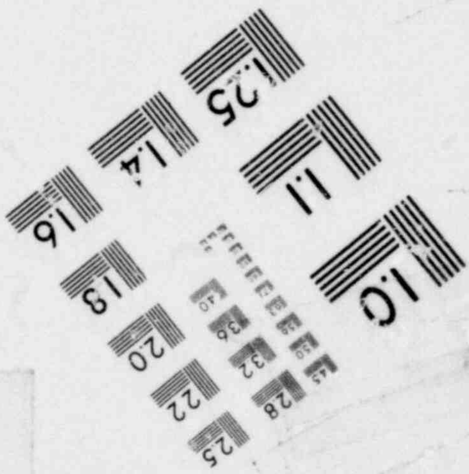


IMAGE EVALUATION TEST TARGET (MT-3)





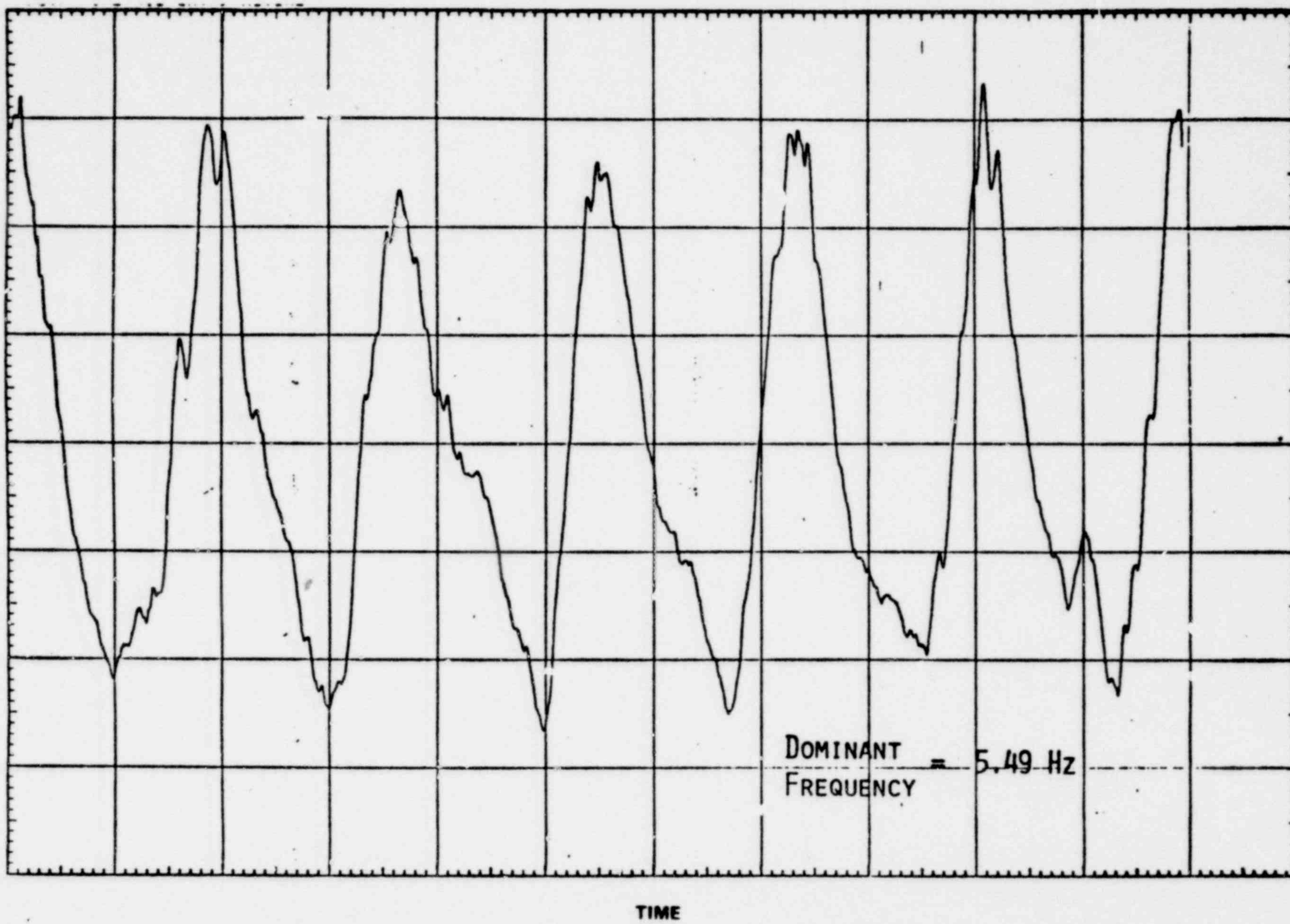
TORUS ANALYSIS FOR CONDENSATION OSCILLATION LOADING

- OVERALL PROCEDURE
  - Basis For Load Definition Is Data Measured In FSTF
  - Periodic Loading. Fourier Expansion Of Loading And Frequency By Frequency Solution
  - Correct Measured Pressures For FSI Effects. Develop Rigid Wall Load Definition
  - Apply Rigid Wall Loading In Plant Unique Analysis. Incorporate Plant Unique FSI In Solution

- DEVELOPMENT OF FSI CORRECTION CURVE
  - NASTRAN Model Of FSTF And Contained Fluid
  - Analyses For Unit Harmonic Sources At Downcomers. Repeat Analysis With Source Frequency Varied In (Approx 1 Hz) Increments Over Range Of Interest
  - Two Series Of Analyses. First Is For Fluid And Actual (Flexible) Structure, And Second Is For Fluid With Rigid Boundary
  - Output Is Wall Pressures. Integrate Wall Pressures To Get Net Vertical Load
  - FSI Correction Curve Is Ratio Of Flexible To Rigid Net Vertical Load, As A Function Of Frequency

1500 001

88



TOTAL VERTICAL FORCE (INTEGRATED PRESSURE)

1500 002

89

FSTF ANALYTICAL MODEL

(DEVELOPED USING NASTRAN COMPUTER PROGRAM)

- STRUCTURAL MODEL
  - One Half Of FSTF (Symmetry Segment)
  - Approx 500 Elements, 500 Nodes
  - Shell Modeled Using Quadrilateral Shell Elements
    Stiffeners And Columns Modeled With Beam Elements

- FLUID MODEL
  - Consistant Mass Matrix Method
  - Fluid Modeled Using Hexagonal Solid Elements
  - Fluid Assumed Incompressible

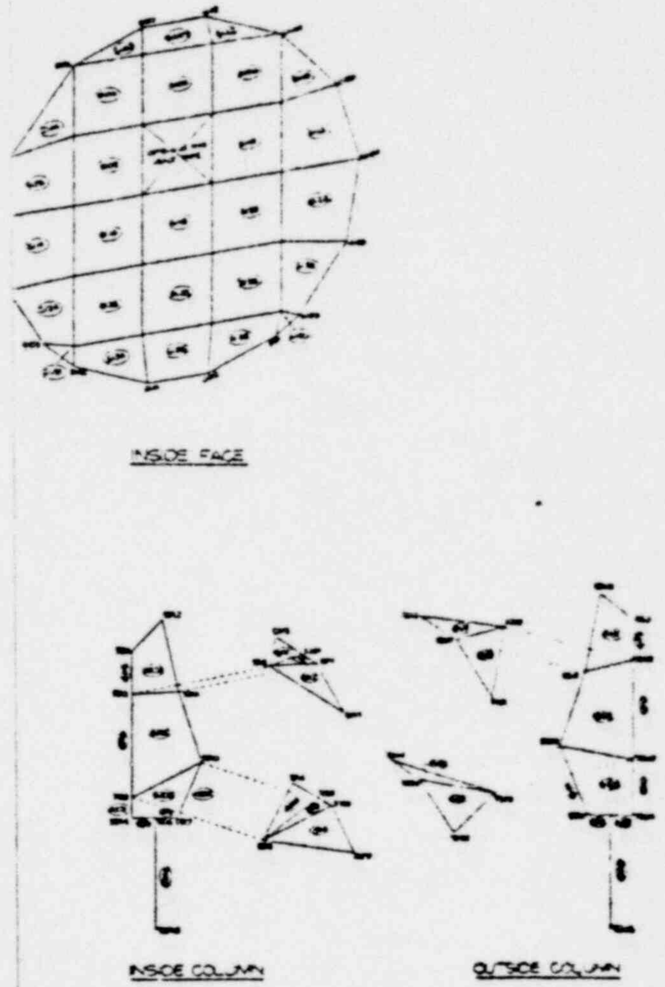
- LOAD APPLICATION
  - Source Forcing Function At Downcomers

    Or

  - Wall Pressure Forcing Function

1500 003

70



MARK I CONTAINMENT PROGRAM
GENERAL ELECTRIC COMPANY

ANALYSIS OF
FULL SCALE TEST FACILITY

FIGURE 3-2

1500 004

71

VERIFICATION OF ANALYTICAL MODEL

- STATIC CHECK CASES

- COMPARISON AGAINST SHAKE TEST RESULTS

- ABILITY TO PREDICT FSTF STRUCTURAL RESPONSE TO CONDENSATION OSCILLATION LOADING

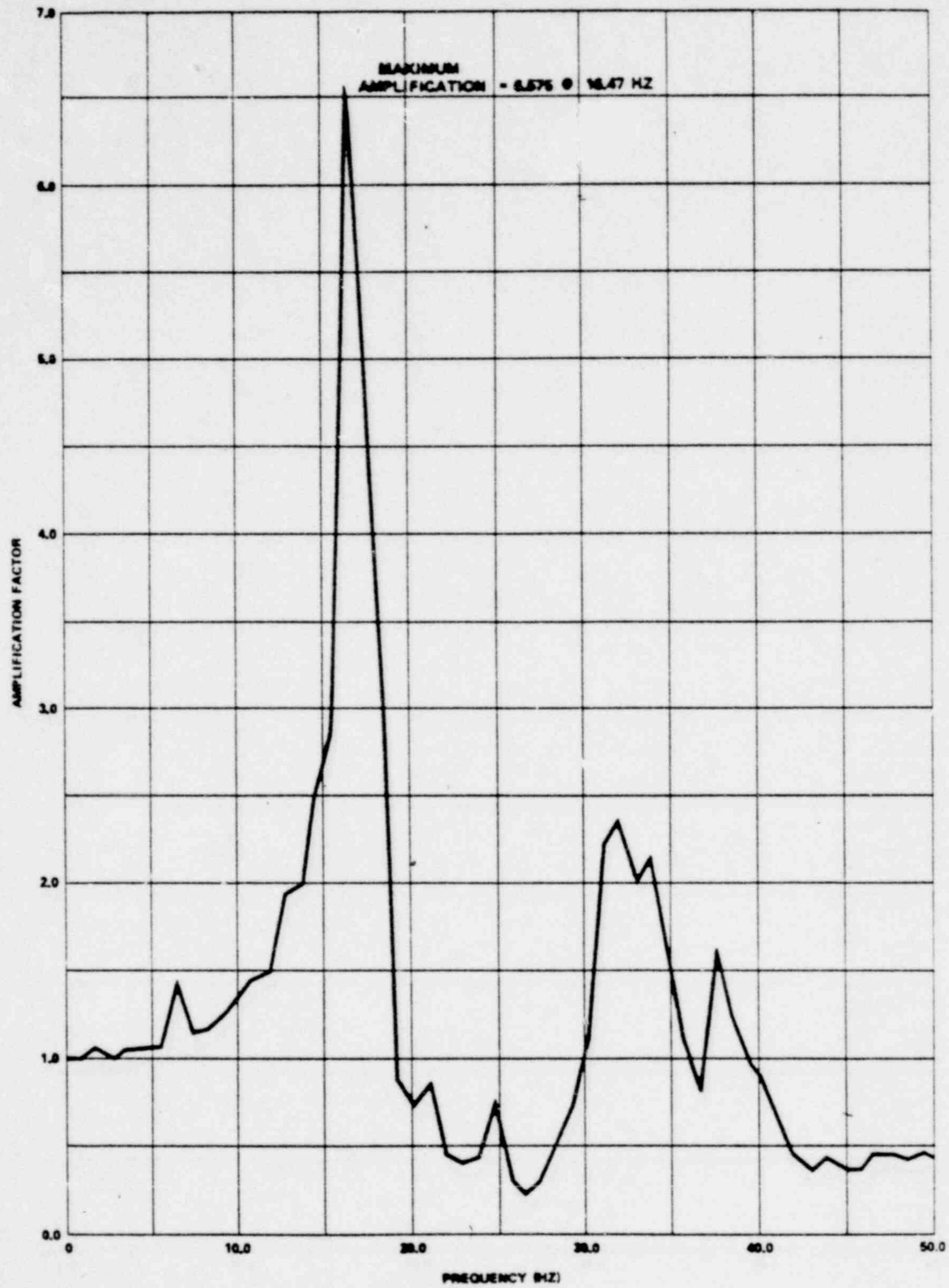
  - Data From Test M-8, Period From 24 To 25 Seconds, Used For Verification

  - Convert Measured Flexible Wall Pressures To Rigid Wall Pressures Using FSI Correction Curve

  - Dynamic Structural Analysis Based On Rigid Wall Loading

  - Compare Predicted Structural Response Quantities With Measured Data

1500 005



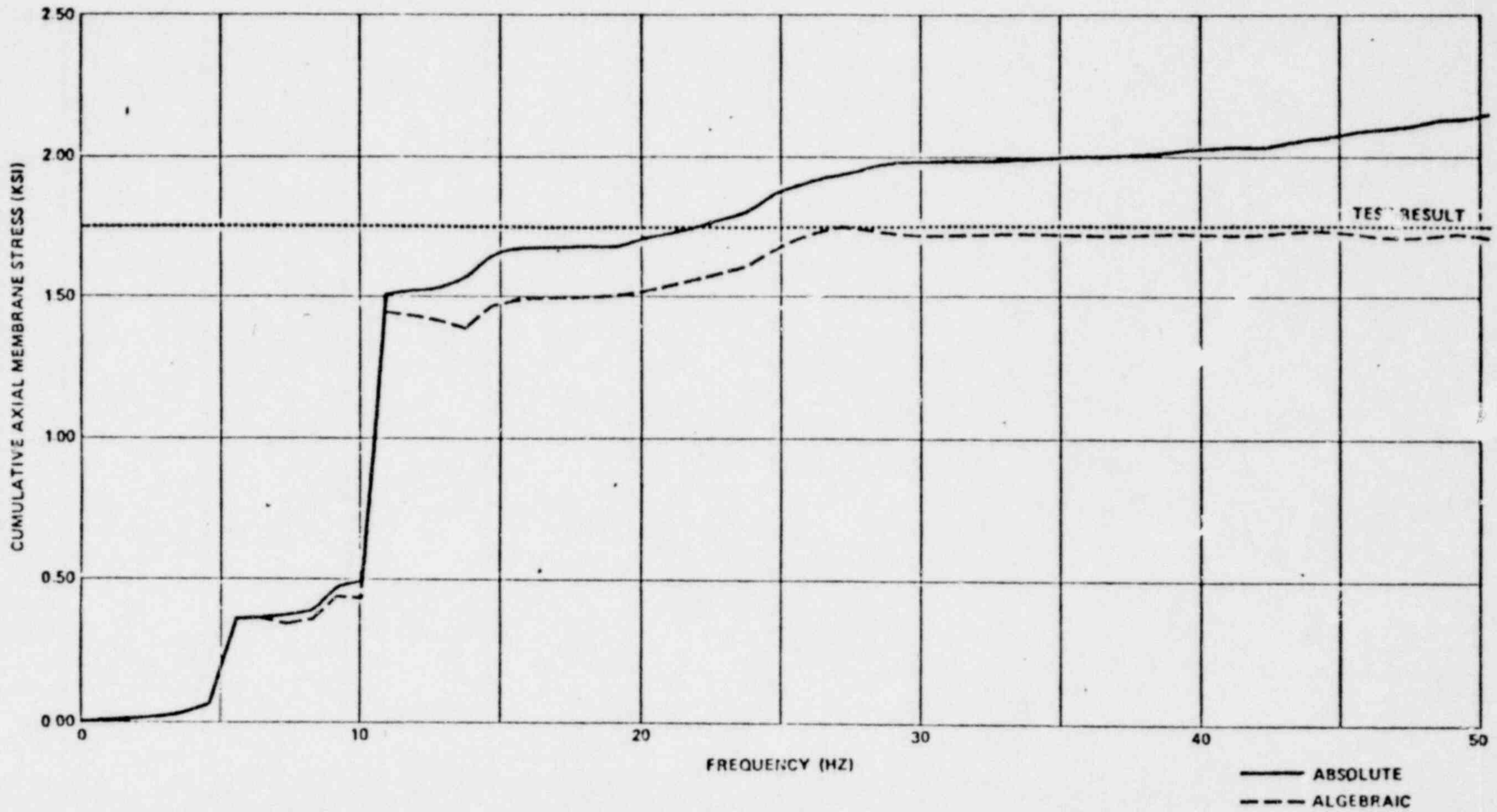
RESULTS OF ANALYSIS

AMPLIFICATION FACTOR FOR TOTAL VERTICAL FORCE VS FREQUENCY
("RIGID" FORCE = "FLEXIBLE" FORCE / AMPLIFICATION FACTOR)

1500 006

RESULTS OF VERIFICATION RUNS

CUMULATIVE AXIAL MEMBRANE STRESS AT BOTTOM MID-SPAN (SOURCE ANALYSIS)



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CONDENSATION OSCILLATION LOADING

FSTF RESPONSE

QUANTITY		TEST DATA (1)	FSTF ANALYSIS [2]		LDR LOADS [3]			MARGIN
			ALGEBRAIC	ABSOLUTE	CASE 1	CASE 2	CASE 3	LDR CASE 2/TEST DATA
BOTTOM DEAD CENTER	AXIAL MEMBRANE (KSI)	1.94	1.80	2.22	3.55	4.57	2.20	2.36
BOTTOM DEAD CENTER	HOOP MEMBRANE (KSI)	2.06	1.80	2.35	3.90	4.60	2.45	2.23
BOTTOM DEAD CENTER	RADIAL DEFLECTION (INCHES)	0.086	0.101	0.129	.230	.275	.141	3.20
INNER COLUMN AXIAL FORCE (KIPS)		93.3	101	136	254	290	172	3.11
OUTER COLUMN AXIAL FORCE (KIPS)		111.5	116	152	278	320	186	2.87

NOTES:
(1) DATA FOR TEST M-8 TIME PERIOD 24.8 TO 25.9 SECONDS
(2) LOAD APPLIED AT MULTIPLES OF .91 HZ. FREQUENCIES 0-30 HZ CONSIDERED.
(3) LOAD APPLIED AT STRUCTURE NATURAL FREQUENCIES FREQUENCIES 0-30 HZ CONSIDERED. ABSOLUTE SUM.

1500 008

TORBECK

MARK I

FULL-SCALE TEST FACILITY (FSTF)

- TEST OBJECTIVES
- FACILITY DESCRIPTION
- TEST MATRIX
- TYPICAL RESULTS
  - CONDENSATION OSCILLATION AND CHUGGING
  - HYDRODYNAMIC AND STRUCTURAL RESPONSES

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1500 009

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MARK I FULL-SCALE TEST FACILITY

PROGRAM OBJECTIVES & APPROACH

OBJECTIVE:

Obtain data to define Hydrodynamic Loads and Dynamic Structural Response resulting from Steam Condensation Phenomena on a representative Torus Sector in a full Scale Test Facility.

FACILITY APPROACH

- Full-Scale 22-1/2° Sector or Wetwell (8 Downcomers)
- Scaled Drywell, Vents, Flash Boiler
- Typical Structural Response
- Hydrodynamic and Structural Instrumentation
- High Speed DAS

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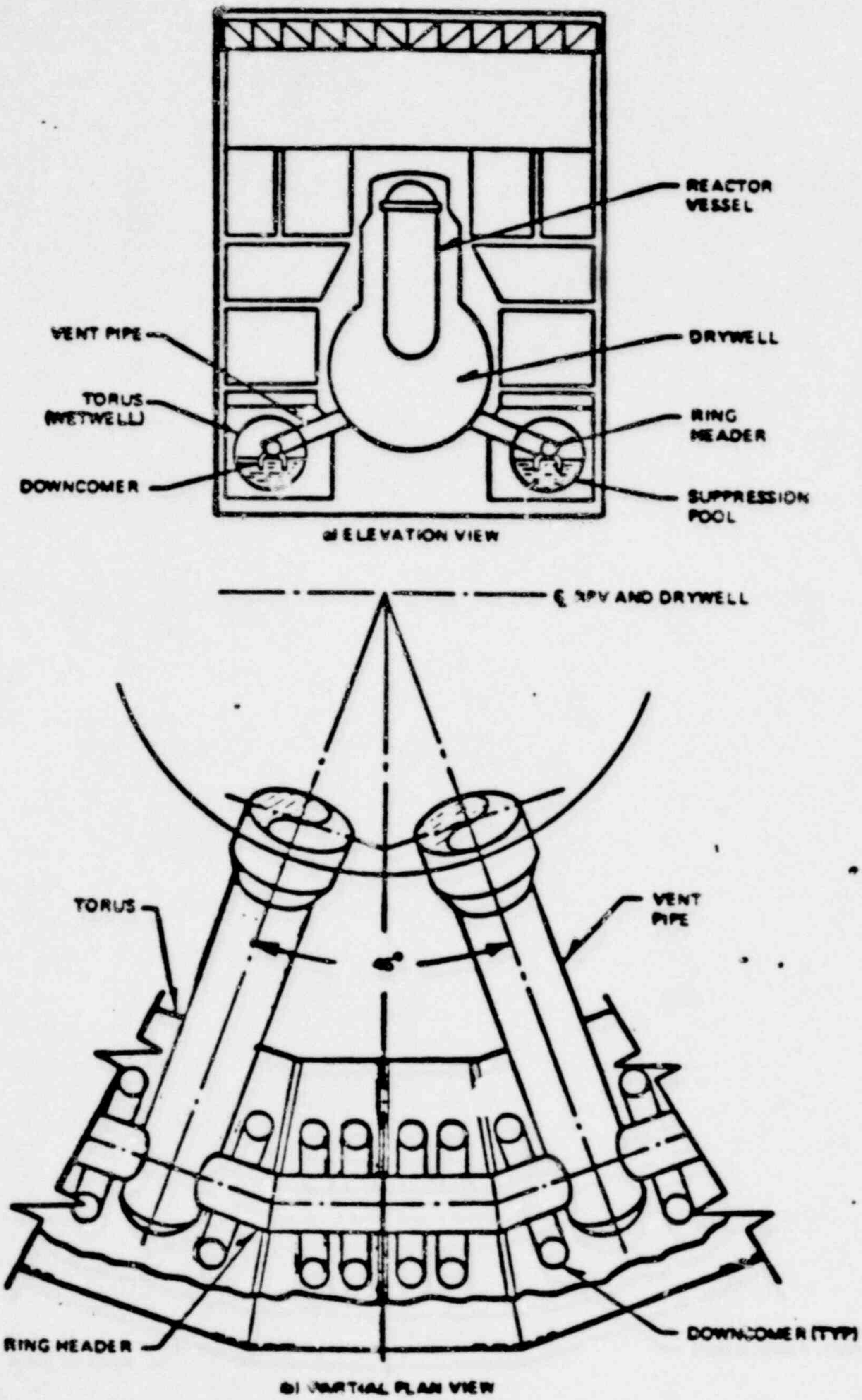
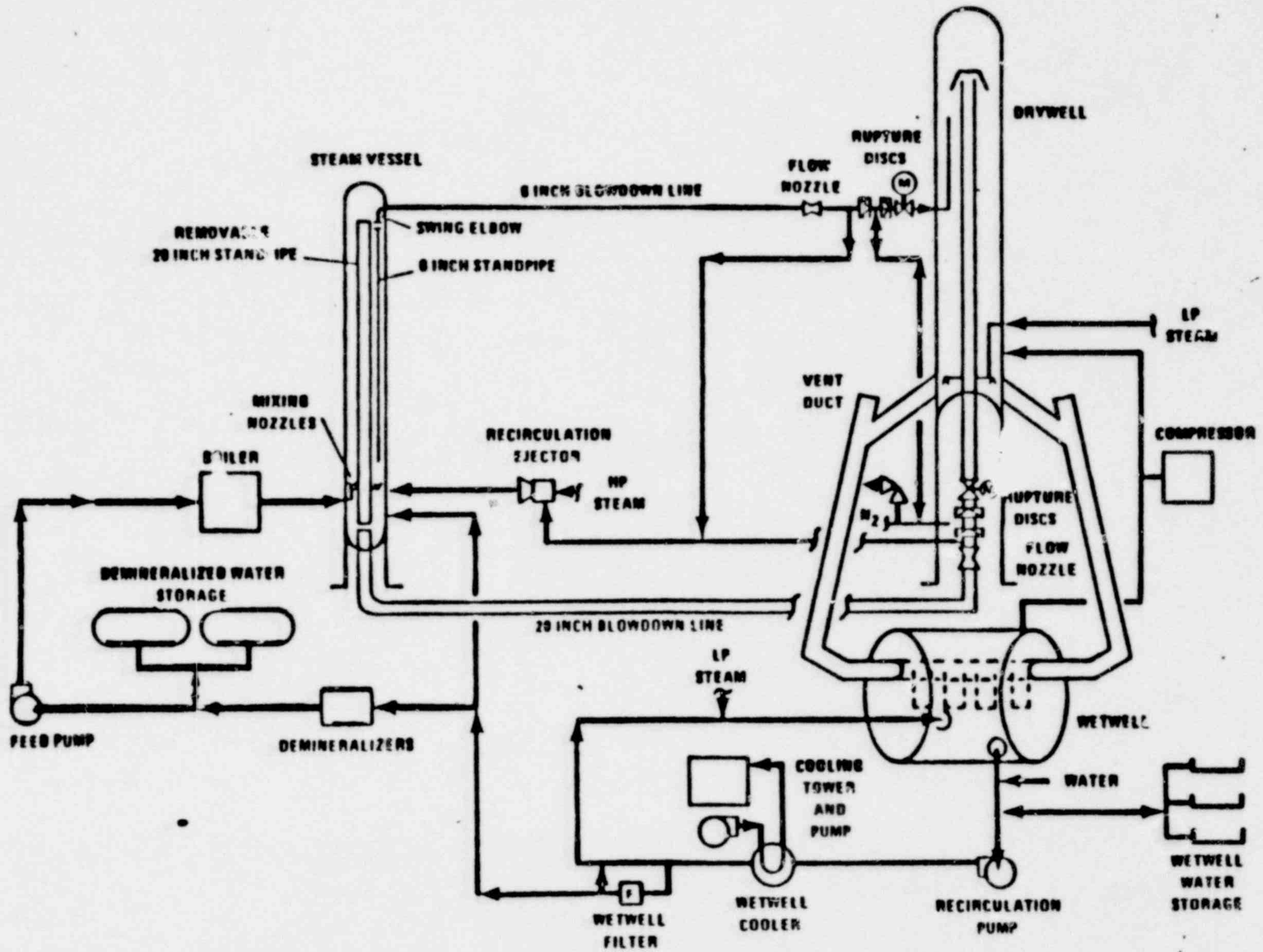


Mark I Containment Schematic

SIMPLIFIED FLOW DIAGRAM



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FSTF TEST MATRIX SUMMARY

TEST NUMBER*	BREAK CONFIGURATION	PARAMETER INVESTIGATED
M1	Small Steam	Reference Test
M2	Medium Steam	Break size increased (steam)
M3	Small Liquid	Break type changed to liquid.
M4	Small Steam	Freespace pressure increased.
M5	Small Steam	Pool Temp. increased
M6	Small Steam	Submergence decreased and pool temp. increased.
M9	Small Steam	Submergence increased.
M10	Small Steam	Vent air content decreased.
M7	Large Steam	Break size increased (steam).
M8	Large Liquid	Break size increased (liquid).

* In order of performance

1500 013

SYSTEM INSTRUMENTAT

DATA RECORDING CAPABILITY

- 256 CHANNELS
- EACH CHANNEL SAMPLED AT 1000 SAMPLES/SEC

PRIMARY MEASUREMENT GROUPS

- TORUS SHELL RESPONSE (ϵ, X, $\ddot{X}$)
- TORUS SUPPORTS STRAINS
- DOWNCOMER BENDING MOMENTS
- RING HEADER STRAINS AT DOWNCOMER ATTACHMENT
- TORUS WALL PRESSURES
- RING HEADER AND VENT PRESSURES
- DOWNCOMER PRESSURE
- DRYWELL PRESSURE
- DOWNCOMER AND RING HEADER LEVEL PROBES
- POOL TEMPERATURE DISTRIBUTION
- SYSTEM FLOW RATES

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TEST INSTRUMENT SUMMARY

	PRESSURE	STRAIN	DISPLACEMENT	TEMPERATURE	LEVEL	ACCELERATION	DIFFERENTIAL PRESSURE	TOTAL
WETWELL								
SHELL	26	122	16	54	6	14		222
HEADS						4		4
VENT HEADER	1	28			4			33
HEADER SUPPORTS		16						16
DOWNCOMERS	13	16			16	9		54
WW SUPPORTS		40						40
VENT DUCTS	4			4			2	10
6-INCH BLOWDOWN	3			1				4
18-INCH BLOWDOWN	3			1				4
DRYWELL	2			9			1	12
STEAM VESSEL	1			2			3	6
BASEMAT						6		6
TOTAL	53	222	16	71	26	33	6	427

MARK I CONDENSATION OSCILLATION

FSTF RESULTS

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163

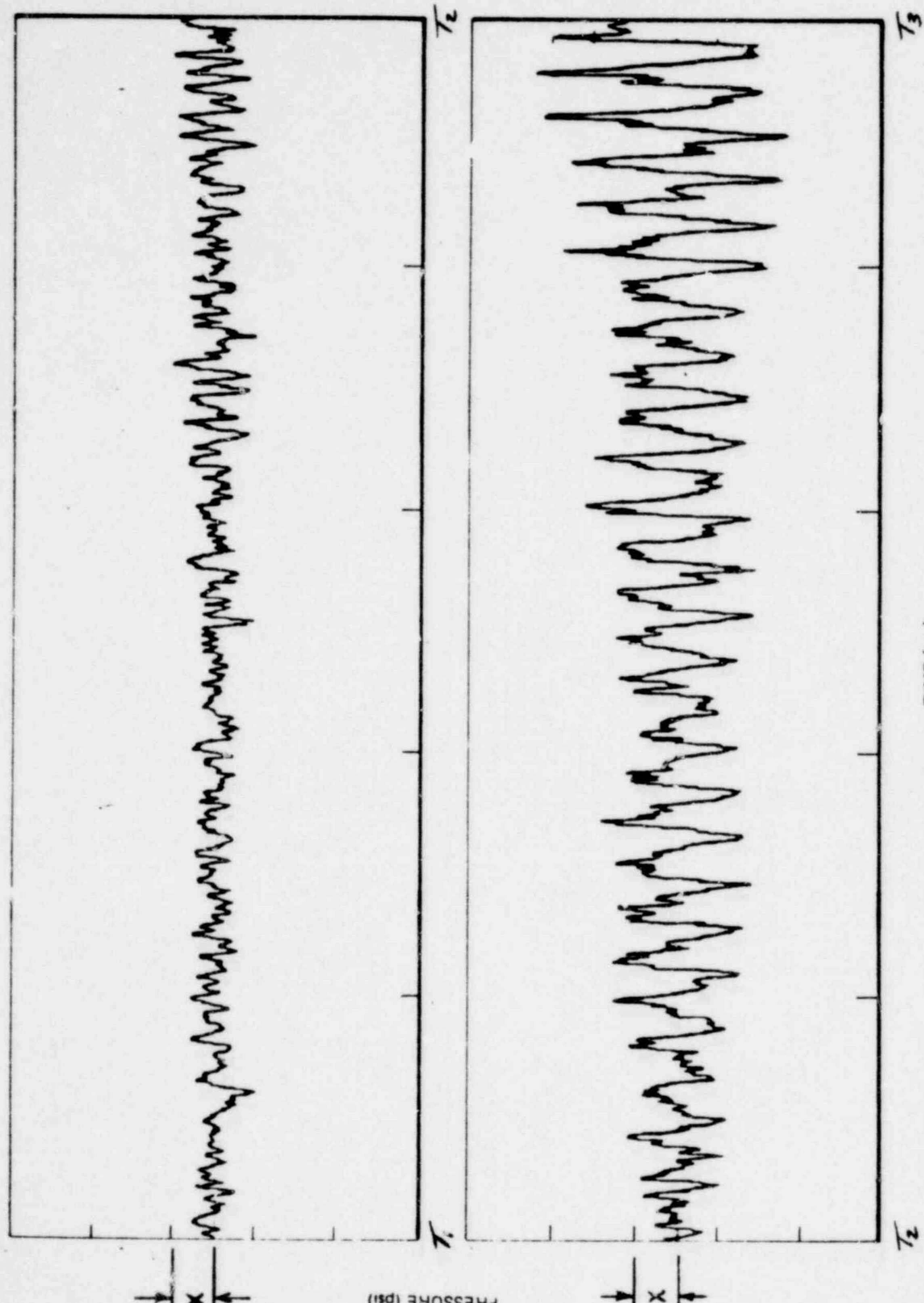


Figure 6.2.2-11. Dynamic Portion of Wetwell Pressure Transducer (P3183) Output

1500 017

104

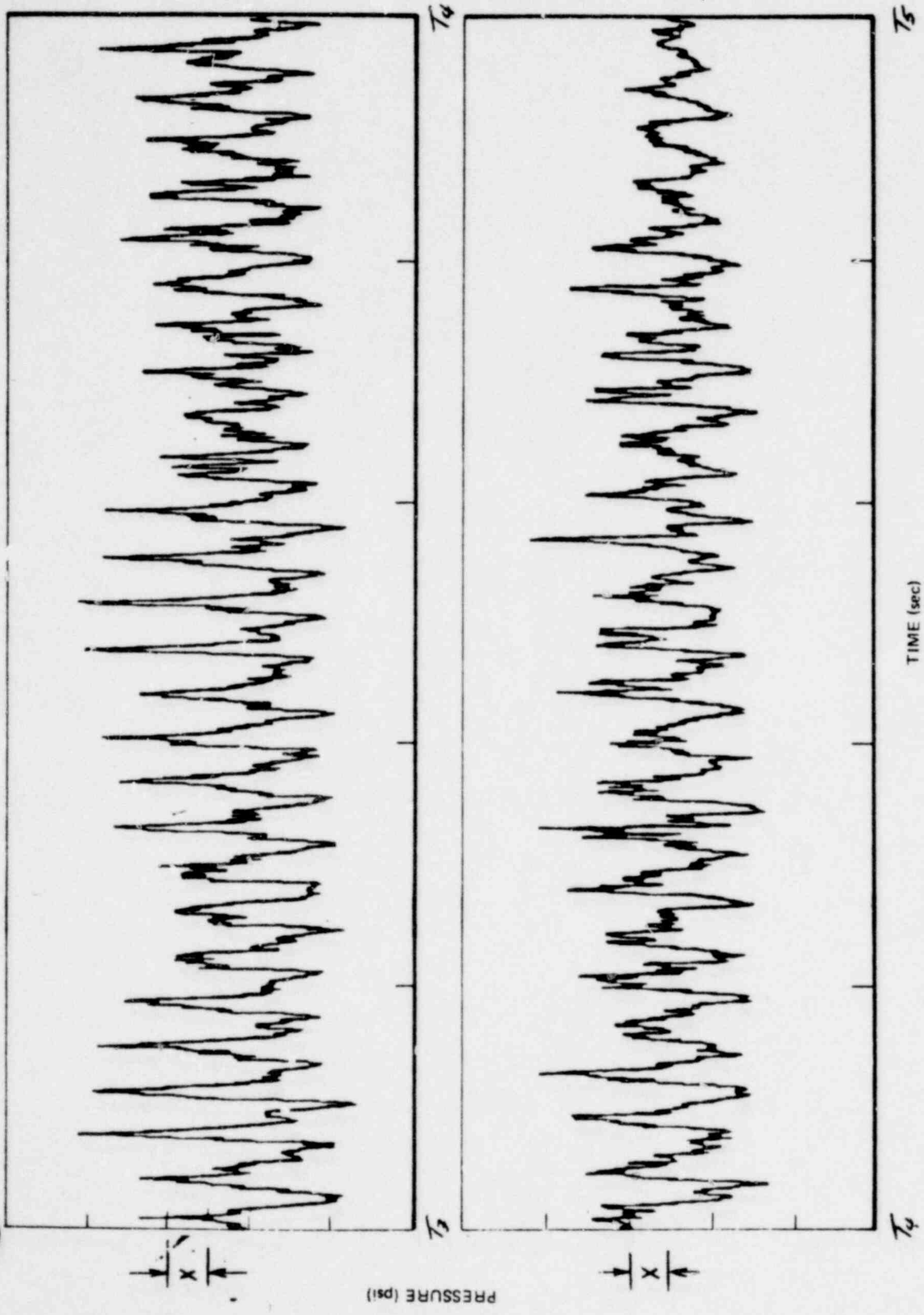


Figure 6.2.2-12. Dynamic Portion of Wetwell Pressure Transducer (P3183) Output

1500 018

105

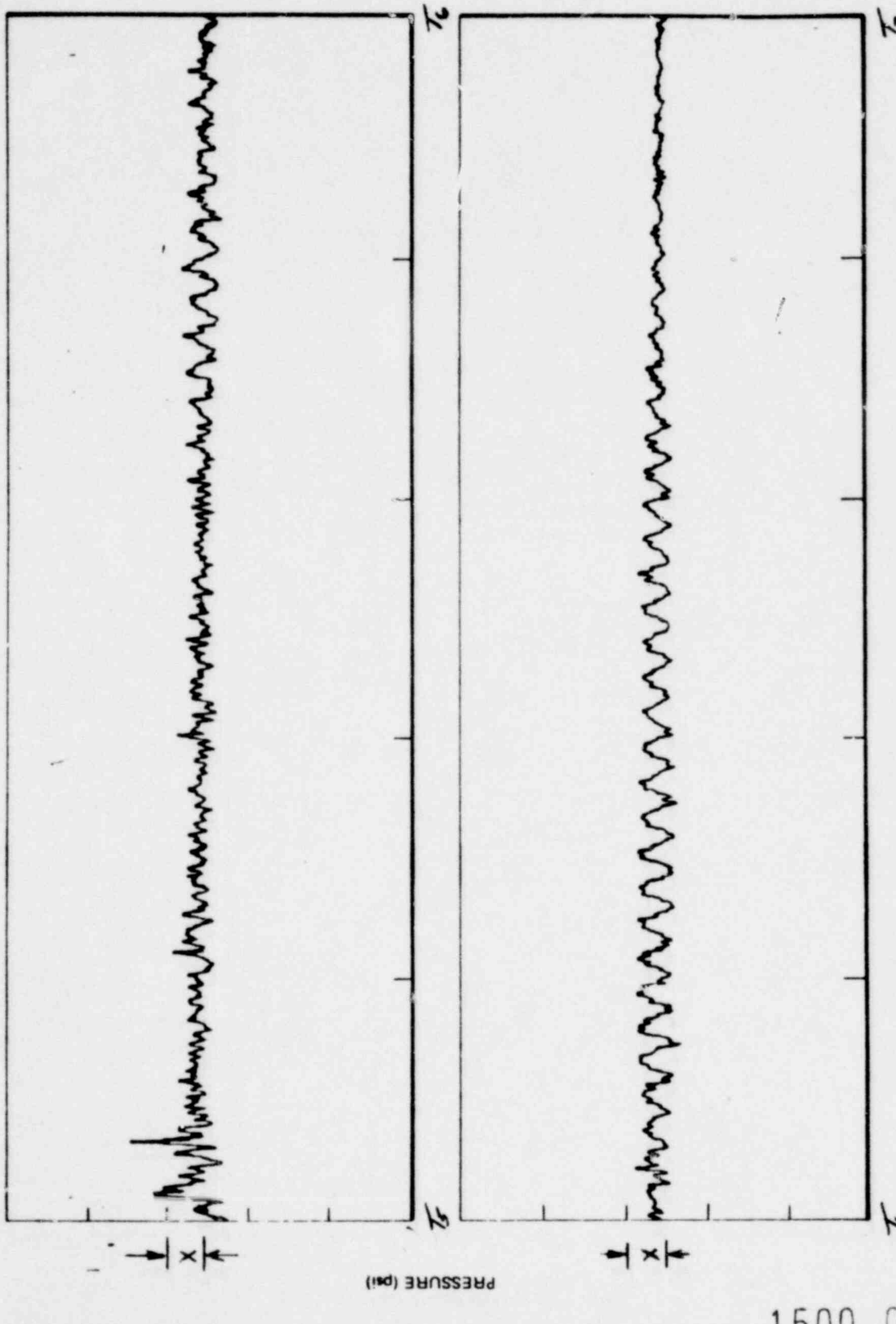


Figure 6.2.2-13. Dynamic Portion of Wetwell Pressure Transducer (P3183) Output

1500 019

106

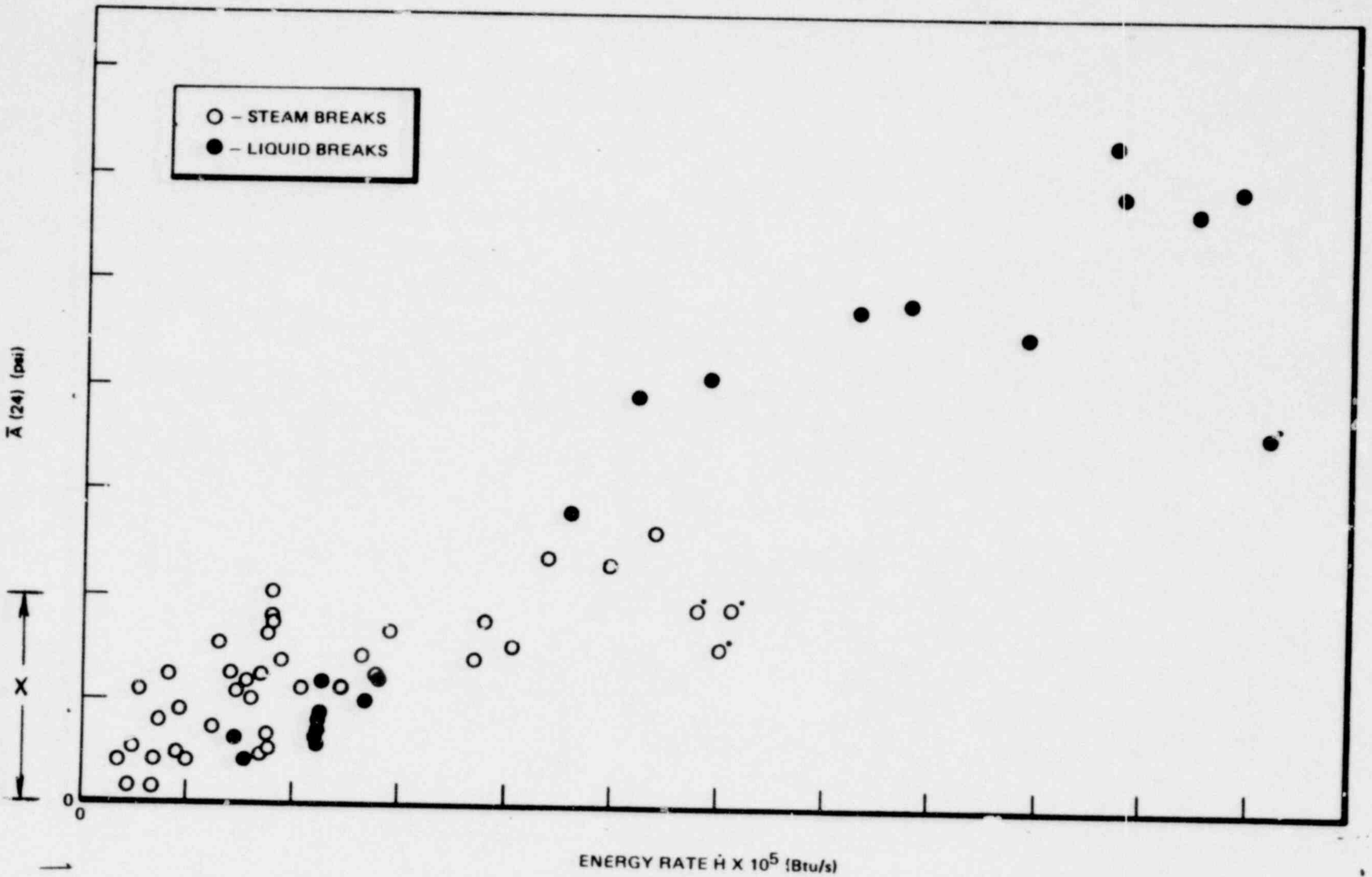


Figure 6.2.2-56. Average Amplitude of 24 Wetwell Transducers vs Energy Rate

1500 020

107

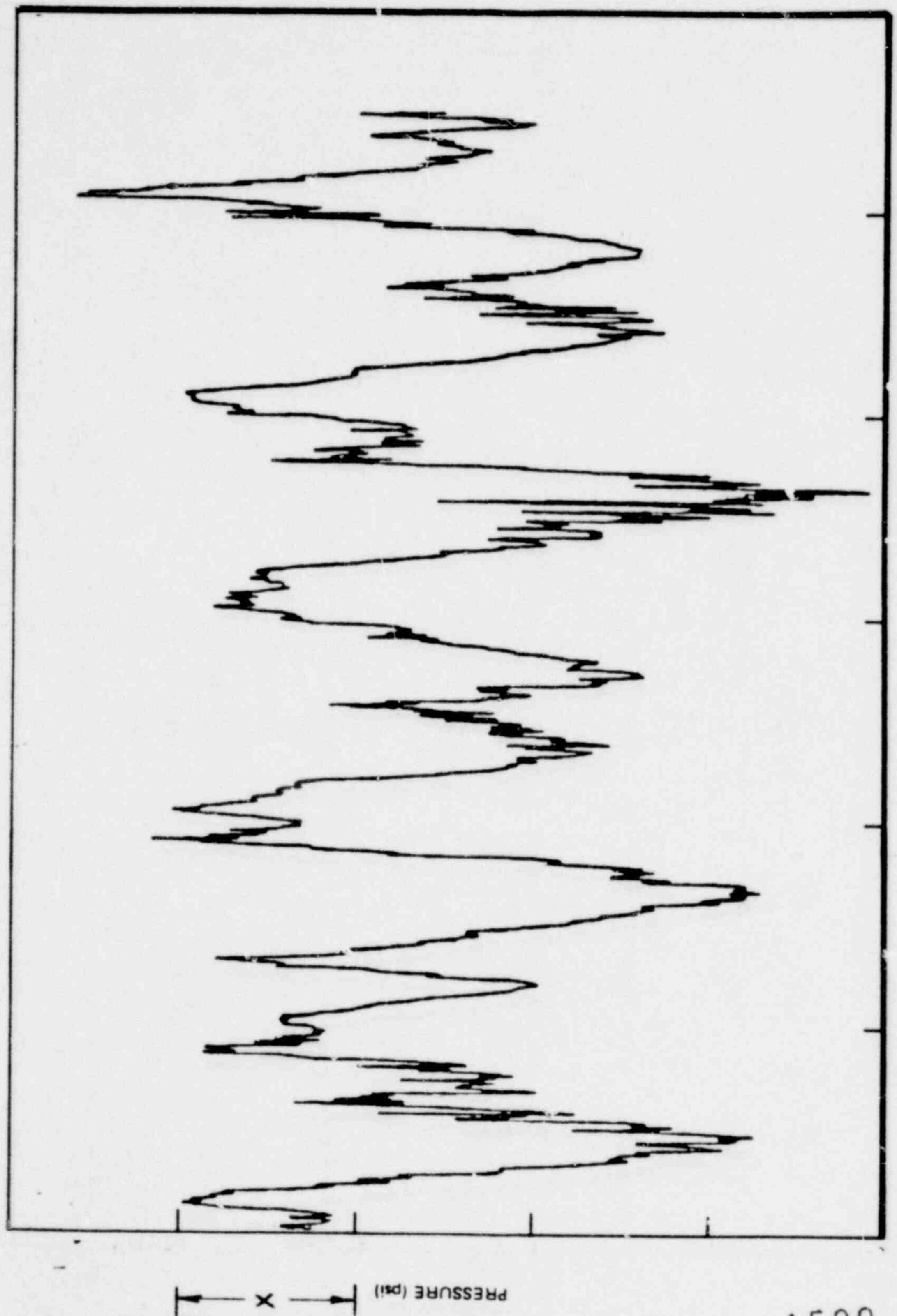


Figure 6.2.2-17. Dynamic Portion of Downcomer No. 4 Pressure Transducer (P5443) Output

1500 021

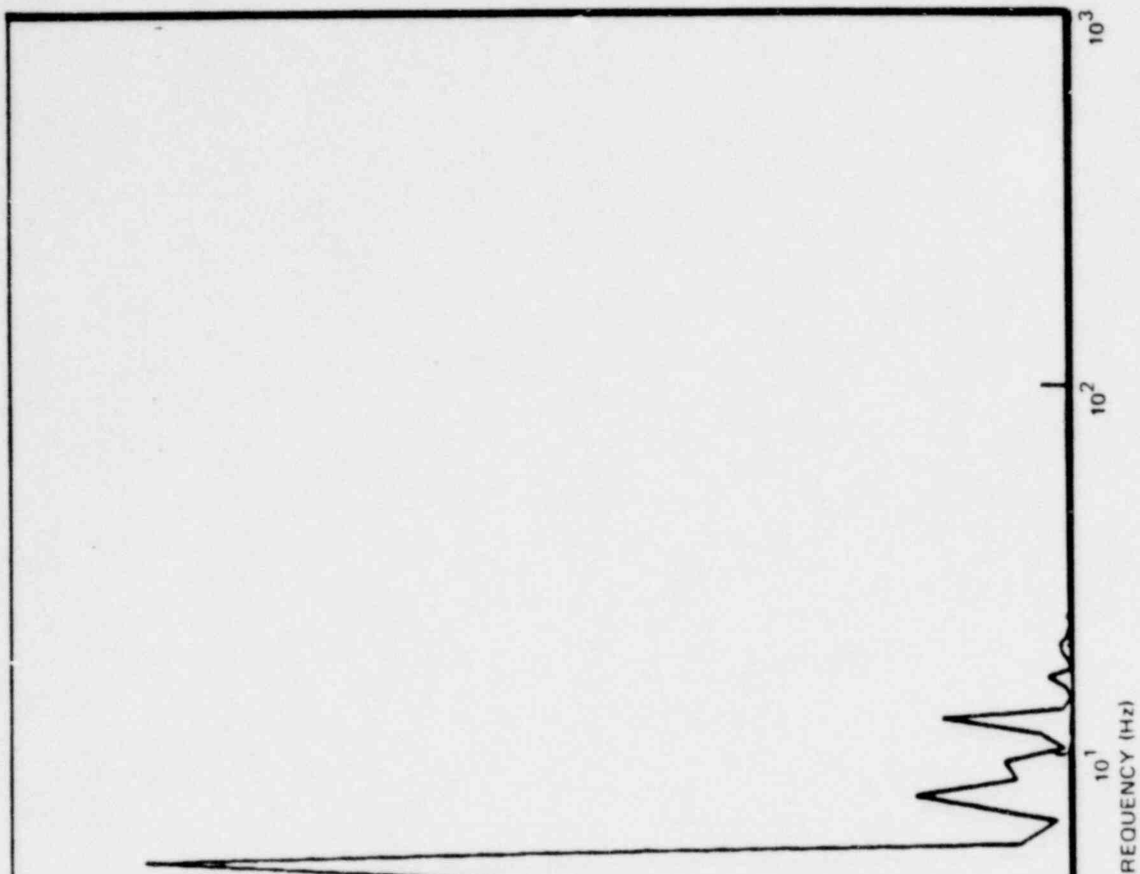


Figure 6.2.2-21. Calculated PSDs for Downcomer No. 4 Pressure Transducer Signal (P5443)

1500 022

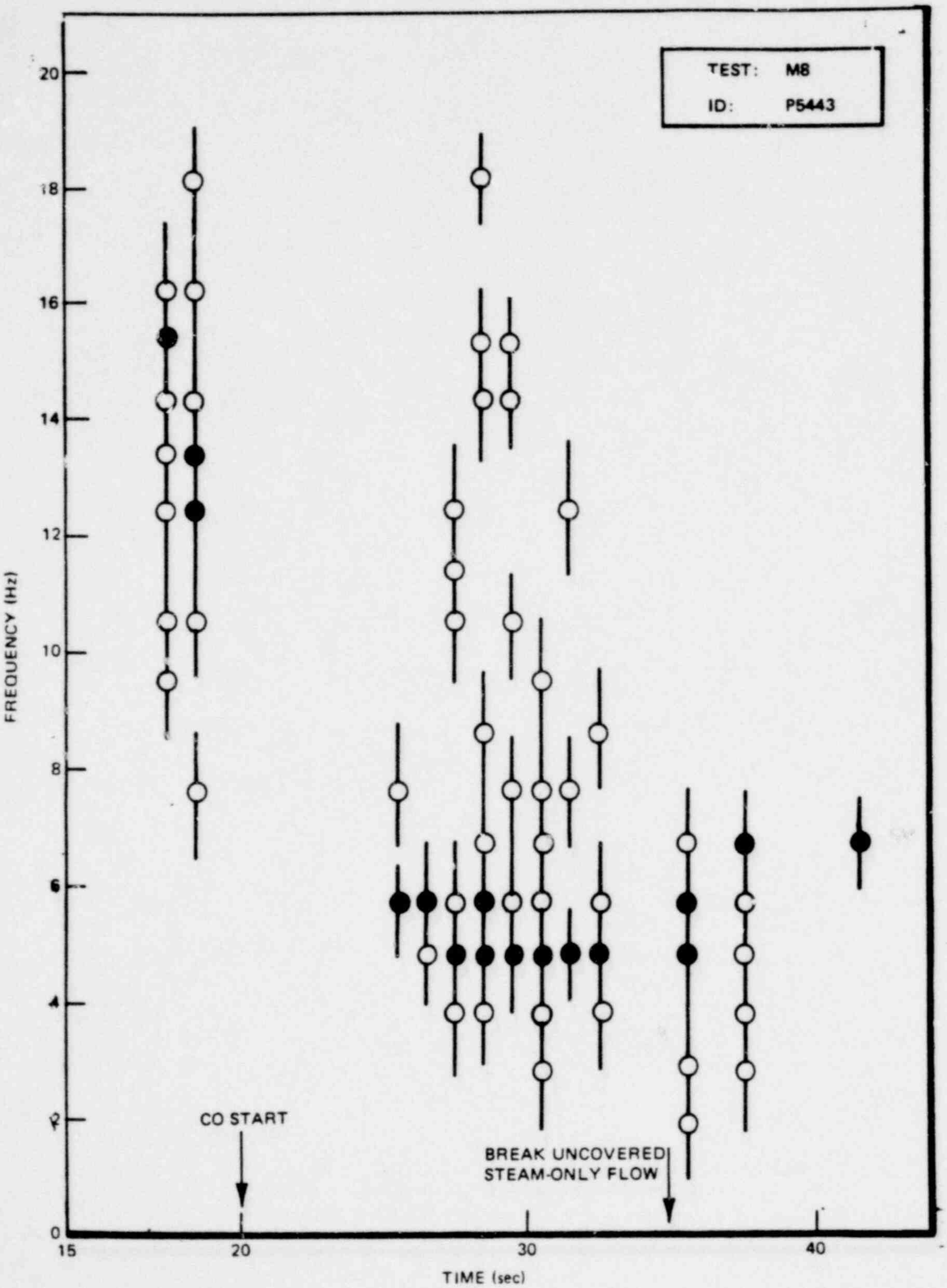


Figure 6.2.2-28. Significant Frequencies in the Pressure Waveform in Downcomer No. 4, M8

1500 023

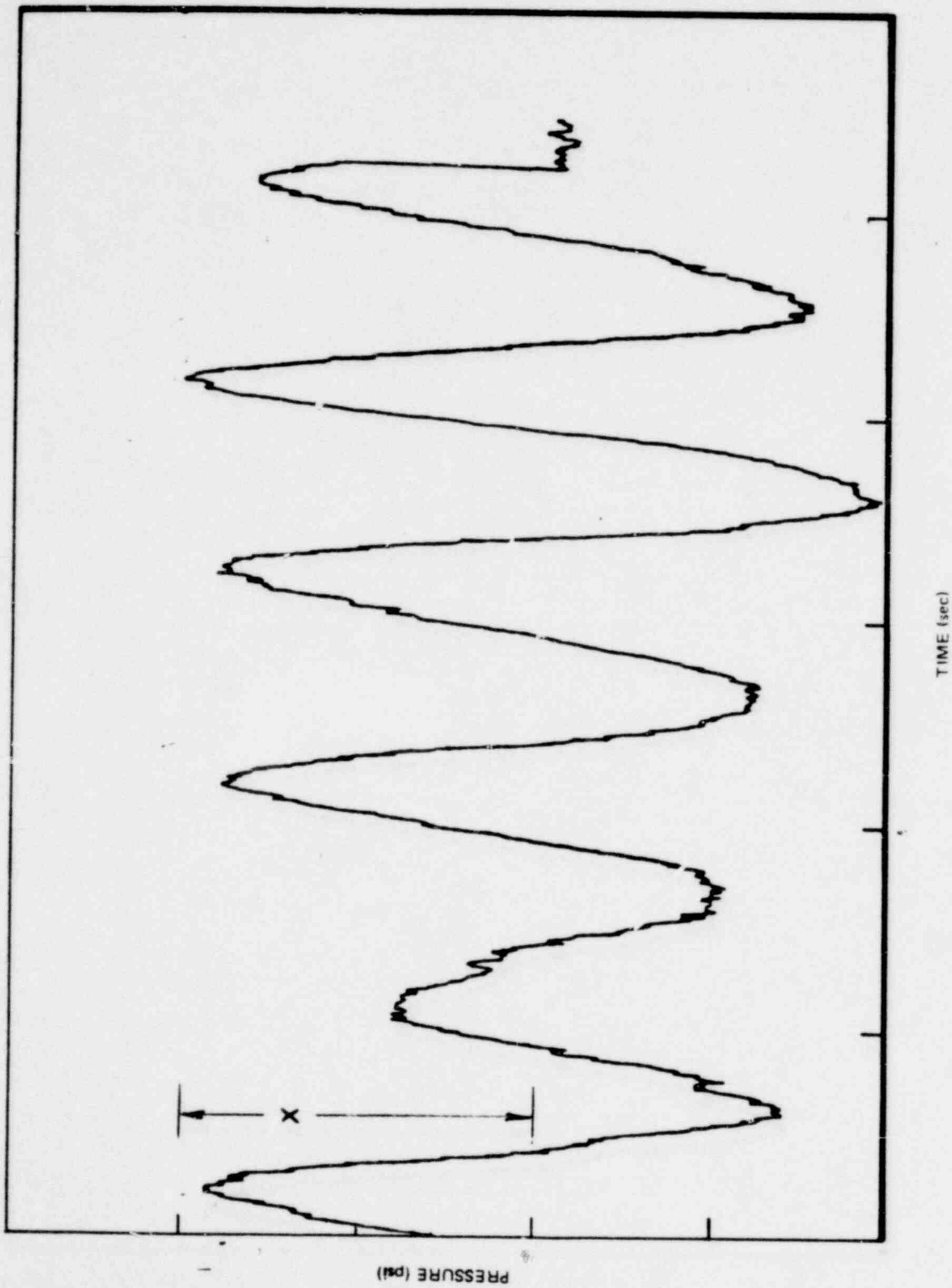


Figure 6.2.2-16. Dynamic Portion of Vent Header Pressure Transducer (P5901) Output

1500 024

NEDE-24539-P
GE[redacted]
Class III

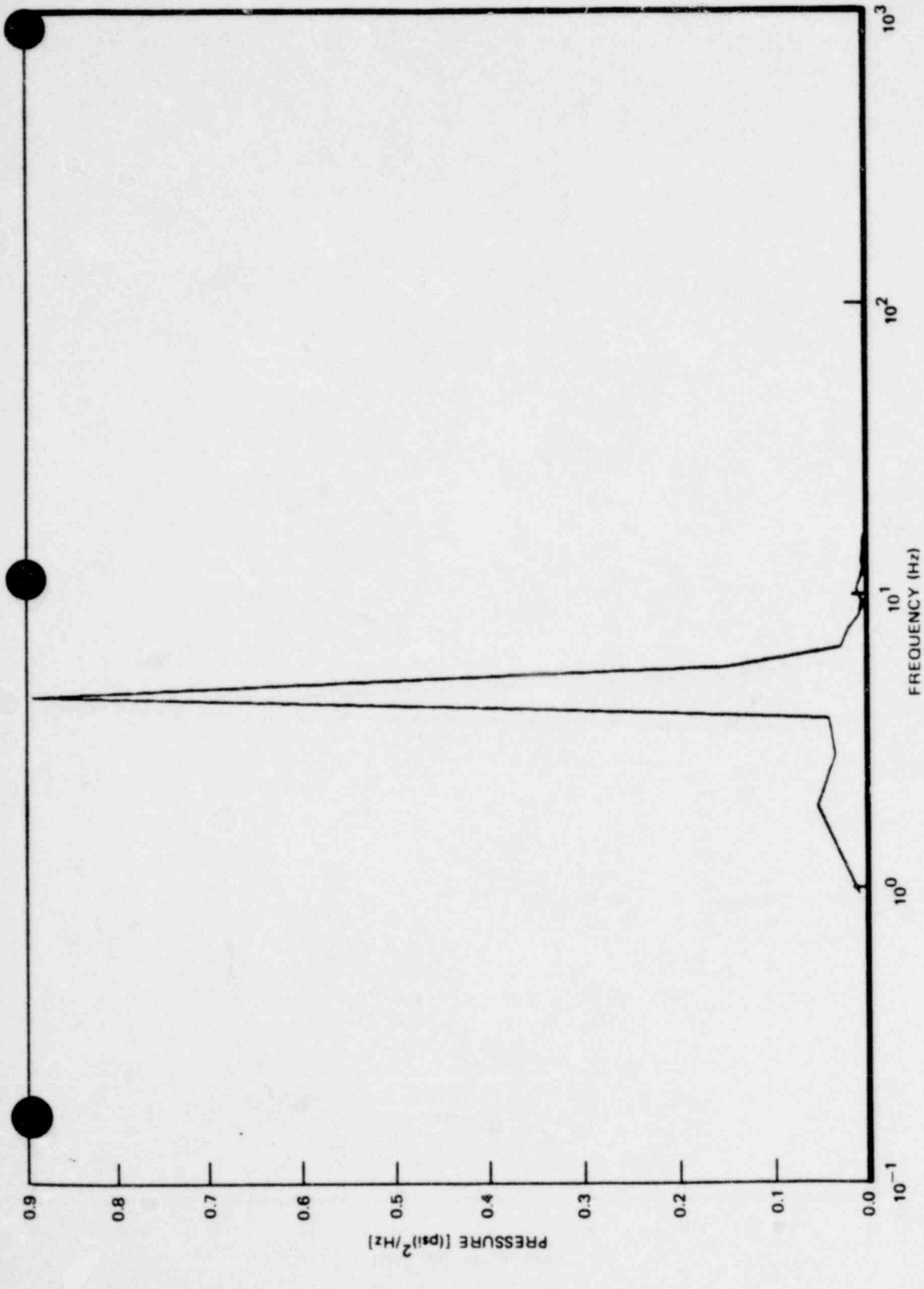


Figure 6.2.2-20. Calculated PSDs for Vent Header Pressure Transducer (P5901) Signal

1500 025

112

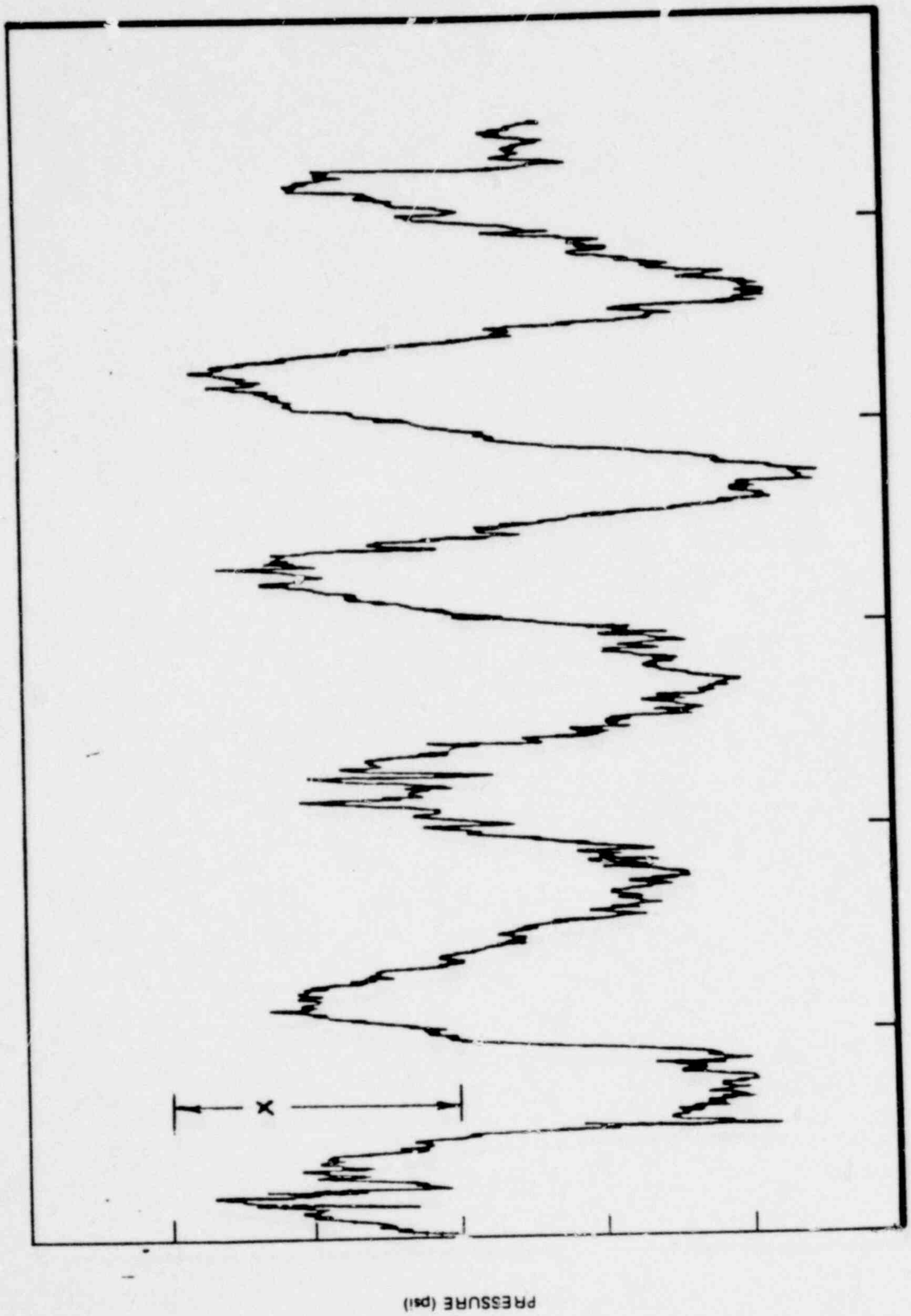


Figure 6.2.2-15. Dynamic Portion of Vent Pressure Transducer (P2004) Output

1500 026

113

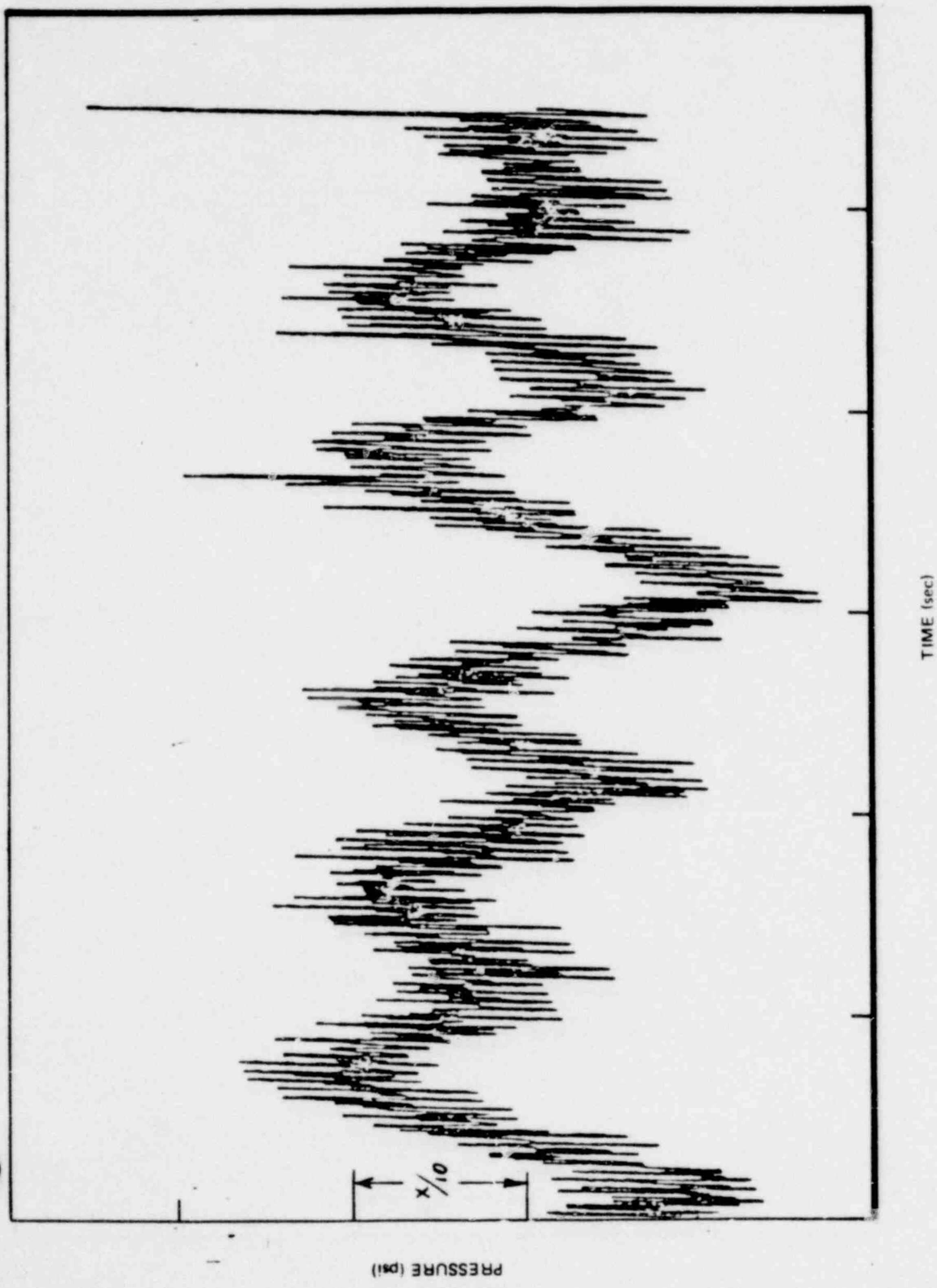


Figure 6.2.2-14. Dynamic Portion of Drywell Pressure Transducer (P2001) Output

1500 027

114

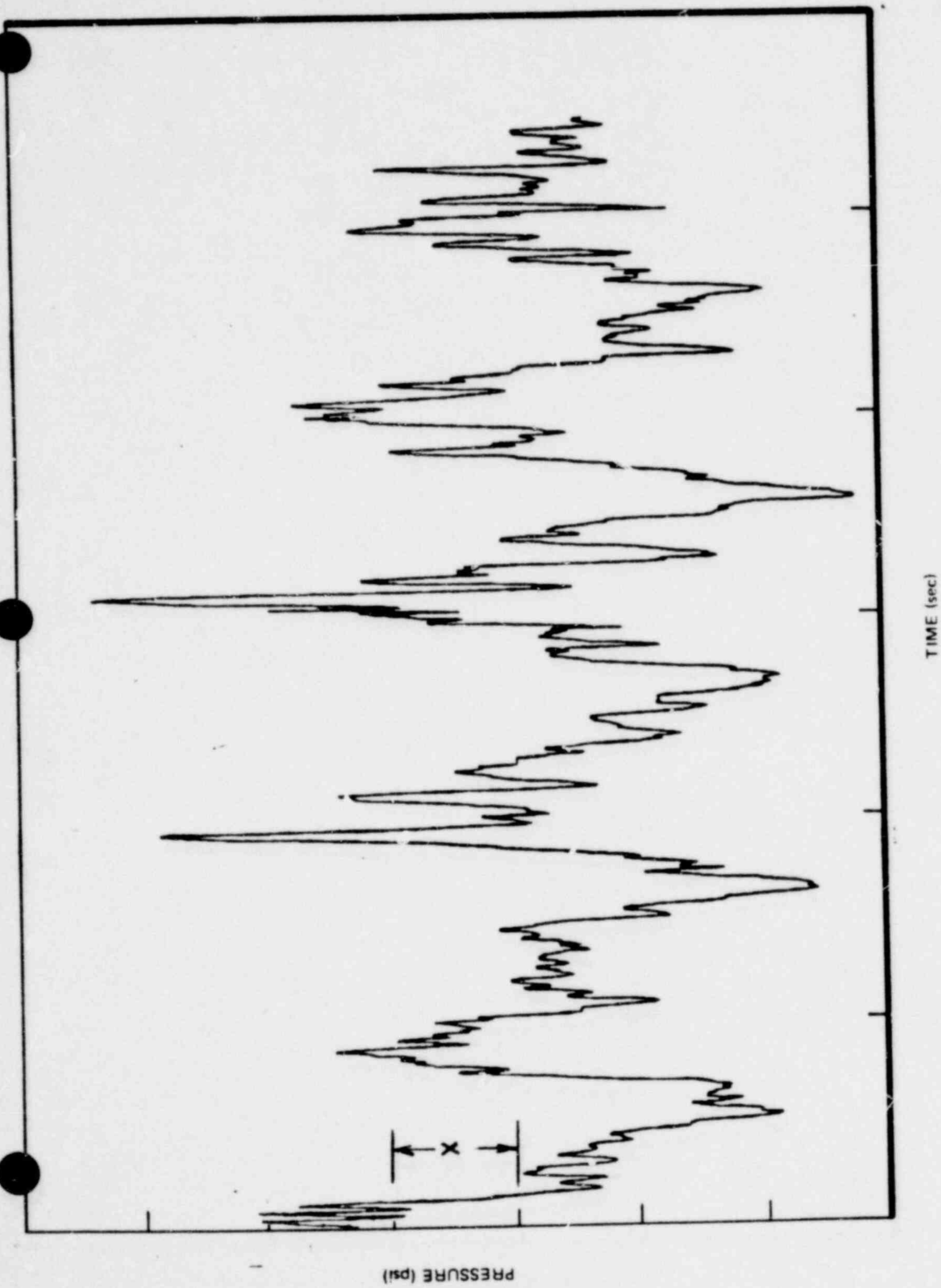


Figure 6.2.2-18. Dynamic Portion of Wetwell Pressure Transducer (P3185) Output

1500 028

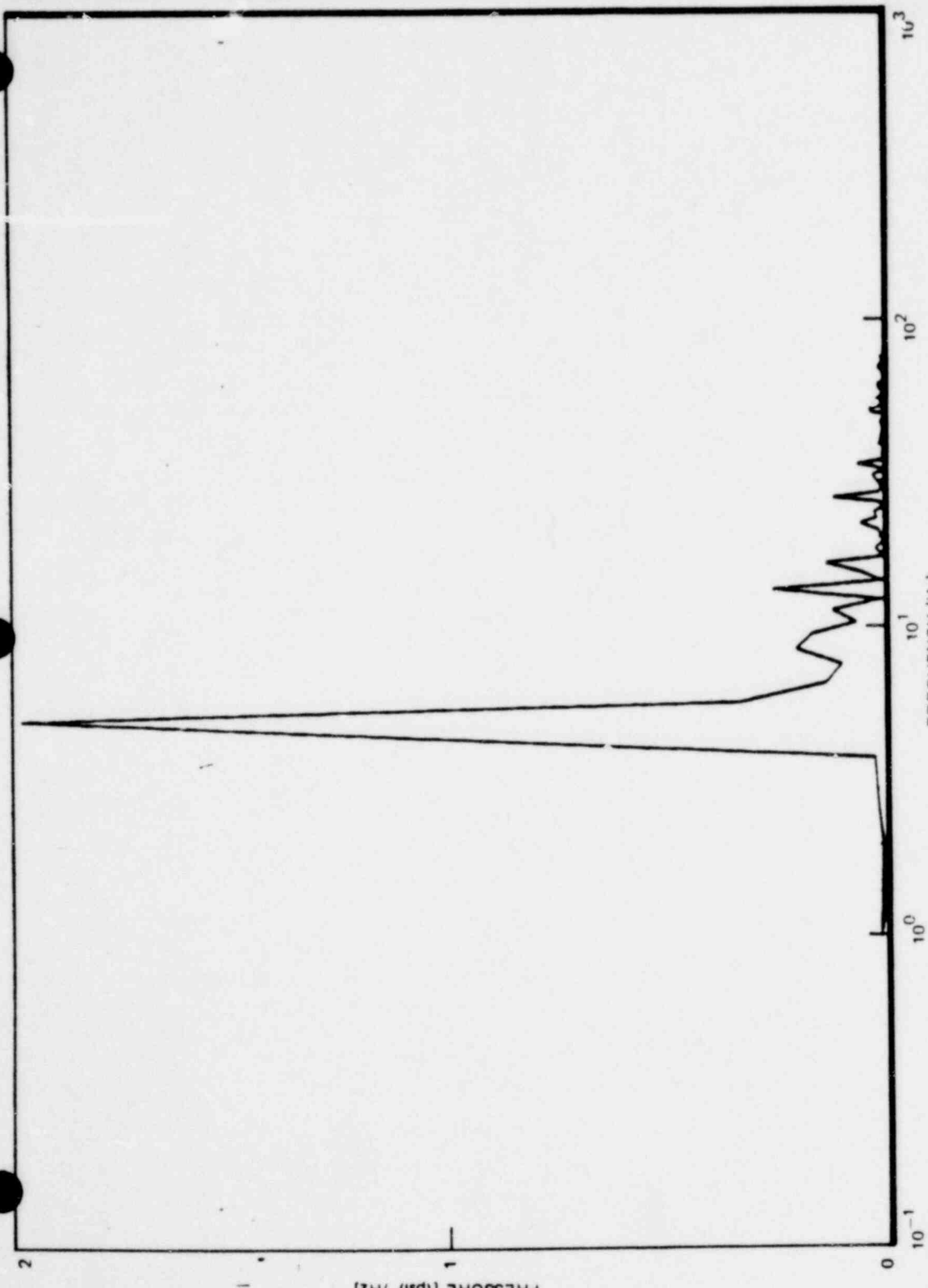
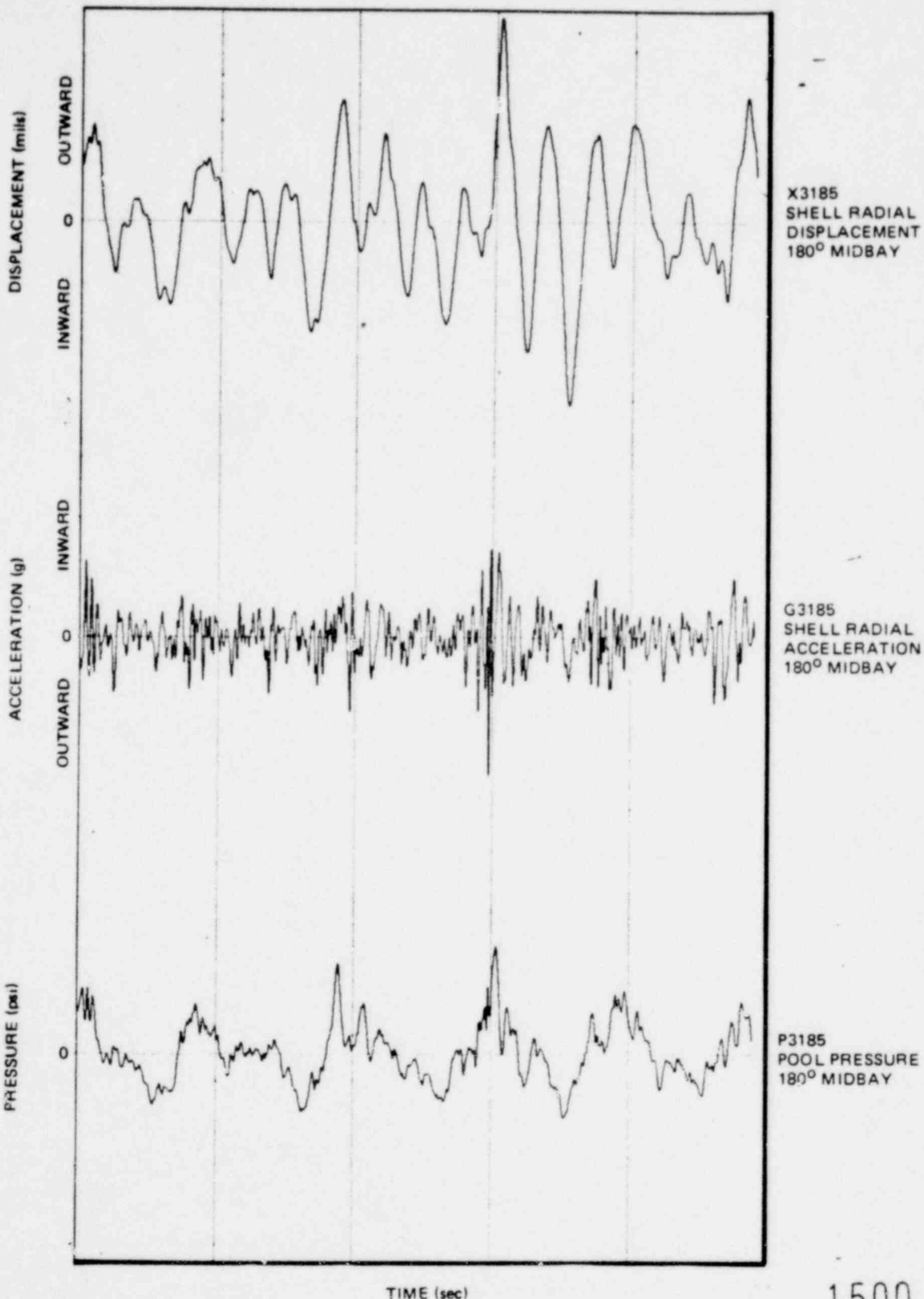


Figure 6.2.2-22. Calculated PSDs for Wetwell Pressure Transducer (P3185) Signal

1500 029

116



1500 030

Figure 6.4.1-11. Wetwell Shell Response at 180° Midbay During Condensation Oscillation (Large Liquid Break Test - M8)

117

MARK I CHUGGING

FSTF RESULTS

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11/16/79

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/18

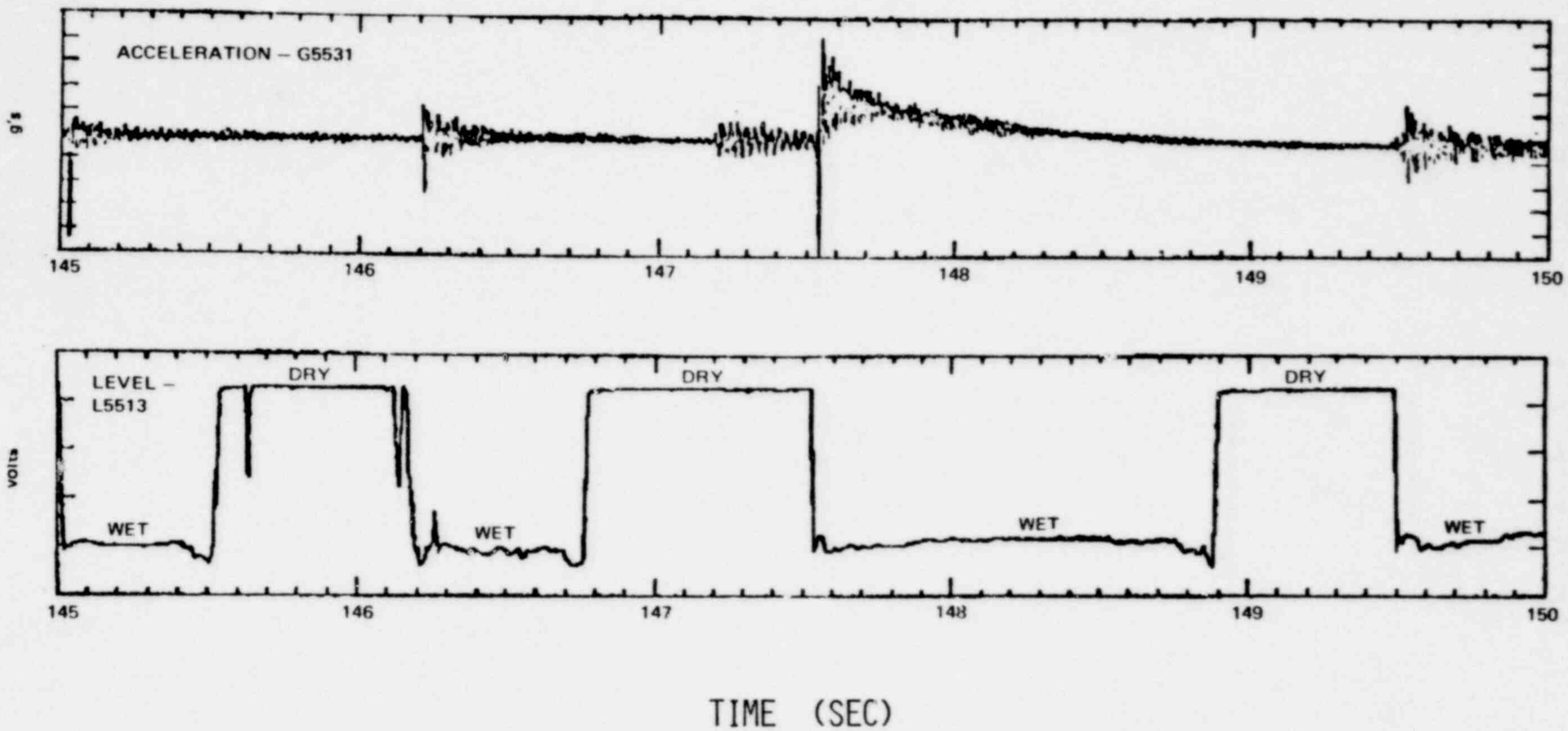
Table 6.2.1-1

SUMMARY OF CHUGGING DATA BASE

Test Number	M1	M4	M9	M10
Initial Conditions	nominal	5 psig free space press.	4.5 feet submergence	no vacuum breaker
*Approximate Chugging Periods, Seconds	30-330	26-116	25-305	20-120 250-305
Seconds of Chugging Data Recorded	300	90	280	155
Approximate Number of Downcomer Chugs	670	110	480	200

*Time = 0 is the start of data recording

1500 032



1500 033

DOWNCOMER ACCELERATION, AND WATER LEVEL DURING CHUGGING

120

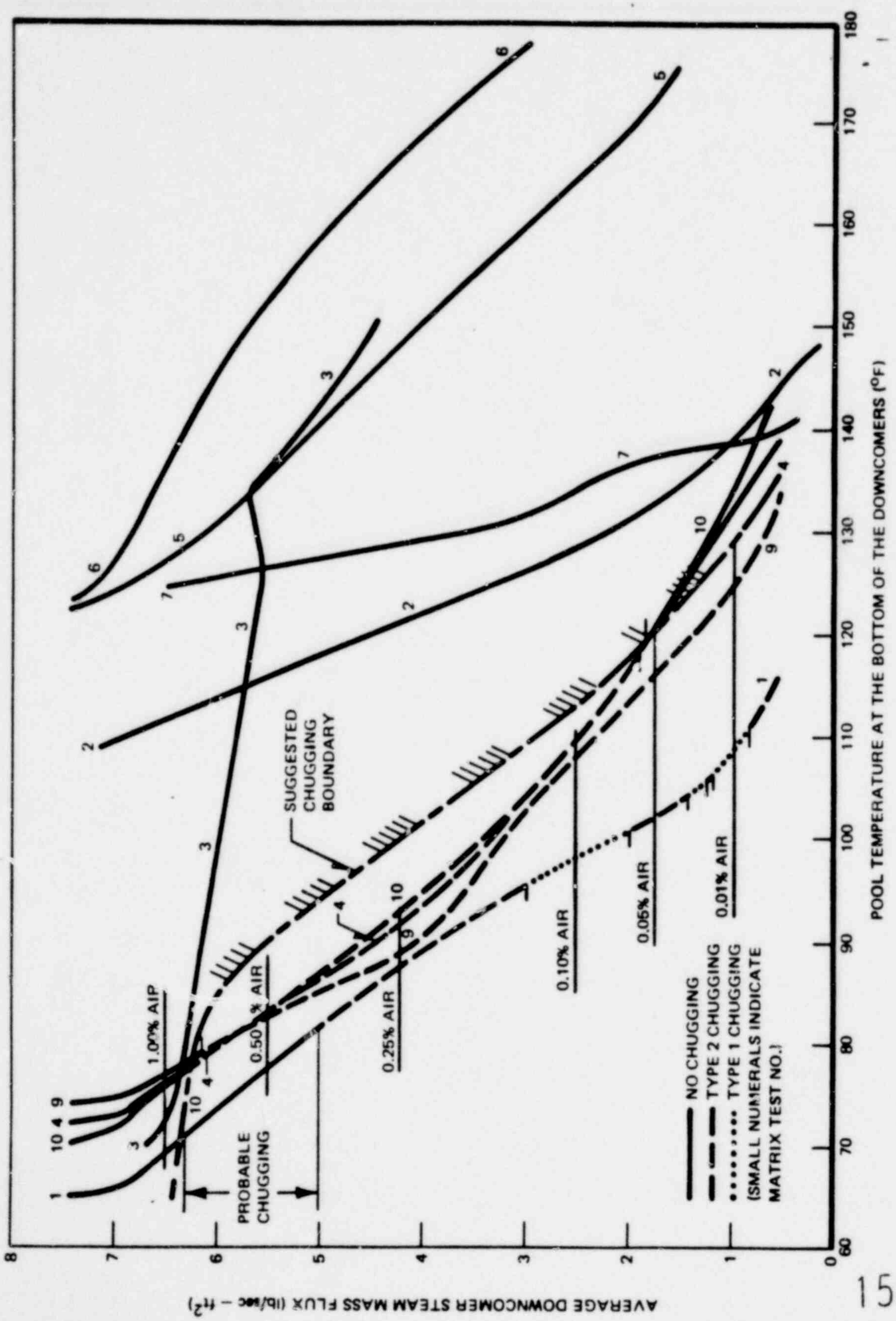


Figure 6.2.1-3. Mass Flux and Pool Temperature Conditions for Chugging

1500 034

121

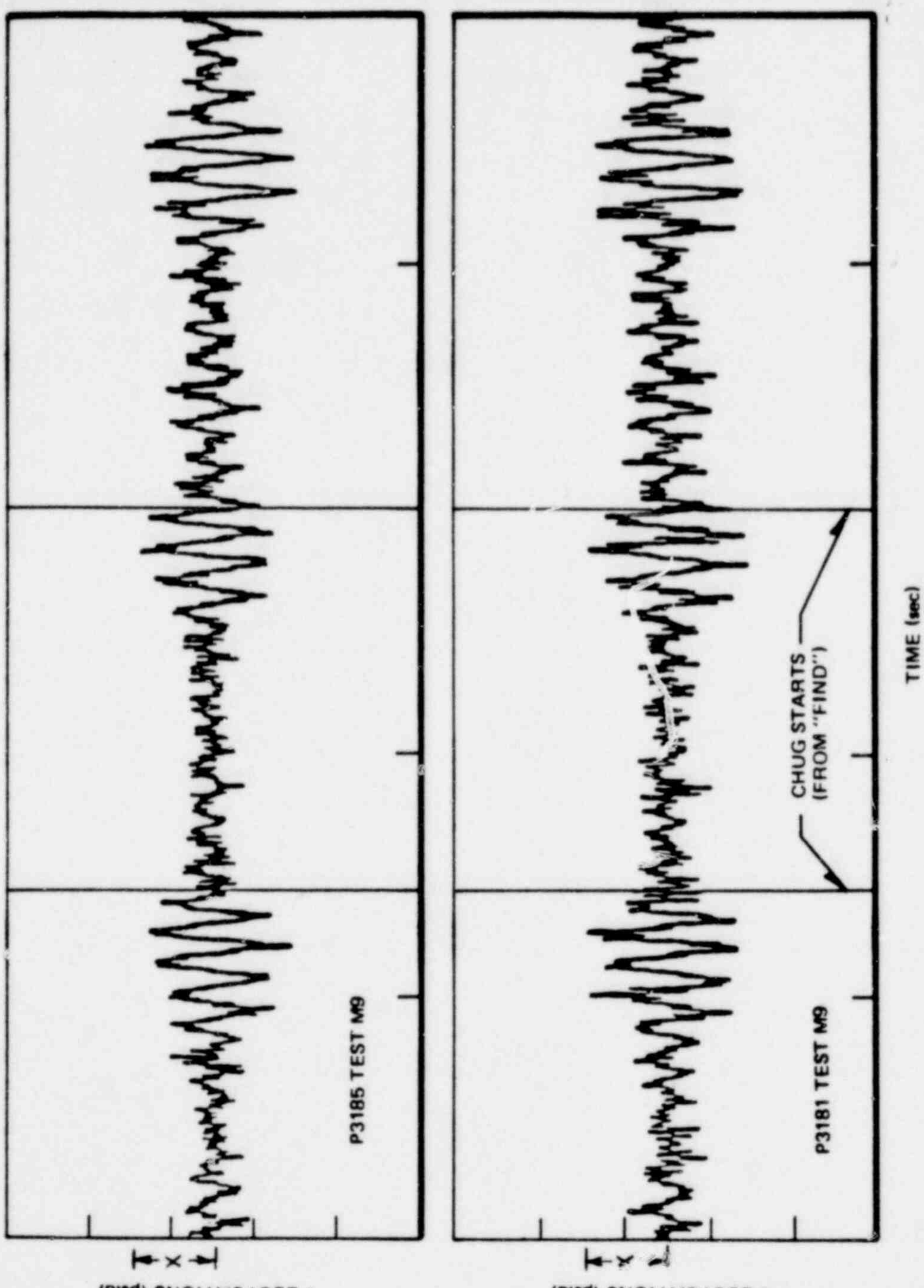


Figure 6.2.1-15. Chugging Wall Pressure Signals

1500 035

Table 6.3.1-2

DYNAMIC STRESSES DURING CONDENSATION OSCILLATION AND CHUGGING

	Condensation Oscillation (M8) (psi)	Chugging (M1) (psi)
Wetwell Shell*		
Wetwell Shell	3,800	2,500
Wetwell Shell/Ring Girder Intersection	14,800	2,900
Wetwell Support Columns		
Radial Bending	1,500	300
Longitudinal Bending	500	300
Tensile/Compressive	1,600	500
Vent Header Shell		
Downcomer/Vent Header Intersection		
• "Tied" Downcomers**	14,000	-
• "Free" Downcomers	46,000	25,000

* Maximum surface stress intensity.
** Monticello prototypical tie-straps.

1500 036

123

SAXENA

MARK I CONTAINMENT PROGRAM

CONDENSATION OSCILLATION LOAD

TORUS SHELL

ACRS MEETING

SAN FRANCISCO, CA

NOVEMBER 16, 1979

124

1500 037

UC SAXENA
UCS - 1
11/16/79

MARK I CONDENSATION OSCILLATION

LOAD DEFINITION

- OBJECTIVE
- APPROACH
- FSTF TEST DATA
- DATA APPLICATION
  - DATA BASE SELECTION
  - DATA REDUCTION/ANALYSIS
  - LOAD DEFINITION
- SUMMARY

1500 038

125

UCS - 02
11/16/79

MARK I CONDENSATION OSCILLATION

OBJECTIVE

- DEVELOP CONDENSATION OSCILLATION LOAD DEFINTION FOR TORUS SHELL FROM THE FSTF TEST DATA

126

1500 039

UCS - 03
11/16/79

MARK I CONDENSATION OSCILLATION

APPROACH

- ENTIRE FSTF CO DATA WAS EXAMINED
- MAXIMUM PRESSURE AMPLITUDE DATA SEGMENTS WERE SELECTED AS DATA BASE
- WALL PRESSURES (24 SENSORS) WERE SPATIALLY AVERAGED → AVERAGE VERTICAL PRESSURE LOADING ON THE TORUS SHELL
- PSD ANALYSES WERE PERFORMED
- FSTF FSI EFFECTS WERE ACCOUNTED FOR
- RIGID WALL PRESSURES AS A FUNCTION OF FREQUENCY WERE SPECIFIED AS LOAD DEFINITION

MARK I CONDENSATION OSCILLATION

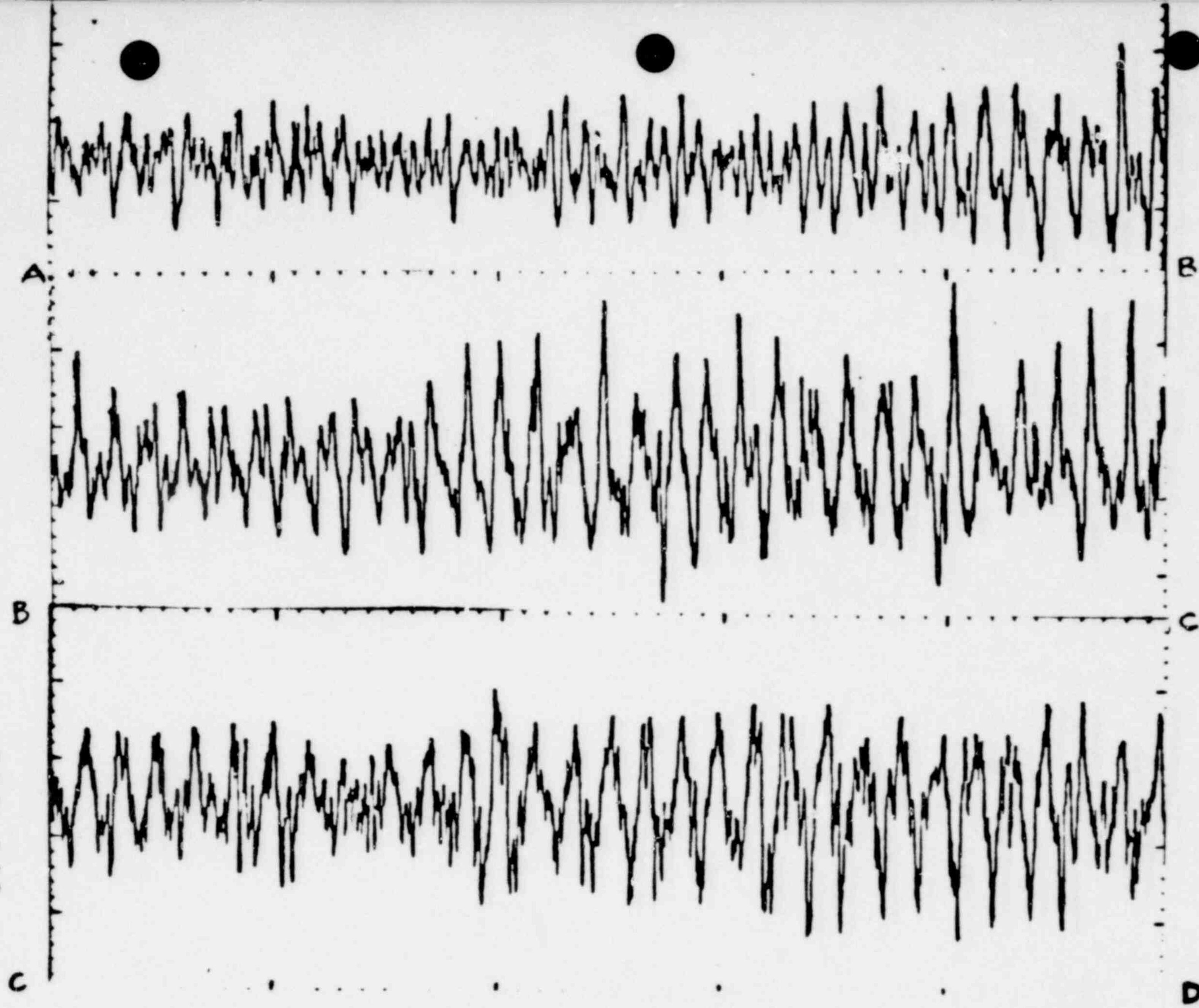
FSTF TEST DATA

- MARK I FULL SCALE TEST DATA - ONE BAY OF THE MARK I TORUS
- LIQUID AND STEAM BREAKS TEST DATA
- LOAD MAGNITUDE BREAK SIZE/TYPE DEPENDENT
- HIGHEST PRESSURE AMPLITUDE OBSERVED DURING LARGE LIQUID BREAK

1500 041

128

UCS - 05
11/16/79



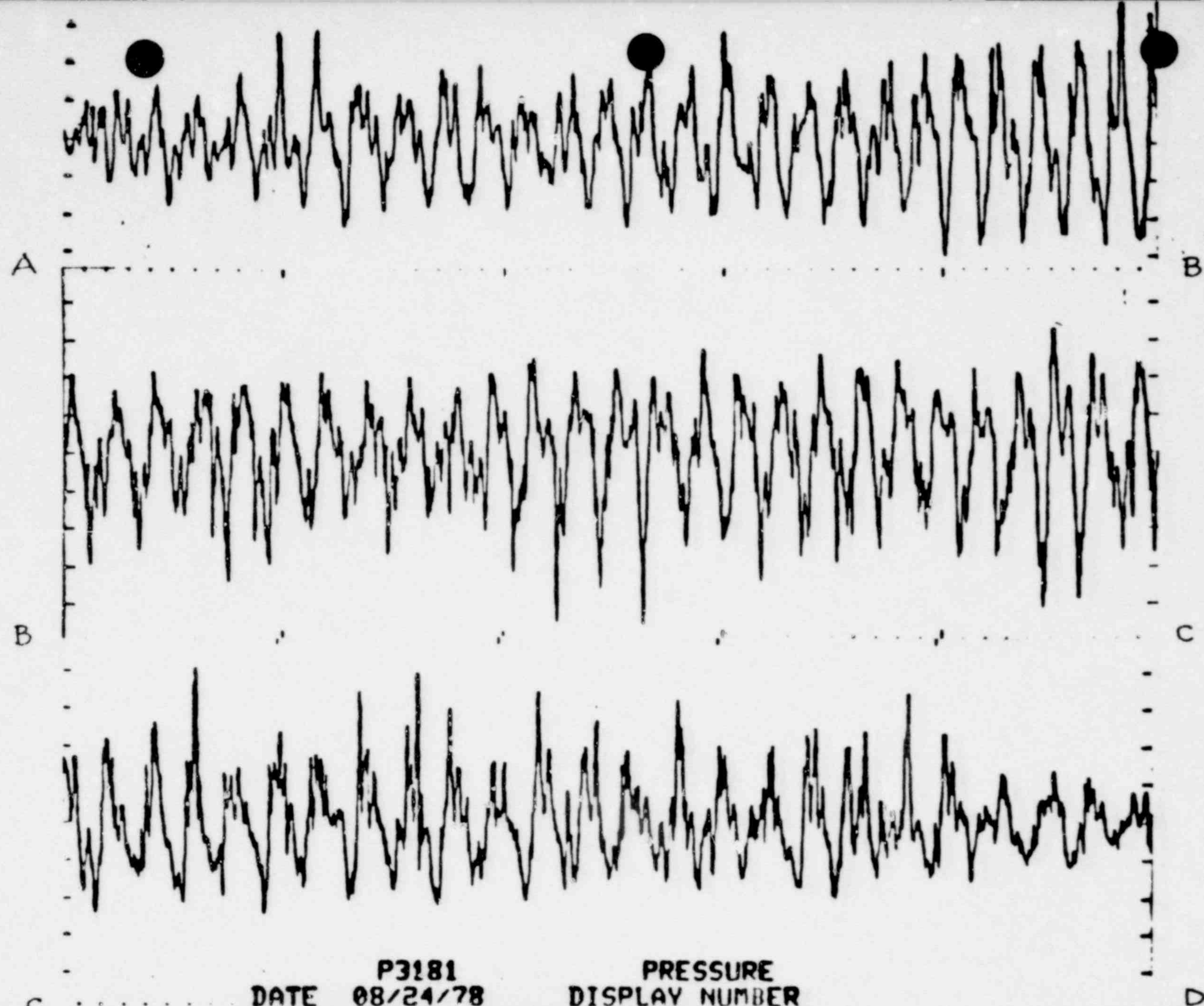
P3181 PRESSURE

DATE 09/01/78 DISPLAY NUMBER 7

M-7 SHORT TERM HISTORIES

1500 042

129



P3181 PRESSURE
DATE 08/24/78 DISPLAY NUMBER
M-8 SHORT TERM TIME HISTORIES

1500 043

130

MARK I CONDENSATION OSCILLATION

DATA APPLICATION

DATA BASE SELECTION

- EXAMINED ALL THE FSTF CO DATA TO SELECT MAXIMUM CO LOADING DATA
- RUN M7 & M8 WERE SELECTED
- THREE DATA SEGMENTS OF MAXIMUM PRESSURE AMPLITUDE FROM M7 & M8 WERE SELECTED AS DATA BASE

1500 044

131

MARK I CONDENSATION OSCILLATION

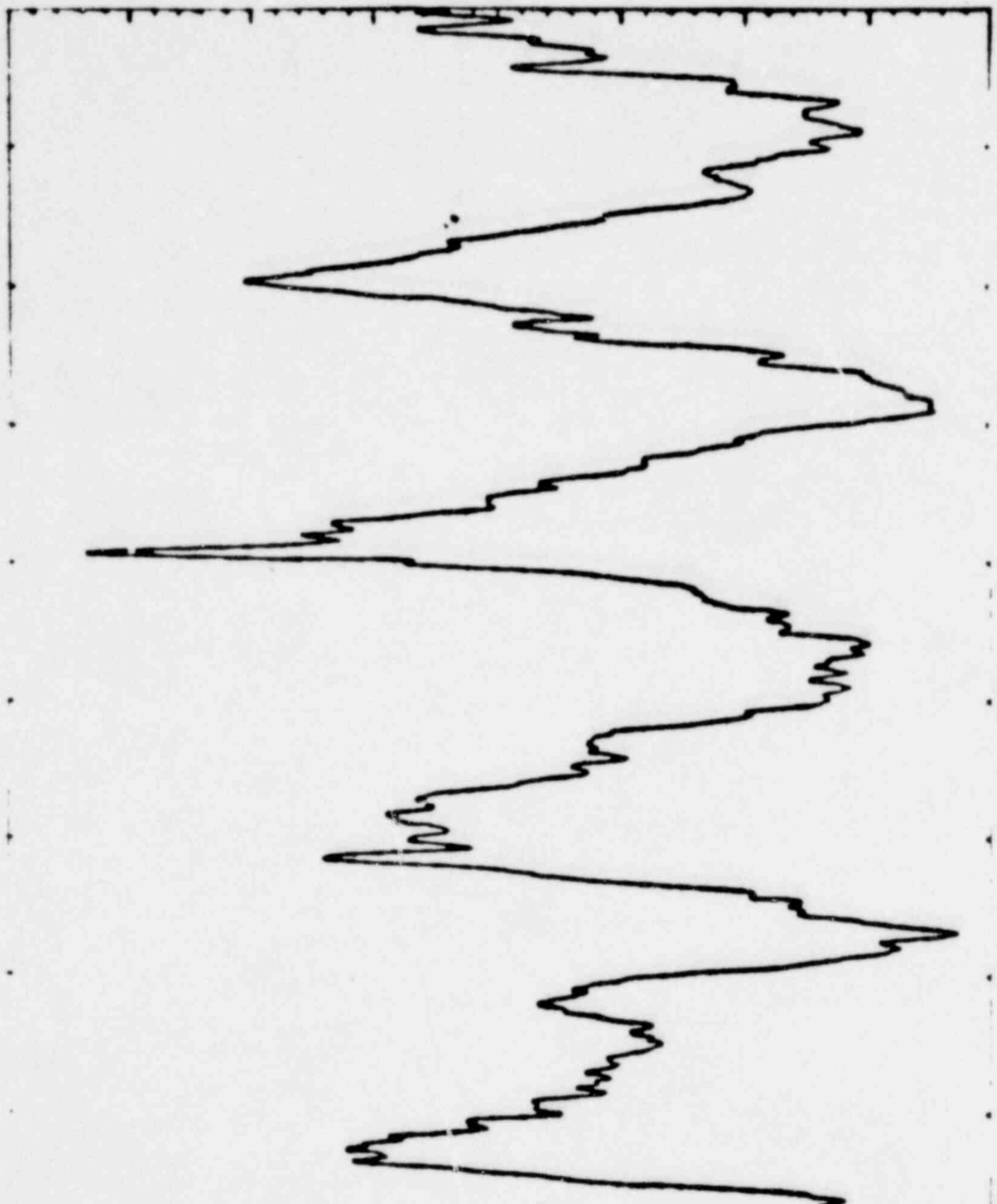
DATA BASE

THREE DATA SEGMENTS SELECTED ARE:

TEST RUN	DURATION	POWER
M8	29-33 SEC	MAXIMUM 4-5 Hz
M8	24-28 SEC	MAXIMUM 5-6 Hz
M7	21-25 SEC	MAXIMUM 6-7 Hz

1500 045

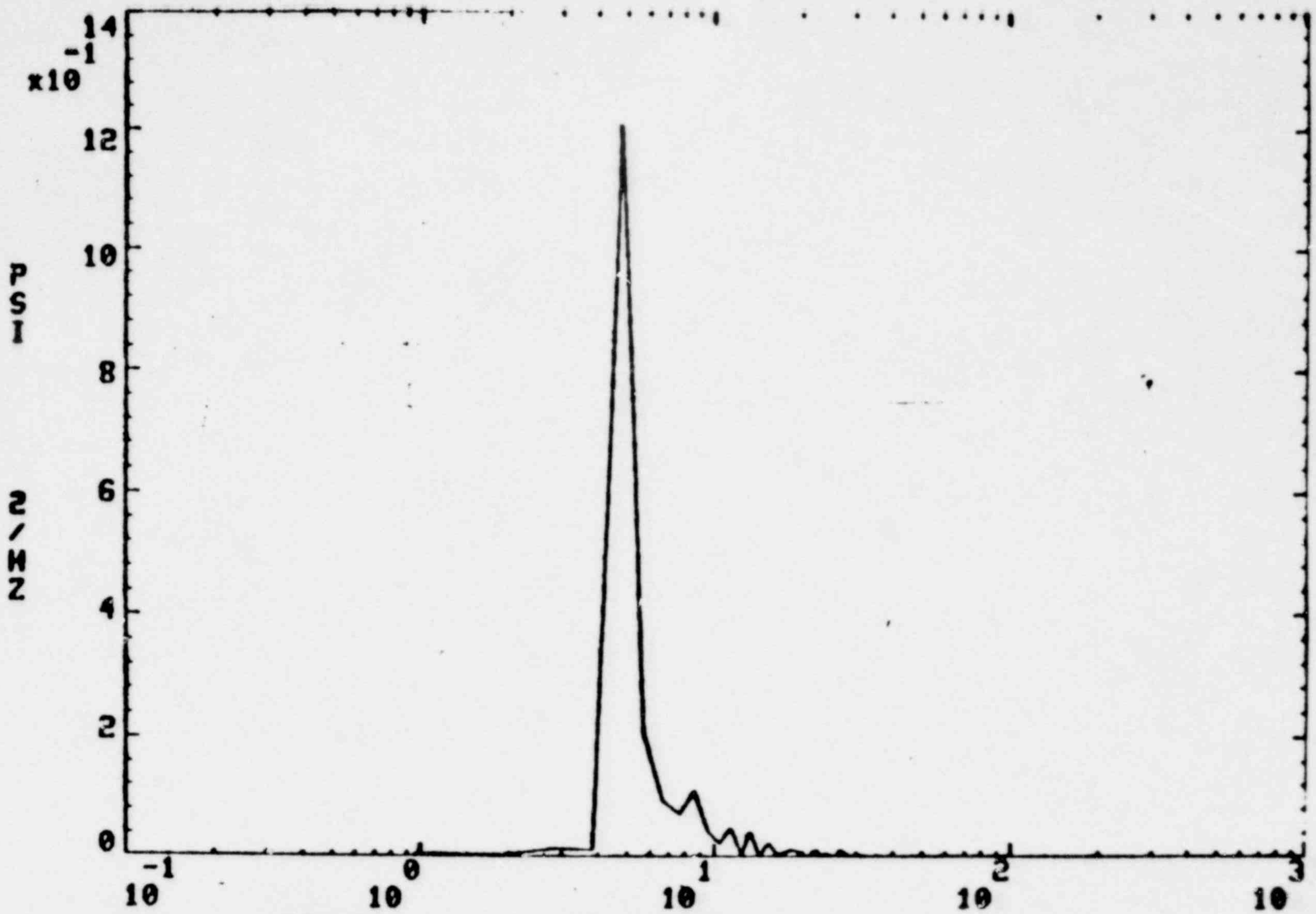
UCS - 07
11/16/79



M8 : DATA SEGMENT 1
AVERAGE PRESSURE

1500 046

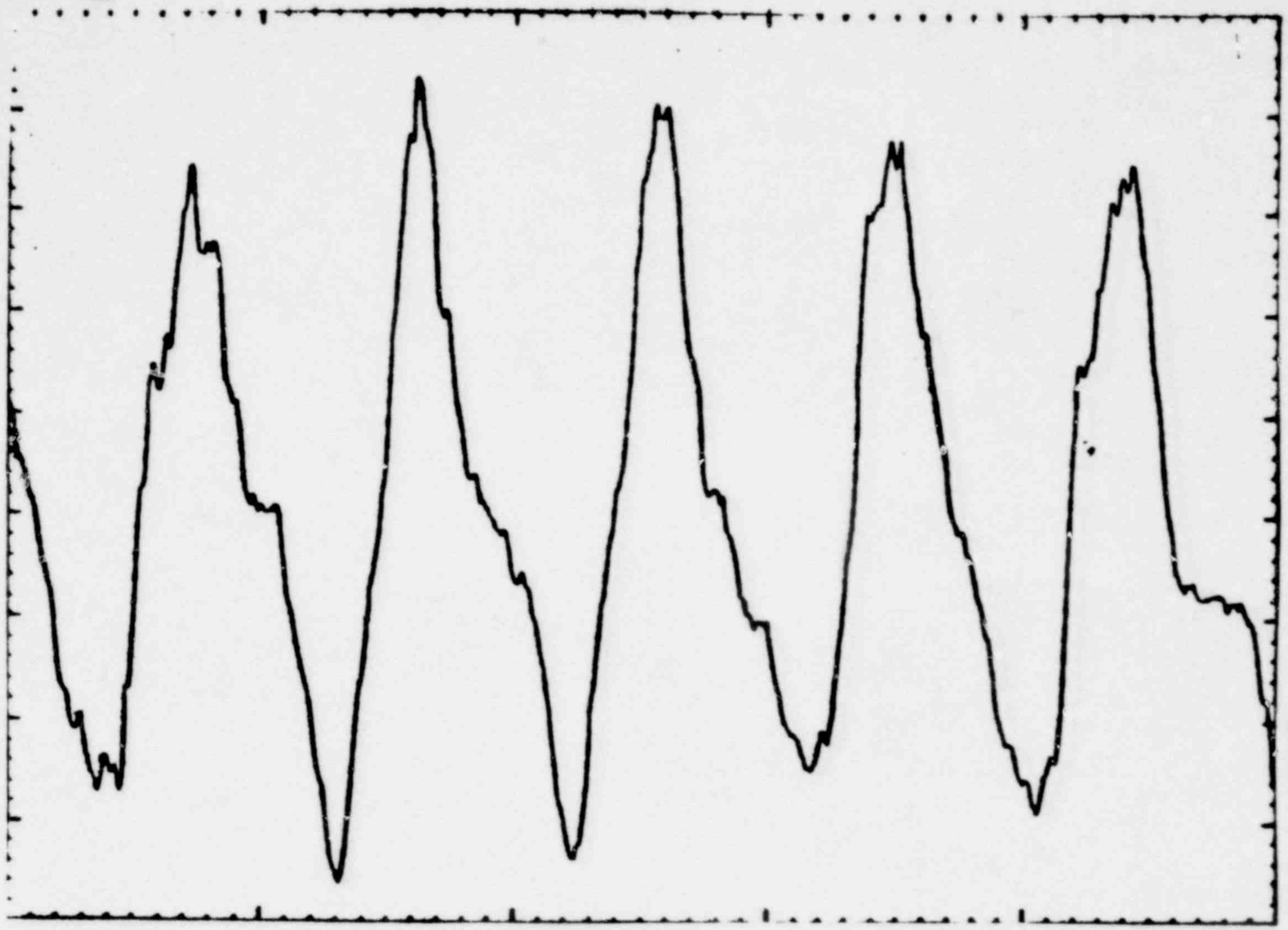
133



P3105 PROPERTIES
DATE 11/22/78 DISPLAY NUMBER
M-8 AVERAGE PRESSURE ANALYSIS

SEGMENT 1

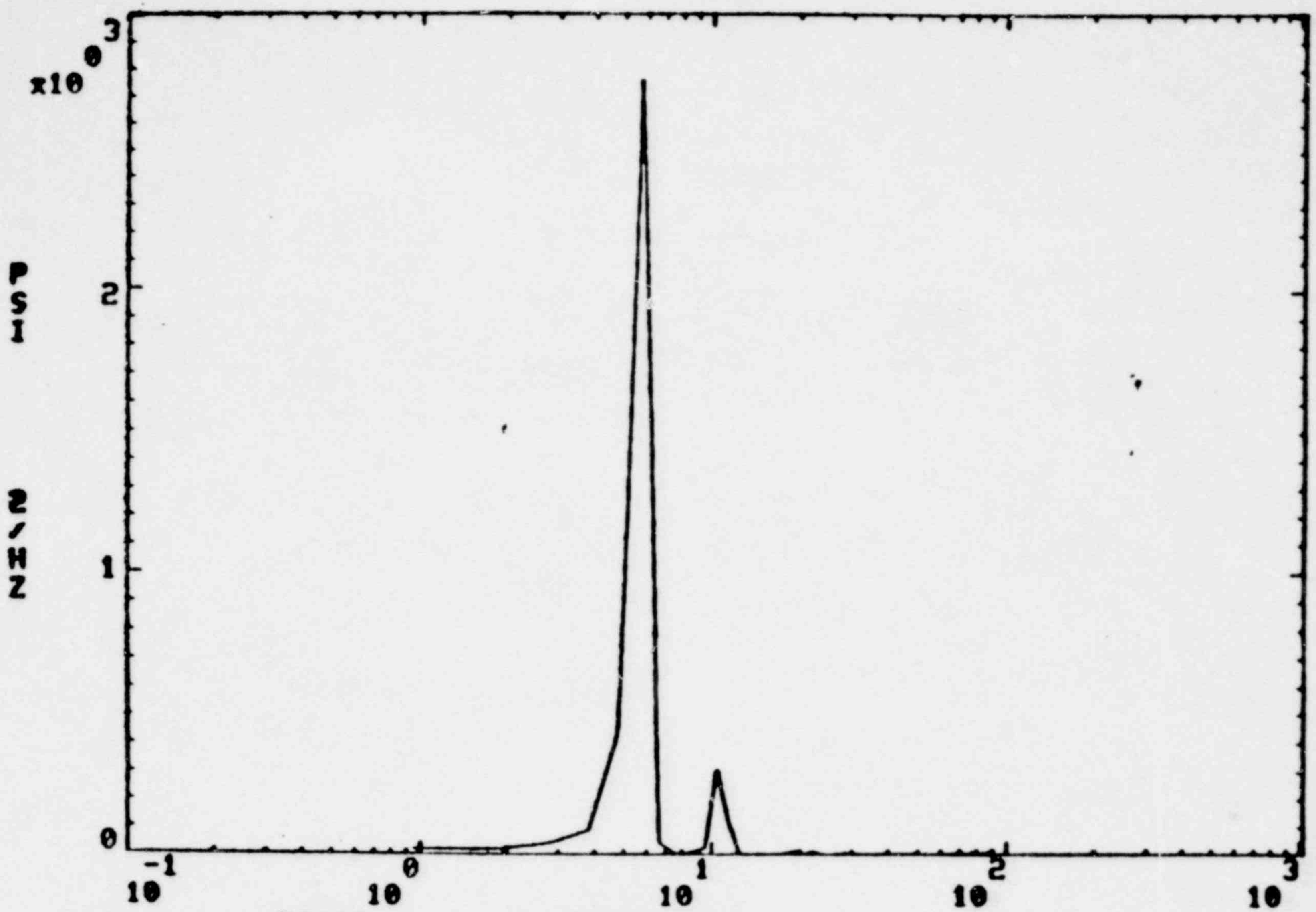
1500 047 134



DATA SEGMENT 2

1500 048

135

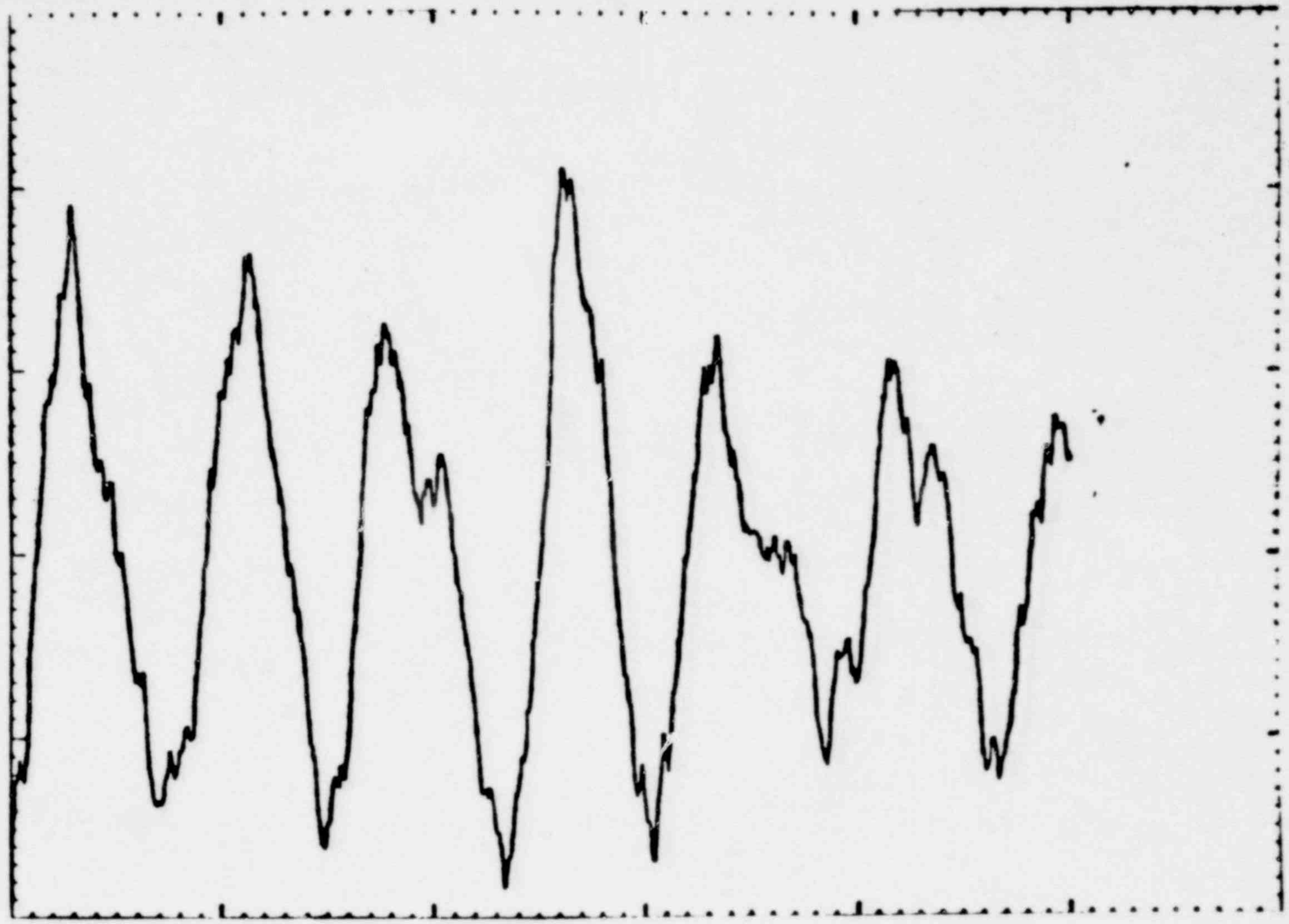


P3105 PROPERTIES
DATE 11/22/78 DISPLAY NUMBER
M-8 AVERAGE PRESSURE ANALYSIS

SEGMENT 2

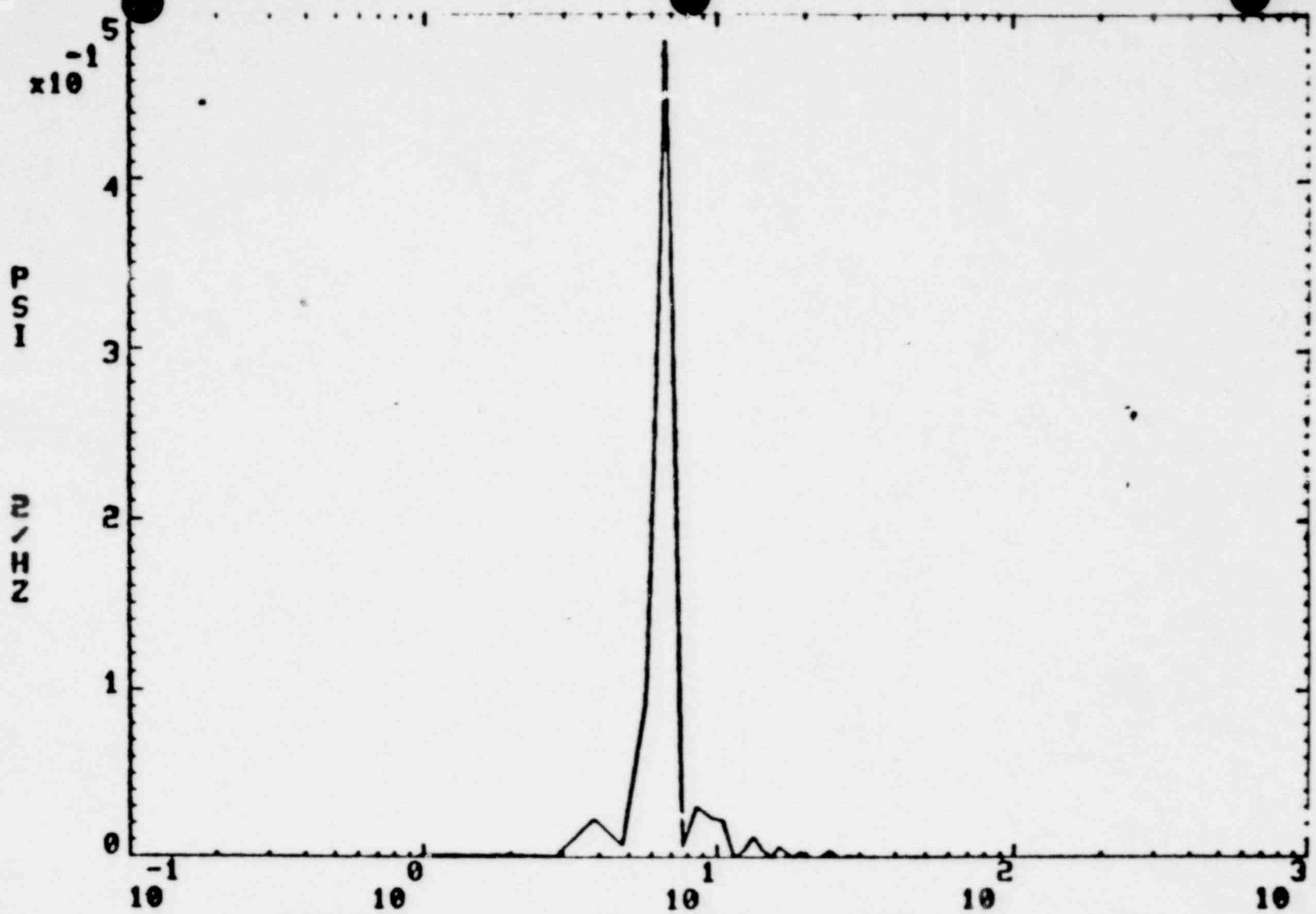
1500 049

136



M-7 DATA SEGMENT 3

AVERAGE PRESSURE



P3105 PROPERTIES PSD HZ
DATE 11/22/78 DISPLAY NUMBER
M-7 AVERAGE PRESSURE ANALYSIS DATA SEGMENT 3

1500 051

MARK I CONDENSATION OSCILLATION

DATA REDUCTION/ANALYSIS

FOR EACH OF THE SELECTED THREE DATA SEGMENTS...

- WALL PRESSURES INTEGRATED
  - MEASURED WALL PRESSURES (24 SENSORS) WERE SPATIALLY INTEGRATED
  - INTEGRATED VERTICAL PRESSURE TIME HISTORY GENERATED
  - OBTAINED TIME HISTORY REPRESENT OVERALL LOADING ON THE TORUS SHELL

- POWER SPECTRAL DENSITY (PSD) CALCULATED
  - PSD OF EACH 1-SECOND SEGMENT WAS GENERATED
  - PSD VALUES WERE AVERAGED OVER THE FOUR SECONDS
  - AMPLITUDE VS. FREQUENCY VALUES WERE COMPILED

- FSTF FSI ACCOUNTED FOR
  - FSI FACTOR AS A FUNCTION OF FREQUENCY OBTAINED
  - COMPILED AMPLITUDE MULTIPLIED WITH FSI FACTOR - RIGID WALL PRESSURES

1500 052

139

MARK I CONDENSATION OSCILLATION

LOAD DEFINITION

- TORUS LOADING DEFINED AS RIGID WALL PRESSURE VS. FREQUENCY
- THREE ALTERNATE FREQUENCY SPECTRA, 4 TO 16 Hz, SPECIFIED
- ALTERNATE SPECTRA BOUND VARIATION OF DOMINANT FREQUENCY WITH TIME OBSERVED DURING THE TESTS
- LOAD DEFINITION:
  - AMPLITUDE VS. FREQUENCY
    - 0 - 50 Hz RANGE
    - INCLUDING ONE SPECTRUM 4 - 16 Hz
  - SPATIAL DISTRIBUTION
    - UNIFORM AXIALLY
    - LINEAR ATTENUATION WITH SUBMERGENCE
  - PLANT UNIQUE ADJUSTMENT FOR POOL-TO-VENT AREA RATIO DEFINED
  - AMPLITUDE COMPONENTS SPECIFIED AS STEADY STATE LOADING

1500 053

140

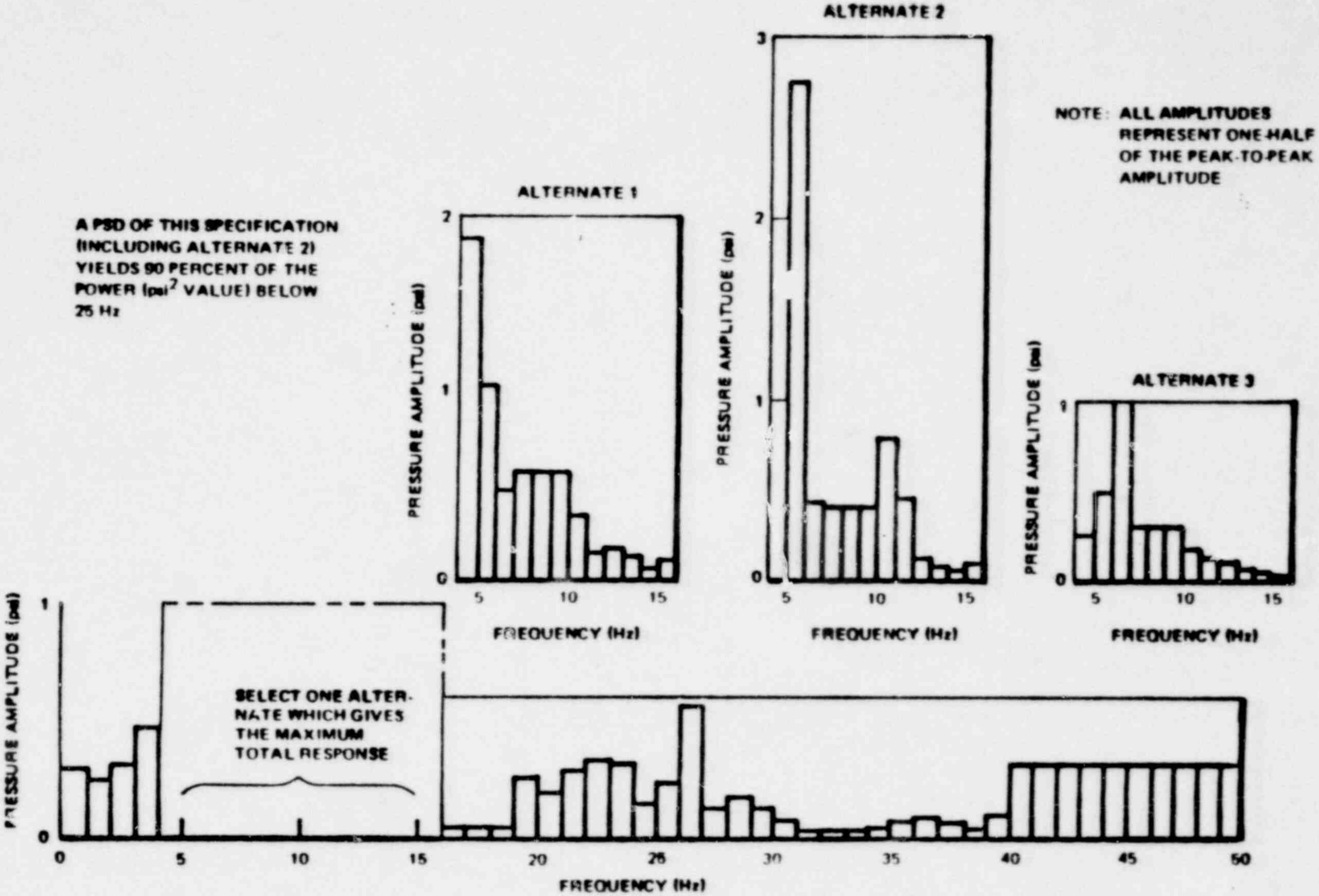


Figure 4.4.1-1. Condensation Oscillation Baseline Rigid Wall Pressure Amplitudes on Torus Shell Bottom Dead Center

1500 054

144

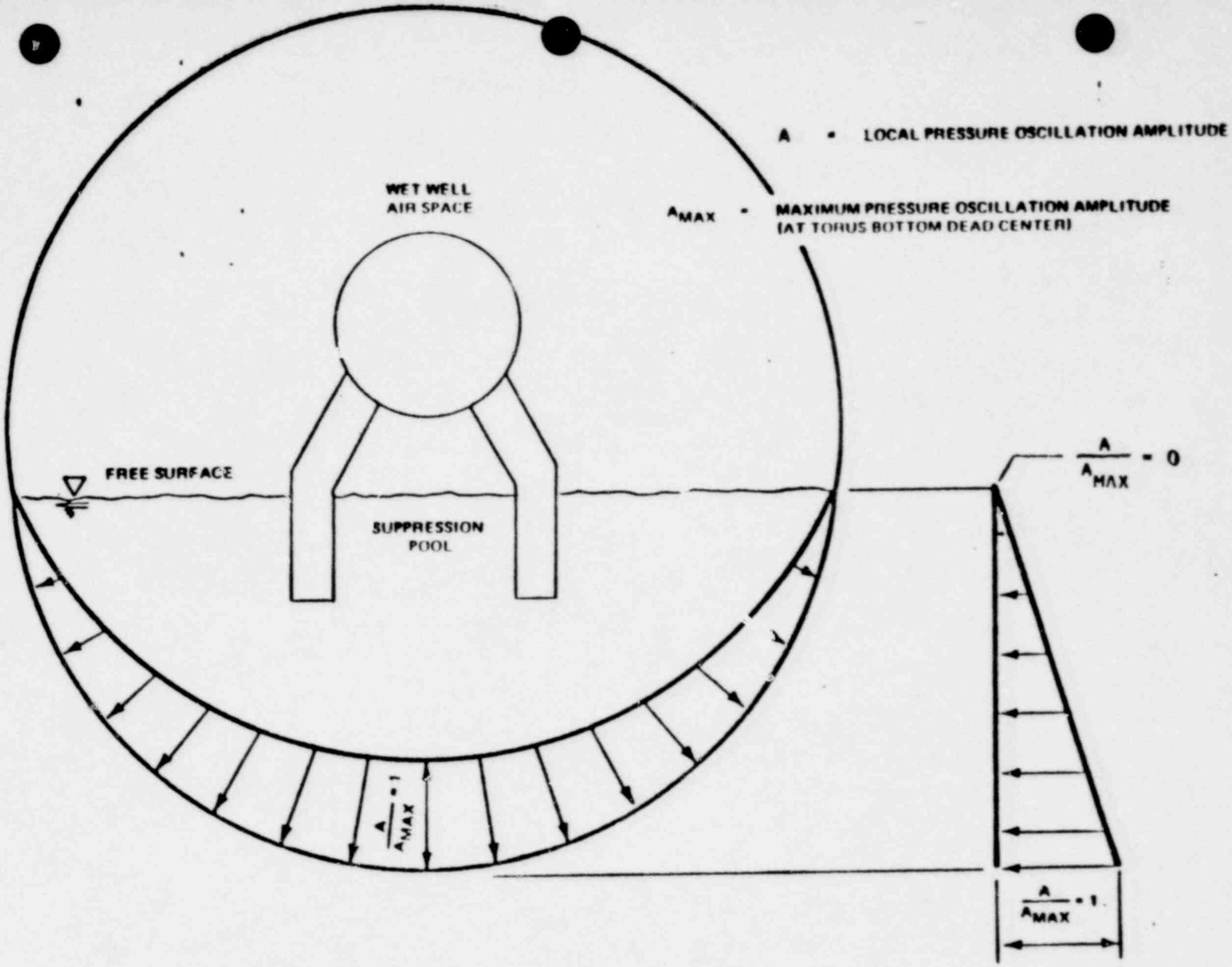


Figure 4.4.1-2. Mark I Condensation Oscillation - Torus Vertical Cross Sectional Distribution for Pressure Oscillation Amplitude

1500 055

142

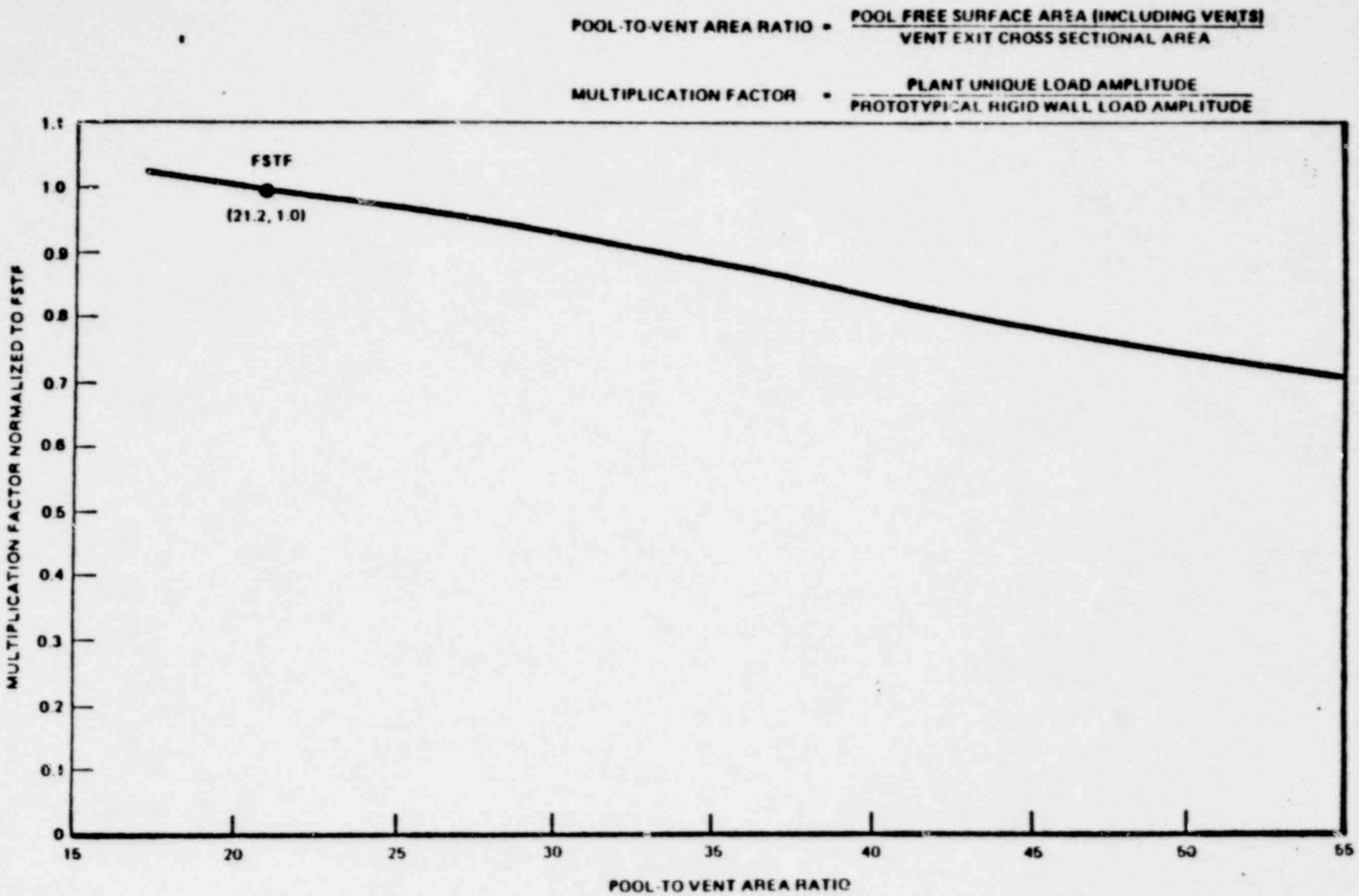


Figure 4.4.1-3. Mark I Condensation Oscillation - Multiplication Factor Versus Pool-to-Vent Area Ratio for Plant Unique Load Determination

1500 056

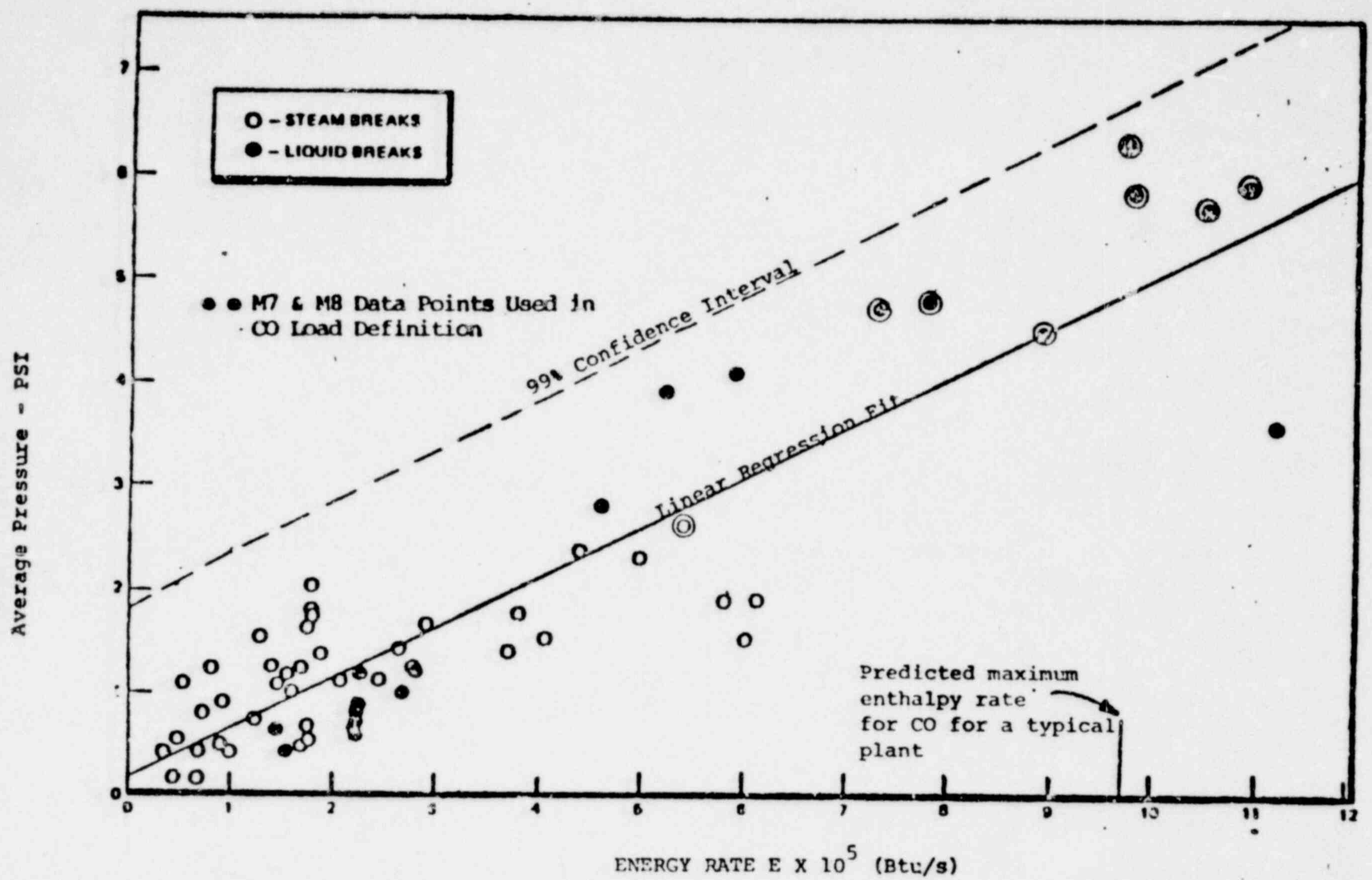


Figure 1. Average Amplitude of 24 Wetwell Transducers vs Energy Rate

1500 057

1944

MARK I CONDENSATION OSCILLATION

SUMMARY

IN DEVELOPING THE LOAD DEFINITON...

- FULL SCALE TEST DATA EMPLOYED
- DATA SEGMENTS OF MAXIMUM PRESSURE AMPLITUDE FORMED THE DATA BASE
- NO CREDIT FOR AMPLITUDE AND FREQUENCY VARIATION WITH TIME (OBSERVED DURING THE TEST) WAS TAKEN
- FSTF FSI EFFECTS ACCOUNTED FOR
- CO LOAD DEFINITON CONSERVATIVELY FORMULATED

BRENNEN

CONDENSATION OSCILLATIONS + CHUGGING

CONDENSATION OSCILLATIONS

- HIGH VENT FLOW RATES
- CONTINUOUS PERIODIC OSCILLATIONS
- SYNCHRONIZATION

CHUGGING

- LOWER VENT FLOW RATES
- EVENTS 1→2 SECONDS APART. TYPICALLY CONSIST OF SMALL NUMBER OF IMPULSES FOLLOWED BY DECAYING RING-OUT.
- RANDOMNESS IN FORM, OCCURENCE AND AMPLITUDE
- LESSER DEGREE OF SYNCHRONIZATION.

LOADS

- TORUS SHELL LOADS IMPARTED TO STRUCTURE BY WATER
- LOADS ON DOWNCOMERS
- LOADS ON OTHER STRUCTURES IN WETWELL POOL
- VENT SYSTEM PRESSURE LOADS (STEAM)

1500 059

146

DATA BASE FOR LOAD DEFINITION

FULL SCALE TEST FACILITY (FSTF) BLOWDOWN RESULTS

- PROTOTYPICAL EXCEPT FOR FSI EFFECTS ON WATER IMPOSED LOADS

- THEREFORE FSI MODELLING REQUIRED TO

  (i) EXTRACT FSTF FSI EFFECTS FROM DATA BASE

  (ii) INSERT PLANT UNIQUE FSI EFFECTS

1500 060

FSTF Test Matrix

Test * Number	Date Performed	BREAK		Wetwell Nominal Initial Conditions		
		Size	Type	Submergence	Temperature	Pressure
M1	5/5/78	Small	Steam	3 ft 4 in	70°F	0 psig
M2	5/12/78	Medium	↓	↓	↓	↓
M3	5/25/78	Small	Liquid	↓	↓	↓
M4	6/17/78	↓	Steam	↓	↓	5 psig
M5	6/26/78	↓	↓	↓	120°F	0 psig
M6	7/6/78	↓	↓	1 ft 6 in	↓	↓
M9	7/11/78	↓	↓	4 ft 6 in	70°F	↓
M10**	7/27/78	↓	↓	3 ft 4 in	↓	↓
M7	8/10/78	Large	↓	↓	↓	↓
M8	8/22/78	↓	Liquid	↓	↓	↓

* Shown in order of performance

** Air sensitivity test performed with vacuum breaker replaced with rupture disc.

Condensation Oscillation Data Base:

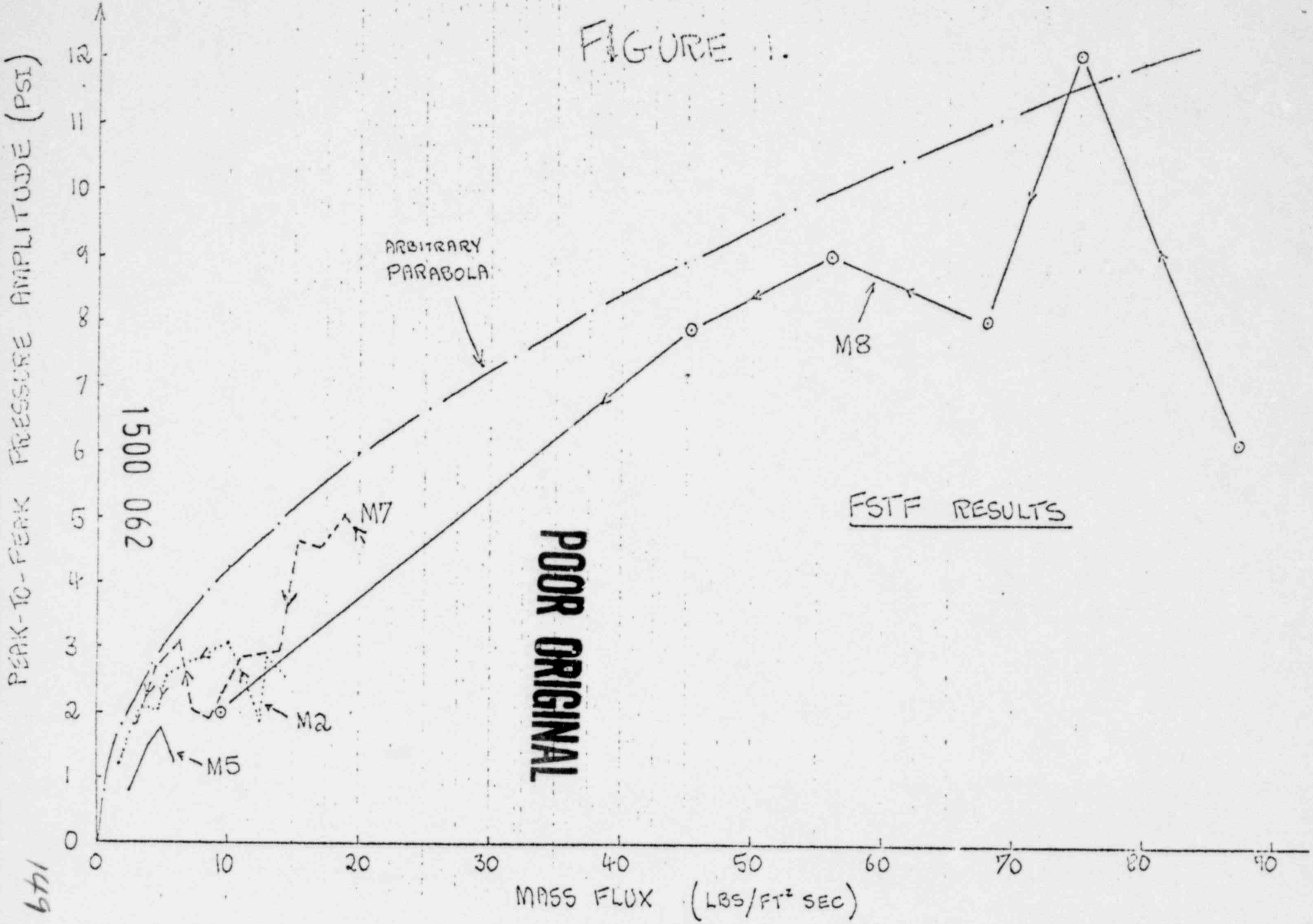
M8 – LARGEST VENT FLOW RATE (DBA LEVEL)

– MAGNITUDE OF CO MUCH LARGER THAN ANY OTHER TEST

Chugging Data Base:

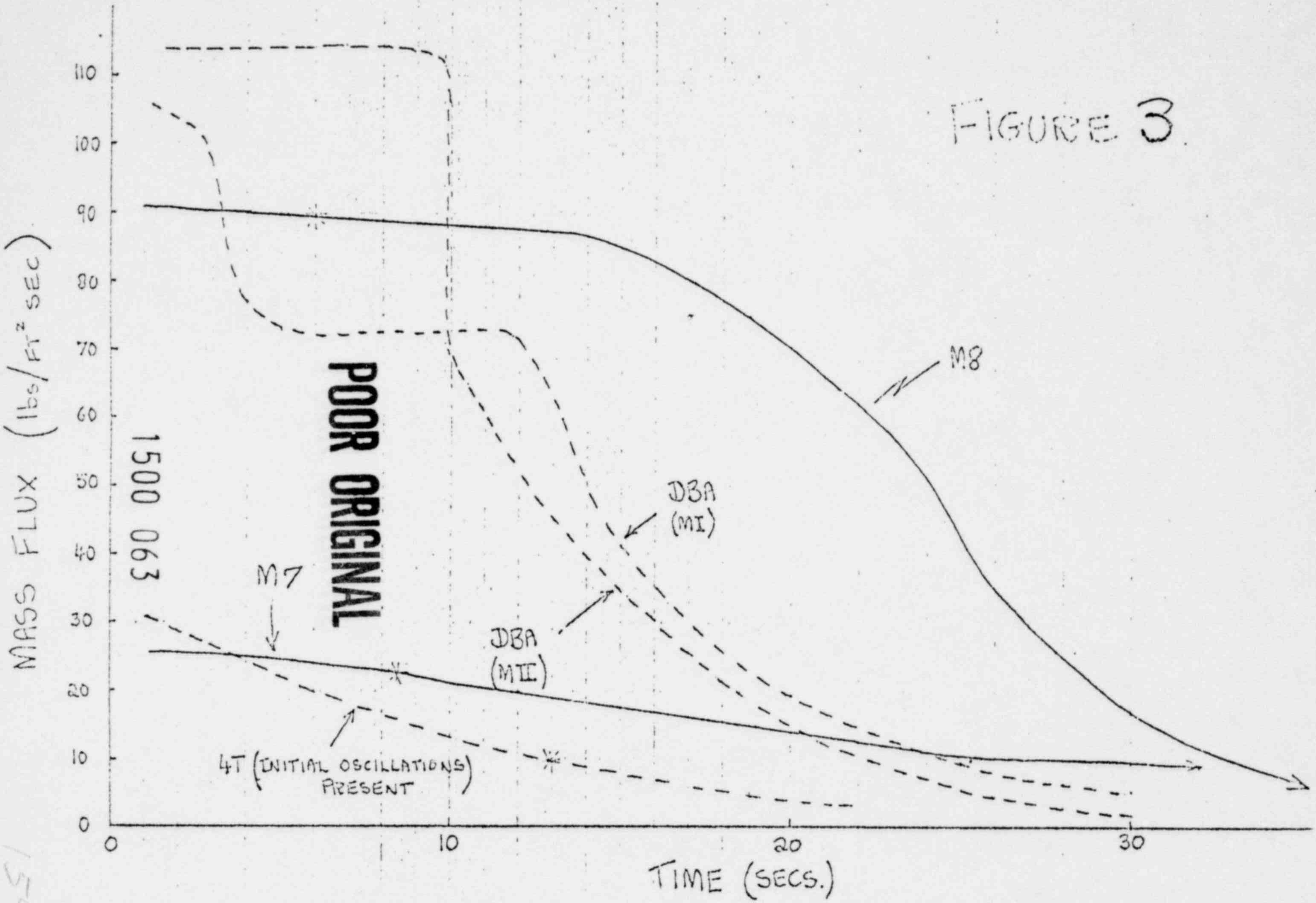
M1, M4, M9, M10 – LITTLE OR NO CHUGGING IN OTHER BLOWDOWNS

FIGURE 1.



POOR ORIGINAL

1500 062

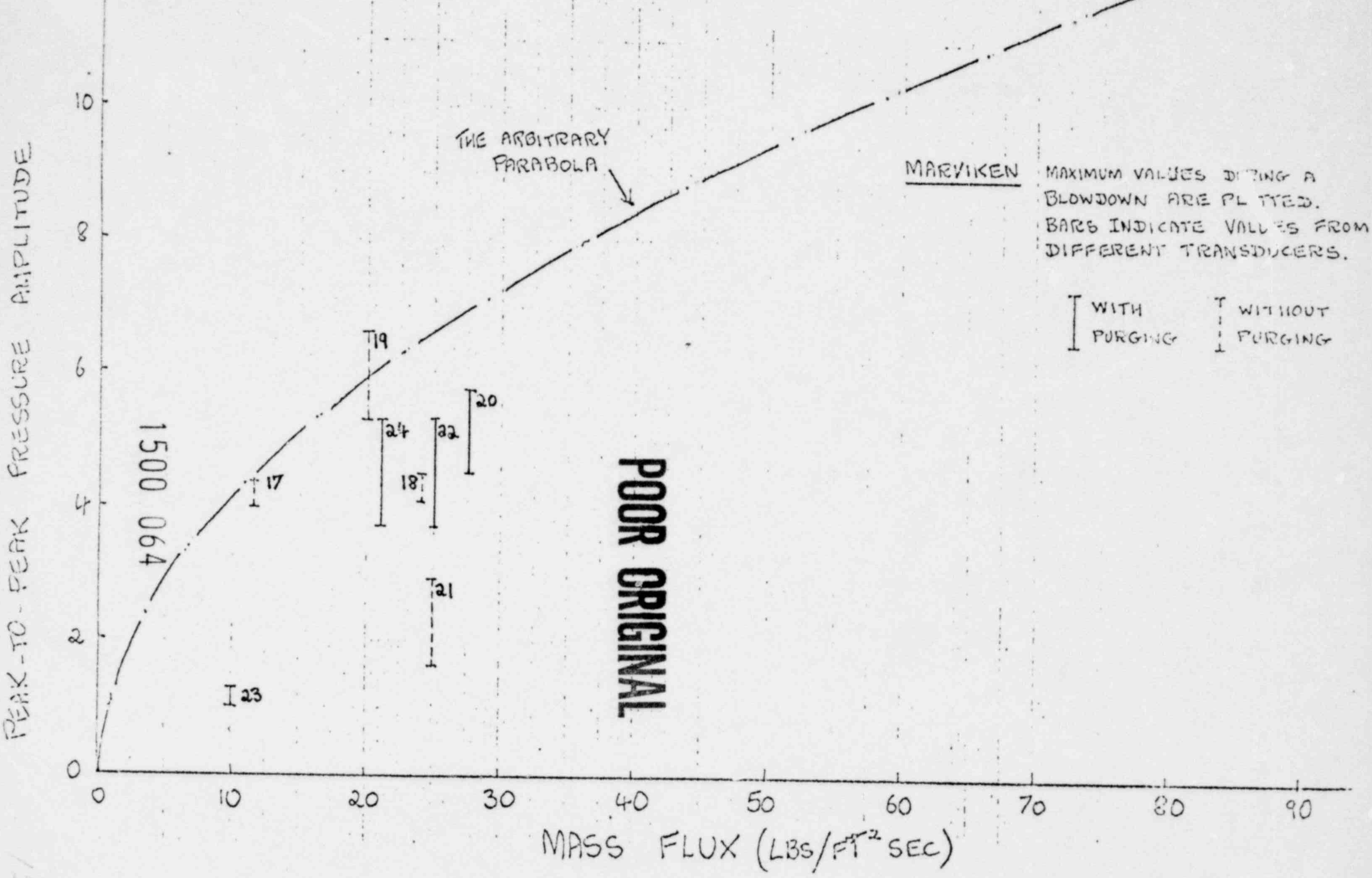


POOR ORIGINAL

1500 063

150

FIGURE 4

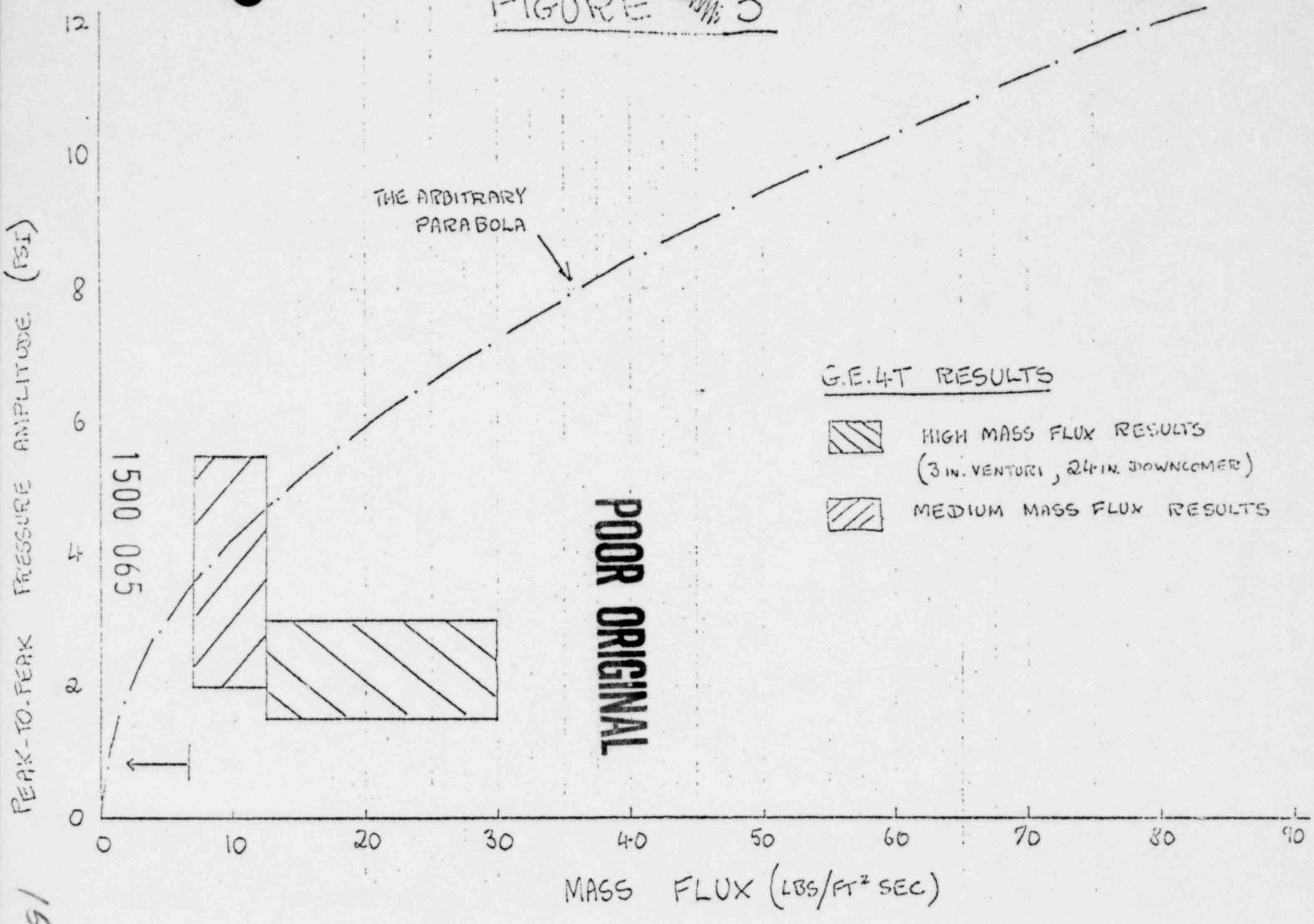


POOR ORIGINAL

1500 064

151

FIGURE 5

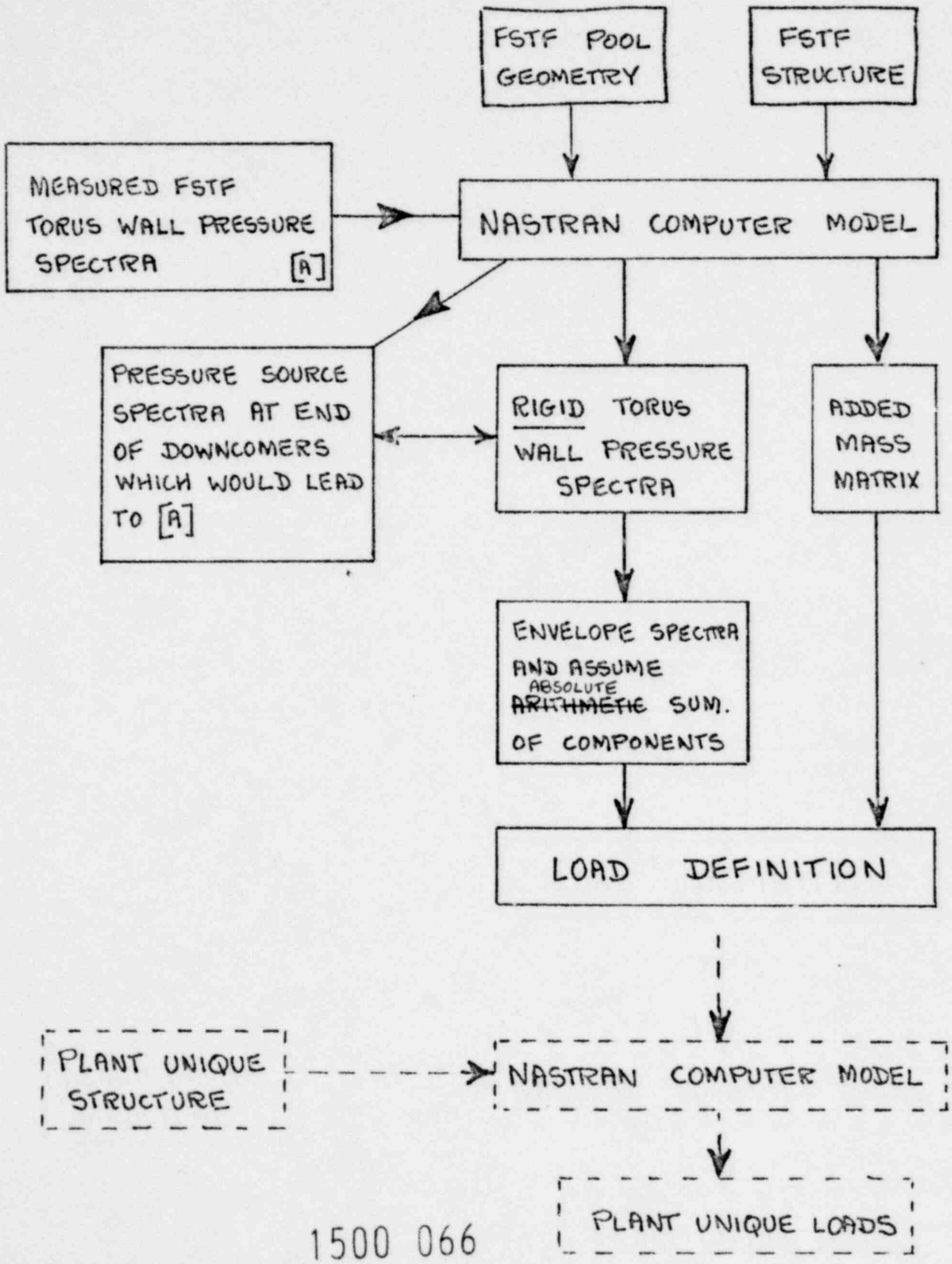


1500 065

POOR ORIGINAL

152

FSI METHODOLOGY FOR TORUS SHELL PRESSURE LOADS*



1500 066

* BASED ON NEDE 24645P FOR CONDENSATION OSCILLATION LOADS

CONDENSATION OSCILLATION LOAD DEFINITION

DURATION:

BREAK SIZE	ONSET TIME AFTER BREAK	DURATION
DBA	5 SECS.	30 SECS.
IBA	NOT DEFINED (CHUGGING LOADS BOUNDING)	
SBA		

TORUS SHELL LOADS

- SPATIAL DISTRIBUTION ← LINEAR WITH DEPTH
  — SYNCHRONOUS AROUND TORUS
- SPECTRA FOR BOTTOM CENTER PRESSURE

1500 067

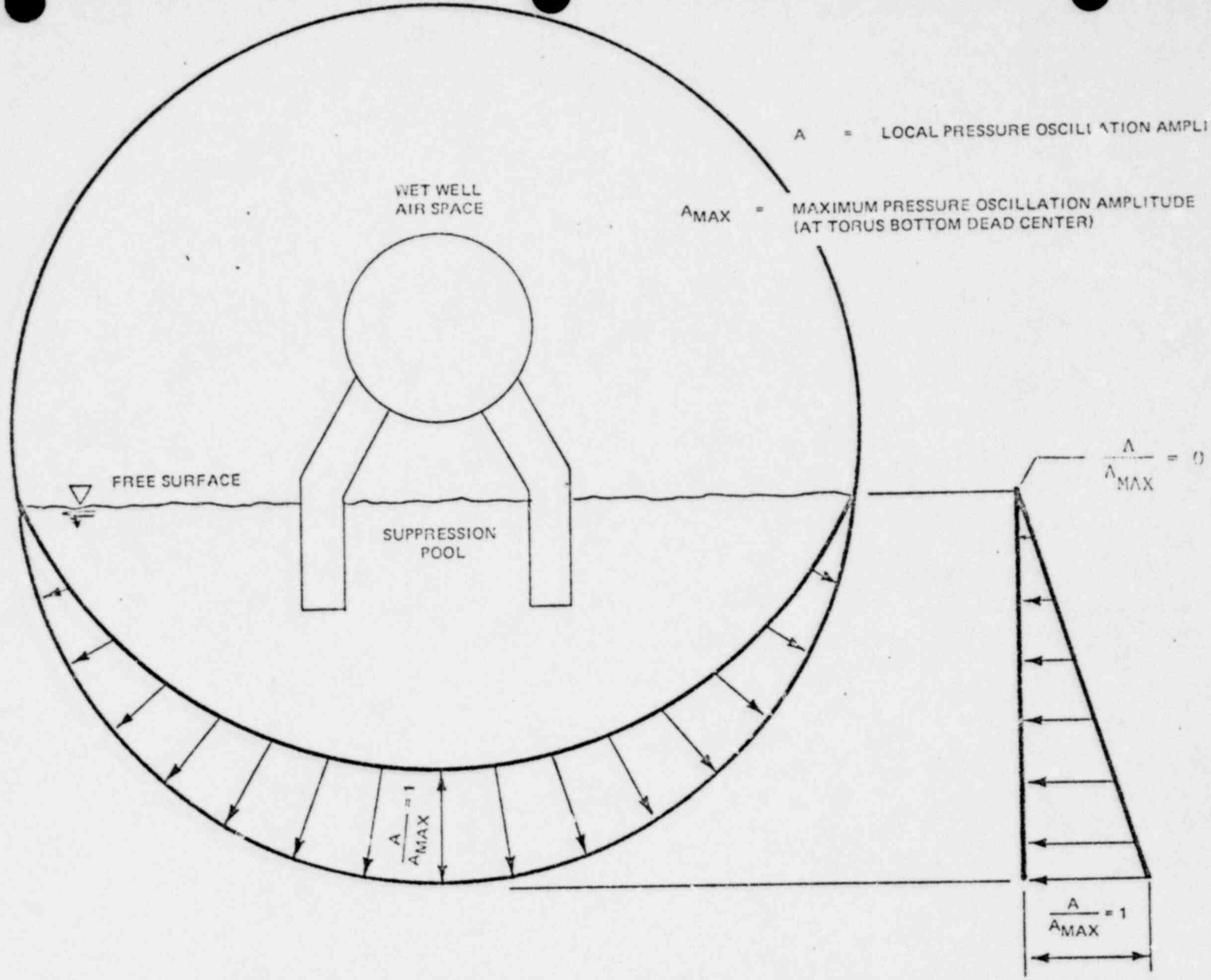


Figure 4.4.1-2. Mark I Condensation Oscillation - Torus Vertical Cross Sectional Distribution for Pressure Oscillation Amplitude

1500 068

155

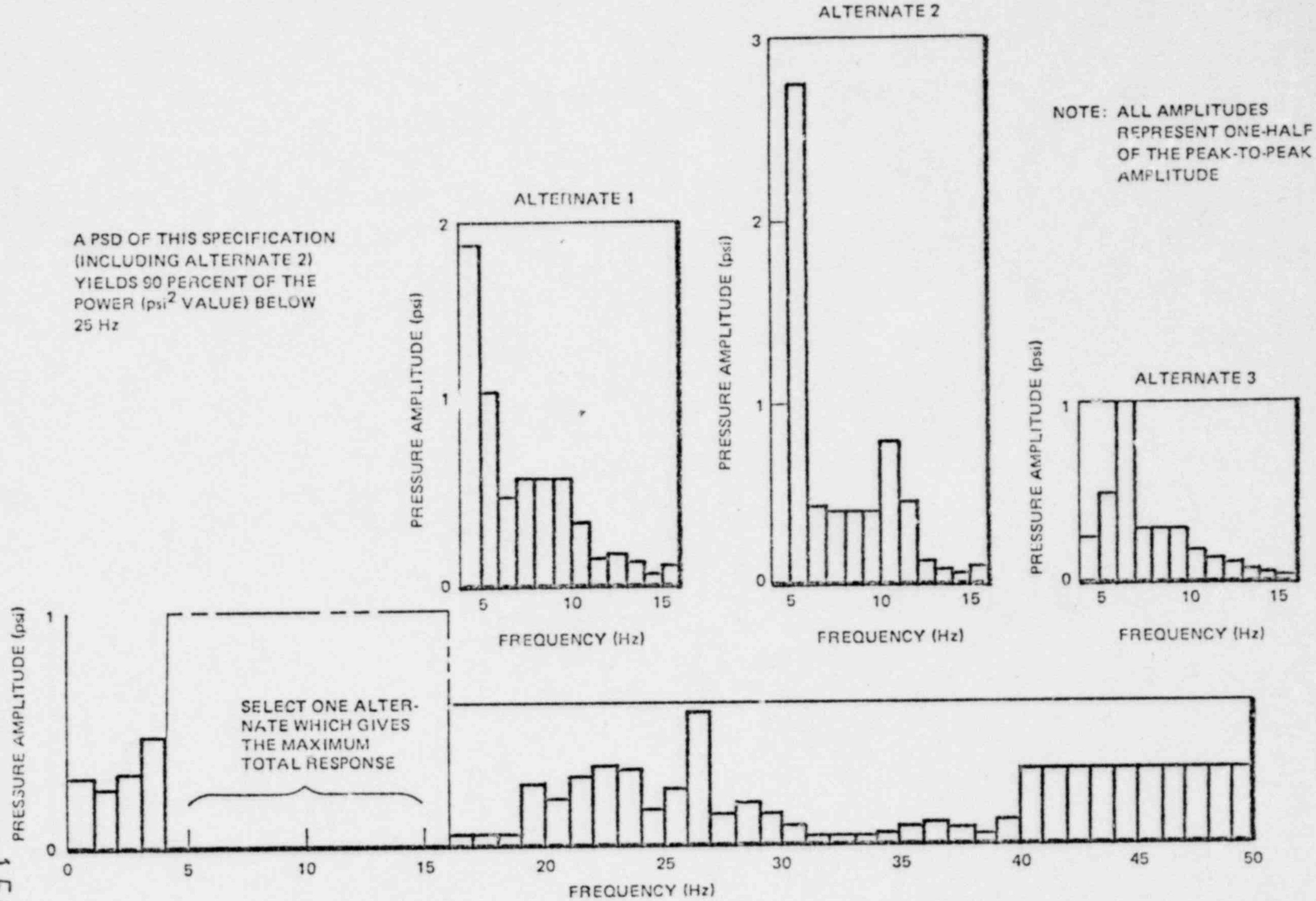


Figure 4.4.1-1. Condensation Oscillation Baseline Rigid Wall Pressure Amplitudes on Torus Shell Bottom Dead Center

1500 069

156

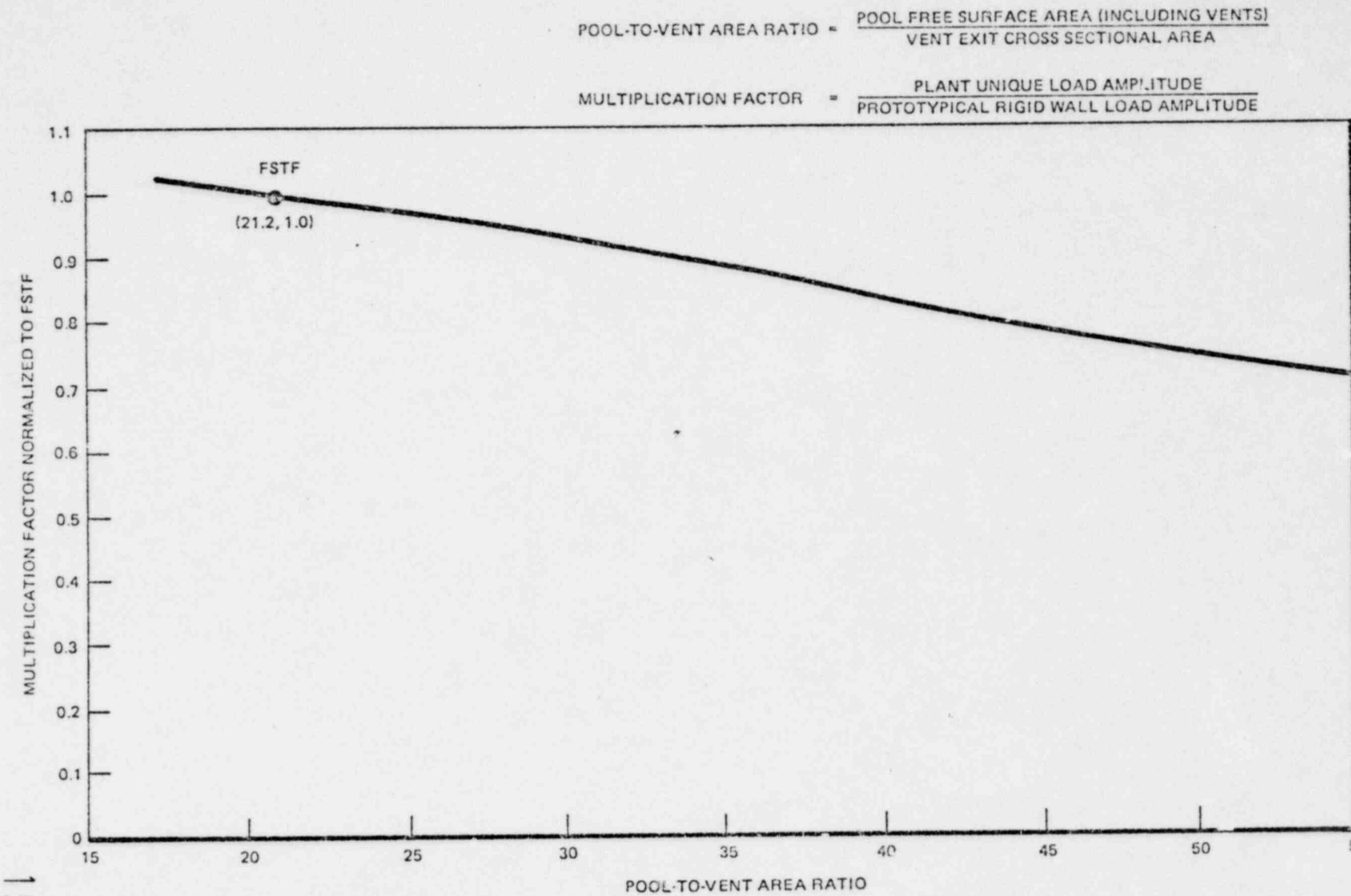


Figure 4.4.1-3. Mark I Condensation Oscillation - Multiplication Factor Versus Pool-to-Vent Area Ratio for Plant Unique Load Determination

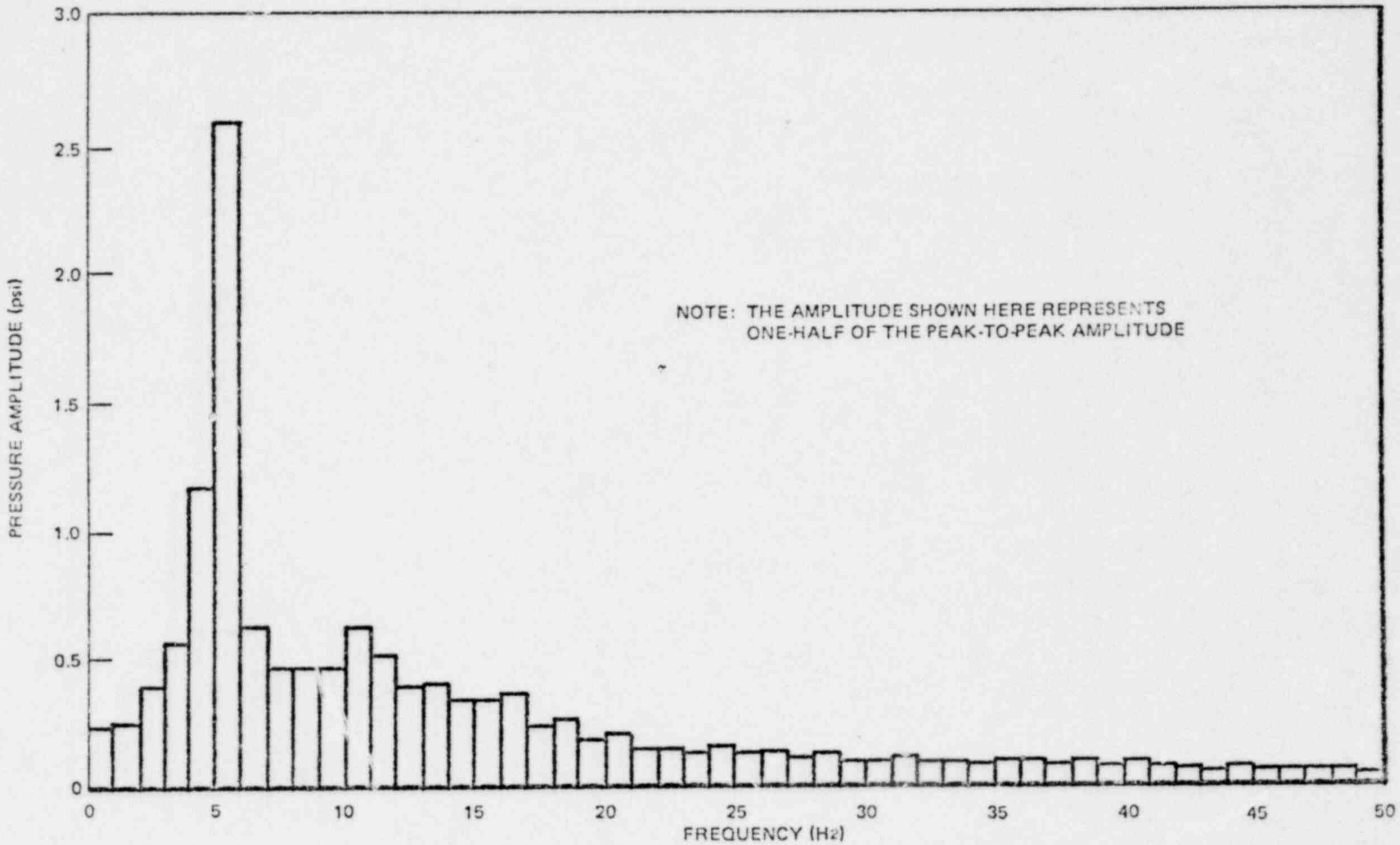
1500 070

VENT SYSTEM LOADS FOR CONDENSATION OSCILLATIONS

Main Vent and Vent Header	
Amplitude	± 2.5 psid.
Frequency Range	The frequency producing the maximum response in the range of 4 to 8 Hz.
Forcing Function	Sinusoidal.
Spatial Distribution	Uniform

Downcomers	
Amplitude Versus Frequency	Values given in Table 4.4.4-1 (Also shown in Figure 4.4.4-1).
Total Response	Resulting responses from applying the amplitude at each frequency given in Table 4.4.4-1 are to be summed.
Spatial Distribution	Uniform.

1500 071



TOTAL = 13.93 Psi

Figure 4.4.4-1. Condensation Oscillation Pressure Amplitudes in the Downcomers

1500 072

159

CHUGGING LOAD DEFINITION

DURATION:

BREAK SIZE	ONSET TIME AFTER BREAK	DURATION
DBA	35 SECS.	30 SECS.
IBA	5 SECS.	900 SECS.
SBA	300 SECS.	900 SECS.

TORUS SHELL LOADS

- SPATIAL DISTRIBUTION — DEFINE A SYMMETRIC AND AN ASYMMETRIC GLOBAL LOAD AS CHARACTERISTIC CASES
- TEMPORAL DISTRIBUTION — REPEATED APPLICATION OF EVENTS OF GIVEN PRESSURE SPECTRA.
  — PRE-CHUG AND POST-CHUG
  — SUPER-POSITION NOT REQUIRED.

1500 073

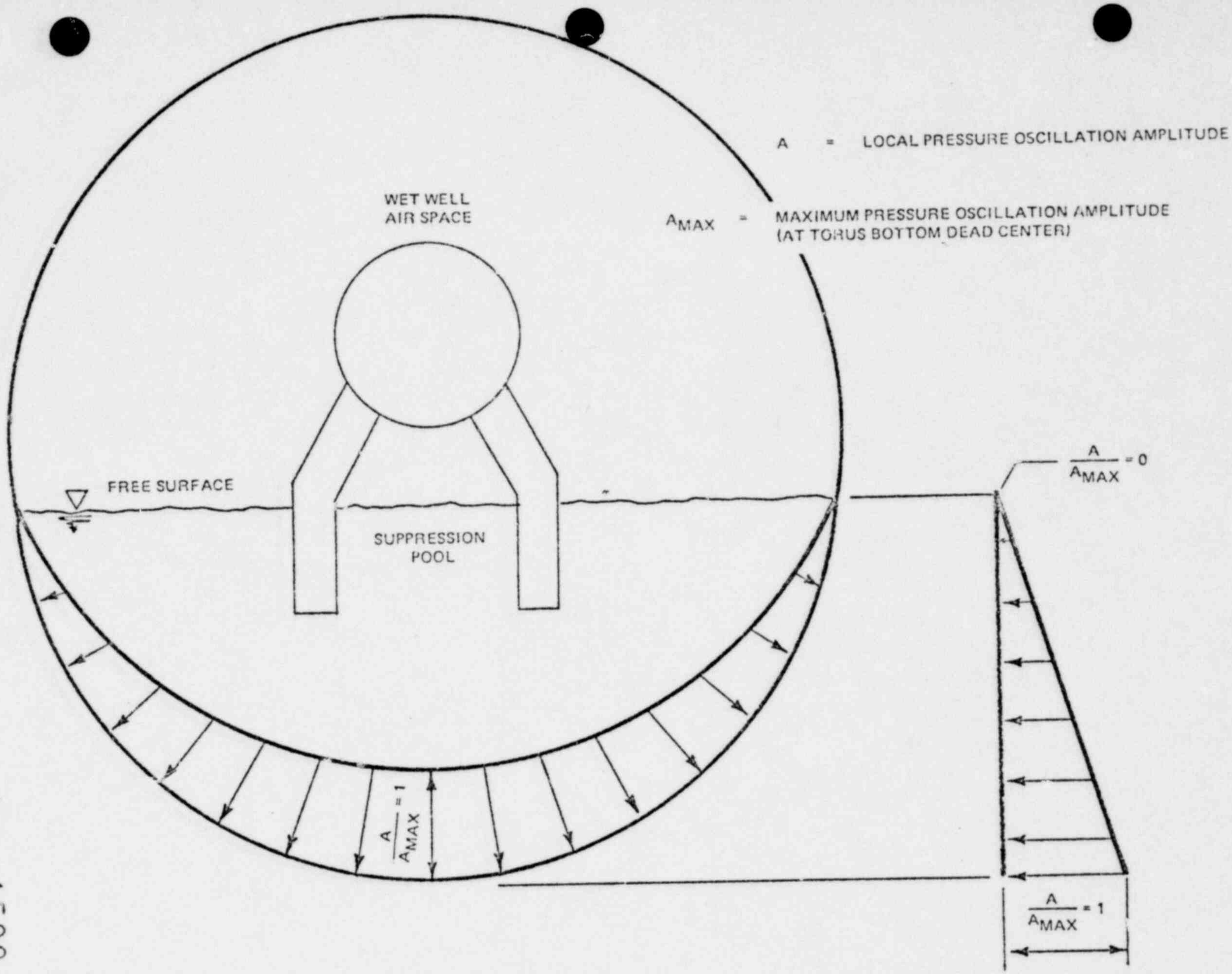


Figure 4.5.1-2. Mark I Chugging-Torus Vertical Cross Sectional Distribution for Pressure Amplitude

1500 074

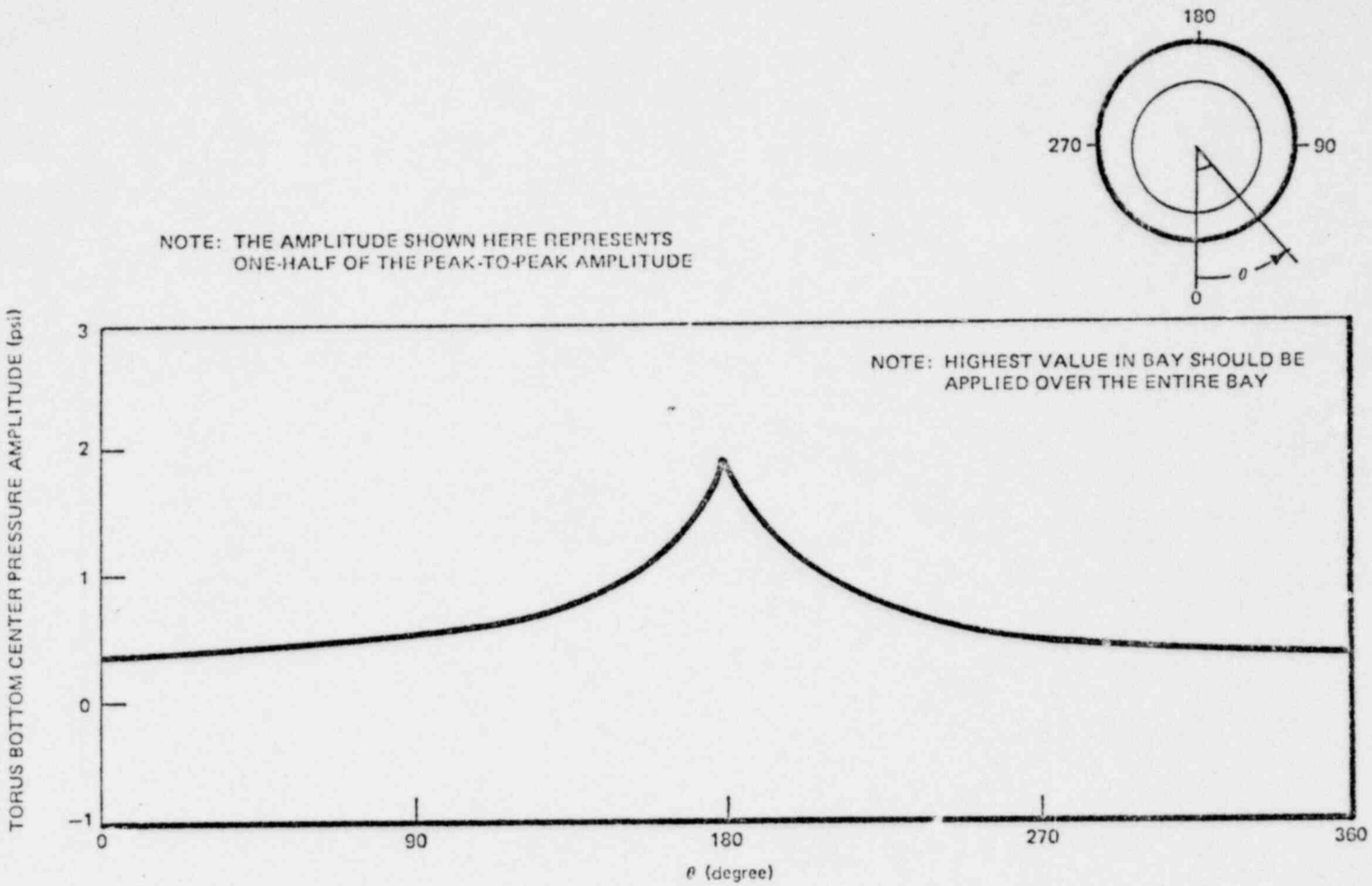


Figure 4.5.1-3. Mark I Chugging - Torus Asymmetric Circumferential Distribution for Pressure Amplitude

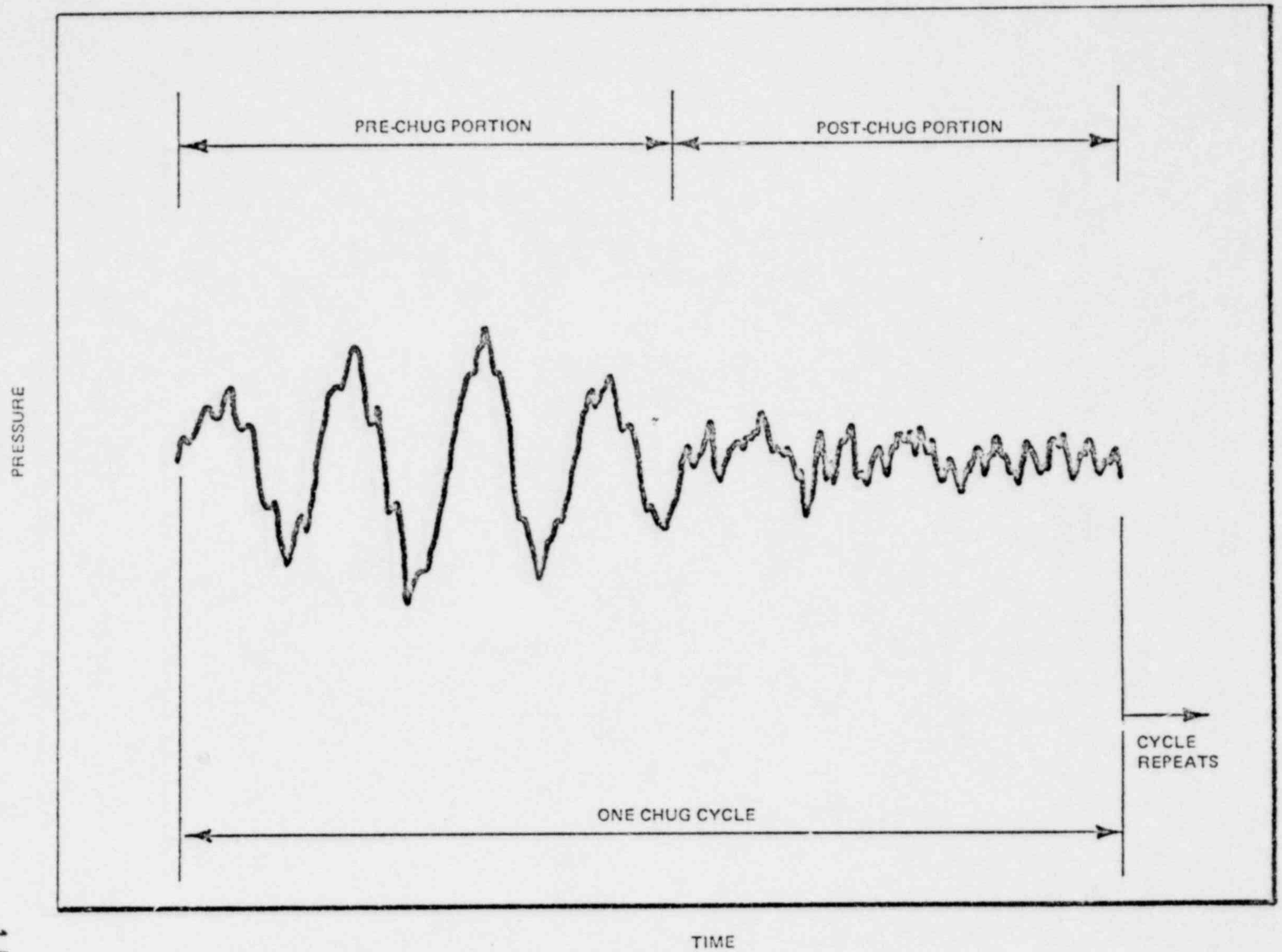


Figure 4.5.1-1. A Typical Chug Average Pressure Trace on the Torus Shell

1500 076

Pre-Chug Load

Amplitude and Circumferential Distribution	Two cases shall be evaluated independently: Symmetric Distribution ±2.0 psi uniform axially along the torus centerline at bottom dead center. Asymmetric Distribution Values shown in Figure 4.5.1-3.
Vertical Cross Section Distribution	Linear Attenuation with submergence along the wetted perimeter as shown in Figure 4.5.1-2.
Frequency	The frequency producing the maximum response in the range from 6.9 to 9.5 Hz. ← deter[illegible] str[illegible]
Pre-Chug Cycle Duration	0.5 seconds every 1.4 seconds for the appropriate total duration defined in Table 4.5.1-1.

These loads are to be applied about the local static pressure at the appropriate times in the blowdown (see Table 4.5.1-1).

1500 077

164

Post-Chug Load

Amplitude Versus Frequency	Values given in Table 4.5.1-2 (Also shown in Figure 4.5.1-4).
Total Response	Resulting steady state responses from applying the amplitude at each frequency given in Table 4.5.1-2 are to be summed.
Spatial Distribution	Uniform axially along the torus centerline. Linear attenuation with submergence along the wetted perimeter at the torus cross section as shown in Figure 4.5.1-2.
Post-Chug Cycle Duration	0.5 seconds every 1.4 seconds for the appropriate total duration defined in Table 4.5.1-1. Pre-chug and post-chug evaluations need not be combined.

These loads are to be applied about the local static pressure at the appropriate times in the blowdown (see Table 4.5.1-1).

NOTE: THE AMPLITUDE SHOWN HERE REPRESENTS ONE-HALF OF THE PEAK-TO-PEAK AMPLITUDE

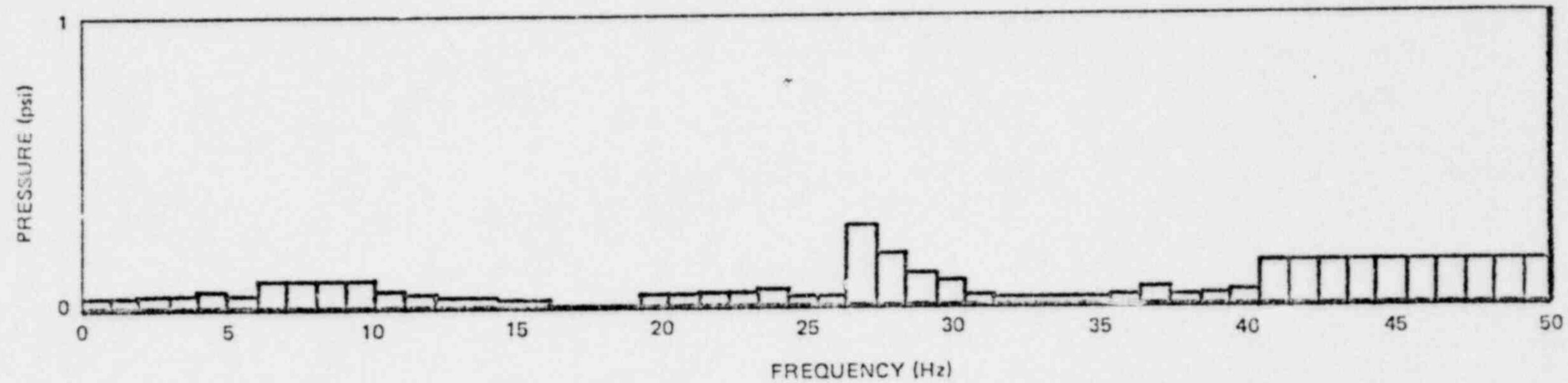


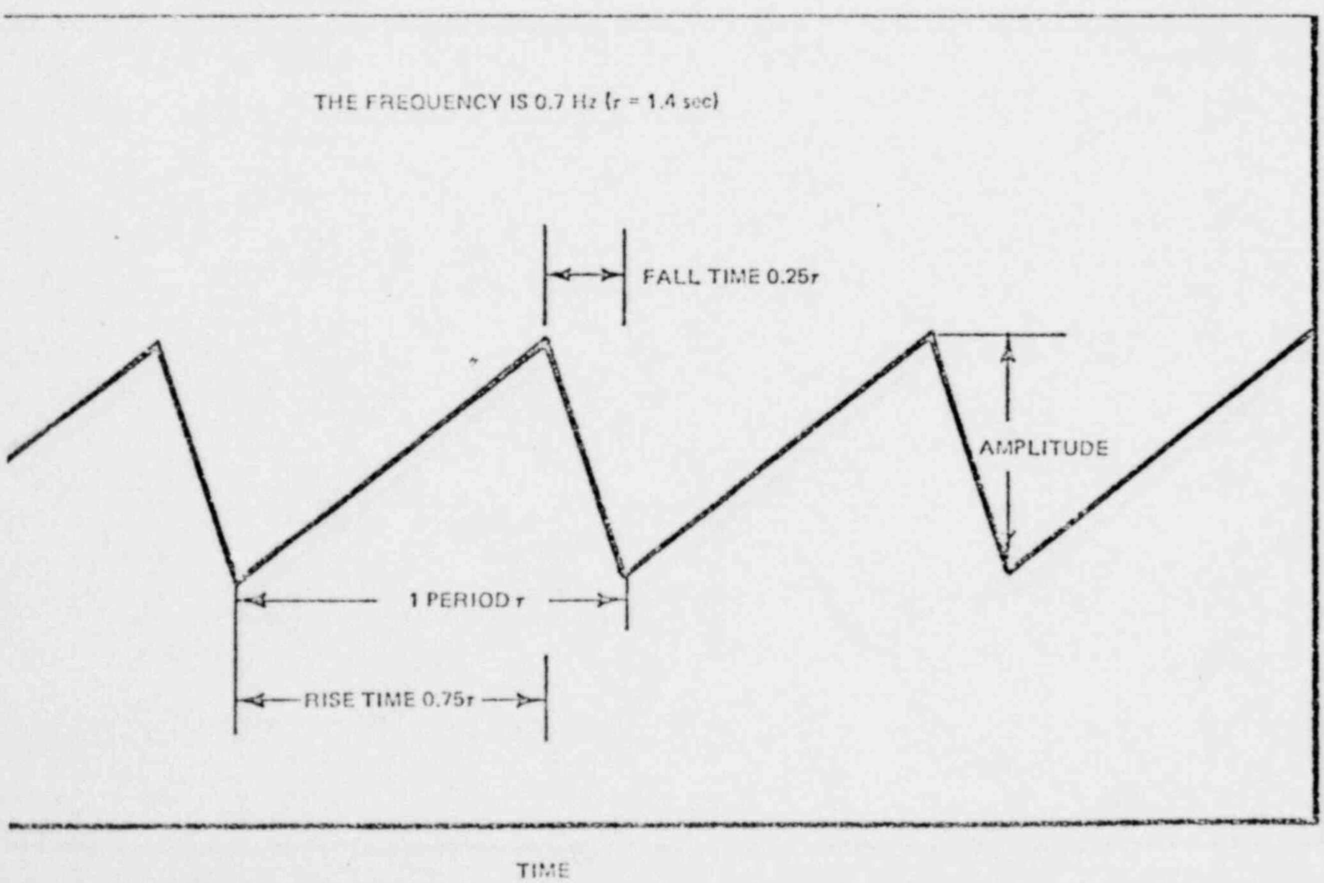
Figure 4.5.1-4. Post-Chug Rigid Wall Pressure Amplitudes on Torus Shell Bottom Dead Center

1500 079

166

VENT SYSTEM LOAD AMPLITUDES AND FREQUENCIES FOR CHUGGING

Load Type	Frequency (Hz)	Amplitude (psi) Main Vents	Vent Header	Downcomers
Gross Vent System Pressure Oscillation	Use wave form in Figure 4.5.4-1 (0.7 Hz)	±2.5	±2.5	±5.0
Acoustic Vent System Pressure Oscillation	Sinusoidal with frequency varying between 6.9 to 9.5 Hz	±2.5	±3.0	±3.5
Acoustic Downcomer Pressure Oscillation	Sinusoidal with frequency varying between 40 to 50 Hz	N/A	N/A	±13.0



BRONAN

EVALUATION OF DOWNCOMER LOADS DURING CONDENSATION OSCILLATION

OVERALL APPROACH

- Finite Element Model of FSTF Header/Downcomers
- Static Verification Based On Downcomer Jacking Tests
- Dynamic Verification Based On Downcomer "Snap" Tests
- Postulation Of Load Definition (Based On Pressure Data Measured In Test M-8)
- Dynamic Analysis For Postulated Loading
- Correlation Of Dynamic Analysis And Test Data

EVALUATION OF DOWNCOMER LOADING DURING CONDENSATION OSCILLATION

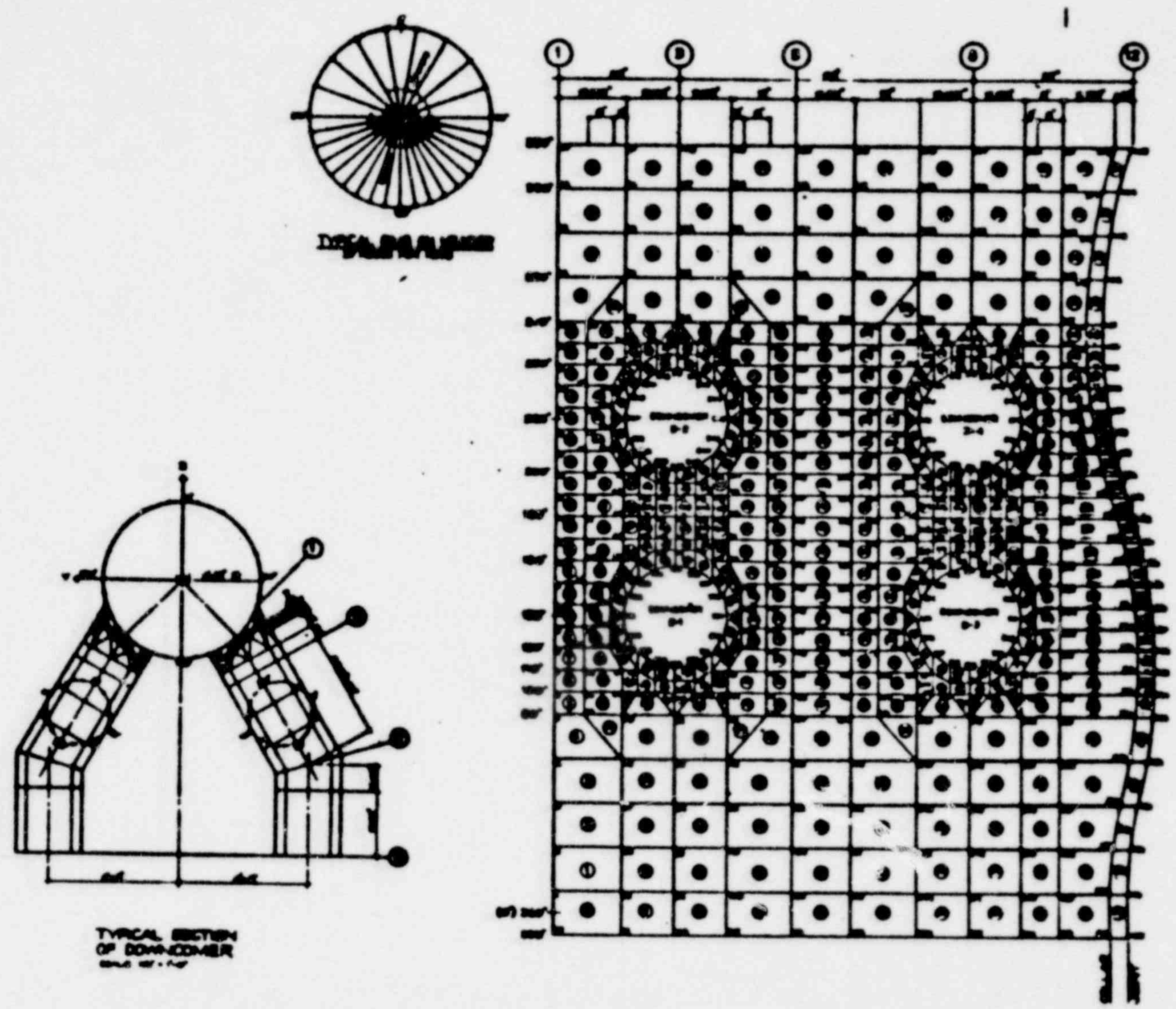
- COMPUTER MODEL
  - NASTRAN PROGRAM
  - MODEL HEADER FROM MIDBAY TO COLUMN SUPPORTS (ASSUME SUMMETRY)
  - SHELL REPRESENTATION (QUAD4 AND TRIA3 ELEMENTS)
  - TEST M-8 CONFIGURATION (D/C 5-6 UNTIED, 7-8 TIED)
  - EFFECTIVE WATER MASS WITH DOWNCOMERS

- POSTULATED LOAD DEFINITION (TEST M-8 FROM 25-30 SECONDS)

  1.5 PSI STATIC DIFFERENTIAL PRESSURE

  ± 2.5 PSI @5.5 HZ IN HEADER

  ± 5 PSI @5.5 HZ IN DOWNCOMER



FINITE ELEMENT MODEL OF VENT & DOWNCOMERS

1500 083

170

EVALUATION OF DOWNCOMER LOADS DURING CONDENSATION OSCILLATION

- STATIC VERIFICATION RUNS
  - Jacking Between Downcomers #5 & 6 (Test #7)
  - Jacking Between Downcomers #6 & 8 (Test #6)
  - Jacking Between Downcomers #7 & 8 (Test #8)
  - Correlate On Load - Deflection Curve
  - Correlate On Strain Guages On Downcomers And Adjacent Header (S5911-S5918, S5921-S5928)

- DYNAMIC VERIFICATION RUNS
  - Modal Analysis To Calculate Downcomer "Swing" Frequency
  - Compare With Results Of Downcomer "Snap" Test
  - Possible Adjustment Of Effective Water Mass In Downcomer

- STATIC PRESSURE RUNS
  - Unit Pressure In Downcomer And Header
  - "Two To One" Pressure In Downcomers And Header

- DYNAMIC ANALYSIS
  - Harmonic Analysis (5.5 Hz Loading)
  - "Two To One" Pressure In Downcomers And Header
  - Correlation With M-8 Test Data (Strains In Downcomer And Adjacent Header)

CLOSURE

Postulated Load Definition Explains Measured Strains ?

Or

Look At Phasing Between Pressures In Adjacent Downcomers

And Finally

Look At Other Tests And Time Periods

GRIMES

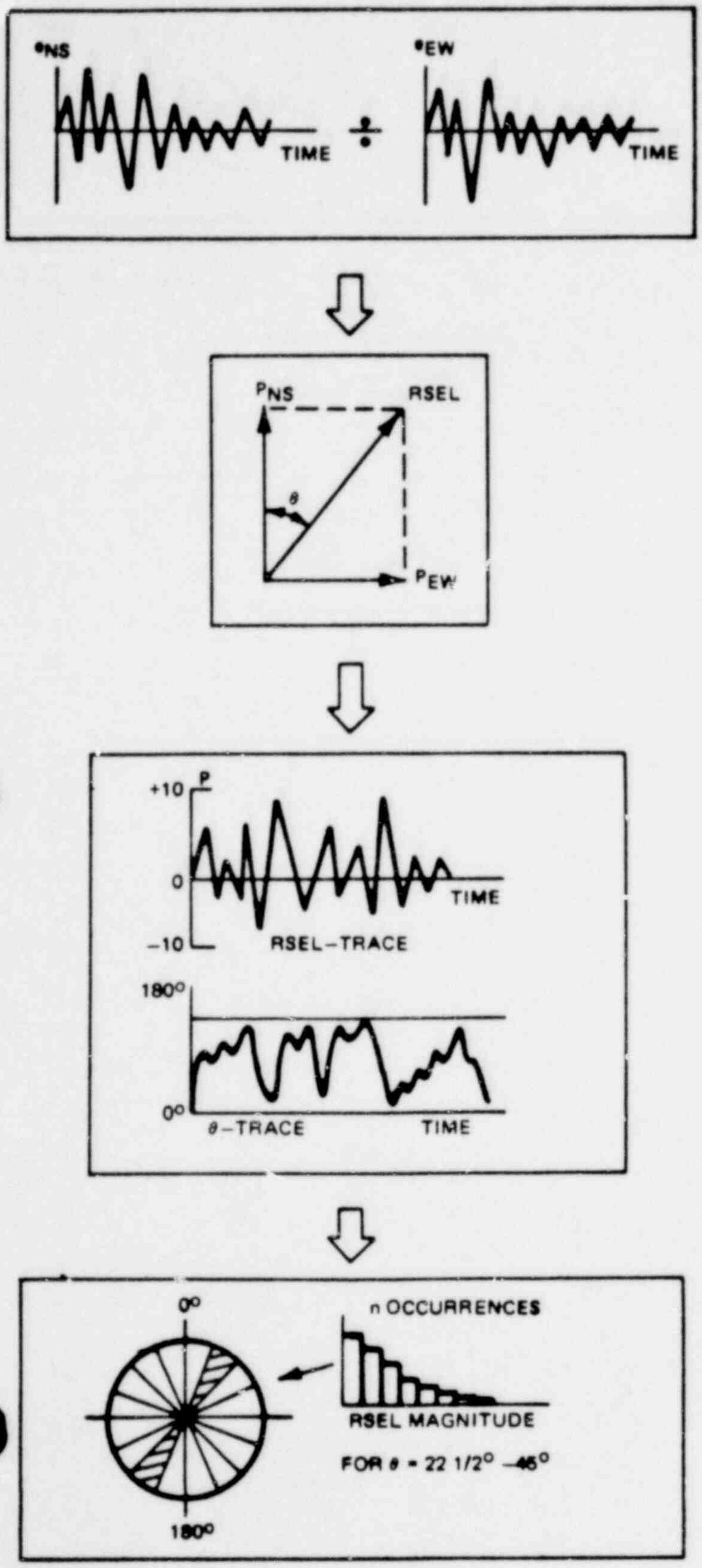
DOWNCOMER
CONDENSATION OSCILLATION
LOAD ASSESSMENT

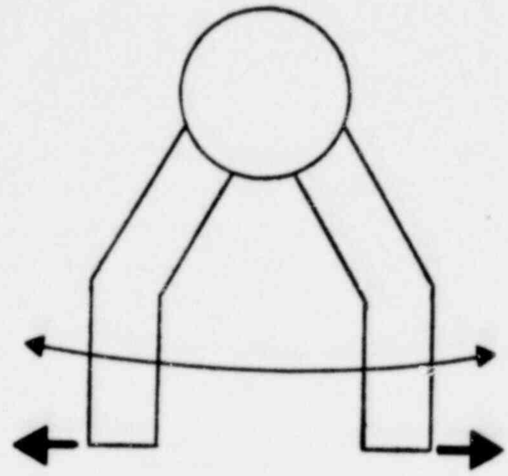
"UNTIED" DOWNCOMERS

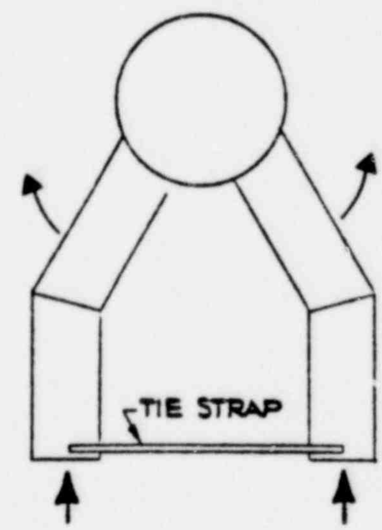
* DYNAMIC LOAD FACTOR SCALING
  DAMPING & NATURAL FREQUENCY
  5.5 HZ DRIVING FREQUENCY
* FUNDAMENTAL RESPONSE MODE - "SWINGING"

"TIED" DOWNCOMERS

* FORCING FUNCTION - PRESSURE OSCILLATION
* RESPONSE MODE - "WISHBONE"
* CONCLUSION - BETTER LOAD DESCRIPTION NECESSARY


NS
TIME
EW
TIME
PNS
RSEL
θ
PEW
+10
P
0
TIME
−10
RSEL−TRACE
180°
0°
θ−TRACE
TIME
0°
n OCCURRENCES
RSEL MAGNITUDE
FOR θ = 22 1/2° −45°
180°




TIE STRAP

1500 086

103

DOWNCOMER

CHUGGING LOAD

ASSESSMENT

"UNTIED" DOWNCOMERS

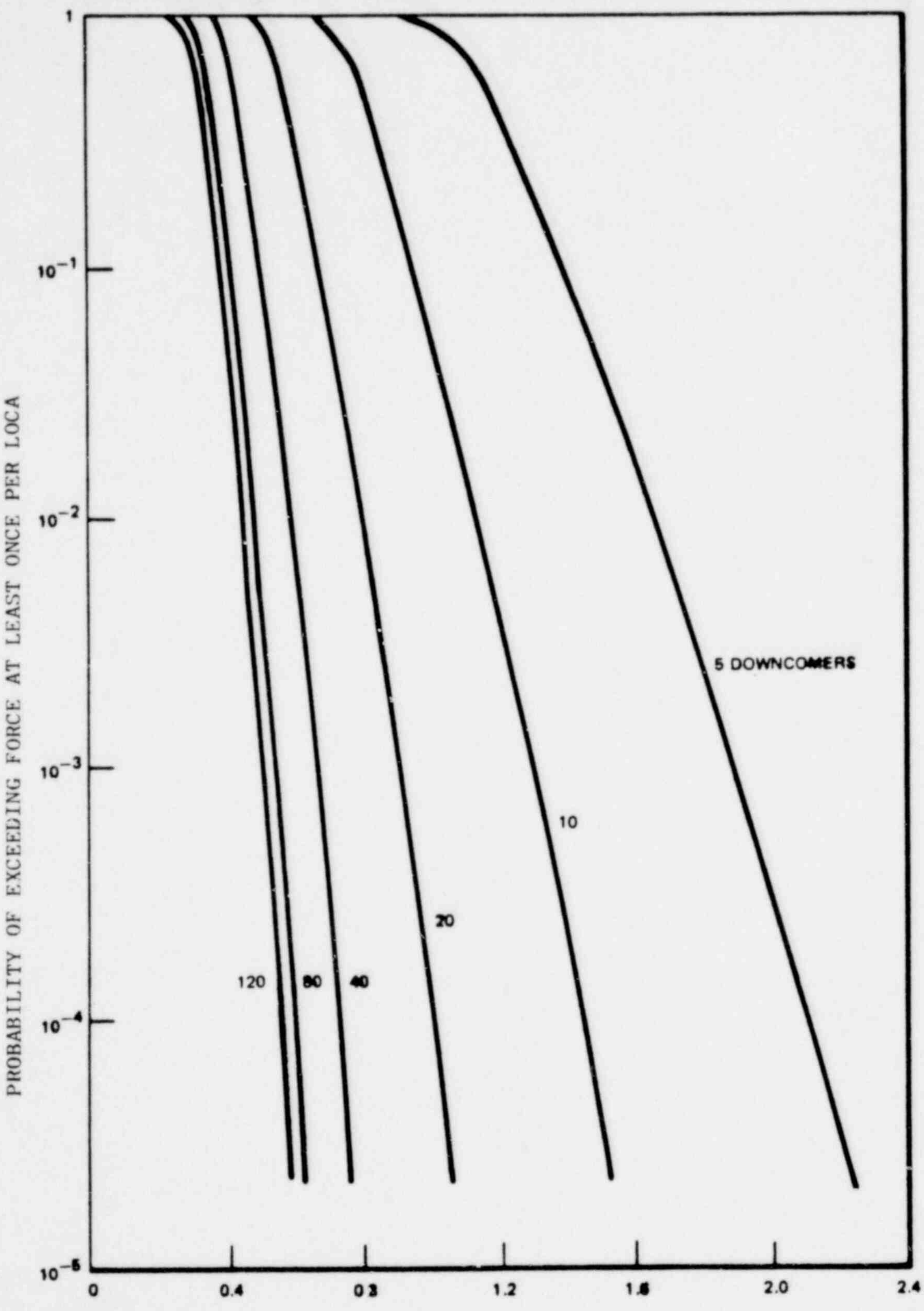
* MAXIMUM RSEL - PRIMARY STRESS
* 95% NEP - FATIGUE LOADING
* 10^{-4} NEP - DIRECTIONALITY

"TIED" DOWNCOMERS

* SPECIFY CONSERVATIVE LOAD FOR TIE-BAR
  DERIVED FROM "UNTIED" LOADS
  WORST DIRECTION - ONE DOWNCOMER
* RANDOM LOADING CONDITION

1500 087

MULTIPLE DOWNCOMER UNIDIRECTIONAL LOADING FUNCTION



1500 088

175

R.H. COGUE

MARK I CONTAINMENT PROGRAM

OWNER'S GROUP PERSPECTIVE

1500 089

176

11/16/79

KEY PROGRAM ACTIONS

•	GE ISSUED LOAD DEFINITION REPORT (LDR) PART A	DEC '78
	PART B	MAR '79
•	WORKING GROUP MEETINGS WITH NRC STAFF	FEB '79 - SEP '79
•	NRC ISSUED LOAD ACCEPTANCE CRITERIA	OCT '79

- CURRENT ACTIONS
  - INSTALLATION OF PLANT MODIFICATIONS
  - PLANT UNIQUE STRUCTURAL ANALYSES

1500 090 177

TYPICAL GENERIC MODIFICATIONS TO PLANTS

- T/QUENCHERS
- VENT DEFLECTORS
- TORUS SADDLES
- COLUMN REINFORCEMENTS
- ANCHOR BOLTS
- DOWNCOMER TRUNCATION

. . . . AND CONTINUED USE OF DRYWELL/WETWELL ΔP

11/16/79

1500 091 178

SCHEDULE FOR COMPLETION OF PLANT MODIFICATIONS *

OWNER		PLANT	COMPLETION DATE
TENNESSEE VALLEY AUTHORITY	**	BROWNS FERRY 1,2,3	JUNE 1983
CAROLINA POWER & LIGHT	**	BRUNSWICK 1,2	JUNE 1981
NEBRASKA PUBLIC POWER DIST.		COOPER	MAY 1980
COMMONWEALTH EDISON CO.	**	DRESDEN 2,3	MAY 1982
COMMONWEALTH EDISON CO.	**	QUAD CITIES 1, 2	FEB. 1982
IOWA ELECTRIC LIGHT & POWER		DUANE ARNOLD	APRIL 1981
POWER AUTHORITY STATE OF N.Y.		FITZPATRICK	JAN. 1983
GEORGIA POWER COMPANY	**	HATCH 1,2	JAN. 1983
NORTHEAST UTILITIES SERVICE CO.		MILLSTONE	APRIL 1982
NORTHERN STATES POWER		MONTICELLO	FEB. 1980
NIAGARA MOHAWK POWER CO.		NINE MILE PT.	JUNE 1981
JERSEY CENTRAL POWER & LIGHT		OYSTER CREEK	DEC. 1980
PHILADELPHIA ELECTRIC CO.	**	PEACH BOTTOM 2,3	NOV. 1981
BOSTON EDISON CO.		PILGRIM	MARCH 1981
YANKEE ATOMIC ELECTRIC CO.		VERMONT YANKEE	NOV. 1981

* AS OF MARCH 1979

** MULTI-UNIT PLANTS

1500 092

179

11/16/79

IMPACT OF NRC CRITERIA ON PROGRAM

- LOADS PROGRAM EXTENDED THROUGH 1980
  - FSTF RETEST
  - SIGNIFICANT LDR REVISION
  - MAY REQUIRE ADDITIONAL VENT DEFLECTOR TESTS
- ADDITIONAL AE STRUCTURAL ANALYSES
  - NEED TO RE-DO SOME PLANT UNIQUE ANALYSES
  - EFFECT ON PROGRAM SCHEDULE
- PLANT MODIFICATIONS CONTINUING ON "RISK" BASIS
  - MAY REQUIRE ITERATION ON EXISTING MODIFICATIONS

/80

1500 093

11/16/79

SUMMARY OF MARK I OWNER'S POSITION

- CONTAINMENT LOADS MORE COMPLEX THAN ORIGINALLY ANTICIPATED

- FURTHER INTERACTION ON LOADS AND STRUCTURAL METHODS REQUIRED - FUNDED THROUGH 1980

- UTILITIES PROCEEDING WITH MODIFICATIONS ON "RISK" BASIS

- EXPECT INTERACTION WITH NRC ON EITHER GENERIC OR PLANT UNIQUE BASIS

- OWNERS BELIEVE CURRENT LDR GIVES PRACTICAL ENGINEERING SOLUTION

- OWNERS REQUEST CONTINUING ACRS/NRC DIALOGUE TO ASSURE BALANCED PROGRAM CLOSURE