NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

IN THE MATTER OF:

SUBCOMMITTEE MEETING
on
FLUID DYNAMICS

Place - Los Angeles, California

Date - Thursday, 13 September 1979 **Pages** 1 - 223

Telephone:
(202) 347-3700

ACE - FEDERAL REPORTERS, INC.
Official Reporters
444 North Capitol Street
Washington, D.C. 20001
NATIONWIDE COVERAGE - DAILY

1030 273

7909260316

CR6841

PUBLIC NOTICE BY THE
UNITED STATES NUCLEAR REGULATORY COMMISSION'S
ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

Thursday, 13 September 1979

The contents of this stenographic transcript of the proceedings of the United States Nuclear Regulatory Commission's Advisory Committee on Reactor Safeguards (ACRS), as reported herein, is an uncorrected record of the discussions recorded at the meeting held on the above date.

No member of the ACRS Staff and no participant at this meeting accepts any responsibility for errors or inaccuracies of statement or data contained in this transcript.

R6841

UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

SUBCOMMITTEE MEETING

on

FLUID DYNAMICS

Century IV Room
Airport Quality Inn
Los Angeles, California

Thursday, 13 September 1979

The ACRS Subcommittee on Fluid Dynamics met, pursuant to notice, at 8:40 a.m., Dr. Milton Plesset, chairman of the subcommittee, presiding.

PRESENT:

DR. MILTON PLESSET, Chairman of the Subcommittee

DR. J. CARSON MARK, Member

MR. WILLIAM MATHIS, Member

PROCEEDINGS

DR. PLESSET: The meeting will now come to order.

This is a meeting of the Advisory Committee on Reactor Safeguards Subcommittee on Fluid Dynamics. I am Milton Plesset, Subcommittee chairman. Other ACRS members present are William Mathis and Carson Mark; and our consultants. I will go around in this order: Dr. Catton, Dr. Wu, who has just stepped out for a moment, Dr. Yao, Dr. Zudans, Frank Zaloudex, Spence Bush. Have I left anybody out?

The purpose of this meeting is to discuss the NRC staff progress in the review of Mark I and Mark II boiling water reactor containment load definitions and acceptance criteria.

The meeting is conducted in accordance with the provisions of the Federal Advisory Committee Act and the Government in the Sunshine Act. Dr. Andrew Bates, on my left, is the designated federal employee for the meeting. The rules for participation in today's meeting have been announced as part of the notice of this meeting previously published in the Federal Register on August 29, 1979. A transcript of the meeting is being kept and will be made available, as stated in the Federal Register notice. It is requested -- and this is underlined, so I am now using italics -- that each speaker first identify himself and speak with sufficient clarity and volume that he can be readily heard.

We have received no written comments or requests for time to make oral statements, from members of the public.

We will proceed with the meeting, and I will now call on Mr. Cliff Anderson, of the NRC staff.

DR. HANAUER: Let the record show, Mr. Chairman, that Ace-Federal Reporters has saved the day.

(Court reporter provides electric extension cord to subcommittee.)

MR. ANDERSON: I am Cliff Anderson, task manager of the NRC's containment review program for Mark II dynamic loads. This is a task A-8.

I want to make one request later on in the day. We are requesting a closed session to have some discussion on some of the foreign testing when that comes up. It is on the agenda. It is the item — it is one of the last items on the agenda. This information is considered proprietary at this point. This is proprietary to NRC.

DR. PLESSET: It will be about a half hour, Carson.

MR. ANDERSON: That is the only time that we have requested a closed session.

DR. PLESSET: We should start by saying that our

BWH

session today is all concerned with the Mark II containment.

MR. ANDERSON: Yes. The purpose of this meeting, the way we see it today, is to give you an update, status report on where we stand and where the Mark II owners stand in the Mark II program. You might recall that our criteria dealing with loads that the staff finds acceptable were issued for the lead plants in September of 1978, last year. We then documented this in NUREG-0487, where we provided the basis for those loads that we found acceptable. That was issued in October of 1978.

We then met with this subcommittee in November of '78, about the middle of November. We had another meeting where we dealt with the implementation of the generic criteria on the first of the Mark II plants, the Zimmer plant, and this was done in February, I believe, of this year. So, this, then, is our first meeting with you since that time.

The purpose of the meeting, we see in four major areas, and we will be addressing each one of these areas today. The first item, as we have indicated, we had found certain loads acceptable for the lead-off, for that matter, any of the Mark II plants; and those were documented in NUREG-0487. There were certain of these loads where the lead plants and the other plants had asked for some consideration by the staff of a revised load specification.

Over the past year, we have worked with the Mark II owners to review these other load specifications, and we now have come to a point where we find certain other loads than those currently found acceptable in the NUREG. These other ones, also acceptable. These are just a few in the SRV, and the submerged structure load area, that will be our first area; and the staff will be doing most of the talking here.

And then there will be some discussion in this area, lead plant dowocomer support. Since the time that we had last talked with you, some concerns have come up with regard to consideration of redesign of the supports for some of the unbraced dcwocomers. This is primarily in the Zimmer and LaSalle facility. Zimmer and the LaSalle people will be giving an update on that to tell you where they stand.

The third major area is a long-term program. A lot of work has been done on this in the last couple of years. A lot of progress has been made in this last year. For the major generic tasks and a couple of the plant-unique areas will be discussed, primarily by the Mark II owners.

And then the proprietary section that we had requested would be this last major area, and the staff will make a presentation here.

(Slide.)

This is a summary agenda, and I notice some changes from the detailed agenda that you have seen before.

First of all, with regard to some of the lead plant load areas, you would recall that we had something listed for discussion of the ring vortex model. We now understand, from the Mark II owners, that the lead plants, all of the lead plants, will use the original load specification that we had found acceptable in the NUREG. And this was the one that we discussed last year. In other words, none of the lead plants intend using the ring vortex model at this point; so this has been moved to the long-term program.

There had been some discussion about having a presentation of this in the long-term program discussions here. However, the Mark II owners' consultant is not available; and, therefore, we -- there is no plan for a formal presentation to deal with this topic. Should the subcommittee wish to express some concerns of have some informal kind of discussion along this line, we might want to leave that to this point here. Again, I emphasize it is not included as an evaluation methodology for any of the lead plants.

DR. PLESSET: We might have some brief informal discussion. I think some of our consultants may be able to make a comment that might be of interest, but we will see how it goes.

MR. ANDERSON: Another thing I might point out is that there are quite a few items here, and we could go for quite a while. We have tried to pare it down to some extent. But what I am going to try to do in this introduction is to give you an overview of each one of these four major areas and in the process of doing that perhaps we can make a determination of which one of the major areas you want to concentrate on.

I think that is pretty much it. There was one other item that was dropped, and this is the item with regard to load combinations, the update on SRSS. As we understand, this has been moved to a different subcommittee, and that other subcommittee had taken up this topic in August. So, there are no plans to make any presentations on the load combination methodology for today.

Let me move on.

(Slide.)

This is our latest update on NRC's view of the facilities scheduled for some of the Mark II plants, a couple of things you might want to note on this. First of all, as you are aware, the safety evaluation report was issued for Zimmer. A supplement is planned. I am not sure on the date of that.

Another point that you might want to note is that, as a result of Three Mile Island-related concerns, most of the fuel load dates have been moved something like five, six months, on the average. We know one thing, as we look at this slide, that there appears to be kind of a grouping of a whole bunch of them coming in about the middle to the end of '80 and the early part of '81. So, there is going to be quite a bit of licensing activity.

One other point: We do intend to address some of these concerns that have been raised with regard to the potential redesign of the downcomer supports for Zimmer, in a supplement, should ACRS want to discuss that after we have had a chance to look at that.

(Slide.)

Just a few background slides, and I am not going to go into it in any real detail. I included this for reference sake, more than anything else. A picture of a Mark II facility showing the major structures, the drywell, wetwell, pipes, the pool.

(Slide.)

And just to give us sort of a road map today, there will be some similar scenario of loads discussed in the Mark I discussions tomorrow. I do present here the chronology of the primary LOCA-related loads. And it might be of some value to just touch on these again. I don't want to agonize on this too much.

BWH

Following a posulated LOCA, you have steam, you have steam enter the drywell. That mixture of steam and air pressurizes the drywell, and this results in expulsion of the water that is originally in the downcomer or in the vent. And that results in loads in the basemat. The air from the dry well is carried through the downcomers, forming a bubble at the end of the downcomers. Formation of that bubble also results in some submerged drag load on components.

The bubbles coalesce. As the bubbles coalesce into a pancake shape under the pool, the pool moves under the action of that compressed air bubble, expanding compression of the air space above the pool, and then also undern the action of gravity. And we get these types of loads. The air bubble drag loads and impact loads, delta T across the diaphragm — we have discussed a lot of these things in previous meetings — and once it reaches the maximum height, as the pool settles back down, we get drag loads associated with the fallback process.

And then, following the air clearing process, where you have taken all of the air from the drywell to the wetwell, which occurs within a period of maybe just a relatively few seconds, we then get the steam loads. The steam loads occur on the pool boundary, submerged structures, and also locally on the vents.

We have these loads categorized into areas. One of the high-mass flux loads, that we commonly call "condensation oscillation," and this is a more harmonic type of phenomena, and then as we go through that period and go into a lower mass flux and lower air content, we have the more stochastic phenomena, what was commonly referred to as the "chugging loads" on the pool boundary, and also on the downcomer.

Just for reference sake, one thing we will be talking about for alternate loads would be the load on the basemat. And during this vent clearing process, a little bit about some of the drag loads. And we'll be doing evaluation of some additional consideration of the maximum wetwell pressure and maximum height of the pool.

There will also be some discussions in some other areas here, as we talk about the long-term program, or rather as the Mark II owners talk about that. Again, just for reference, I am not really going to go into this, this sequence of events, the time that these various phenomena occur, what the phenomena is and the resulting loading condition.

(Slide.)

The first of the major topic areas, the alternate lead plant loads, we have alternate loads. We will be talking about alternate loads of three areas: LOCA, SRV,

BWH

S submerged drag.

We have been having discussions with the Mark II owners since the NUREG was issued. The LOCA area, there are three pool swell-related phenomena that were addressed here.

The first one is, as I mentioned before, when we clear the vent of the water during the early process of the LOCA, one gets an induced load on the basemat and the pool boundary. The original specification was 33 psi. The new specification that we will be talking about is 24 psi.

In this area, pool swell elevation and wetwell air compression methodology that we had in our criteria was to use the pool swell analytical model. There was some additional looking at the 4T data and some other data to put some other restraints on the use of that pool swell model, so that under certain conditions you would use the pool swell model up to a certain point based on this 4T data.

And then the last one is the air bubble-related asymmetric pool swell. What we are talking about here is the potential air bubble pressure variations that can occur on the pool containment boundary resulting from potential maldistributions of the steam as it goes into the drywell.

In the SRV area, there is only one area we will be talking about, and that is, as you recall, there are five load cases that we asked the Mark II people to evaluate their plants to. These include a single SRV valve release,,

BWH

two for an asymmetric case to relief valves, ADS and several all valve sequential and all bubbles in phase.

One of these cases appeared to be excessively conservative and this one — and was giving some difficulty in evaluation of piping and things along this line — and this was the load case five, all bubbles in phase, each of the same frequency, covering a range of frequency of four to 11 hertz. And we recall at the time we put this, our NUREG, together, the only real data that we had at that time was Ramshead data, so we put together a very conservative specification based on Ramshead load magnitude, recognizing there would be — there was good indication of substantial reduction.

What we have effectively done now is that now that the KWU-T quencher data is available and the CAORSO data is available, we have backed off on the magnitude. We think it is appropriate to reduce the magnitude of the load specification. And the submerged structure drag load, there were a number of small criteria here. And a couple of these areas we have done some refinement — like in the standard NUREG, how does one account for interference effect and separate unsteady and oscillating flow — by looking at some specific Mark II considerations and some additional data. Here with the vortex shedding, these are the transverse loads on structures, equipment, and under what conditions should this be considered.

And then finally, structural nodalization. What kind of nodalizations should targets be subjected to in order to get accurate calculations of submerged structures.

(Slide.)

The next significant area that will be discussed by the lead plants — Zimmer, Shoreham, LaSalle — relates to support of the downcomer. And a few words on this. There are 11 Mark II plants. Of those 11 Mark II plants, there is a variation in the support arrangement for those downcomers. They have — some of them have different types of bracing arrangements. Two of them did not have — were not going to use bracing. Those were Zimmer and LaSalle. Since the time that our criteria had been issued, these criteria have now been folded into the design evaluation of those two facilities, and the determination was made that there was erosion of some of the margins and it would be prudent to consider putting bracing into those plants. That was one of the options. There are several other options that they are investigating, to the best that we understand, with regard to this concern.

The concern relates to primarily submerged structure drag loads resulting from condensation oscillation phenomena and SRV air bubbles. In particular, the condensation oscillation load is occurring right about the

BWH

natural frequency of the downcomers, and you are having some pretty high dynamic load factors associated with that.

Their startegy is to consider some — installing some bracing in those plants, and also considering refinement in the submerged structure drag load, and other considerations to change the natural frequency of the downcomer. They may have some other options.

This is something that we only have been involved with in the last couple of months, and we are not currently doing any evaluation of any reports or anything there. They are still doing some work in this.

DR. PLESSET: Will the Mark II owners people give us some kind of an informal brief presentation of some of their ideas today?

DR. BRINKMAN: Yes, sir. Dr. Crawford is here from Sargent & Lundy, and he will be discussing this.

DR. PLESSET: That's good.

MR. ANDERSON: There are some complications when one considers putting braces into a plant that was originally designed to not have braces. These are: The plants were originally designed to take those downcomer loads and transmit them up to the diaphragm; the other plants were considering transmitting those loads to the containment, not just up to the diaphragm, but also to the containment walls through the bracing system.

Now, the concern here is that with our current very conservative load specifications for lateral loads, when one comes up with a multiple vent load specification, you can get some pretty significant loads that have to be transmitted to the containment walls. That is one of the things there.

(Slide.)

A couple of slides here, just to provide a status report on the total generic program.

This slide shows that portion of the generic program still to be completed in three areas: generic SRV, LOCA, and some of the miscellaneous items.

One of the major things I want to point out here is that with the completion of documentation of Phase 2 and CAORSO test -- that is about the last generic category, related to SRV -- we should be getting that at the end of '79. For the LOCA area, one of the last tasks here in mid-'80 is the documentation associated with the 40 condensation oscillation tests.

Another point that might be made here is that our current schedule for these two programs -- the lead plant and the long-term -- the lead plant is essentially complete with the exception of our documenting these new alternative criteria and the bases for these criteria. We have found these alternate criteria acceptable. However, we will document this in a supplement to NUREG-0487 that is currently scheduled for November of this year.

For the long-term program, we have moved a lot of our review efforts from the lead plant to the long-term program efforts, such that we are spending over half of our time now on the review of those other efforts. Our current schedule calls for completion of our review efforts in October 1980. I think you can see here that there could be some difficulties here in our completing all of this, assuming we would only be getting the reports at these stages.

One other point one might note is that this looks at the generic programs. It does not look at programs falling outside of the Mark II generic program. There are a number of these, and I will talk about that in a second.

(Slide.)

The next slide shows you some information with regard to plant-unique programs that the Mark II owners non-lead plant, Mark II owners — in other words, not Zimmer, LaSalle, and Shorehamn — have identified. Note that there are also lead plant plant-unique programs such as Zimmer, in-plant tests for safety relief valve loads, the LaSalle in-plant test, and also the KWUT quenchers test, that are not included in this.

Just for illustrative purposes, one that we are talking about here, the WPPSS-2 program, we are going to be talking today, or the Mark II owners, will be talking about a generic program with regard to an improved chug load specification. There is also a plant-unique program unique to WPPSS-2. We will be talking today about the generic 4T condensation oscillation tests. There is a plant-unique counterpart to that to get prototypical data that is specific to Susquehanna. That is the GKN-2 condensation oscillation test that will be discussed briefly today.

Many of these plant-unique programs are not that well defined at this point. We are in the process of sending letters to each one of the Mark II plants. The purpose of this letter is, one, to request that they give us a clearer definition, on a plant-unique basis, of all of their programs so that we can plan our necessary resources and see what impact this may have on some of the generic work; and, two, we are also trying to encourage them to do as much grouping as possible to come up with generic or semigeneric approaches.

DR. ZUDANS: Are these tests, are these scale tests, or are these to be done in-plant?

MR. ANDERSON: These are not all tests. These are a combination of tests, analytical programs, and things like that. We don't know all of what some of these things are, to be very honest. This is a full-scale test of one vent.

There are some other tests that are included in here. I wouldn't try and identify them.

DR. ZUDANS: When you say "full scale," you do not mean in-plant tests?

MR. ANDERSON: That's right.

DR. CATTON: Full-scale plant simulation.

DR. ZUDANS: And it is all simulation, whether analytical or theoretical.

DR. PLESSET: We will hear more later about the details.

DR. ZUDANS: He says he doesn't know.

MR. ANDERSON: Some areas I know.

DR. PLESSET: Some he does.

DR. ZUDANS: Fine.

MR. ANDERSON: Other areas, we are just not sure.

We recognize that in some areas there is a need, but we are trying to encourage the Mark II owners, where possible, to do some grouping.

(Slide.)

The third major area is status report that is to be presented by the Mark II owners on some of the primary long-term program tasks, and they have made some significant progress in these tasks. We have had a number of meetings with them, and in just about each one of these generic and plant-unique programs.

Looking at this list of programs, there are essentially, I think it is 5 out of the 6 programs are related to improved specifications of the pool boundary load or steam-related load on the containment.

One of them is giving you an update on the CAORSO safety relief valve test.

Just a word or two on these. You will hear a bit more about this.

With regard to the 4T CO test, the purpose of these tests is to address the non-prototypical nature of the 4T with regard to vent length. The original 4T plant had a 90-foot vent. The typical Mark II plant has a vent length of 40 feet.

So this is to look at vent acoustic effects and their effect on CO, the CREARE multivent test. Recall that the lead plant approach was to develop loads based on a single cell test. These are the 4T tests, the full-scale test.

We felt that there was need to confirm the bounding nature of that and also to give us a better handle on the margin associated with using single vent as opposed to multiple vent loads.

So that is the purpose of these various multivent tests. They have completed the first phase of these tests, including tests at 10 scale and 6 scale. They are now in the

BW

process of doing their second phase of tests where they will do some more multiple vent tests and some single vent tests at larger size, including quarter scale and 5/12ths scale.

Improved chug load. The purpose of this program is to recall — you recall in the lead plant program the approach was to take the loads measured on the 4T wall, take some of the worst loads there and then apply them directly to the plant at the individual plants.

We were concerned about identifying some of the FSI related aspects of the 4T facility and how that might affect transmitting those loads to an individual plant.

In the process of their looking at that, they tried to come up with a source specification that was free of 4T effects. And they have done a lot of work here (indicating).

And last of all, you've got some information on the CAORSO tests at our last meeting. Those tests were in progress at that time. Since that time, the tests have been completed. We do have documentation on the first phase of those tests.

The second phase, the multivent tests, the report there is due in at the end of this year. And then as I indicated, in addition to the generic programs, there are some similiar plant unique programs very similar to the 4T CO test, the GKM II CO tests.

BW

These are, again, much closer to the Susquehanna conditions, since it is sponsored by them.

In the WPPSS-2 improved chug load, again, there are some similarities to the generic improved chug load, a little different way of handling some of the library of test data, statistical treatment of that to come up with a source and some somewhat different analytical models with a lot of similarities in them.

(Slide.)

The last area related foreign tests. Foreign tests that are being conducted in Japan and in Germany that are related to the Mark II design are in progress now. They have been in progress for some months. There will be a discussion first about these JAERI tests, which are prototypical of the Mark II. They are full scale, they are steam tests with testing programs started in '77 and scheduled to be completed in 1982.

The tests represent a 1/16th sector of a full Mark II plant, including 7 vents. Four of the shakedown tests have been completed. They have also completed some of the earlier tests, the regularly scheduled tests. And we have had a chance to look at data from one of these tests, one of the shakedown tests.

We have looked at it with regard to these areas, pool swell, and some of the steam modes. With regard to pool

BW

swell, again, this is just looking at one test under some nominal conditions. Our observations are that with regard to pool swell, there are no particular surprises. They do establish the nature of the loads that were used for the lead plant.

With regard to condensation oscillation, there has been some attention to condensation oscillation loads on the pool boundary. We have not observed any significant CO loads in that particular test.

And then with regard to chug load, our primary emphasis here is to look at some of the detailed data with regard to how it might be used as part of the confirmatory process for some of these improved chugging load specifications where they are taking credit for reducing the lead plant load.

A couple of observations about this.

In general, we would say that the average load that was observed on that facility, even though it has its own unique FSI, fluid/structure interaction, it has about the same average load as we saw in the 4T facility.

There are a couple of high localized loads. One of the major things that we are concerned with the potential for a number of large chugs occurring at the same time. The current plans for the improved chug load specification generally assumes that you can use the library of

BW

chugs as taken from a single vent test. It assumes that these loads are somewhat random in magnitude. And you can apply any load's magnitude for any vents interchangeably.

Some of our preliminary observations indicate to us that there is a potential for having some large chugs occurring at the same time.

DR. CATTON: Are you saying that the chug is — random in time and random in amplitude?

DR. ANDERSON: The current — the lead plant — excuse me. The plans for the long-term program are to assume that the loads are random in magnitude but occur at the same time.

Now, again, we will get into some of this later on. We do see gross pool chugs occurring together. But as far as exact phasing is concerned, we have to look at the data a little more carefully with regard to that.

The question that we are looking at here to confirm what the Mark II owners want to do in the long-term is can we confirm that they are random in magnitude?

And we have some reason to believe that it is possible you can get some large chugs occurring at the same time, but we have to look at this data very carefully.

DR. CATTON: Will we hear more about the JAERI tests?

DR. ANDERSON: Yes. We will make a presentation in a closed session. We are getting reports in at this point.

BW

We have a report on one of the tests. This information, again, at this point, both the JAERI and the GKSS proprietary to NRC. Then we will make sure that the ACRS does get copies of these reports.

We are just getting them now.

With regard to GKSS tests, these also are related to Mark II. They are steam tests. They have three large vents. They are not exactly prototypical. Vent length is a little off. The drywell volume is a bit too small. But the test facility was available and we do feel that we can get some good qualitative information out of this.

Where they stand is they were going to do four shakedown tests. They have done three of those. They are now in the process of embarking on their regularly scheduled tests. They have something like 12 tests scheduled over the period of the next year and a half. The first of those tests should have been done last week, as far as I understand. And we have some observations on that, too.

Again, on the average, even though it has its own FSI, we see on the average the same chug load, average chug load on the pool boundary, as we saw on the 4T.

That completes my discussion related to this introduction. And I would like to move now into some of the —

DR. PLESSET: Cliff, I think with your concurrence, we will have a break so that we can get some more chairs in

BW

here.

So — and we will come back to this. So let's have a few minutes break and try to arrange to have a few more chairs in here.

So we will have about a five-minute break.

(Recess.)

DR. PLESSET: Let's reconvene. We have some more chairs, and if we sit down, we will see how many people still have to stand. Hopefully, nobody. I will give them a moment, Cliff, to get settled.

MR. ANDERSON: We are moving now into some of the lead plant load areas and some of the work that has been done in these areas.

The first topic we will be taking up is the alternate LOCA loads. Following my presentation, Dr. Economus, our consultant from Brookhaven, will talk about the alternate safety relief valve load specification. And then following that, Professor Bienkowski from Princeton, also our consultant, will talk about some of our alternate specifications there.

I might make one point before I get into this. The original loads that were proposed for the Mark II plants were documented in the Mark II owners dynamic force and function report. We reviewed that, and as a result of our review, we came out with a NUREG-0487, loads for the lead plants.

BW

As I indicated before, we have been working with them to refine some of those loads to come up with alternate loads.

What we mean by "alternate loads" is that we would find either these loads or the original loads conservative and acceptable. We have documented these alternate loads in various letter reports that have been issued over the last year, and you should have those letter reports.

I'm not going to try to identify them one by one during this meeting, but if you're interested in the specific ones, let me know and I will mention them.

With one exception -- there is one area, the submerged structure drag load area, where there is not a public — a report out yet. We have a draft of that report and we will be telling the Mark II owners to get that report submitted and documented as a formal report before too long.

As I indicated before, we are going to be dealing with three pool swell related loads: the low load on the pool boundary; the maximum pool swell elevation; and the asymmetric pool swell resulting from asymmetric bubble pressure on the pool boundary.

I will do several things. First, to give you the origin of the load, the original specification and the basis for that, and then the revised or alternate specification and our basis for accepting that alternate specification.

I might point out again that we have concluded that all of these loads we will be talking about are acceptable.

(Slide.)

The first of these deals with the event clearing load. The origin of this, again, is when you expel the water that is originally in the downcomer and induce pressure on the pool boundary, the original specification included a 33 psi overpressure statically applied to the containment on the basemat.

This was in their DFFR. The basis for that was the assumption that one had formation of a jet which could penetrate to the basemat and then doing a conservative calculation with a very conservative vent clearing velocity, one came up with 33 psi.

We looked at the test data and based on the test data, we felt that this definitely was a conservative specification. We did feel, however, that it should not be limited to the basemat. There should be a specification associated with this induced pressure also on the containment walls.

So we extended the specification to be applied to the containment walls.

(Slide.)

The Mark II owners agreed with us regarding the appropriateness of the specification of a vent clearing induced pressure on the walls. But they felt that the 33 psi, after looking at the 4T data a little harder, was a bit on the conservative side, and a significantly lower load could be justified.

It was justified primarily in light of test data instead of doing these conservative calculations. There was good indication that you would not have jet penetration based on looking at these like EPRI data. More like three vent diameters as opposed to a total vent clearance to the basemat of something like 10 feet.

So as a result of looking at the 4T data, they came up with a 24 psi pressure to be applied over the hydrostatic pressure on the basemat and on the walls up to the exit of the downcomer and then a linear tenuation of zero to surface of the pool.

They formed this load specification on the basis of the highest basemat pressure observed during this time period during the 4T test. The highest value they had observed was 20 psi.

I should note that on the average, they observed something in the order of magnitude of about 12 psi, 12-1/2 psi. However, they did do a little bit of modification of this to reflect the fact that while 4T is a factility that is prototypical of Mark II plants and has been designed so that

BW

it would be bounding its parameters pool area to vent area ratio, things like that. It was conservative but there was one area where there was potential non-conservatism for certain plants.

This non-conservatism is that the total test matrix did not necessarily go to the maximum drywell pressurization that one would calculate using conservative models for the -- some of the Mark II plants.

If one looks at the Mark II plants and picks out for the limiting one the maximum drywell pressure at the point of vent clearing, one sees that that can be as high as 4 PSI higher than the maximum value seen in the 4T tests.

So they added the 4 psi to the 20 psi. We feel that there is some basis for that methodology coming up with a pool boundary load. But we felt that we might take a little harder look into it.

And what we did was our consultants did a least square fit of both the 4T points and also some Marviken data, where our consultants did a least square fit of the overpressure, induced overpressure with these parameters, the one we thought would be important, ones related to pressurization.

In other words, the energy flux submergence, and then indirectly to the pool-area-to-vent-area ratio.

We concluded the 24 psi specification was

BW

conservative and a 99-99 non-exceedence competence limit, as long as one did not exceed a value of this parameter of 55.

But I think that none of the domestic Mark II plants exceed that parameter of 55.

So we find that 24 psi is acceptable.

The next area, I don't want to go over too much of previous ground that was covered in this particular load specification, but I think there is a little bit here — the next one refers to the specification relating to the maximum pool swell height and the associated maximum wet well compression.

(Slide.)

There are two things with regard to pool swell methodology. One, they used a pool swell model that had been benchmarked against a number of tests to calculate velocities in individual plants. They used that — this was in their original dynamic forcing function report methodology. They used that up to the point of maximum velocity and then they kept a constant maximum velocity up to 1-1/2 times submergence where breakthrough was assumed to occur, and then the pool would settle back.

In our evaluation of that originally, we had found that in a few cases, primarily for low submergence, those plants with maybe 9-foot vents, you could exceed that 1-1/2 times vent submergence criteria.

And in addition, we had some concern with regard to the method they used for calculating or backing out the elevation. They backed it out kind of indirectly based on the observed we well pressures in the test.

So as a result of that, we had specified last year, discussed with you in the NUREG specification for velocity and pool elevation, maximum elevation based strictly on the pool swell model with a little bit of fooling around with the model.

We did basically two things. To account for some uncertainties in the velocity measurements, we increased the velocity by 10 percent. But that is not a problem really here

The other thing we had done is we felt they should use the exponent of 1.2, and when our consultants had taken the pool swell model and used it, and used it in predicting 4T tests, they concluded that the pool swell model bounded all velocity measurements and in addition, provided a 4-foot margin on all measurements of pool swell elevation, including froth.

DR. BUSH: That figure isn't in your presentation, incidentally.

DR. MARK: We had a different version earlier.

DR. ANDERSON: I have three of these and I will check with you later on and see which ones. If there is something missing, I will try to get it to you after the meeting.

I just might mention what has happened in the interim. Most of the Mark II owners have been able to accommodate this load specification without any real problem. A few had some little areas up at the higher elevations and they have agreed with the use of the pool swell model, but with a couple of constraints based on the 4T data.

This is the last slide.

Again, they would use the pool swell model up to the point that, based on 4-T data, you could get some maximum wetwell pressurization. And that maximum wetwell pressurization would determine, based on the maximum upload on the diaphram, this is the differential between the wet-well space and the drywell and we have a criterion for that and the associated drywell pressure. And you combine those two and come up with a maximum wetwell pressure.

So you run the pool swell model until you get that maximum wetwell pressure and you would not run it higher than that.

There would be one other constraint that they put on this. That is that you would not use a lower termination height than 1-1/2 times the vent submergence.

(Slide.)

We requested that they check that methodology against selected Phase 1 and 2 4T tests. We picked out the ones where we thought that they would have the most difficulty in calculating using that methodology and we concluded after they did that comparison that the methodology was, in fact, conservative.

We should point out one thing: That in one case for saturated vapor run no. 35, the measurement did exceed the calculation, methodology calculation by about 6 inches.

However, in light of other conservatisms, we still felt that the pool swell that revised alternate criteria was acceptable. Those other conservatisms included conservatisms in the 4T tests, conservatisms in the methodology for calculating the drywell pressurization when you use the model according to this NEDM-10320 prescription, when you use that on an individual plant.

In addition, one should point out that the measurements do bound also the froth measurements, not just when the pool is as a solid ligament. And you do not really get any substantial loads at that point. And that froth region is the order of magnitude of maybe about a foot.

One other point is that, typically, design breaks are saturated liquid breaks where you have much more conservatism in the methodology.

Run no. 35 happened to be a saturated vapor run. One limitation on the whole thing is that there is some freedom that the Mark II owners have in how they would do their

DW

drywell pressurization calculation as an input to the pool swell model.

We had checked this methodology by checking the two together with the conservatisms of NEDM-10320. Should something else be used, that does not have these types of conservatisms in it. Then they should take that combined drywell pressurization models with the pool swell model and do the same type of check.

But if they use this methodology, we find it acceptable.

(Slide.)

Just for background purposes, this is the test data that was used for that comparison of the methodology, comparing that methodology, the measured height against the calculated height for Phase I 4T test. And you know that this shows two points that are above the specification.

However, one should recall that they have resized the model to say that you will not be less than 1-1/2 times submergence. This is 11-foot submergence. You go over here and it does bound all of this one point, which is just a little bit above. And one sees that for the Phase II test, again, that the methodology is conservative.

(Slide.)

And then the last of the pool swell related area is the asymmetric pool boundary load. This is the bubble pressure related pool boundary load that could potentially result in circumferential variations and steam as it goes in the drywell and then into the vents.

There was not originally any specification in the DFFR. We felt that it should be addressed. We came up with an excessively conservatively specification based on some of the earlier proposals for the Mark III.

And what we had ended up specifying was that all of the air would be vented on one-half of the containment, all of the steam on the other. And that would result in maximum bubble pressure from all of the air on one side as calculated by the pool swell model at the time of vent clearing on one side, and then assuming complete condensation of steam on the other, zero pressure on the other side.

We recognize that this was a very conservative specification.

(Slide.)

A revised specification was proposed, and as a result of our review of that, we concluded that 20 percent of that maximum calculated bubble pressure vent clearing would be acceptable.

That is 20 percent of the bubble pressure on one side and nothing on the other. And the basis for that are some calculations that were done, and also some qualitative arguments. The calculations were based on a simple two-vent model where one assumed a break that occurred close to one of those vents.

The steam exiting from that break would undergo homogenous mixing with the ai . That homogenous mixture would go — would enter the first of the air, initially air-filled vents.

Immediately, it would move at a speed such that in 4/10ths of a second, the second vent would be supplied with that homogenous steam-air mixture and be supplying compressed air to the far vent up to the 4/10ths of a second.

At 4/10ths of a second, it was inferred from a couple of things. The maximum distance between two vents for a Mark II facility and the velocity of propogation of the steam front was inferred from some PWR 1/64th scale tests where they had similar shock velocities. And they estimated the velocity of the --

DR. YAO: How do you determine the 4 seconds?

DR. ANDERSON: That came from two areas. It came from looking at the Battell tests, the 64th scale Battell tests. One can infer what the steam front velocity was. And then you can pick out what the worst distance is between two vents.

And they came up with 4/10ths of a second.

BWH

You mean two tenths?

DR. ANDERSON: That gave us a good idea of what the steam front velocity would be, the Battelle test.

DR. YAO: What is the scale?

DR. ANDERSON: 1/64th.

DR. YAO: You are talking about a time constant, actually, uniform for all of the scales, scaled down test.

DR. ANDERSON: What they did was they looked at the shock wave velocity for the two facilities, for a prototypical Mark II and also for that facility.

DR. ZUDANS: But the .4 would be significantly long time for this process anyway, so it probably wouldn't matter how much longer.

DR. ANDERSON: This model had a number of other conservatisms in it. It is kind of a difficult thing to get a handle on. And we felt we should make some attempt to upper bound this thing, so that is what they did.

DR. YAO: It is a conservative estimate.

DR. ANDERSON: Yes.

DR. YAO: Thank you.

DR. ANDERSON: Actually, in their calculations they came up with no more than 10 percent of the maximum air bubble pressure as this asymmetry. We tried to reproduce that using the pool swell analytical model and our consultants weren't able to come up with exactly the same number. They came up a little bit higher. And rather than spend a lot of time on this the Mark II owners did agree to the use of the 20 percent because they could accommodate this without any major problem. That was the basis for that.

There are some other qualitative arguments, as I indicated, for our not having substantial maldistribution of the steam air that would give this bounding, instead of the specification we had before including if we had a break it would be turbulent flow and some good mixing. In addition there are enough structures so that you would — that would aid in the mixing process. And they took a look at the Marciken data. It is hard to infer much from this but they did not see any major pressure variations within the Marviken multivent test.

That concludes my presentation on the alternate LOCA loads. Now I will turn it over to Dr. Economus from Brookhaven.

DR. ZUDANS: Could I ask a question?

DR. PLESSET: Before we let you go, let's see if there are any questions.

DR. ZUDANS: All of this reasoning, really, in bounding the asymmetric boundary loads was based on non-uniform -- let's say time lag in feeding one of these two vents. What about the aspects that would be subsequent to this type of deal, where you have variable submergence at the same time?

DR. ANDERSON: Recognize that we did talk about potentials for variable submergence. If you want to talk about that some more I guess we could. There are various things that can result in asymmetries. There is an asymmetric chugging load. There is an asymmetric safety relief valve load. This one, we believe that that was taken care of. All of the other ones were taken care of. This one just deals with an asymmetric bubble load because of different steam content in different parts of the containment.

DR. ZUDANS: Because of time lag, but let's say if you reduced the asymmetric pool swell itself. That situation would reinforce the asymmetry because you would not -- you would have the vents at a different level.

DR. ANDERSON: But you can get into a number of different arguments that if you have low submergence and you are going to have a quicker clearing time. And this is the one that we concern ourselves with. We did not impose -- what is the mechanism for the variable submergence?

DR. ZUDANS: Thank you.

DR. MARK: Just a matter of semantics. You take the pressure going back to your first item here, at the downcomer level at 24 pounds and extrapolate it to zero at the pool service. Is that the original pool service or the service that the pool might get to after you empty the downcomers? What is the basis for saying the original is an adequate fixed number there?

DR. KUDRICK: When you are talking about this you are talking about very early, at the time of vent clearing. You don't have much motion at that time.

DR. MARK: This is a phenonena that you are worried about before there has been any actual fluid displacement.

DR. KUDRICK: That's correct.

DR. ANDERSON: Any other questions?

DR. PLESSET: Any other questions? Thank you.

DR. ECONOMUS: I am from Brookhaven national laboratories. In the area of alternate load for SRV the only open issue is the so-called load case five. And I would like to give a little bit of background on what that is.

In NUREG-0487, each of the applicants was required to do design evaluation for a series of load cases. There was the single valve and so on, and load case five required that the evaluation be done for an all-valve case where all of the bubbles are assumed to enter simultaneously and oscillate in-phase. In addition, pressure loads would be computed using the Ramshead model. The amplitudes would be combined from the various valves using absolute sum and a range of bubble frequency would be considered.

Now, as Cliff indicated earlier, this NUREG specification was developed prior to any information regarding the actual performance of the T quencher, which would be utilized by the lead plants. And so the use of Ramshead model for estimating the bubble pressure, bubble amplitude. We recognized that was conservative but in the absence of any definite information as to the performance of the actual device that would be used, this was the only alternative we had.

(Slide.)

The lead plant had done evaluation for all valve load cases. However, phasing was -- there was phasing permitted in these all-valve load cases that came from mechanistic considerations taking into account different set points, line volumes and so forth. The results of those design evaluations showed that the containment was adequate.

The lead plant applicants felt that the use of the Ramshead loads combined with this simultaneous in-phase oscillation was excessively conservative and they proposed an alternative which consisted essentially of satisfying the criteria of NUREG-0487 for load case five in terms of the simultaneousness of the bubble and the in-phase oscillation, that replacing the pressure loads with something that more realistically represents the performance of the T quencher.

(Slide.)

I think you all know that the lead plants have committed to the use of this so-called KWU T quencher, and our evaluation of this alternative is that it does comply with all aspects of the criteria with the exception of the use of the pressure amplitude basemat and the T quencher load, T quencher results which were obtained experimentally, and again, the frequency range which bounded all of the frequencies that had been observed. And 329 more correctly represents what was observed with the T quencher.

DR. CATTON: What about plants other than the lead plants?

DR. ECONOMUS: Yes.

DR. KUDRICK: WPPSS-2 has the GE X-quencher, which is supported by the CAORSO test program.

DR. PLESSET: Is that the only one?

DR. KUDRICK: That we are aware of.

DR. ZUDANS: It is not Ramshead. It is quencher. The question was with respect to Ramshead.

DR. ECONOMUS: Anyway, if the alternative proposal is acceptable the pressure amplitudes that are utilized, that they propose to use in the design evaluation, are supported by the results of the KWU tests and similarly the frequency range which is proposed to be used for design evaluation is also supported by the results observed at KWU.

(Slide.)

The current status as far as the design evaluation is concerned, the lead plant applicants have taken this new T quencher load specification and have done design evaluation for piping, particularly for critical piping systems, those in the frequency range of the particular T quencher device, and have found that the design is adequate. Shoreham has documented the evaluation formally. The LaSalle and Zimmer plants made a presentation at a July meeting and we expect that documentation for the piping systems evaluation will come in by the third quarter.

All of the lead plants are currently making their evaluation of the — of their equipment. They indicate to us that the design is adequate and we are not too certain at this point when that evaluation will be in a document, but it should be in the not-too-distant future.

DR. ZUDANS: Just one question. In terms of this frequency from three to nine cycles, that has been now observed based on KWU tests, are there any structures sitting in that pool that would have natural frequencies in that range?

DR. ECONOMUS: There may be, but they indicate that their structures are capable of taking these —

DR. ZUDANS: That is not the question. The question is, are there any structures where there is reason to believe that --

DR. ANDERSON: The downcomers currently are somewhere below the 7 hertz.

DR. ZUDANS: They are in that range.

DR. ANDERSON: Yes, I believe so. I am not familiar with other ones. There may be some other ones.

DR. ZUDANS: There is not much else there, anyway.

DR. ANDERSON: Right.

DR. ZUDANS: Thank you.

DR. ECONOMUS: If there are no other questions, I will turn it over to Professor Bienkowski of Princeton, who is going to update you on alternate submerged structure drag loads.

DR. PLESSET: We may want to hear from the owners groups about this natural frequency question.

DR. ZUDANS: And associated questions.

DR. PLESSET: Later.

DR. ANDERSON: I believe in the discussions of the downcomer design, we will be hearing some discussion with regard to the natural frequency.

DR. PLESSET: That's good.

DR. BIENKOWSKI: I guess I wasn't informed that I was supposed to have copies of the slides for everybody, so I am afraid I didn't bring any. Maybe we can try to get some later today and give them to the committee later.

DR. BATES: If you get them to me I will distribute them to people.

DR. BIENKOWSKI: I am sorry about that.

(Slide.)

I would like to talk about an update on the submerged structure loads. The first slide just sort of gives an outline of the format that I would like to use. The first thing I would just bring you up to — to remind you of what the origin of the loads is. I will go over them rather quickly because we presented that in November. I have a slide showing the history of the load specification, which essentially corresponds to the next four items, which is the initial owners' methodology, what the NRC acceptance criteria were, what the owners' response on some of those issues were where they did not wish to accept the criteria directly, and finally as to what the supplement to acceptance criteria will show.

I have additional slides in more detailed technical basis for these various things and I will only show those if there are specific questions on those issues where somebody has a specific question as to the basis.

(Slide.)

This one, I would like to go over very quickly. This is a slide I had in November and you have already seen the origin of the loads for other LOCA earlier on SRV-related loads. These are really the same. There is a vent clearing phase where a jet comes out of the vent. There is an air bubble formation phase where LOCA, where bubbles of entry coalesce and pool swell for SRV, where bubbles which separate and oscillate and rise up, and all of these can induce submerged structure loads.

And finally, there is a steam condensation oscillation chugging loads. These were left in the acceptance criteria to be plant-unique, and I believe they still are; however, the owners have indicated more or less the direction in which they are going from these loads, so when the occasion arises, I will just indicate a little bit about that although we have nothing informal that we have been able to evaluate on that.

(Slide.)

This slide is mainly to show you where the information is that indicates the history of the load specification submerged structures. The initial proposed methodology was essentially based initially on the DFFR and at least at the time of the writing of the NUREG-0487 an applications memo which give specific ways of calculating submerged structure loads, I believe was later incorporated in supplement three to DFFR.

The NRC acceptance criteria essentially accepted the major procedures of the owners' proposed methodology but had a fair number of small exceptions and changes to guarantee conservatism in application of these. These essentially involved jet loads, the computation of the induced pressure arising at the jet front and acceleration drags that could be produced from this in front of the jet.

The second issue which was a fairly major one had to do with what are the appropriate standard drag coefficients and the issue there was essentially one of not wishing the owners to use only data from steady flow, but rather using drag coefficients from flows which were more like the flows induced by LOCA or SRV, either oscillating or accelerating flows, the result of the issue of interference effects between structures which were sufficiently close to each other.

And finally, there was an issue -- the owners propose to use the velocity calculated, the geometric center of the structure, as an equivalent uniform flow for computing drag on structures. The NUREG criteria said, it is not always conservative, certainly if the structure is very large, and it has some flow which is substantially higher over portions of it in the geometric center and the proposal was to use the highest velocity rather than the geometric center. This produced, apparently, quite a lot of difficulty in terms of the implementation for the owners because there are many sources and it is sometimes difficult to find exactly where the highest velocity would exist in a structure. And so they wanted to — they proposed an alternative way of showing how small a segment they had to divide the structures to still be able to use the geometric centers. So that is why we are calling this nodalization now.

All of the acceptance criteria, of course, are in NUREG-0487. Now, the owners' response has been more or less in a direction of some of the issues that we have raised in the acceptance criteria. They have just accepted directly. Others, they have addressed the concerns but have chosen to do it in a somewhat different way than the way that was done in the acceptance criteria.

And the main information for this is in a draft report that Cliff Anderson mentioned. That is not yet, I believe, in formal form. We have a draft report on submerged structure methodology.

DR. CATTON: Are you going to tell us what areas they have an alternative formulation for?

DR. BIENKOWSKI: Yes, I will discuss that.

(Slide.)

What I think I will do is I had some more slides re'iewing the — what the owners' original methodology and acceptance criteria were, but I think what I will do instead now is go through the water jet loads, bubble loads, condensation loads one by one and highlight only those areas where there has been some difference, where there is some alternative methodology that the owners are proposing.

(Slide.)

In this particular case, things have been changing rapidly, so since I made this slide there has already been — some of what I am saying is not quite accurate.

The original NRC acceptance criteria for LOCA water jet loads was, as I mentioned, primarily to modify the strictly one-dimensional model which was in the owners' methodology to include induced flow at the jet front. It was a pressure induced by the accelerating water out in front of the jet. And to include the acceleration drag as well as the steady drag for SRV jet loads — we felt that these were not going to be a very important point and we proposed a sphere of influence around the quencher arm where if no structure was in the sphere of influence, or did not have to consider jet loads — the owners' response, I understand, now for all lead plants is for the LOCA jet loads they will essentially follow the NRC acceptance criteria.

So there is — there was at one time talk of one plant following a plant-unique path of using the ring vortex model. This, I believe is in the long-term program. The owners did propose in the SRV quencher jet load to modify the sphere of influence, rather a cylinder of influence around the quenchers to a five-foot cylinder.

We have examined this, based on test data, and found this acceptable.

(Slide.)

DR. PLESSET: Has the staff looked at this vortex analysis?

DR. ANDERSON: We have done some preliminary review of it. We have not received any reports dealing with how the methodology would be applied to plants, but just a basic description of it. Perhaps Professor Bienkowski might want to say something about preliminary observations. Would you want to hear that?

DR. PLESSET: Sure.

DR. BIENKOWSKI: I think —

DR. PLESSET: I think the owners group may talk about this, too.

DR. ANDERSON: They have no formal presentation. Only in response, I think, to your questions.

DR. PLESSET: Fine.

DR. BIENKOWSKI: I have examined some formal reports on the ring vortex model and on the basis of that, not the issue of how it would be applied to actual plants, the phenomenological influence of what is going on, it appears that the comparison to EPRI data seems good, including things like pressure time histories on the floor.

The difficulty, as I saw it, with that and how it would be applied to plants that the methodology is essentially formerly rigorously valid only up to the time of vent clearing. And therefore it cannot say much more to — it leaves sort of a space between when do you go from the jet model to the air bubble model and the time of vent clearing is not the time of maximum pressures necessarily, maximum accelerations in the pools.

So the issue there was, how would it be applied to plants in a conservative way to take care of the transition from the jet model to the air bubble model. As I said, this is all based on a relatively brief informal report at this stage.

DR. CATTON: I noticed in looking through, in Chu's model and all of his predictions, they were never carried to the peak pressure that was measured.

DR. BIENKOWSKI: Because that occurs after vent clearing in his model, he is not capable of directly in the model of taking — when all of the water has come out of the vent and air is now entering into the jet and mixing with the jet, he is not capable of carrying that calculation within his model. He cannot have two-phase flow.

DR. CATTON: Then the model doesn't really help you a whole lot, if you are interested in the peak load.

DR. BIENKOWSKI: That is the issue I was referring to about the transition. If you really ask yourself, what is the transition, when do you go from a jet to the air bubble, there is always a problem in any one of these models.

DR. CATTON: I guess I would have to say that I am not covinced that the peak load occurs after the air bubble begins to grow.

DR. BIENKOWSKI: In all of the data analysis with EPRI the peak load occurred after vent clearing, and indeed all of the comparisons of Chu's models carried only as far as air clearing. He has some — that part I have not heard. He has some ways of trying to take account after vent clearing and predict what the pressure is, and I have seen some slides which I would hate to stake my reputation on, because I have just seen some slides showing the continuation beyond the up-to-peak pressure and so bounding the peak pressure as well, but that is something that I have not seen the details.

Up to vent clearing, all of his pressures have been not only bounded the EPRI tests, but have actually followed the trends very well.

DR. PLESSET: Peak loads where? Which peak loads are you talking about?

2-3

DR. BIENKOWSKI: Take loads off the pressure on the bottom, the basemat.

DR. PLESSET: Other comments?

DR. WU: Is there any preliminary -- any follow up work after the -- my question is, has there been any follow up work right after the vent clearance, followed by the bubble expansion into the lower plenum?

DR. BIENKOWSKI: Maybe the Mark II owners can respond to that better than I can.

DR. PLESSET: That is a good point. We will let them talk about that when they make their presentation. We don't need to --

MR. KUDRICK: They have no presentation on the point.

DR. PLESSET: But they are willing to talk, I guess, when they get their turn.

DR. CATTON: I have one more comment. The report by GE, "Analytical Model for Liquid Jet Properties for Predicting Forces on Rigid Submerged Structures," discusses the particular process of transient formation of a jet.

DR. BIENKOWSKI: That is the one dimensional model.

DR. CATTON: They refer to data or observations which indicate a physical process that is somewhat unlike what is modeled in Chu's paper. I am wondering if there are any attempts to bring the two closer together. One is on one extreme, and I think the other is on the other extreme.

DR. BIENKOWSKI: The NRC acceptance criteria attempted to do that. The NRC acceptance criteria, we have said you can use the model for the jet within the major portion of the jet, but the front in this one dimensional model has an infinite extent and is infinitely thin, because when you do the conservation momentum, you get a shock front at the front of the model where the particles catch up. So you have said they must somehow model the front differently by saying that you take whatever was in the mass at front and create something like a hemispherical or spherical cap, which propogates with a shock front and induces the flow in front of it.

The idea was to allow for objects which are not directly impinged by the jet, but still in front of the jet, to feel some pressure, because this one dimensional model would show no forces on an object until the jet had actually impinged.

DR. PLESSET: But you are using the words "shock front."

DR. BIENKOWSKI: It is used in there.

DR. PLESSET: That's good. I'm glad. But I would think that some calculation like Chu's might be quite a bit better than that until the vent is clear. What do my experts say?

DR. CATTON: I would agree that this is at one extreme.

(Laughter.)

DR. PLESSET: You don't disagree with it?

DR. BIENKOWSKI: I think in most instances this is more conservative than Chu's model. I would say that the physical phenomena after vent clearing is certainly better represented by Chu's model.

DR. PLESSET: I think that is a good place to leave it, until the owners group might want to make a few comments.

DR. CATTON: I am not sure that Chu's work was on the conservative side. If I had to make a guess, I would say that it probably falls on the other side, and I am not sure why. I am sure there is a great deal of numerical confusion, so he is essentially looking at a — and his model, even though he is attempting to model with this —

DR. BIENKOWSKI: The comparisons I have seen of the propogation of the ring vortex, both forward and to the side, comparisons with EPRI tests have looked quite good.

DR. CATTON: The EPRI tests are small diameter.

DR. BIENKOWSKI: I don't thing the Reynolds' number — I think it is high enough. I don't think real viscosity — numerical viscosity is a separate issue. I don't think real viscosity plays a very significant role.

DR. CATTON: The issue was not real viscosity but numerical.

DR. BIENKOWSKI: The EPRI test did not have numerical viscosity.

DR. WU: Is it proper to say that the Chu model is almost on the best estimate, intended in that direction, and the other is more conservative.

DR. BIENKOWSKI: That is what I was implying. I think it represents the physical phenomena much more closely, and the issue of how to guarantee that it is conservative is what I was leaving to the issue of if and when the Mark II owners want to use the model, and they want to say how do you provide conservatisms into that to make sure that all of the data is bounded. That is the issue of what kind of a source terms — how you can provide conservatism with a faster velocity with water-air interface, and I think there clearly would be questions answered as to just what numbers do you put in to provide conservatism.

All I was really referring to is I think the basic phenomena, in terms of what is going on, in terms of the shape of the cloud, the time at which it happens including the pressures on the floor, the phenomena seem to very well model experiments up to that clearing.

DR. PLESSET: The analysis seems to be much better in the sense that people call mechanistic. In other words, it uses a real physical description, but there are these points that Professor Bienkowski mentions.

DR. CATTON: I would agree with that, and the Mark II owners, if they don't use it, it is academic.

DR. PLESSET: Yes, in a way it is.

DR. CATTON: An interesting academic problem.

DR. YAO: I have one comment. We generally know the vortex type calculations, that it is unstable, numerically unstable. But I think it has been demonstrated, if you introduced a small numerical viscosity, you can get a stable result and a result quite accurate.

DR. BIENKOWSKI: I think I will accept the comment without additional comment.

(Laughter.)

DR. PLESSET: Why don't you go on?

(Slide.)

DR. BIENKOWSKI: I spent all of that time on what I was not prepared to talk about.

(Laughter.)

On the LOCA air bubble which presumably occurs sometime after vent clearing and is based on essentially a spherical bubble, the original -- there were a number of issues that we were -- addressed in the acceptance criteria, and a couple of them were found acceptable by the owners, and I will not discuss them again because I already mentioned them in November.

We wanted to provide additional conservatisms associated with the bubble asymmetry, since the model is based on a symmetric bubble, and the data indicates they are not that symmetric. Another was the blockage effects in the pool swell portion. These are sort of typical wind tunnels which you have for drag due to the fact that the flow is constrained to flow between -- in tighter quarters.

These they found acceptable. I will not discuss more about that.

The main three issues which not only refer to LOCA but also SRV quencher air bubbles and condensation loads, I will discuss all together instead of separating, because they are essentially the same issue. What is the use of the standard drag coefficient?

The Mark II owners proposed use of a steady flow drag coefficient for the standard drag was not acceptable because of data that indicated there are unsteady conditions in certain situations. These drag coefficients could be substantially higher than the steady flow coefficients. The owner's response essentially has been -- and so we propose -- I step back. We proposed essentially, based on the data that we had available, that the owners either could do more detailed study of this and produce, justify the drag coefficients, or they could use what we considered conservative upper bounds on these coefficients, which were essentially like three, three times the standard drag coefficient, which was bounding all of the data we had available at the time.

The owners have essentially proposed to do this differently for LOCA and for SRV. The reason is actually quite sound. A LOCA situation is essentially a uniform accelerating flow where the flow direction and the acceleration are in the same direction, and , in deed in both of those instances, the drag coefficient, if anything, is slightly lower than higher for such an accelerating flow. So they want to use the data for such a uniform and impulsive flows for the standard drag coefficient, and that brings them back to using the steady flow drag coefficient.

However, for SRV bubbles and for condensation oscillation loads where the flow actually oscillates back and forth, there is a flow reversal. The appropriate data is data from oscillating flows. And in those situations, that is where the upper bound factor of the three came from. They will, indeed, use the relevant data so that they will use the drag coefficient appropriate for the particular period parameter. This is a function of the period parameter, which is nothing else but the velocity times the period divided by the diameter of the body.

And, indeed, it makes sense to do that, because for many of the larger structures, this parameter is quite low, and the drag coefficient of three times the steady flow coefficient would have been ultraconservative in those situations. It turns out that to some extent for many structures it is a non-issue, because for large structures it is acceleration drag that is important, not the standard drag.

So you are talking about worrying about a factor of three on something that is only ten percent of the total load.

The other issue that was raised in the NRC acceptance criteria were interference effects. And, again, we provided a rather — the possibility of a conservative bound, saying that the structures were closer — if the structures were further apart than three diameters of the largest structure, they did not have to worry about interference effects.

But for structures closer than that together, they could either do a detailed analysis or have a conservative multiplier which is essentially a factor of four on the draggage, which came for structures which clearly were very close together. They chose, again, not to use the conservative multiplier, and, indeed, the draft report, as I mentioned — about two thirds of the report is based on a fairly detailed literature study of the information interference effects and categorizing of different conditions.

So they have answered by saying they will use appropriate data and analysis for those four structures which are closer than three diameters.

DR. BUSH: For clarification of the statement regarding the LOCA being different, is that equally applicable to a small LOCA. I would think you could get fluctuation effects.

DR. ANDERSON: You get the same kind of strain phenomena for the condensation oscillation over range.

DR. BIENKOWSKI: I think the question was about the air bubble.

DR. ANDERSON: We don't think we get any substantial air bubble.

DR. BIENKOWSKI: Those loads, the air bubble loads, would be bounded by the DBA loads. Even if they were there, I would assume --

DR. PLESSET: I think Dr. Bush's point was, have you really thought carefully about any problems that might arise from something smaller than the DBA? Isn't that what you were thinking, Spence?

DR. BIENKOWSKI: In connection with submerged structures?

DR. PLESSET: Or any other part of the containment problem.

DR. BIENKOWSKI: Certainly, I think chugging loads. Everybody agrees that it is not the DBA that is the bounding consideration. I think I am going outside of my expertise to answer other parts of the submerged structure .

MR. KUDRICK: Relative to chugging, it really does not matter whether it is a small break, medium break, or DBA break. You have basically the same phenomenon when you get into that flow regime. CO is more pronounced at the higher mass fluxes, so it is more conservative looking at it from the DBA standpoint. So we have looked over these loads over the spectra to ensure that we have selected conservative breaks.

DR. PLESSET: I think that is the answer that we want to hear -- that you have thought about it.

DR. BIENKOWSKI: In connection -- as a matter of fact, I was somewhat deficient in explaining all of the details, because I didn't want to get into all of the them. Actually on the LOCA air bubble, when they get to the pool swell portion where the pool rises and comes back down, they do, indeed, consider that to be half a cycle of an oscillatory flow and use the drag coefficients from the oscillatory flow, even for a regular LOCA. It is the expanding bubble portion they consider to be a uniform accelerating flow.

(Slide.)

I did not mention the issue that we addressed in the acceptance criteria with the equivalent uniform flow assumption. That is to be applied at a geometric center. I think the issue there was really a question of geometric center of what, and we tried to cover that and be conservative by saying for any particular segment of the structure, just take the maximum flow velocity and use that position. That turned out to be not easily implemented, so what the Mark II owners have done -- and it is also included in the draft report -- they have done a sensitivity analysis of segmenting structures into smaller and smaller segments, basically a numerical study to find out at what point the load, the total loads, in a structure no longer change.

They included structures -- the ones that were going to be closest to the sources. It turned out as long as you kept within one to two diameters of the structures, the loads were changed by only a fraction of a percent or so for going to any tighter segmentation. And I will talk about this when we talk about the supplement. We will find that procedure essentially acceptable.

DR. ZUDANS: When you say about segmentation, meaning then you would use some geometric center for each of the segments, rather than look for maximum velocity for something that is non-describable.

DR. BIENKOWSKI: They are going to use the geometric center. The difficulty with the maximum velocity was not so much that most of the structures are long cylinders, pipes, downcomers, so it wouldn't be too hard to find where the geometric center or the maximum velocity was, if I had only a single source and a single structure.

The difficulty is that in their numeric model, you may have a structure, but you have many sources. And so now if you take literally what you mean by the maximum velocity point, you sort of have to hunt where that maximum velocity point is. It turns out that if you segment the structures in segments of about one diameter to one and a half diameters, the effect -- there are theoretic studies to show that if you have a nonuniform flow and you have a cylinder, just a nonuniform flow, that taking the geometric -- the velocity of the geometric center is conservative or at least for theoretic calculations, is actually -- it is correct to pick the velocity at the geometric center for the acceleration drag at least.

DR. ZUDANS: The segmentation is longitudinal?

DR. BIENKOWSKI: Yes.

DR. ZUDANS: You pick a piece and then the geometric center and so forth, rather than taking the entire structure and picking a single geometric center?

DR. BIENKOWSKI: Clearly, if there was a pipe and the source was here and you picked the geometric center, you would not necessarily be conservative. That was clearly the concern that we were trying to address in the acceptance criteria by placing restrictions on the segmentation of about one to one and a half diameters. We feel that concern has been met.

DR. ZUDANS: There is no segmentation within each of the segments?

DR. BIENKOWSKI: No, they are treated as cylinders.

DR. ZUDANS: Since this is on velocity, there was a discussion of fallback velocity. Are you going to talk about that, or it doesn't represent part of your presentation?

DR. BIENKOWSKI: The issue of the fallback velocity is not part of my presentation. The treatment of the submerged structures during that portion, they treat essentially as -- by the same procedure, the drag coefficient chosen for oscillating flow.

DR. ZUDANS: I would have one question, but maybe there is some other question for it. The question is, the draft report says that velocity will be based on the free flow velocity throughout the upper surface shown directly above the subject structure. I am thinking in terms of — that sounds to me okay.

DR. PLESSET: Hold that until tomorrow. That is Mark I.

DR. BATES: That is Mark I acceptance criteria.

DR. PLESSET: We will get to that for sure.

DR. ZUDANS: I would say that Mark II has the same question. I have just used the words out of that section. There is presumably a similar situation for fallback velocity in Mark II, and if it is calculated from what point. It is from a point that the water reaches and it starts falling back or what happens if it impacts some structure? It is under some angle? Is that impact velocity then taken into consideration? And you can impact laterally structures with higher velocities than you expect the fallback velocity would be.

DR. ANDERSON: I don't understand the full question, but as I recall the point for calculating the velocity was the point of maximum elevation. Did I miss some of the other points?

DR. ZUDANS: Maybe it does not have application to Mark II as clearly as it does in Mark I.

DR. PLESSET: That's right. The question is not without meaning, but I think it is significant really for Mark I. So I think we will get some —

DR. ZUDANS: Tomorrow.

DR. PLESSET: Right.

DR. BIENKOWSKI: I am almost done.

(Slide.)

This is a copy of a slide that was presented to us by Mark II in terms of how the concerns in connection with the drag coefficients interference effects, and nodalization has been addressed.

I am putting it up for those of you who may want to know where the data and references are. For the unsteady flow, we basically have two sets of references: accelerating flow and oscillatory flow. This is actual a number of papers of Sarpkaya. I would actually myself add also a paper by Keulegan and Carpenter, because that happens to be the only paper that I know of that has sharp structures rather than just cylinders. So it is important for one of the issues.

Interference effects, they divided for standard drag and accelerating drag. Some of these are theoretical. This on is an experimental review paper. For accelerating drag, it is mostly experimental, although there is also -- mostly theoreticl, although there is some experimental work by Sarpkaya.

These interefence effects are basically of two types: one, for structures close together; another for structures close to walls. We — the NRC acceptance criteria, we had included the transverse forces, lift forces, as part of the conservative coefficient on the drag. In other words, taking the maximum total force on this subject, Mark II owners have chosen to separate these, so indeed they are including the lift due to vortex shedding and unsteady flow which can produce significant transverse forces, at least for the oscillating type flow.

For most of these situations, the Mark II, for the LOCA air bubble where the flow is just accelerating, most of the phenomena are over before you have had enough time for the vortices to separate, so there is no lift force.

But for the SRV and condensation oscillation, one has to consider these.

I already discussed structural nodalization, and essentially the owners have done a study showing that if the length of a segment is on the order of one to one and half diameters, the numerical values are not changed.

(Slide.)

To summarize, then, the supplement to the NRC criteria requires no changes now in the net loads, since the owners have effectively accepted them as they are for the lead plants. On the LOCA and SRV air bubble loads, we find the data and theoretical calculations for both the drag coefficients and interference effects that the owners have proposed for cylindrical structures are acceptable. They are based on data relevant to those structures. For non-cylindrical structures, the owners propose to just use a circumscribed cylinder for computing correction factors between, let's say, unsteady flow and steady flow or correction multipliers for interference effects, and then using those correction factors of the actual drag coefficients for the particular structure it had.

We found this to be somewhat worrisome in the sense that the little bit of data that is available for sharp edged structures was clearly — the vortex separation is different for unsteady oscillating flow, which is the Keuleagan and Carpenter paper for a flat plate -- indicates much higher drag coefficients compared to steady flow than you would get from just the circumscribed cylinder.

So we have said we are accepting the draft report for cylindrical structures. For structures with sharp edges, we feel that drag coefficients or standard drag should be taken from relevant data which, if they can find other than Keulegan and Carpenter, we would be happy to see. But if not, at least for something like flat plates which at least has the effect of sharp edges in it, no lift coefficient — the other thing is clearly that if you have a circumscribed cylinder, the only lift you can get is from vortex shedding. But if I have a, let's say, an I-beam or a rectangular structure on which the flow impinges at some angle other than an angle of symmetry, I can get lift on that structure even without worrying about the unsteady effects.

So clearly doing the circumscribed cylinder does not account for that effect. So in the supplement we would include criteria that will require it to either get such a lift coefficient from data or some approximate theory, or we felt that a bound of something like 1.6 from all of the data I have been able to see would clearly be a conservative bound. And you would have the coefficient on a reasonably non-streamline structure.

On the quencher, the only issue there is what the source strength — how the source strength for the quencher is chosen. And that requires some evaluation. But it appears that the procedure is essentially acceptable.

For condensation levels, we have only been given a glimpse of the — as I said, these are to be plant-unique, so they are not part of the NRC acceptance criteria at this stage. We have been given only a glimpse of what direction the Mark II owners are going. It appears that the approach appears reasonable to us now.

The issue will certainly again revolve around what is — all of the other issues are still there. The main issue will be, what is the source strength.

DR. WU: Is it easy to define the Keulegan-Carpenter number for this kind of problem, involving bubble —

DR. BIENKOWSKI: The period parameter?

DR. WU: Yes.

DR. BIENKOWSKI: It is not for LOCA. They are not using that data. It is clear it is going to be very hard to say what you are going to do about sharp-edged structures in uniformly accelerating flow. But it appears that oscillating flow bounds things for uniformly accelerating flow, and the only parameter that would be comparable in accerating flow would be the time times the maximum velocity, divided by parameter.

DR. WU: I thought in the original paper they used the -- they used the velocity farther away from the object as relatively easy.

DR. BIENKOWSKI: You are talking about the experimental issue.

DR. WU: Is it really significant? That's one. And if it can be fairly well defined, then what is the range of the Keulegan-Carpenter number cover for this type of calculation. And thirdly, is it still following a similar approach, namely, the linear position of the proportion of the acceleration? And the other is to the absolute velocity times the velocity type of drag coefficient.

DR. BIENKOWSKI: You can argue that the -- in dimensional analysis, you can argue it is invalid if you use drag coefficient and acceleration coefficient as functions of all other nondimensional parameters. So in a sense -- so the issue is, can I pick one drag and one acceleration coefficient.

In the Keulegan-Carpenter and Sarpkaya's work, the hydrodynamic coefficients vary with parameters. So you can say it is not a totally linear superposition. You're asking essentially a philosophical question. I don't know the answer to your question. I wish there was more data, and indeed, I don't know why there has not been more data, why

BW mte

rather than sharp edged structures.

It seems to me there is a very significant issue of the vortex shipper separation with sharp edges that will be quite different. I was referring to the only paper I'm aware of, is the Keulegan-Carpenter paper. It is the best data I know of. And it is true that it is probably subject to some questions.

DR. PLESSET: Any other questions?

(No response.)

Thank you. And I think this would be an appropriate time to have a ten-minute break. So we will reconvene in ten minutes.

(Recess.)

DR. PLESSET: Let's reconvene.

I would like to say that Dr. Bates would appreciate it if those who haven't signed this attendance sheet before would do it as soon as possible and give it back to him.

I think that we will go on with the rest of our agenda, and we are going to go to presentations by the Mark II owners group. And I think that Mr. Crawford is going to start off. Is that correct?

DR. CRAWFORD: Yes.

DR. PLESSET: Before you begin, Mr. Crawford,

mte

Professor Bienkowski, you were going to give Dr. Bates your slides, or somebody, so we can have them?

DR. BIENKOWSKI: We are getting copies.

DR. PLESSET: Fine.

Proceed, Dr. Crawford.

DR. BIENKOWSKI: My name is Ray Crawford. I am from Sargent & Lundy, and I would like to speak with you now and tell you what the status of our analysis and assessment for the effects of the submerged structure loads on the downcomer, main downcomer vents is.

I would like to follow what Mr. Anderson introduced earlier, and I would like to briefly review the type of design that is employed in LaSalle and Zimmer for the downcomer bracing. We have a pre-stressed concrete structure with an integral diaphragm floor. The downcomers themselves are anchored into the diaphragm floor, and that provided the main support for those downcomers against any lateral loads acting on the downcomers, any dynamic lateral loads.

In the case of LaSalle, there was a restraint or bracing system just underneath the diaphram floor above pool swell, maximum pool swell height, and that equally distributed the load on the floor for the lateral loads that existed.

(Slide.)

That system of design for the downcomers was analyzed for the submerged structure drag loads of LOCA and SRV according to the initial load specification, as Mr. Bienkowski pointed out, contained in the DFFR. That assessment included the effects of inertial drag, and it also included the localization of the local flow field effects.

The assessment of the structures to this load definition was contained in the design assessment report submitted approximately in the first quarter of 1976. More recently, in 1978, to update the design assessment report, there was a closure report prepared which accounted for any changes in the load definition that was contained in revision two of the DFFR, and it does provide additional justification for the methods of predicting the submerged structure loads.

We have not completed our assessment for providing the results and assessment for all of the loads, and that was to be contained in a design assessment report amendment. At that time, all of our assessment work indicated that the criteria was satisfied on all of the structures.

There have been some recent changes, however, and I would like to briefly review what those changes are and what we are doing about them. There has been three rather significant developments since that time. One is the NRC acceptance criteria. Secondly is the adoption of the KWU T-Quencher for SRV discharge. And thirdly is the steam condensation drag loads.

Mr. Bienkowski has summarized very well how we have addressed the criteria for unsteady flow effects on drag and lift for the interference effects and how we have addressed the non-uniform flow field. In LaSalle and Zimmer we do not have any sharp edged structures where we are concerned about the vortex shed. We use round cylinders.

In the case of LaSalle and Zimmer, adoption of the KWU T-Quencher for SRV discharges has required relocation of all of the SRV lines, and so it is immediately obvious that we would have to take into account the local effects caused by the relocation of these lines.

It is true that the KWU T-Quencher produces lower bubble pressures, and it is also true that the bubble frequency or the oscillation of the bubble tends to go toward lower frequency. And I want to come back to that in just a moment. But let me finish here pointing out that, for the LOCA steam condensation drag and the LOCA events, we do consider the water jet and vent clearing as well as steam condensation events of chugging and condensation oscillation.

And in the case of condensation oscillation, the magnitude of that pressure oscillation is a low magnitude, and it is also — the bubble oscillation is of low frequency.

Now, the downcomer system that we assess to does have some natural frequencies that are in the lower range, and so these shifts of frequencies by the SRV discharge, as well as the steam condensation flows, was of concern to us. The natural frequency of the downcomer was, I believe, around two or three hertz. And we felt that these loads, with these lower frequencies, were something that we needed to examine as to the impact on these structures.

And our approach to that was to consider the then available criteria and apply it in a very conservative way.

(Slide.)

The load definition criteria that we used has been explained in the closure report, and we have included the acceptance criteria. And because of the frequency shift, even though the magnitudes are low, we felt uncomfortable without examining that further. And so we have been considering a restraint system design for that downcomer.

The design of the restraint system that we are looking at now — we have convinced ourselves that it can accommodate the NRC recommendation for the lateral loads, and so our concern at this point is simply to finalize what that design will be.

DR. PLESSET: What are your preliminary ideas about that, about this restraint system? What kind of restraint system will that be? Any idea?

DR. CRAWFORD: Yes. I can just briefly describe it, and if you want more details, I can call on one of our other people. But basically, we are thinking of a restraint system design that is located near the pool surface. It consists of eight-inch extra-strong pipe, tying the downcomers together.

DR. PLESSET: I saw an arrangement in Japan where they are tied together near the bottom of the downcomer. Have you looked at that?

DR. CRAWFORD: We did look at that, and that is what led us to examine a reevaluation of the lateral load criteria, because the acceptance criteria for the lateral load is a function of the frequency of the system, and putting restraint down near the tip stiffens the system and increases the lateral load. And we felt that we would be better off to have a more flexible system by putting in the restraint system near to the pool surface.

DR. PLESSET: Have you looked at that possibility of where these restraints might be near the bottom or higher up?

DR. ANDERSON: No.

DR. PLESSET: You are not concerned?

DR. ANDERSON: We just haven't received any substantial information.

DR. ZUDANS: On this question of natural frequencies for your downcomer system, are these natural frequencies computed considering the fact that these downcomers are submerged?

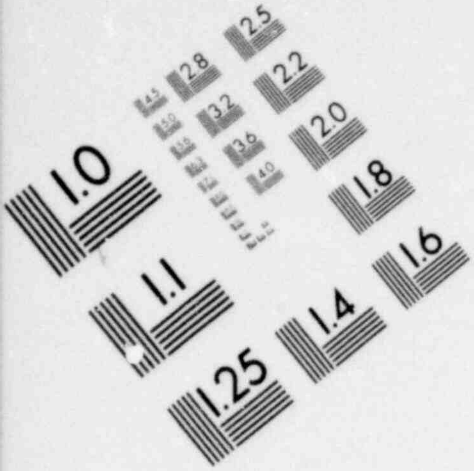
DR. CRAWFORD: We have considered both the submerged and the non-submerged, full of water and empty.

DR. ZUDANS: Do you have any concerns relative to the effects of the interaction and therefore your load definition? Your current load definition is based on rigid boundaries?

DR. CRAWFORD: Yes.

DR. ZUDANS: Once you have a situation in range where your resulting frequencies of load forcing function and natural frequencies of structure which was assumed originally, do you have any concerns about the validity of such forcing functions?

DR. CRAWFORD: We have considered the coupled system of the fluid and the structure and the net effect that we have found from our analysis thus far would indicate that the load would not be as severe as the way we are currently doing it. And I was trying to stress that we have taken, at this point in time, a very conservative approach in the method of the load application. And it is our



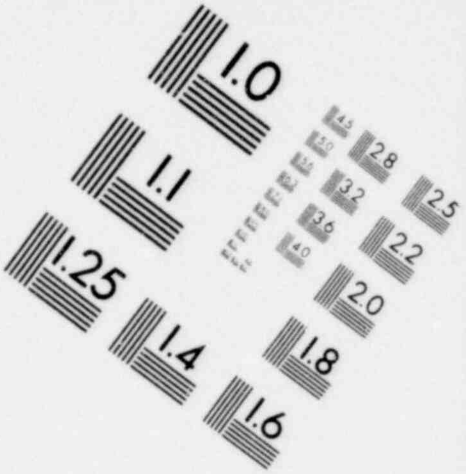
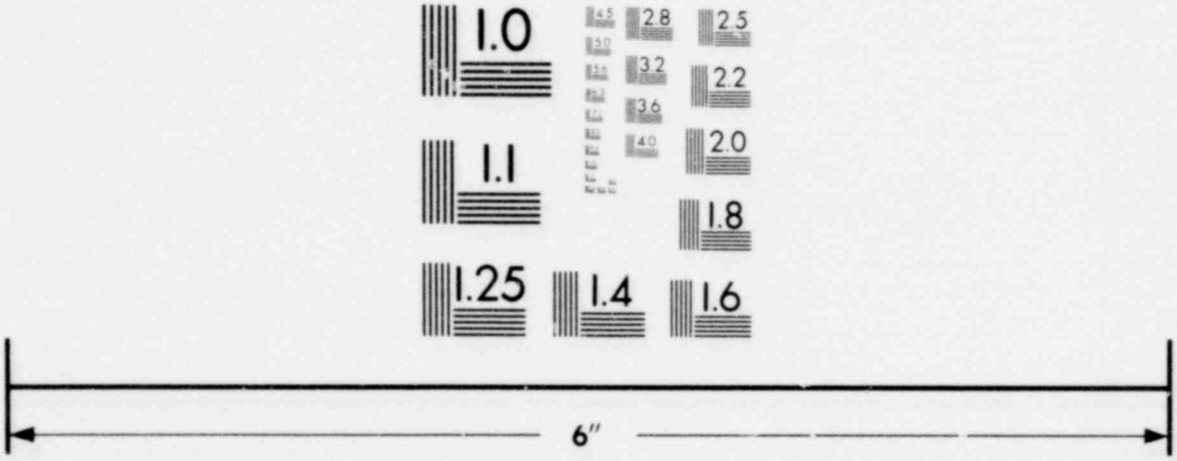
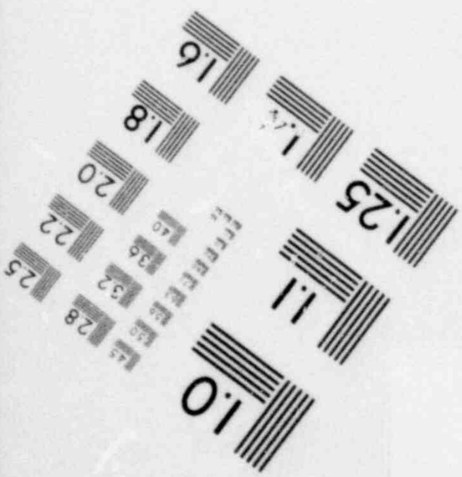
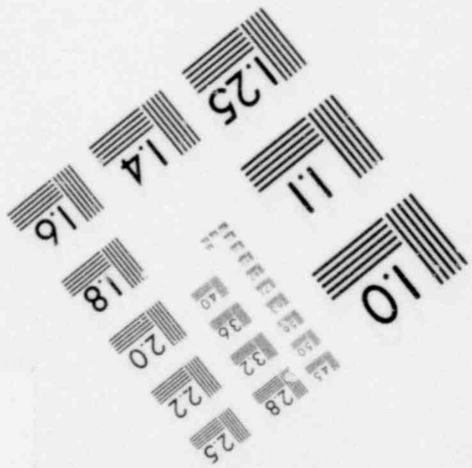
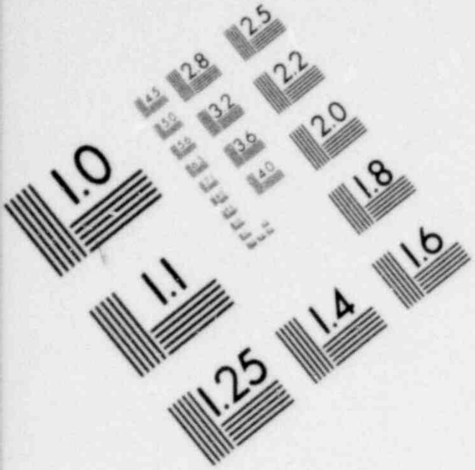


IMAGE EVALUATION
TEST TARGET (MT-3)









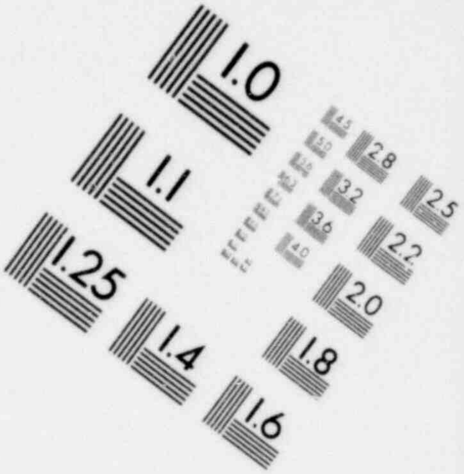
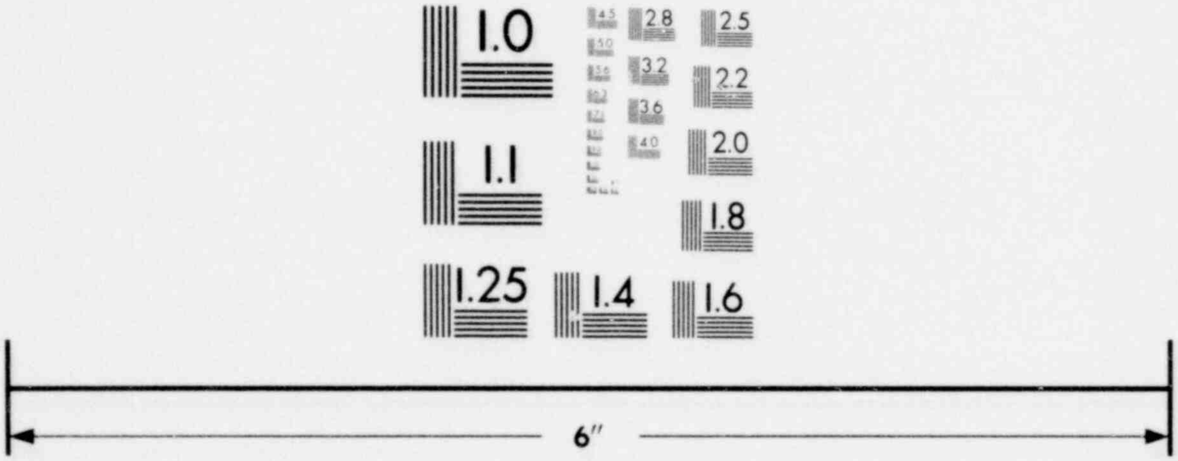
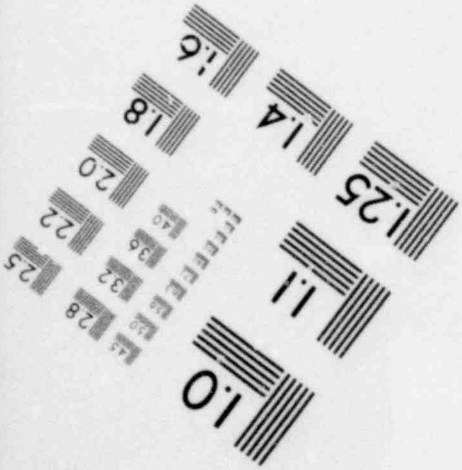
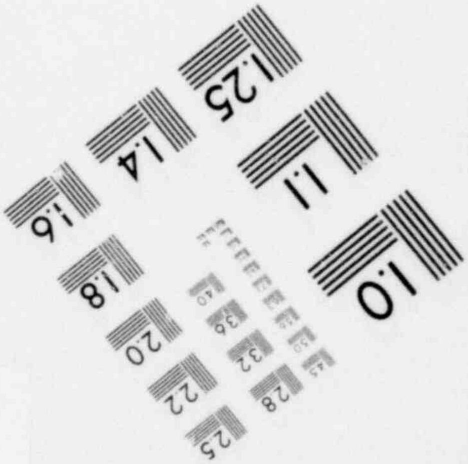


IMAGE EVALUATION
TEST TARGET (MT-3)







intention to pursue this further, to see if we can convince ourselves as well as the NRC that perhaps there is not a need for a very substantial restraint system on the downcomers.

DR. BUSH: How do you handle vertical motion?

DR. CRAWFORD: On the restraint system?

DR. BUSH: On the downcomer. As I understand your system, what you are doing is you are coming out with a web, essentially, of piping, which I presume is welded to the downcomer; or is it? I hope not, but I suspect it is.

DR. CRAWFORD: I don't believe it is.

DR. ZUDANS: I am not finished with my question.

DR. PLESSET: Identify yourself.

DR. SRINIVASAN From Sargent & Lundy.

(Slide.)

DR. SRINIVASAN: This is one of the schemes we are currently examining. This is a system where the downcomers would be tied together, as Dr. Kudrick explained, by eight-inch pipes. But you see here, they are tied to the containment on one side and on the other side.

Now, we will include in the design of this system any drag loads that you would have in either vertical motion or lateral loads on the bracing members themselves. Those will be incorporated into our design.

DR. PLESSET: Dr. Bush is interested in the attachment method. Weren't you, Spence?

DR. BUSH: Yes, I am concerned with the growth of the downcomer. And then I have a situation where there are a series of welds. So I get this kind of an accident with the possibility of a tear-out.

DR. SRINIVASAN: We do consider that. The connection to the containment is not rigid. It can transfer shear forces. But it has a pin, so there is a rotational capability of the system about the containment. So any thermal growth is accounted for.

DR. BUSH: That would help on your seismic loads, too?

DR. SRINIVASAN: Yes.

DR. ZUDANS: I would like to come back to the same question. Then you say --

DR. PLESSET: This relates --

DR. ZUDANS: To the frequencies. This structure is an interesting cartoon to look at.

(Laughter.)

DR. ZUDANS: A starship. I am concerned about at least apparent lack of concern about the possibility of resonances and feeding the energy into that vibration mode. Now, maybe you have some test results where the downcomer natural frequencies were in the range of condensation oscillation frequencies, and maybe you can get some observations from that. And when you have a range of frequencies of a structure in the range of frequencies, the only thing that can save you is damping. Otherwise, you can feed regardless of how small your excitation course is. But there is lots of damping.

But the question is raised, are there any tests where you would have any such confirming answers, any tests where you have structures that really had the actual frequency in the range of exciting forces.

DR. CRAWFORD: I would like to try to answer your question with two points I tried to make clear earlier. The reason for us to consider the restraint design was to stiffen up the downcomers, to get out of the frequencies of the forcing functions. That was our first approach. And I certainly concur with you that damping is a very important part, and we are looking into that further.

But with the restraint system design that we see here, that clearly moves up the natural frequencies of the system above where the primary forcing functions are.

DR. ZUDANS: I would agree with that, there is no question.

DR. CRAWFORD: The other comment that I wanted to mention was that in the 4T test, where there was some condensation oscillation observed, the natural frequencies of that downcomer, I believe, was of the order of

7 to 10 hertz, something in that range, which is apparently — apparently was close to the forcing functions observed in that test. But it appeared — at this level of going into the detail of that, it doesn't seem that that answers all of our questions yet, that we can totally eliminate that. So we are looking for additional analysis.

DR. ZUDANS: One comment more than a question: Since this is not precisely quantified phenomenon as yet, what effect it has, maybe you could think of some tests where you could vary the frequency of the downcomer by simply stiffening for the purposes of a test, and maybe you will find out that all of your loads disappear laterally.

DR. CRAWFORD: I think that is a good suggestion.

DR. PLESSET: Spence, did you have other comment?

DR. BUSH: I was concerned with the pinning effect, the rigid aspect. That answered my question.

DR. SRINIVASAN: Another scheme we are looking at would involve not attaching it to the containment or to the pedestal, a system which would primarily tie all of the downcomers together. This is the current bracing at the upper elevation at LaSalle that Dr. Crawford pointed out, which is a segmented system.

This is — we would envision a system at the pool surface. We may want to have a continuous ring. So you would end up with two concentric rings and some cross-members. This has the advantage of not inducing additional loads on the containment.

DR. ZUDANS: However, you would probably, in this arrangement, find a sympathetic mode of motion which would have the same low frequency.

DR. SRINIVASAN: But that would be the overall mode, and we do not anticipate for the structural loads to be acting in that direction. This is more likely to be an excitation where you would see that mode coming into the picture. The submerged structure modes will be directional. We believe the higher modes would be what is more important and not the fundamental sway node of the system.

DR. CATTON: I thought — I am hearing two stories. I thought one hypothesis was that the submerged loads were random in direction. And yet you are indicating that you are assuming they are directed.

DR. SRINIVASAN: I want to clarify. What I meant was these loads are directional, meaning that they are not in the same direction but multi-directional innovators, random.

DR. CATTON: That is a random excitation?

DR. SRINIVASAN: Yes. All of the downcomers going in the same direction. That particular mode would not be excited by the submerged structure loads. That is what I meant.

DR. CRAWFORD: Could I add something to that? Remember, we are considering both the chugging loads, condensation oscillation loads, and the SRV loads. So I think the more correct expression is to say that the loads are directed, like an SRV load exists at a position near the quencher. And we know the kinds of directions that it would be facing. They wouldn't all be in the same direction and they wouldn't be random, either.

We are trying to treat it mechanistically, having direct —

DR. CATTON: I understand what you are doing. I have not seen any clear demonstration that it is one way or the other.

DR. CRAWFORD: For the SRV load?

DR. CATTON: For the LOCA load.

DR. BUSH: What occurs to these systems if only part of the SRVs open? You assume you get a homogeneous mixing of the pool, so essentially — otherwise, you would get a differential expansion aspect.

DR. CRAWFORD: You are speaking of the terminal effects due to SRV discharge. This restraint system we are considering would be up near the pool surface and for — I think we would anticipate that we would have sufficient mixing, even for an extended blowdown, that we wouldn't run into any severe —

DR. BUSH: You would have a series of cold legs and hot legs, and you would almost have to depend on some degree of homogeneous mixing of the pool, I would think, which you probably would get. I am not arguing.

DR. CRAWFORD: We are anticipating there will be thermal mixing with the quencher. It discharges deep into the pool. We think the thermal plume will spread out and provide mixing.

We do not assume homogeneous mixing, but we assume a reasonable amount of mixing will occur.

DR. ZUDANS: These restraints would be in a single plane?

DR. CRAWFORD: Yes, sir, they would.

DR. ZUDANS: And therefore you would have considerable links of downcomer left between this plane and the floor. So you actually could possibly accommodate significant delta T's in each of the restraint places, and still not be critical, because there is lots of free length.

DR. BUSH: I am worried about some of them not changing the length and others changing the length. So it takes your horizontal members and it begins to do this to them (Indicating).

DR. ZUDANS: The downcomers themselves are changing?

DR. BUSH: Yes, because if the SRVs don't open, some of the downcomers —

DR. ZUDANS: Then you have local bending.

DR. BUSH: Yes.

DR. SRINIVASAN: In a situation where this is not continuous and segmented, it would solve that problem. Where you could have these segments would be located such that they would be centered at about a quencher, so you could accommodate the situation where you only have some of the valves going off. So you have a localized temperature here and it does not affect the other ones that are cold.

We are looking at several options.

DR. CATTON: But it affects the one that is in the same grouping. Your region of influence of the relief valve is not going to extend through 30 degrees.

DR. SRINIVASAN: This is something we would address in our design.

DR. CATTON: What is the reasonable assumption on the size of the plume rising above the SRV?

DR. CRAWFORD: We tried to consider the — well, the quencher is deeply discharged and discharged in the horizontal plane out, and I don't remember the exact numbers. But I would anticipate that that plume rises up, and I would anticipate it would cover at least 20 to 30 degrees.

DR. CATTON: How far — it is a highly bullient jet and your steam coming through the quencher is going through a lot of little holes, so it is going to lose most of its momentum. If I had to guess, I would guess it is only going to go a small distance beyond the end of the quencher. That steam jet is not going to extend very far into the water.

DR. CRAWFORD: The steam jet itself will not extend into the water very far. But I am anticipating that the thermal plume will go several feet away.

DR. CATTON: What is going to drive it?

DR. CRAWFORD: It is not going to go several feet in the horizontal. It will be going upward, obviously. But the anticipation —

DR. PLESSET: I think we have a comment here.

DR. KUDRICK: I think one comment should be made, and that is that Zimmer and LaSalle have committed to an SRV testing, and one of the objectives of the test which we will be looking for will be a demonstration of the pool mixing potential for an SRV discharge.

In addition, they have tested in Germany quencher-type devices that is somewhat analogous to the Mark II, and they have found fairly good mixing potential in the pool. I don't know if that answers all of your questions, but at least we will be getting some preliminary data.

BWH

DR. PLESSET: Have Zimmer and LaSalle chosen a particular restraint system?

DR. CRAWFORD: For the downcomers?

DR. PLESSET: No.

DR. CRAWFORD: No, it has not been shelled yet.

DR. ZUDANS: Have they committed to doing the SRV testing in-plant?

DR. KUDRICK: Yes.

DR. ZUDANS: You would possibly be able to instrument, to take care of questions like Dr. Bush asked?

DR. PLESSET: They promised to do that; isn't that right, Mr. Brinkman?

DR. BRINKMAN: Yes, sir, that is right. We have promised to measure temperature gradients. And maybe I could volunteer something about your concern of one safety valve going off and the next one not going off. I can't give you any numbers, but to give you some more feeling, the tests were done in the CAORSO plant in Italy. They did have heavy bracing systems over there that you may be familiar with, and the bracing system survived the test, and that is not to say the problem goes away, but some tests were done already.

Another thing I think that might be worth considering is that as I look at this existing test data that I have seen from Mark Is and other in-plant tests, the maximum temperature gradients I saw were maybe in the order of from right at the quencher to the water some distance away, might be 20 Fahrenheit degrees, and that tells you relatively, it seems to me, how important or how severe of a temperature gradient you would get from one quencher, from one downcomer pipe to the next downcomer pipe.

What I am trying to get at is there is some existing basis for design. Sargent & Lundy lays out this final quencher arrangement. There is existing data that gives them, I think, some fairly good guidelines as to how much would be the maximum temperature gradient for downcomer No. 1, downcomer No. 2.

I am volunteering that the water temperature differences aren't tremendous; and, therefore, the difference in thermal expansion I wouldn't anticipate to be tremendous, either.

DR. CATTON: I think the temperature differences you are going to find will depend strongly on where you put your thermocouples, and unless they are properly located you are not going to find the maximum temperature differences. And Dr. Bush's question, I think, is important.

DR. CRAWFORD: We have extensive temperature sensors in the pool for the in-plant tests for LaSalle and Zimmer. We have planned extended blowdown, and we have submitted, in the case of Zimmer, the proposed in-plant

BWH

test.

DR. CATTON: What are you going to do if the concerns are valid?

DR. CRAWFORD: If what? The severe temperature gradients exist?

DR. CATTON: That's right. From one downcomer to the next.

DR. CRAWFORD: You have all of the hot water going up around the quencher?

DR. CATTON: Yes.

DR. CRAWFORD: I think our concern would be more about the ability to condense the steam than with the restraint system, if that was the case.

DR. ZUDANS: If you don't have restraint systems that hold out, you don't have condensing systems.

DR. CRAWFORD: You mean if --

DR. ZUDANS: If you have a structure that does not survive the discharge, you do not have a condensing system. So, your concern really should also be, quite seriously, on the whole system and downcomers, not so far as to how effectively you have condensed steam, whether or not the structure can take it.

DR. CRAWFORD: We are concerned about temperature gradients.

DR. ZUDANS: I wanted to shift the emphasis in my direction; right?

(Laughter.)

DR. PLESSET: Did you have another comment?

DR. SRINIVASAN: I was only going to make one comment: that we are not necessarily tied to using a design where these braces are going to be ridigly tied to the downcomer; we could have the option of having a capability so that the rigidity is not a problem.

DR. ZUDANS: I would suspect that would be a good idea. What about the Shoreham type of design? They have already designed that. We saw it.

DR. CRAWFORD: Yes. Shoreham does have a restraint design.

DR. ZUDANS: Have you looked at that design?

DR. CRAWFORD: Yes, we have looked at it, but not in detail.

DR. BUSH: I didn't do a very good job of explaining my concern. Your last solution, I think, would solve it. And that is, if I got a very long pipe that is tied at the top and not at the bottom and the water level is halfway up that pipe, if I don't run any water down the pipe, I don't get any expansion in that first 10 feet or so of pipe, or whatever it is. And as a result, if the next pipe is hot and that one is cold, I certainly am going to have a difference. But if you can, as you suggest — all of my concerns disappear.

DR. SRINIVASAN: We will address that question in our design.

DR. PLESSET: I hope, Steve, you don't mind our getting into this now. It might be helpful for us to have heard early.

DR. HANAUER: No problem.

DR. CRAWFORD: To just conclude, I wanted to indicate that we are also considering the possibility that more realistic load definitions resulting in lower pressure and better definition of the frequency range and accounting for the energy dissipation and attenuation could result. It may not even require a bracing system, although this is what we are continuing on.

We are looking very carefully at the load definition to convince ourselves that we need to install a restraint system design. These concerns about the natural resonance.

DR. PLESSET: Thank you.

We have an item here for staff comments. Do you have any more comments you want to make?

MR. ANDERSON: Not now.

DR. PLESSET: Then we will go to the next item from the Mark II owners group, which is, as I have it, on the ring vortex model. I gather there was no organized presentation.

MR. ANDERSON: Yes, there is no formal presentation, as I understand, from the Mark II owners on this. It has been moved to the long-term program. There consultant is not available. Maybe now would be a good time to address some comments to them regarding this.

DR. PLESSET: I was going to suggest that maybe we have some comments from the consultants and have a chance to talk to some of the people who have worked on this to have the floor for a bit. Maybe I will call on Prof. Wu and then Prof. Catton for different viewpoints.

(Laughter.)

DR. WU: I don't know if the problem treated by Dr. Chu and Lee -- are you familiar with this, with this analysis?

MR. ANDERSON: Yes.

DR. PLESSET: The staff is aware, right.

DR. WU: In this paper, though not expressly stated, it is intended to simulate the flow of the downcomer out of the suppression flue of a pool of a Mark II pe of reactor. I think, to speak of it very briefly, it is based on an -- on any of the viscous vortex sheets, frozen without a viscous attenuation and fusion.

However, it does include a vortex sheet generation. The viscosity is generated within the downcomer pipe and transported into the lower plenum and the — it is a numerical calculation, and based on the equation that the transport of the viscosity is a material property and that retains this property all the way through. So, the vortex sheet would be generated and then rolled up into a vortex core, and this would in turn be wrapped in a — in a mushroom head.

The numerical procedure take a four-corner weighting function which has been fairly standardized and well developed in numerical schemes, but otherwise there is no further numerical diffusion, as I understand it. So, the procedure is a fairly well known one in the profession, and based on this I believe the problem is well formulated.

There are a few things, perhaps, I could comment on. One is the boundary calculation. It is taken as a unit cell and axially symmetric with the downcomer pipe central axis symmetry, and it is cylindrically symmetric and bounded by a cylindrical surface. I believe it is like eight feet or so in radius. And the downcomer pipe is extended. Related to the Mark II atypical case, nine feet from the downcomer exit plane and upper to the upper plenum, with nine feet below to the basemat. It is an unsteady flow calculation with a switch on, starting from time T.

Now, the velocity condition is as follows: There is no normal component of the velocity at all of these bounding walls, and the initial velocity comes — is prescribed at the exit plane of the downcomer pipe. And the vertical velocity is assumed or prescribed to be uniform. And at the same time, the free surface in the suppression pool is also assumed to move uniformly upward. These are the two assumptions.

DR. PLESSET: What was the first vertical velocity?

DR. WU: It is prescribed, instead of at the free surface within the downcomer pipe, it is prescribed at the exit plane, and that is prescribed to be uniform in the radial direction. So, from then on —

DR. PLESSET: Then the problem is defined.

DR. WU: Yes. And then the problem is defined.

In the report, I think the velocity distribution along the axis has been given. And then, also, the positions of the stream lines, the stream surfaces, are given in a time sequence.

Very recently, there has been a further new numerical result, probably not included in the original report. And that involves some of the transverse velocity at a few vertical planes. And as I have it here, it is one or two feet below the exit plane of the downcomer pipe. And another one is at 2.6, and that is given at a point of 55 seconds, and in that time the mushroom head occupies the position 2.66. So, there is a radial recirculating flow.

And also, with a velocity distribution along the axis — and Dr. Catton and I have looked at this — there is certain rate of velocity decay. This is vertical, the velocity gradient in the vertical direction; and, of course, that should be a reasonable physical result. That probably would require a further investigation into it.

But on the whole, it looks like the problem has a few new features: One is it is highly unsteady; the other is the unsteady — the three-dimensional figure around the mushroom head is very conspicuous, it is probably quite important; and the third one is the boundary condition due to the lateral wall in the proximity of the basemat would change some of the — our earlier concept of that to the generation of a jet that would come from the downcomer pipe to be established within a short distance.

So, those are some of the new physical aspects that might not follow with our earlier conventional experience. And aside from these feuding aspects that might require further investigation or thinking to understand the problem, it appears the numerical work is done with high confidence. Otherwise, the results are quite reasonable.

That is a brief summary of my reading of that.

DR. PLESSET: Ivan?

DR. CATTON: For the most part, I agree with

Dr. Wu. After having gone through the paper, I have an uncomfortable feeling about some of the results, and, in particular, the fact that the predictions of the pressure don't extend to the region where the peak pressures occur — as a matter of fact, they cut off quite a bit earlier.

There was some comment earlier about Dr. Bienkowski, that this had to do with when the vent clearing occurred, but I am not sure there is a lot of agreement in that, either.

The main point I would like to mention is that certain aspects of the solution don't appear to be correct. I think —

DR. PLESSET: You mean physicall?

DR. CATTON: Physically.

DR. PLESSET: Not as far as the numerics.

DR. CATTON: I think the way the problem is set up is a step in the right direction, but somewhere between setting the problem up and getting the solutions, things don't look quite right. And the axial velocity, as measured from the exit plane to the bottom of the model or the floor, seems to drop off much too fast. And in particular, the results that Dr. Wu recently got and that were transcribed over the phone — it may be the telephone was part of the problem; I am not sure.

(Laughter.)

DR. CATTON: That shows that the derivative in the axial velocity is non-zero at the exit plane, and that is just incorrect.

So, one has to kind of wonder why this could be so. I will appeal to Dr. Yao's comment that this kind of problem is very difficult to solve numerically. It is inherently unstable. So, you have a tendency, when you look at these kinds of problems, to build in a lot of numerical vamping or even though it starts out to be inviscid, if you start checking, it is a very viscous fluid.

I don't know where all of this leads, but as long as it is not being I guess used on a particular plant at this time, it is somewhat academic.

The non-zero derivative looks to me as if there is an error somewhere. The imposed boundary condition is not reflected in the solution.

DR. PLESSET: I think, since there has been an effort, it has been worth our looking at it so that we would have some basis for an opinion. I think that both of you, we are grateful to you for your looking at this. And who knows, they may want to use it again. I don't know. Presumably not.

As far as the staff knows, it is not going to be invoked; is that correct?

MR. ANDERSON: Not for the lead plants.

BWH

DR. PLESSET: Not for the lead plants.

MR. HEDGECOCK: Hedgecock, chairman of the Mark II owners, from Washington Public Power Supply System.

In response to comments I heard earlier, our position at the moment is that this is not an academic question. There is at this time some intention of the non-lead plants to use this model. We would prefer to leave it at this stage at this time.

DR. PLESSET: So, then, this was a useful discussion. And I think that the points that Professors Catton and Wu have made are perhaps helpful in further consideration by you and your consultants.

MR. HEDGECOCK: We certainly appreciate it.

DR. PLESSET: Fine. Any other comments on this point?

(No response.)

DR. PLESSET: We are in an awkward situation. We can go to lunch earlier or break a little less logically. I am open to suggestions.

Carson, do you want to have lunch now? It seems to be agreed by my weighty colleagues that we are going to adjourn for one hour for lunch.

(Whereupon, at 11:40 a.m., the meeting was recessed, to reconvene at 12:45 p.m., this same day.)

AFTERNOON SESSION

(12:45 p.m.)

DR. PLESSET: We will reconvene.

There is one item that Dr. Crawford wishes to make a brief comment on for the record. So, I will ask Dr. Crawford to do that.

DR. CRAWFORD: I would like to clarify the discussion of the downcomer restraint system that we had earlier. I would like to point out that the safety relief valve lines are entirely separate than the main vent LOCA downcomer vents. Because of the separateness of the two and because the main vent downcomer vents are all used at the same time in the event of some kind of hypothetical LOCA, we would not anticipate any thermal gradients.

And furthermore, the restraint system design is up near the surface of the water, and the only portion of the downcomer vents is about a 10-foot length of pipe extending down into the pool, and we don't anticipate any large temperature gradients from one downcomer vent to another.

I think that would clarify the discussion we had earlier.

DR. PLESSET: I think that clarifies the record.

DR. CRAWFORD: Thank you.

DR. PLESSET: We are glad to have that.

So, we can go on now to our next agenda item,

BWH

which relates to the long-term program.

MR. HEDGECOCK: I would like to introduce the long-term program this afternoon, and I can list the speakers for you to aid in the transcription. The overview will be presented by Mr. Alan Smith, General Electric, our program manager. And the 40 CO test program will be presented by Mr. Ray Muzzy, General Electric Company. We then go on to the CREARE multi-vent test, an update on those from our consulant, Dr. Hottel, from CREARE. This will be followed by the generic improved chugging load program, presented by Dr. Jim Fitch, of General Electric. And then we will apprise you of the progress in the reduction of the CAORSO test data, tests themselves having been completed, and Mr. Mac Davis, of General Electric, will present that. Since we had covered the ring vortex model before lunch, we don't intend to say anything further about that.

e-6

DR. PLESSET: Not at this time, but later.

MR. HEDGECOCK: Later. And then we will go on to the plant-unique programs, and Mr. Dale Roth, of Pennsylvania Power & Light will talk about the GKM-2 CO tests, followed by Dr. Bedrosian, our architect engineer, Burns & Roe, to talk about the WPPSS-2 chug improvement program. And then comments.

I would like to introduce Mr. Alan Smith.

MR. SMITH: Our Mark II containment program has been explained to you by the NRC and others, and I would like to give you a bird's eye view of where are we in terms of the number of tasks that we have been working on and the different areas. The total number of tasks that we have been working on in this program -- and bear with me, there is subjective judgment in that, but there are over 400 tasks and if we can break those down by categories, possibly eight percent of those lie in the lead plant SER area, perhaps 32 percent in the non-lead plant area, 34 percent lie in a combination of the two, and we have about 12 percent of our program in the confirmatory area, and perhaps 14 or 15 percent in the so-called informational category, the point being that the informational category is really more for the owners and it does not necessarily constitute a necessary part of the program.

Overall where we are right now, as you can see on the chart, we feel that we are about, as of July of this year, 70 percent complete. And we are probably a few percent beyond that as of today.

(Slide.)

The next chart, I believe you have seen this earlier this morning. I would like to comment on it. The area beyond the final LOCA information to the staff really represents basically licensing support kinds of activities, that is, basically the program in terms of the analytical and the testing work is done and completed and submitted. And this is additional time probably necessary to be spent working with the staff, answering questions and so forth and we expect that to be completed by the middle of 1981.

(Slide.)

I would like to show you now in a bit more detail where we are with respect to our specific tasks in the LOCA area. I have listed for you the percent, which means I won't waste time going through each one of those. Each triangle represents a discrete or tangible output of the program, whether that be a report or a model, some discrete tangible output to the NRC. And obviously, the triangles that are filled in represent those things that have been completed as of July and the white triangles represent those things yet to be completed.

As you can see from this chart, our final output from the CO test program, task A-17, is about the end of the third quarter, which with much of our earlier actual test information being available sooner than that. That is the longest program task item that we presently have in the Mark II program.

(Slide.)

I have a few other charts but you don't have copies of them, mainly because there is a problem in the reproduction of color, but I thought it might be of value just to quickly show you where are we in living color, if you will. And bear with me.

The green obviously means it is done or completed. And what appears to be gray here has come — are the areas we are still working on. This is just to give you a bird's eye view of what does the program look like, what are those areas that are still requiring some work. In the steam chugging and main vent loads it is well completed in many areas. The seal program is underway and of course we are not yet complete with that. The chug load definition program is in task A-16. You will hear more about that later. It is well beyond the midway point.

Dr. Patel from CREARE will discuss our subscale multivent program and it is also well past the midway point. There is one confirmatory program that we are working at.

(Slide.)

This is the safety relief valve program, generic safety relief valve program, I should say, that was originally conceived by the Mark II owners group. It does not include the T quencher program because that came along later. It has been adopted by most of the Mark II owners but this represents rather the Ramshead program and the X-quencher program and as you can see the Ramshead program is completed. The safety relief valve quencher program is very nearly completed. There are some plant-unique aspects of the X-quencher program that Washington Public Power is working on and then I show where the T quencher program that is being used by the other seven utilities fits into the process. They have their own program which has already been discussed. And so everything, the Ramshead program, the X-quencher program, the T quencher program, feeds into the plant evaluation by their design analysis reports.

(Slide.)

The next area is submerged structures and I really put this together more for my own benefit than most people's because it is a very complicated program and I tried to identify the simpler elements. It has three basic elements, analytical, models, Mark II unique applications memoranda and the testing aspect of this program.

(Slide.)

And where are we on the analytical modeling work? Our LOCA Ramshead air bubble work is completed. LOCA and Ramshead water jet work is completed. We are still working to complete our response to the staff's inquiry on submerged structures criterion. As we mentioned earlier, Mr. Hedgecock indicated to you, I think there are some plants beyond the lead plants that are considering using the vortex. And of course there will be more work in that area.

There are two areas that are plant-unique ideas, the water jet and the LOCA steam condensation, that are not part of the generic program. Mark II has determined that those are more relevant to plant-unique work and the quencher air bubble work is nearing completion.

(Slide.)

The testing program that we had is a 1/4 scale test that is totally complete.

(Slide.)

And we have a miscellaneous category that is nearing completion. Load combinations and functional capability, the task has been completed. Again, we are continuing to address items that the NRC staff had in their submerged structures criteria. We are nearing completion in this blue zone in answering all of the NRC's formal questions. We have completed most of the SRSS work. There is a supplement that Drs. Newmark and Kennedy have been working with us on that will be complete, and our world test monitoring activity is continuing. That is a general understanding of what is going on throughout the world, to keep advised of what is going on.

That concludes my very brief overview of the status of the program. I would be happy to answer any questions you have.

DR. PLESSET: Are there any questions of

Mr. Smith?

(No response.)

DR. PLESSET: I assume that somebody is going to watch closely what Brookhaven is doing on the study of the SRSS?

MR. SMITH: Yes. We are vitally interested in that.

DR. ZUDANS: On this discussion of vortex before lunch, I am wondering in which areas this particular research or definition of — you plan to use that particular part?

MR. SMITH: For application for the ring vortex?

DR. ZUDANS: It is plant-specific or generic?

MR. SMITH: I would say it is more plant-specific and I think we would probably have to ask each plant to speak for that and probably they are not prepared at this time to speak directly to that. You will no doubt find that there would be some commonality but I would expect that it would be also unique.

DR. PLESSET: The lead plants aren't involved in the question there?

MR. SMITH: These will be non-lead plant applications.

DR. ZUDANS: The reason I mentioned this is because from what I gathered before lunch there are many questions not yet resolved with respect to capability of this model to predict reality.

MR. SMITH: Yes, and we were very interested to hear what Dr. Catton and Dr. Wu said, and we will take those into consideration for any application.

DR. PLESSET: Thank you, Mr. Smith.

DR. MUZZY: I am Ray Muzzy from the General Electric Company.

(Slide.)

The program I am going to talk to you about today is the 4T CO program. As a result of examining the lead plant assessment report, NUREG-0487, there was a question which was mentioned this morning concerning the potential vent length because of the scaling of the 4T test equipment. The vent length system within the 4T was about 90 feet long, whereas prototypical was about 45 feet. As a result of examining considerable subscale test data and analysis in attempt to resolve that issue, we concluded that from the existing data, that we did not have enough information at that time to resolve the issue and considered two paths for possible closure of this particular question.

The first path would have been to go through some additional analysis and subscale data, which we believe would have been a long and lengthy closure, whereas the better would be to go to full scale tests for unique and generic information for the Mark II plants.

(Slide.)

Some of the objectives of the program are the following: to confirm the adequacy of the existing 4T specification we used an existing test facility, the 4T, and made up modifications in that facility which I will describe in the next slide. We went to a prototypical configuration and we varied the test conditions to make the test data generic to all of the Mark II plants. We considered various types of breaks as well as vent submergence at full temperatures, max fluxes and vent rises.

(Slide.)

The test configuration is as follows: this is the existing 4T 10, the wetwell for the previous test, the vent system, its bracing, as well as the wetwell tank here in the previous test. This was the drywell and there was a vent length that consisted of pipe that came from here all the way up and down through here and that created the 90-foot length that I talked about.

For the existing system we took the drywell and put it on top of the wetwell in a prototypical configuration. This is the existing steam generator that is used in the old 4T test and will be used as the basis for the source of steam and liquid for these tests. Also contained in the test equipment is typical vent riser with jet relector, which is prototypical of the Mark II plant and will be studied as one of the test parameters.

DR. ZUDANS: I previously asked a question about the natural frequencies of the downcomers. Do you know the natural frequency of this downcomer?

DR. MUZZY: No. It is identical to the downcomer that was used in the previous 4T test.

DR. ZUDANS: It is feasible or possible or required, really, to vary this particular parameter, the natural frequency of the downcomer to see how it affects the things that you observed, because of condensation oscillation frequencies, and go through the range where it becomes synchronized with that? This seems to be in my mind the only reason why you people decided to consider bracing of downcomers in plants that did not have bracing before, because you could not anwer the question of what happens if they are of the same frequency and you could, in fact, study this particular parameter, could you not?

MR DAVIS: The bracing system that Zimmer and LaSalle is considering is a result of considering submerge structure loads, which is the impact on one vent from oscillations of another vent. It is not caused by oscillation in a single vent acting on itself. From the previous 4T data, we have observed that during condensation oscillation there is very little, if any, movement of the vent itself from its excitation to itself.

DR. ZUDANS: It doesn't have to be from itself. There is a condensation oscillation from multiple vents and it operates at a given frequency. And if the structure is of the same frequency you do have a question of what happens if the resonance occurs. You will never have a single vent postulating a single vent in a real plant.

MR. DAVIS: True.

DR. ZUDANS: So there is other chances for other vents to feed the energy, so to speak. I am asking a question, is it feasible to think that you could get some light shed on this resonance that might exist because you stated that some of the frequencies in the structure are in the range of frequencies of condensation oscillations.

Of course, if you make it very stiff and brace it you will move it out of that range. And maybe that is the solution.

MR. DAVIS: In the previous 4T test we did have essentially different frequencies of the vent in the test, in that we changed the elevation of the bracing in the tank from — I will have to guess at the numbers, like eight foot from the bottom of the vent to 24 foot, so there was quite a range of vent frequency. And in none of those tested, we see any excursions or significant loads during condensation oscillation. I am not sure I am answering your question.

DR. ZUDANS: You're not.

(Laughter.)

DR. ZUDANS: All you have to do is go home and dig. You have the information. Because you don't know what the frequencies were, even with this support shifting. You would have to look at the oscillation condensation oscillation frequencies and look at the frequencies of this structure as you had it, and if you can show that you really went through the resonance. I didn't observe anything signicant. That might be all you need to do.

In other words, you do have the information, I assume, on natural frequencies of those downcomers. Have I made myself clear?

MR. DAVIS: I believe so. Maybe I could try again on why the bracing system at Zimmer —

DR. ZUDANS: Don't try that.

(Laughter.)

DR. PLESSET: I'd like to hear it anyway. Go ahead.

(Laughter.)

DR. BRINKMAN: I would like to make sure — I would like to understand for sure what the question is. The question seems to me to be, if you would do a test with various bracing arrangements in this tank, perhaps you could demonstrate that you don't need a bracing system to the

BWH

power plant.

DR. ZUDANS: Maybe that would be a result. That could have been a result. But what you would have to demonstrate is that nothing adverse happens if you happen to have resonance between the natural frequency of your downcomer and the oscillation frequency of condensation oscillation.

DR. BRINKMAN: You are concerned about an individual downcomer, then, loading itself due to the oscillations at the bottom.

DR. ZUDANS: Each of these downcomers is, in principle, identical to the other. If one of them has a frequency, natural frequency, and submerged state of 9 hertz so will the others. If your condensation oscillation frequency is between 6 and 14 hertz or 6 and 11 hertz, that means that you do have exciting frequency that is exactly in resonance with your structure.

What will the structure do if it is subjected to such exciting force? You do not have any tests that indicate such a situation. You don't have that information.

MR. SMITH: I think we do. We have exhibited that in the original 4T program.

DR. ZUDANS: What did you have? I asked whether you had frequencies and you said --

MR. SMITH: We don't have the information here as to the exact frequencies, but we did experience condensation oscillation in that test series. And there was no deleterious structural effects.

DR. ZUDANS: Do you know what the frequencies of the structure were?

MR. SMITH: I can't tell you here. We probably have that information.

DR. BRINKMAN: I think it is fair to say that the frequencies covered by the test facility bound the frequencies that we would expect in the power plants. And if we went home we could dig up the data and give you the numbers.

DR. ZUDANS: I want you to convince you, yourself, that resonance is not a dangerous situation. That's all.

DR. PLESSET: Mr. Crawford.

DR. CRAWFORD: Perhaps I could — I would like to try to clarify what I think the question may be, and why we are still having a concern and are still considering the design. In the 4T test with the single vent, we tested over a range of test frequencies --

DR. PLESSET: Including resonance?

DR. CRAWFORD: Yes. But our concern for examining the restraint system design is that the load magnitude at the resonant frequency may cause an adjacent downcomer to be excited. The reason for that concern is that the self-induced load is not the same load magnitude as the load induced on that downcomer by an adjacent downcomer. So that is really the concern.

In this particular test we have tested over the range and we don't feel there is any self-induced load that excites the resonant condition. I would feel more comfortable before — if I were to do a test to determine that a neighboring downcomer would not induce a resonant if I had a multivent test.

DR. ZUDANS: That is a very good answer.

DR. PLESSET: If Dr. Zudans is satisfied, accept it.

(Laughter.)

DR. PLESSET: I think that you clarified very well.

DR. ZUDANS: The question is not being ignored.

DR. PLESSET: No, that's for sure.

DR. ZUDANS: That was my concern. Nothing else. He answered my question that a single vent test is not the avenue to find the answer for this question.

DR. PLESSET: That's right.

DR. MUZZY: In terms of instrumentation, the primary objective of the tests are to address the questions concerning vent acoustics and wall load information so the instrumentation was concentrated in that area. We had from our existing 4T facility approximately 64 channels of total instrumentation available, of which the mix was about 3/4 of high speed channels and about 1/4 low speed channels, low speed being devoted to thermocouples and bubble probes and the high speed with pressure measurements throughout the system.

In addition to instrumentation on the wetwell and suppression pools, the downcomer drywell blowdown line and steam vessel, we have also added instrumentation for measurement of the air content. We have two methods, one method being a grab sample technique where we grab a sample of air downstream of a vent inlet and also a backup technique where we continuously monitor the air.

DR. CATTON: The high speed measurements, how many per second on a given pressure transducer?

DR. MUZZY: There are similar to high speed instrumentation used for the last 4T test.

DR. CATTON: I don't recall what that was.

DR. MUZZY: I don't have the exact answer.

BW

DR. CATTON: Does anybody have the answer?

DR. MUZZY: Yes.

DR. PLESSET: It will be in a report.

DR. MUZZY: There is a test plan in the procedure document. There is a document that will address the question.

DR. PLESSET: Maybe we could get the number later.

DR. MUZZY: I may be able to answer that question right now.

DR. PLESSET: That's best.

(Pause.)

DR. MUZZY: I would like to check it later and give you the answer.

DR. PLESSET: Yes.

DR. ZUDANS: Could I ask a question on the previous slide?

DR. MUZZY: Yes.

DR. ZUDANS: I noticed that you don't have anything indicated on downcomer.

DR. MUZZY: We do have instrumentation for strain gauges on the lower downcomer. I do have a back-up slide.

DR. ZUDANS: You do have strain gauges?

DR. MUZZY: Yes. These are the strain gauge locations on the lower downcomer and we will be recording that information — on the bracer system. And we have two accelerometers located —

DR. CATTON: Our Part 3 strain gauge is sufficient to locate the direction of the force.

DR. MUZZY: Yes.

DR. ZUDANS: No braces?

DR. MUZZY: Yes.

DR. ZUDANS: Don't you plan to run it without braces?

DR. MUZZY: No, there are no plans to do that at this time.

DR. ZUDANS: Why?

DR. MUZZY: We are primarily interested in measuring the CO wall loads and addressing the question concerning the vent length. That is an objective of the test — that's why we have used the equipment the way we have.

MR. ANDERSON: From the beginning of the 4T test, there has been an attempt to look at and establish the loads and to put together a facility that was prototypical of Mark II plants. However, not structurally prototypical.

There has never been an attempt to make it structurally prototypical because you have quite a variation from plant to plant in the bracing systems.

I think you might hear something with regard to the Susquehanna presentation where, in that particular case, for that — that is prototypical of that plant. And they are trying to make that structurally prototypical.

Here you would have to run a whole series of tests.

DR. ZUDANS: I understand that. My comment would be that if you plan to license any plant without any braces — which right now Zimmer is without braces and they are not committed to use the braces yet. They are only studying it. You don't have any single test for — without braces.

DR. ANDERSON: But that doesn't necesarily affect the load.

DR. ZUDANS: It affects the load dramatically. The load is affected. There will be effect.

DR. ANDERSON: The data for establishing load for all of these plants comes from a variety of tests. The 8.8 kip static equivalent load didn't come from the 4T; it came from GKM. And there are many other test facilities. So we looked at those with bracing configurations to come up with single-load specifications.

DR. ZUDANS: Is there any test at all without braces any place?

DR. ANDERSON: I don't think so.

DR. ZUDANS: I suggest that we think about it.

DR. ANDERSON: If they do not make a modification to include bracing, this will become — we will look at that area at that fluid structure interaction concern.

DR. MUZZY: We have structured a text matrix to investigate a range of parameters for the Mark II conditions and have investigated break type, break size, pool temperature, vent submergence, and we have inserted a riser to establish the effects of a riser, which rises up into the dry well.

We have a two-phase program, an initial set of about four tests. We get early-on indications of the behavior of the system which will help us to formulate and finalize the remaining Phase II test which will be done after that.

I will give you some schedule indications on my last slide.

DR. CATTON: Is there any reason that you have no steam break with high pool temperature?

DR. MUZZY: No. It is a matter of packaging the various tests that we have available from the matrix, which is 23 tests to maximize the information.

DR. CATTON: So you feel that the loads associated with the liquid break will be greater than those associated with the (inaudible).

DR. MUZZY: That has been borne out in experience.

(Slide.)

To give you an idea of how the test matrix covers the range of blowdown conditions for Mark II plants, we have plotted the air content versus the vent steam mass flux.

(Slide.)

In terms of the usage of the data and its interpretation in measuring that exit pressure history, we determine the presence of the standing wave.

And we will also be examining the pool wall pressure histories to establish the CO amplitude versus frequency content and interpret it in terms of Mark II applications and compare that to the DFFR.

(Slide.)

The schedule for these activities are as follows: We have developed the functional specification and the test plan. The facility completion, facility modification will be completed this month.

At that particular time, near the end of the month and in through October, we will be doing shakedown tests. After that, we will initiate our Phase I test, the four tests I talked to you about. Then there will be a time period for about a month when we will examine that data and see how it would possibly influence the Phase II test.

In December and through March, we will do our Phase II testing. The data reduction will take place during this time and the final test report will be out in the third quarter of 1980.

DR. PLESSET: Are there any other questions of Mr. Muzzy?

(No response.)

DR. PLESSET: Thank you.

DR. CATTON: How did you arrive at the high pool temperature being 110 degrees?

DR. MUZZY: Pool temperature will increase during the test. That is the initial temperature. And as the blowdown proceeds, it will increase. That is how it was dictated.

DR. PLESSET: The next item is CREARE tests.

DR. PATEL: I am with CREARE, Incorporated, and I will be presented to you today the multivent test program that we are performing for GE.

(Slide.)

The objectives of the multivent test program are basically to obtain a single vent and a multivent data base which can be used to obtain the transient loads with a number of vents during chugging. And secondly, we plan to show that the trends that we observe in these sub-scale data will be applicable and valid for application to the full-scale

And we will do this by comparing the single-vent data at four subscales and comparing multivent data at two scales.

(Slide.)

To meet the objectives of the program, we are doing single vent tests at 1/10th scale, 1/6th scale, 1/4 square, and 5/12ths square. The multivent tests at 1/10th scale, 3, 7, and 19 vents, and 1/6th scale, 3 and 7 vents.

DR. CATTON: When you change the scale, you change all the geometrics?

DR. PATEL: I will get into that shortly.

We also did tests to see the effect of the parameters like drywell size, pool size, and the location of the vent in the pool.

(Slide.)

The test program was broken up into two phases. In Phase I, we developed the test facility instrumentation data acquisition procedures to do these tests, and then we did the tests at the 1/10th scale, 1, 3, and 7 vents. And at the 1/6th scale, we did the 1 and 3 vents.

We also did the special test in order to evaluate the effect of drywell size, pool size, and the location of the vent in the pool.

Phase II, which we are doing right now, at CREARE we are doing the 5/12ths single vent, 1/4th, the 10th scale for 9 vents and the 6th scale for 7. And this will complete the data base for the objectives of the program.

The schedule for this test program is as follows --

(Slide.)

DR. CATTON: Will you show a cross-section?

DR. PATEL: Yes. The schedule of the program is as follows: Phase I is essentially completed. The test report is in the works. Phase II, testing has been started. We are at approximately 40 percent complete on that. And we hope to produce the test report on that sometime in the second quarter of 1980.

The test facility, a schematic of it, is shown here. And we have shown some typical geometries that we tested in Phase I.

The drywell is essentially located on the top of the wetwell and we did essentially multivents at the same scale.

So we did a single vent test and a multivent test, which looked essentially similar, except for the sizes being changed according to the number of vents.

Do you have any questions on this one?

DR. CATTON: I see what you have done when you scaled up. You built yourself a completely new system.

DR. PATEL: That's right.

DR. CATTON: When you go from the one vent to the three vents, you are increasing the area, cross-sectional area by a factor of 3.

DR. PATEL: Yes. The drywell is increased by a factor of 3 also.

(Slide.)

The test matrix was extensive for this program and the reason being that we wanted to cover the wide range of parameters so that if we needed them for the scaling work, we would be able to basically fall back on the data base.

The submergence and the clearance were scaled by a scale factor. The wetwell diameter was such that the pool to vent area ratio was kept constant. The drywell was scaled by the cube of the scale factor from the corresponding single vent full-scale drywell.

The wetwell space pressure was varied from sub-ambient to the full 45 psia, which is a protypical value. The steam mass flux range was from .1 pounds to 16 pounds, which is expected t lower the entire chugging range.

The pool temperatures varied from 90 to 200 and the steam air content was changed from zero to .5 percent by mass.

(Slide.)

I will be showing you some of the Phase I data that we obtained and I will show it to you in terms of the mutivent multiplier, which is defined as the peak over pressure measured at the bottom of the pool for the mutli-vent geometry divided by the corresponding one for the single vent geometry.

Basically, at the same test conditions for the same value of steam mass fluxes, what will air space pressure and so forth —

Here is a composite bar which indicates the multi-vent multiplier for a range of steam mass fluxes and the trend is fairly clear.

The mutivent multiplier is essentially less than 1.

That means the load in the multivent geometry are less than those in the corresponding single-vent geometry. And further, the loads go down as the number of vents is increased.

This is the 1/10th scale data.

DR. ZUDANS: Which loads?

DR. PATEL: The peak over pressure at the bottom of the --

DR. YAO: Does steam mass flux indicated for single vent or for both tests?

DR. PATEL: The mass flux is essentially non-dimensional by the total area of the vent.

So in the case of a single vent, it is the cross-section area of one vent. For three vents, it is three times that. The mass flux stays the same.

DR. CATTON: When you do one vent, you have a particular vent to pool horizontal area. And you go to two vents on this diagram here -- do I maintain that ratio vent area?

DR. PATEL: Yes.

DR. ZUDANS: This is just one of the load parameters that you observe the pressure at the bottom of the suppression pool?

DR. PATEL: Yes.

DR. ZUDANS: Did you look at other things and they showed the same type of trend?

DR. PATEL: We looked at peak underpressures where basically the program is geared toward measuring the wall of pressure loading.

We are not making any measurements of loads on the vents and so on, like through strain gauges, et cetera.

So it is essentially making — drawing conclusions based on the wall pressure increases.

DR. ZUDANS: You look at the sidewall pressures. Do they exhibit the same thing?

DR. PATEL: Yes. We have a total of six pressure transducers located on the pool walls. Some of them are at the pool bottom elevation and some are at mid-submergence around the circumferential locations and so on.

We have a lot of data at various parts of the pool. And all of them are generally exhibiting the same trend.

DR. ZUDANS: Do you measure anything that would tell you what the vent itself is?

DR. PATEL: The vent in terms of —

DR. ZUDANS: Pressure?

DR. PATEL: We measure the static pressures in.

DR. ZUDANS: How about outside?

DR. PATEL: The outside surface of the vent does not have a pressure transducer. So we do not have a pressure measurement there. We do have three transducers located on --

essentially put close to the vent. So we have some pressure data in the pool close to the vent, but we don't have a pressure transducer on the vent looking at the pool wall pressures itself.

DR. ZUDANS: You do not measure the motion of the vent itself in any way at all?

DR. PATEL: We have accelerometers on each of the vents, so we do have acceleration data.

DR. YAO: How do you define "multivent multiplier"?

DR. PATEL: This particular slide shows the mutivent multiplier based on the peak pressure at the bottom elevation.

That is essentially defined as the peak pressure at the bottom elevation for the multivent geometry, divided by the peak overpressure for the single vent geometry at the same test conditions.

DR. CATTON: When you run your single vent test, where are your pressure transducers relevant to the single vent?

(Slide.)

DR. CATTON: Let's compare one with three.

DR. PATEL: I will do it at a larger scale. Suppose there is a transducer here and pool elevation — do you have a pen that I can use on this? Let me show you a typical transducer location. There would be one here and one there indicating).

There are essentially three around the circumference at this location. There is one transducer there. So you have four plus two. That is six transducers. For the multivent geometry, you have one there, one there and one there. Basically three around the circumference — again here and one there (indicating).

The data I am showing you is showing the peak overpressure here versus the peak overpressure there (indicating).

DR. CATTON: When I look at the three, the location of the pressure transducer is a lot further away from the left most vent.

So I would expect that its impact on that particular transducer to be much less. There is no way that I would take a single vent and multiply it by 3 to get the load because there are area considerations that have to be taken into account.

Have you done any of this kind of thing?

DR. PATEL: That is exactly why we did the test, to see the effect of pool size with the event essentially centered in a different size pool. And the offset vent test where we took the same pool that we use for the three-vent test and we took these two vents out and then we further — moved this vent around so we could quantify the effects of the distance between the vent and the transducer measurement location.

It turns out that the predominant factor which governs the peak overpressure are the magnitude of the wall pressures, is the size of the pool. The distance is a parameter.

The closer you move to the transducer, the higher the --

DR. CATTON: So the pressure source at the vent is probably the same in both cases. You really just have a geometric --

DR. PATEL: Exactly, and probably the vents are not chugging in phase. So each vent is --

DR. CATTON: Are you able to separate that? I think you have a combined lack of synchronization. And if you can't separate them, then I think there is a bit of a problem in accepting either one.

DR. PATEL: We have a single vent test where we took the single vent and put it in the same size pool. Then we measured the pressure.

In general, we find that that is --

DR. CATTON: Did you run three tests with the single vent in everyone of those locations and look at the pressure?

DR. PATEL: Yes.

DR. CATTON: That is a problem that you have to work out.

DR. PATEL: Yes. And by taking a look at that data, we will be ablo to answer —

DR. CATTON: You haven't done it yet.

DR. PATEL: I haven't done that completely. But we have the data and that was the purpose for taking this data, was to sort out what causes the mutivent -- as opposed to just showing that it does go down.

DR. CATTON: you have a broad range of parameters, from 90 to 200 degrees, mass flux to 2.8. Air content, 2.5 on one graph.

Are you going to sort this out or find the maximum value type of curve?

DR. PATEL: There is a report in progress right now and it will be given to the NRC sometime at the end of the year. We don't have complete cross-plocks for each of the points you see here will be plotted against steam mass flux, how it varies and so on.

For the presentation here, this is to give you a flavor as to what this — this band represents something of the order of 50 data points.

DR. CATTON: It represents a tremendous range in important variables.

DR. PATEL: That's right. The point I am trying to

gsh

make here is that the pool size effects seem to dominate the effects of the other parameters.

So you still get most of the data fitting into these kinds of bands.

DR. ZUDANS: Generally, you find that the bottom pressure reduces as pool size increases.

DR. PATEL: Yes.

DR. ZUDANS: The number of downcomers, the number of vents does not have a linear effect on the pool pressure. The effect may be —

DR. BUSH: When you say pool size, you are talking about area relationship or body?

DR. PATEL: We keep the submergence the same.

DR. BUSH: You change the submergence level for the same area of content? Does it change anything?

DR. PATEL: In this particular test program, we are not taking a look at the effect of submergence on the wall pressure.

We have done that in a previous test program and we found that submergence in general does not affect the peak overpressure to a large extent.

For this phase of the program, the submergence was not a variable.

DR. BUSH: Therefore, when you talk about the size, you are really talking about area of content?

gsh

DR. CATTON: Really, what you were saying is that the monometer models are not valid.

DR. PATEL: The monometer models for the chugging.

DR. CATTON: Yes.

DR. PATEL: I seem to feel --

DR. CATTON: I don't want to lead you to a conclusion.

DR. PATEL: It might play an effect, although I think the monometer is essentially going by the condensation process. And as long as things are done, you keep that fairly similar from scale to scale. You find that the other parts of the geometries do not seem to affect it.

DR. CATTON: The monometer effect is not the important one, and I would agree with that.

DR. PATEL: At one-sixth the scale, we see a similar trend. We only did the one, the three vents here. So we have the one data point. Again, you see that the loads are going down.

(Slide.)

DR. PATEL: To conclude --

DR. CATTON: One more question. I don't mean to keep beating on this. If you think that you are dealing with area and you take the particular vent area to pull area ratio, you can go to two vents, double the area, pull area, and get roughly the same load. The pressures will be different, but the load will be the same.

What are you telling me? Are you telling me the loads are the same or the pressures are the same?

DR. PATEL: The peak overpressure are the same.

I do intend to --

DR. CATTON: When you integrate over the surface you may come to a different conclusion with respect to the loads.

DR. PATEL: That may be.

DR. CATTON: You have pressure distribution and you have by no means have enough pressure transducers to determine what it is. You really need another phase to test to conclude that the loads behave as you say they do.

DR. PATEL: This is not a load aspect, per se. It is more what is happening to the pressure at the wall.

DR. CATTON: Your pressure transducers, as you indicated, is in line with the three vents and don't have three vents unless you have located them in some symmetric way that doesn't show on your drawing. I am not sure again what it means.

DR. PATEL: The three vents are essentially put like that.

DR. CATTON: The pressure transducer should be located so it is as close as possible to all three in order to get a peak pressure.

DR. PATEL: We have the three circumferential pressure transducers, the vent elevation.

DR. CATTON: They are higher.

DR. PATEL: We have a pressure transducer there (Indicating.)

DR. CATTON: I noticed that one, yes. You indicated that you were using the bottom one.

DR. PATEL: For the purpose of the data. I made the comparison for these two (indicating.) If I plotted the other transducers --

DR. CATTON: You may not be telling me about the peak pressure then, the pressure that is closest to the vent exit. Doesn't it -- doesn't it read higher? Could you locate that circumferentially around the tree where it would read the peak?

DR. PATEL: We do have that. The three vents are essentially placed like so, and the pressure transducers at the vent exit elevation. (Indicating.)

DR. CATTON: If you bisect one of those vents, how do you know that a pressure transducer where that intersects the wall wouldn't retire due to super position?

DR. PATEL: I don't know that, except when I plot the circumferential location with the three transducers they sort of give the same answers.

DR. CATTON: I would expect that. That is a question I basically cannot answer.

Can't you rotate the lid of this thing?

(Laughter.)

DR. PATEL: We could do that. The purpose of the program was to just see how are the pressures affected at the various locations, and we happened to pick these.

DR. CATTON: It is an acoustic problem and you could calculate where the peak pressure is and locate your pressure transducer then.

DR. PATEL: Right.

DR. YAO: I have a question on the slide on this multivent multiplier.

I tried to understand the meaning of this sentence. So, from this chart if I have a single vent, let me see the total load. The single vent is one unit.

DR. PATEL: Total load or the peak overpressure? The wall?

DR. YAO: This chart indicates peak load.

DR. PATEL: The peak overpressure at a particular wall location.

DR. YAO: Let's assume pressure is almost uniform, so it is one unit. So, I get two vents. Two vents is about .57, something like that. So, is the .57 multiplied by two then multiplied by another two to give you a total area? So actually for two vents, test data indicates this .57 multiplied by four.

DR. CATTON: So, the load is twice, and that sounds reasonable.

(Laughter.)

DR. PATEL: The total load, if you integrate the pressure around the wall, and I am sure what you are saying is true. But the fact is the pressure which is measured at the wall in which I believe the stress -- what one wants to work with and goes down by half. So, there is a distinction between the total load --

DR. YAO: Let's assume from your curve, the curvature, let's say approximately four vents. The curvature starts to change. This means where you increase the number of vents, the load either increases or decreases for the number of vents less than four and the tendency reverses for number of vents bigger than for.

DR. PATEL: Excuse me? I didn't follow that.

DR. YAO: You have a curvature there. This curvature actually changes, the steep curvature it changes to -- the slope changes.

DR. PATEL: First of all, the line has been sort of drawn through some data. I don't know how accurate it is. I don't know whether you want to go to the extent of pulling slopes from it. I would look at the line. But when you start to differentiate a line which has been drawn through a set of data, it worries me.

DR. YAO: The reason I don't interpret your result -- I get the immediate impression that the increased number of vents the load decreases, and --

DR. CATTON: That is correct.

DR. YAO: Somehow this violates my intuition. This is why I try to understand the meaning of that curve.

DR. HANAUER: The ordinant on your curve. This is a number which I have to multiply by what to get the peak pressure?

DR. PATEL: From a single vent multiplied by, for example, .4 will give you the peak pressure at the same location for the multivent geometry.

DR. HANAUER: Do I have to multiply by three?

DR. PATEL: No, you just multiply it by the multivent multiplier.

DR. HANAUER: I am having the same trouble.

(Laughter.)

If I have one downcomer in one plot of Area 1 and I measure a certain number of pounds per square inch peak pressure, if under the same circumstances I have three downcomers in a plot of Area 3 -- will I measure a larger or smaller absolute pressure?

DR. PATEL: You will measure a smaller absolute pressure which will be forty percent of the absolute pressure you measure for the single.

DR. HANAUER: The principle reason is the large part and the fact that they don't reenforce.

DR. PLESSET: I think Dr. Hanauer has the floor for the moment. Continue.

DR. HANAUER: I am finished.

DR. PLESSET: Will you feel better if that multiplier were one-third for three vents?

DR. HANAUER: I just wanted to understand the scale, and I didn't think people were communicating.

DR. PLESSET: Who is next? Let Dr. Yao continue. He started this.

DR. YAO: What I am trying to get across, I think probably, let me suggest something. If you are going to analyze the data, be careful about this curve because the slope change indicated there is the optimum number of vents. You will get the lowest load. That sounds strange.

DR. PATEL: All of this curve in my opinion is showing is that basically when you go from one vent to three vents you see the largest decrease in load, and when you decrease the number of vents you still see a decrease in load, but the decrease is not as large.

Now, I think that if you multiplied those numbers by the number of vents and tried to measure your load, you will find a decrease and an increase character. Of course, as you say, if you draw a line through a wider range of data, maybe -- maybe this curve is not represented in that, but my suggestion is that when you analyze your data you want to be more careful to locate. This may be the mean value of the -- the best estimated value of this curve.

DR. PATEL: I think there is a little misconception here. These numbers do not get multiplied by the number of vents in order to give the peak overpressure. This is just a direct ratio of what I measure in the single vent. I mean, what I measure in the multivent divided by what I measure in the single vent. As far as the pressure transducer is concerned, it doesn't care whether it is one vent, three vents or whatever. It is a direct ratio.

DR. PLESSET: What he has is an area factor which is going up with the number of vents. The load could very well rise as you say, and it will.

For example, if the multiplier was .5 and if it has three times the area it will get one and a half times the load.

DR. YAO: It seems the slope changes --

DR. PLESSET: Don't worry about that.

DR. PATEL: The thing I have shown here is the peak overpressure, and if I did say load several times, I am definitely wrong.

DR. PLESSET: We have that straight.

DR. PATEL: It is a peak overpressure. This is the way the peak overpressure behaves. The total load on the containment, if you assume to do that you would have to make an assumption of what a pressure distribution on the entire wall was and integrate with the area. Therefore, this curve is not to be confused with the total load. This is what happens to the pressure when we measure in a single vent in a multivent geometry. The pressure trend shows that it is increasing with the number of vents.

DR. PLESSET: Increasing?

DR. PATEL: Decreasing.

DR. PLESSET: Your area is going down, too. Your area is going up at the same time.

DR. PATEL: Right.

DR. CATTON: Some of the vents are further away from the pressure transducer.

DR. PLESSET: If you were going to get a load roughly by taking the peak overpressure, multiplying by the area, it is most likely going to go up.

DR. ZUDANS: I would like to make this discussion very short.

(Laughter.)

Ivan pointed out why these data cannot be used to make this conclusion. The transducers are located incorrectly.

DR. CATTON: They may be incorrect.

DR. PLESSET: He doesn't have the pressure field yet.

DR. ZUDANS: The only thing you measure correctly is a single vent peak pressure. You do not have a field pressure for multiple vents. Therefore, you do not know where your peak is. You measure it right behind the triangular path on the diameter. You shaded that point from all of the other vents. I think if you listen to what Ivan says you have to rotate so that you measure in between and you would find out that there is a pressure variation around the circumference. I think this confusion is premature.

DR. PLESSET: For the point at which he measured it.

DR. PATEL: For the point at which I measured which is pull bottom elevation. This is what I see.

Now, I think in the following presentations you will see how they use the 4T data in order to predict the Mark II plant loads. At that point you can see how this --

DR. PLESSET: I think that Dr. Catton's suggestion about getting a couple of other points or one other arrangement would be helpful. You have done so much here with this arrangement that a little more might not be too much.

DR. CATTON: When he goes out to eight vents or seven, now you really wonder where the three pressure transducers are located relative to the --

DR. PATEL: I think that the important point here is that at the same location in the -- there will be a variation around the circumference.

DR. CATTON: You are referring to peak pressure. The only thing I am convinced of is that you know what peak pressure is for the single vent. Unless you run some other experiments with different locations.

DR. PATEL: I think there is a confusion of that peak pressure.

DR. YAO: Maybe this will clarify the point. I think from those peak pressure datum you show us, you can show definite correlation between this peak load pressure and the total load.

DR. CATTON: I think you understand what the concern is. There was only one other comment, and I think you want to make sure that the synchronization of the bubble collapsed in the area factors are separated. If you keep them together, then I am not sure what one can do with the information you are generating. If you can separate the two, I think that they might become meaningful for other aspects of the LOCA loads.

DR. PLESSET: I think we have belabored this a little bit.

Mr. Sobon?

DR. SOBON: It seems to me a lot of this discussion has taken place because it was not clear what the objective of this test program is.

DR. PLESSET: Are you going to tell us now?

(Laughter.)

DR. SOBON: At this point it is narrow, and that is to simply justify that the maximum load measured in the 4T full scale test facility is bounding -- the maximum pressure -- it is simply to demonstrate that that is a bounding pressure and it is suitable for conservative use in plant evaluations.

DR. PLESSET: That is using a single vent peak pressure?

DR. SOBON: Full scale single vent pressure is bounding in a conservative or a multivent geometry.

DR. CATTON: That depends on how you are going to use it. Are you going to take the 4T test pressure and multiply it by the number of vents to get the pressures?

DR. SOBON: We are considering that maximum as conservative and as a maximum for what you would see in a multivent geometry. And this test simply show that the multivent pressure would be low.

DR. PLESSET: We accept that.

(Laughter.)

DR. PLESSET: Would you quickly -- you had a lot of good time.

(Laughter.)

(Slide.)

DR. PATEL: The overall characteristics of the multivent -- of the chugging is very similar to the single vent chugging. The multivent pool wall pressures are lower than those observed in the single vent geometry and the multivent multiplier is a ratio at the pool wall pressures and is less than unity and decreases with an increasing number of vents.

DR. PLESSET: Thank you.

We have another presentation on the improved description of the chug loads.

Mr. Fitch.

DR. ZUDANS: I have a serious question on this.

DR. PLESSET: Just one question.

DR. ZUDANS: I would like to ask one more question of the previous speaker.

When you listed your conclusions, were they based on the same mass flux rate for 136?

DR PATEL: That's correct.

DR. ZUDANS: Then it is not too surprising.

MR. PATEL: It is the same mass flux.

DR. ZUDANS: Total mass flux, the same?

DR PATEL: It is the same. The mass flow in the multivent geometry will be larger than the single vent geometry by the number of vents.

DR. ZUDANS: Is the number of kilograms per second per meter squared?

DR. PLESSET: He said that, but he got a lot of other things.

MR. FITCH: I am Jim Fitch with General Electric.

(Slide.)

I am going to be describing the so-called Task A-16 of the Mark II program to develop an improved chugging load. And the work that I will be describing is basically the result of a joint effort between Bechtel Corporation and General Electric. And we have Bechtel representatives here to help with any questions that you might have.

I would like to begin by briefly describing the history of chugging in the Mark II program.

(Slide.)

I think we can certainly date this to the original 4T testing of 1975 and 1976 which identified the existence on this load, and resulted initially in an application memorandum describing a load to be applied to the wall of the Mark II containment. This load being basically a damped sinu-soidal signal with a frequency range of 20 to 30 hertz.

This load specification was later finalized, and the bounding loads report. There was another development along the way known as the multivent hydrodynamic model which represented an attempt to bring to bear under the problem of the essentially random nature of the chugging phenomena.

This model we now believe was based on an overly simplified representation of the fluid in the containment, mainly the neglect of compressability and the possibility of developing characteristic diversions in the fluid itself. But it nonetheless did indicate that if one took account of the random nature of chugging, the wall loads would be considerably less than the bounding loads.

Another development was the 4T fluid structure interaction study that was conducted by Anamet Laboratories. I will be referring to some of the results of that which bear on this methodology as we go along. There was at the end of that portion of the history, however, still some basic NRC concerns on fluid structure interaction. Namely, that they were concerned that the difference between the fluid structure interaction features of the 4T tank and of the Mark II containment had not been vigorously taken into account in developing wall load for the Mark II.

There were also some results coming back which indicated that the responses calculated on the bounding load were rather high. And as a result of these two areas of concern, the Task A-16 was initiated with the objective of developing an improved chugging load.

(Slide.)

The approach that has been taken in the A-16 program is first of all to pursue some additional study of the 4T data. Second, to develop a model of the 4T system. Third, to develop vent exit forcing functions or what might also be called sources to be applied at the vent exits of a model of the Mark II containment. Fourth, to actually develop a model of the Mark II containment. And finally, I will be talking about some calculations that have been done of actual Mark II responses.

(Slide.)

The 4T facility, I think there has been a similar picture of already. It was, as you know, designed to represent a single cell of a Mark II plant in the sense that it consisted of one vent and that volume of the suppression pool which one might think of as being associated with a single vent. The range of geometric parameters, such quantity as vent diameters submergence clearance cover the range of the Mark II parameters. And the thermal dynamic parameters, initial pool temperature and brake size for example covered the range of what was anticipated to occur during a postulated LOCA.

(Slide.)

The data base that we have concentrated on consists of a library of 137 individual chug events from the total of some 600-odd taken during the entire 4T test. These chugs, we believe, are representative of the worst chugging events that took place during the test in the sense that they contain chugs from those runs in which there was the highest probability of getting a large amplitude event, by which I mean a large maximum peak pressure on the bottom center of the 4T tank.

The specific data that we focused on was the bottom center pressure tracers. In examining these pressure tracers we have arrived at the conclusion that they can be divided somewhat subjectively, of course, into four basic categories. The first of these is probably what most people think of when they think of the chug, a rather large amplitude damped sinu-soidal signal with frequencies in the 20-30 hertz range.

The second category is a lower amplitude sinu-soidal event that persists for perhaps some three and four cycles with frequencies of five, thirteen and twenty-one hertz, typically.

The third category is sort of a combination of those two with the damped sinu-soidal signal separate on a lower amplitude sinu-soidal signal and showing frequencies of five, thirteen and twenty-one hertz and frequencies in the 20-30 hertz range.

And finally, we have what you might think of as the garbage variety chugs which were basically low amplitude events containing those frequencies that I have already mentioned. And, in addition, frequencies -- a whole host of other frequencies ranging up to maybe 50 hertz. The examination of this data and analysis of it has led us to conclude that the dominant frequencies, mainly the 5, 13 and 21 hertz and the 20-30 hertz frequencies, can be attributed to the critical elements of the system which are the vent pipe and the tank pool portion of the facility. And that if one analyzes those separate elements as one dimensional acoustical system containing either steam or water, you can predict that a natural vibration analysis very close to the frequencies that were actually observed in the data. And you find the 5, 13 and 21 as being the fundamental and first two harmonics of acoustic vibrations in the vent. And with some proper additional considerations, we found that the 20-30 hertz frequencies can be explained in terms of an organ pipe mode of the fluid in the wetwell.

With this conclusion at hand, we then embark on a modeling activity to represent the 4T system. And the essential elements of this sytem, which I will be discussing now are, one, that the fluid behavior is modeled as that of a linear acoustic fluid. The chug excitation is presented as a point source excitation at the location of the vent exit.

We hypothesize after the vent is decoupled from the tank pool system, in the sense that frequencies which are seen sls-18

as a result of the acoustic excitation of the vent do appear in the pool, and subsequently on the wall.

However, those frequencies which aren't excited by an impulsive loading of the pool water are not seen in the vent. The theoretical reason that one would have for believing that is that there is a very substantial impedance mismatch between the water and the steam. The kind of handy-dandy way we have adopted in talking about this is that the fisherman has a great deal of difficulty hearing the fish talk, but the reverse is not true.

End t-9

We think this applies to analysis of the 4T system. Another element of the model is a sonic speed adjustment which we have used to account for the effect of fluid structure interaction, and I will be saying a bit more about that.

And finally, as an element in the development of the model, I would like to mention some chug simulation activities. I think calling this a chug simulation is a bit of a overstatement, to say the least.

What it really represents is a wall pressure simulation. And I don't want to place too much weight on what this means, so far as the validity of the model. But let me tell you what we did.

We arrived at the judgment of looking at the 4T data and the way this model responds, that a meaninful impulsive excitation of the system, that you could represent that by a 36 millisecond duration under pressure. The actual functional form we used was a triangular under pressure of 36 millisecond duration -- and let's think of the amplitude as being undetermined at the moment.

Using an acoustic model of the vent, excited the vent with this impulsive underpressure, and say how it responded, mainly in terms of how the first two harmonics appeared in relation to the fundamental, the amplitude of the fundamental.

Then, regarding both the overall amplitude of the vent signal and the amplitude of the impulse as adjustable rmc 2

parameters, leaving alone the relation between the vent harmonics, we attempted to simulate actual chugs in the 4T data base. I am going to show one example of that.

(Slide.)

The bottom portion of the figure here is an actual time trace and associated power spectral density plot of the bottom center pressure from chug No. 30 of the 4T test sequence. This is a chug we would place fairly clearly in our No. 1, Category No. 1.

The top portion of the figure is the time trace, and power spectral density that we were able to achieve by making an adjustment of the overall amplitude of the vent contributions and the impulse contributions.

I think maybe all this says is that with an input function that fits kind of on intuitive feeling about what a chug excitation might look like, we are able with this simplified linearized model of the system to produce bottom pressure signals which appear to be very good simulations of those actually seen in the 4T.

DR. CATTON: Do you separate out the 4T's separate characteristics from characterization of the chug?

DR. FITCH: No. The model, as it was used to do this simulation, incorporates what we believe to be an adequate representation of the 4T's structural characteristics. I am going to say a little bit more about that.

DR. CATTON: I can wait.

DR. FITCH: I would like to turn now to some other verification work that was done on the model. And the first thing I would like to mention is some studies that were done with the computer called KFIX which represents the Navier-Stokes equations in a rigid container. And what was done here was to model the steam in the vent, the water in the pool, the actual geometry of the boundary of those two elements of the system.

The model was excited, both the vent and the pool were excited with a pressure signal which was of sufficient amplitude to give us the kind of signals that are seen on the bottom of the 4T. And the calculation of the response was made both with and without the viscous terms and the nonlinear convective terms.

And we found that those, deletion of those two terms from the representation of the fluid did not have any effect on the response.

So we concluded from this that the assumption of a linear acoustic fluid is being satisfied.

What this means is that the particle velocities were very small in comparison to the sonic speed and the density and the pressure peturbations relative to their background values, were very small.

Additionally, with the use of this model, we confirmed or added confirmation, let's say, to the assumption that frequencies that are generated by impulsive excitation of the pool do not propagate back into the vent, whereas the frequencies that are excited in the vent do propagate into the pool and are seen on the wall.

And while we were working with KFIX, we were working on the other hand with our simplified model, using the point source. And we concluded that so far as the character of the fluid response is concerned, that we were able to excite the response of the fluid just as well with a point source consisting of an impulse plus the input to the pool that one would get from the vent, that you could excite the same response as you could in the FKIX model by modeling both the steam and the water.

I guess part of that result was the fact that the physical presence of the vent type on the axis of the tank did not have a significant effect on the response. And this was partly expected, because of the one-dimensional character of this working type load response of the pool.

Some additional confirmation of our assumptions came from two of the tests that Anamet performed in the 4T facility, one in which they imploded a bell jar, an evacuated bell jar tied to the end of a vent pipe; and a second one in which they dropped a projectile-shaped weight down through the fluid and impacted the baseplate at its center.

The response of the 4T to both those excitations was a damped sinusoidal signal, much as we saw in the data, with a frequency of -- that was downshifted from the one that you would calculate for the fundamental organ pipe mode in a rigid tank.

And by turning to some centered types of analyses, the type that are done to account for the effect of pipe wall flexibility on the propagation of a pressure pulse, we find that with a relatively simple formula that involves the properties of steel and the fluid, we are able to deduce an adjustment in the summed speed input and an input parameter to the model that will account for the effect of the flexibility of the walls and the baseplate, which is primarily to reduce the frequency of the fundamental response mode of the tank.

As a final step in the verification of that method of accounting for fluid/structure interaction, we are pursuing some NASTRAN studies where we have modeled with finite elements both the acoustic fluid and the elastic boundary with full coupling -- and I don't have the final results from those studies at this point; we are just in the middle of this.

But that will form, we think, a significant additional confirmation, that our method of accounting for fluid/structure interaction is adequate.

(Slide.)

The next step in the chronology is to get to what we were after, which was actual load specifications. And the way --

our starting point for this was to establish what one might call a simple vent forcing function or source using the model of the 4T and the 4T data.

And in doing this, we had the thought in mind when we came to the actual Mark 2 containment application, that we would have a load case in which all of the vents were excited synchronously with the same amplitude signal, so that in trying to come up with a load case that we think is a bounding representation of what could happen during chugging.

We tried to balance how the bounding we needed to be with this single vent source against the fact that we were going to be applying this to the Mark 2 plant with all of the vents synchronously firing off at full amplitude.

The criteria that we used to develop the single vent source are in terms of the total -- they are derived by looking at the bottom center pressure signal produced when the single vent forcing function is imposed at the vent exit location in our model of the 4T.

And we established criteria on the total power in the signal produced, criteria on the power by frequency, and an additional criterion on the peak pressure of the time history of the signal.

DR. CATTON: Are you taking that signal that you showed us previously?

DR. FITCH: No.

DR. CATTON: Or are you decoupling it from the structure, asking yourself what was the source strength in 4T and taking that source strength and going back to the Mark 2 and incorporating the structure? Is that the procedure?

DR. FITCH: We believe the model we are using to compute this source has incorporated the structural effects by virtue of the adjustment in the sonic speed.

There is an additional consideration which involves the damping associated with the structure.

DR. CATTON: I don't understand your answer.

DR. WU: With this structural bending included, would that be the measured data --

DR. CATTON: You have a measured pressure on the boundary. Are you going from the measured pressure on the boundary through a structural analysis and saying, gee, what was the source at the vent exit, and then taking that vent exit source and putting it into a Mark 2?

DR. FITCH: Right. I guess I didn't -- I complicated it.

DR. CATTON: Yes, you did.

(Laughter.)

DR. FITCH: The source has the same character as the source that I described earlier in the simulation study. We did not constrain ourselves, as I mentioned, at that point we were trying to simulate an actual chug, one individual chug, mg 8

so that we were adjusting the amplitude of the impulse of the vent signal to do that. We now play that game all over again, trying to satisfy the specific criteria.

I haven't told you specifically what they were, but I have outlined them in general. And having determined the source which satisfied those criteria, we then needed to account for differences between the 4T and the Mark 2.

And the most notable of those is the difference the shorter vent lenghts of the Mark 2. So that we recognized that we had to adjust those frequencies that we associate with the vent, the excitation of the vent in the 4T during chugging by a factor to account for the shorter vent length in the Mark 2 plant.

Having done that, we then assigned -- we identified Load Case 1, I have called it, where that vent exit forcing function is assigned to all of the vents synchronously. And we subsequently developed a Load Case 2 which is an attempt to again look at the random nature of chugging and make some allowance for the possibility that during the course of the chugging portion of a blowdown, you could develop an asymmetry in the force exerted on the containment.

So that we have a second load case which again uses this same single vent forcing function as its basic building block, but applies a circumferential multiplier which is intended to cover the possibility of an asymmetric loading.

I will show you a little more detail on a single vent forcing function.

(Slide.)

I think the most revealing way to look at it is in terms of the power spectral density of the bottom center pressure produced by the single vent forcing function applied in the 4T model versus what I have compared it with here, the average power spectral density on the 137 chugs in the data base.

Now, I think the first thing to observe is that the PSD produced by the forcing function envelopes all of the peaks of the PSD, the average PSD from the pit data base. It also obviously included a considerably greater total signal power.

The main reason for that is this spectral peak at 28 hertz which kind of stands out. The reason why we felt it was necessary to put in that additional signal power in that frequency range is to cover the possibility that all of these vents could simultaneously produce one of these major ring-out type chugs --

Those chugs form such a relatively small portion of the data base, even when you are conservative in the choice of your data base, that that spectal peak tends to be squashed out in the averaging process so that you don't see it in the PSD of the data, that we know it is there, in individual chugs.

And we felt that it was necessary to make allowance for it.

We do have a couple of locations where we are not absolutely enveloping the average PSD, and we think those need attention. But we don't think that they are going to be a serious problem as far as the load definition is concerned.

(Slide.)

One more picture on the single vent forcing function.

DR. ZUDANS: How did you get that first peak of the average to coincide with your forcing function?

DR. PITCH: By tuning the knob that we have available on the amplitude of the portion of the forcing function that we attribute to the acoustic signal excited in the vent. We just cranked it up alone -- we were working with the amplitude of the impulse, and the amplitude of the sinusoidal portion of the signal, and we cranked them up until we got the desired response.

DR. ZUDANS: You give me an answer to the next question, and that is, how did it get on your average? When you averaged 137 individual chugs, it has to be pretty much of a coincidence or pretty much of a playing with the time shift between chugs to get the first peak on your recorded chugs, not on your forcing function, the first big peak.

DR. FITCH: The fundamental of the vent was very consistent.

DR. ZUDANS: You didn't have time shift, nothing?

DR. FITCH: No. The 5 hertz signal was always there.

DR. ZUDANS: And the second single vent forcing function peak, is that realistic? When you played with the first one to match it, I guess you got the second one as a fringe benefit.

(Laughter.)

DR. FITCH: That would be attributing too much to what we did. I mentioned that when we did the simulation study, I fixed the relative amplitude of the fundamental and the first harmonic. When we played this game of getting the forcing function which enveloped the data, I removed that constraint.

So that we, as it turns out, I think the relative amplitude of those two peaks is not unlike what it would be if you excited the model of the vent separately, but that was not an imposed constraint when we did this.

DR. CATTON: This is not your vent exit source that we are looking at?

DR. FITCH: No, this is the bottom center pressure signal produced when we crank that source into the model.

DR. CATTON: You are going to take this and back out a source?

DR. FITCH: Yes. We have backed out a source, and a source produces the dashed line.

DR. CATTON: I understand.

DR. ZUDANS: If you remember our previous discussion of the resonance and things of that nature, if you eliminate that possibility, because actually you took the structure flexibility by adjusting your sonic speed, the pipe only, right?

DR. FITCH: On the water.

DR. ZUDANS: Now, in this case your forcing function frequency content is strictly determined by the structural and water characteristics, right, not by forcing function -- it might account for some other physical reasons in there?

DR. FITCH: The frequency content accounts for the response of the steam in the vent. That is the first. And the dominant signals that we see in the 4T data --

DR. ZUDANS: The structural and acoustic.

DR. FITCH: And the vent pipe acoustical frequencies. So the forcing function which is not applied as a point source must incorporate both the vent pipe excitation -- one might think of the vent pipe now as a separate element exciting the tank pool system.

The source must incorporate those frequencies and also have the capability of exciting the tank pool system in such a way as to envelope or bound in an appropriate sense the frequencies that we attibute to that portion of the system in the data. That is where the impulsive part of it comes from.

And the importance of modeling the fluid in the tank as a compressible fluid is that you now have the capability of picking up this resonance where we see it in the data.

DR. ZUDANS: I think what you say is clear and precisely stated. What bothers me a little bit is chugging being something that you could see that had more source than water and steam did at these relative volumes of the system.

Now, you have the dominant frequencies strictly defined by certain modes of the critical combination of water and space that volume would respond to?

DR. FITCH: That is essentially correct.

DR. ZUDANS: And then when the water condenses in the steam, it excites these things and that is what we see, for the most part.

There is nothing inherently physical in the steam condensation process itself that you could -- that you would have to identify when you describe the configuration accurately. In terms of the linear multiple response, you have all you need; is that a correct statement?

DR. FITCH: Yes. That is what this method is based on. I think -- the answer is yes.

DR. WU: Similar to this, have you tested the -- your point source representation by checking with the predicted pressure elsewhere -- how many places have you checked the rr 4

end #10

pressure measurement versus the energy frequency distribution a the site of the wall or other places?

We have not done that in any kind of detail. That should be done. The pull response, according to the prediction, is essentially a quarter sine wave, standing from the bottom to the top, and it should be possible to ascertain that. We have started on that, but I just don't have anything concrete to say about it at this point that is good additional verification.

DR. WU: This might explain the mechanism of the chugging to see if it is really a point source or if the accoustic effects might be important. You might have some other mechanism involved like the vortices.

DR. FITCH: If we get a tilt when we make the comparison --

DR. ASHLEY: The library we used to develop these sources from consisted of bottom-center pressure. There wee some tests done by Anamet Laboratory with the same tank where the bell jar collapsed where they measured and have produced and sent to the NRC the standing wave pattern, and it is the quarter standing wave, and it is produced by this, because it is the solution of the acoustic wave in the tank.

So insofar as that confirmation has been done, but not for each chug of the 137.

DR. WU: Standing wave with respect to what?

DR. ASHLEY: In the fluid portion of the 4-T test tank. It is a quarter standing wave.

DR. FITCH: That was the implosion of the evacuated bell jar. That was the response. In that test, they had a nice sequence of transducers up the wall and you can fit them with the quarter wave pressure variation.

DR. WU: That is very interesting. Thank you. It is easy to match the standing wave and the point source easily?

DR. FITCH: We can reproduce the quarter standing wave pattern as measured.

DR. ASHLEY: Yes, we developed some impulsive source and then normalize it to unity pressure.

(Slide.)

DR. FITCH: One more figure on the single vent forcing function. The bottom pressure trace. In time it may not look an awful lot like any individual chug, but I think that the reason for this can best be explained by saying that we have here a composite chug in an attempt to suitably bound all of the power to all of the individual frequencies, and that is the essential basis on which the validity of this source rests.

We have, as I mentioned earlier though, established a criterion on peak pressure because we thought that was important. The predicted peak pressure of 9.5 psi is quite conservative, relative to the data base we were working with. I am not stating that that is an absolute bounding peak pressure. I think it is appropriately conservative for the way these loads will be applied in containment.

DR. ZUDANS: Since you have really worked with a linear system, you could apply this to multiple vents just as easy.

DR. FITCH: Right.

DR. ZUDANS: I'm sorry.

(Laughter.)

(Slide.)

DR. FITCH: The final step is the Mark II containment model which has again been based on this linear acoustical representation of the fluid chug excitation sources and each of the vent exit locations. The calculation of the Mark II force response I am going to be showing you has been carried out with a procedure described, among others by Professor Sonon at MIT, in which you first calculate rigid wall loads and then apply those rigid wall loads to a model of the flexible structure with appropriate account taken of the fluid.

I might mention also as part of the program, we have a NASTRAN model of the Mark II containment being cranked up in order to verify the particular set of computer programs and procedure that is now being used to do that.

WH

The results I will be showing you from Mark II response will be an acceleration response spectra at a couple of selected locations.

(Slide.)

There are a couple of points of importance here. I am going to show you a couple of pressure histories on containment from rigid wall pressure histories from this location and then acceleration response spectra on the pedestal near here.

(Indicating.)

And on the containment wall about here.

(Indicating.)

(Slide.)

The next two figures show the rigid pressure wall pressure traces associated with our symmetric, so-called symmetric, all vents in unison load and the asymmetric load. And I apologize for changing units. These are plotted in roughly 7 X psi. That is for that location down at the bottom of the containment wall by the base.

I think I will just skip over the asymmetric one. There is nothing significantly different about it. This is, perhaps, not surprising when you think about it, but we really, in terms of a rigid wall load, no longer have a perfectly symmetric wall on the containment.

(Slide.)

This is due to the nonuniform azimuthal distribution of the vents in the Mark II plants. There is actually some variation in the rigid wall load with angle, not -- it is about half of what the variation is with the deliberately asymmetric load. But nonetheless it is significant.

(Slide.)

Here is the acceleration response spectrum for the point that I showed you on the top of the pedestal compared with the result obtained by using the current bounding load specs. There has been achieved with this load a significant reduction in the response.

(Slide.)

And a similar plot for the containment locations that I showed here. And finally I would like to get a quick summary up here.

(Slide.)

I would like to say that these are the things we have achieved with the A-16 program. First, we have developed forcing functions which we believe we can establish our bounding representations of what an actual Mark II containment could be subjected to during the chugging phase of a LOCA, that we think we have provided a basis for modeling the differences between the 4-T system and the actual Mark II plant, and unscrambling that part of the problem.

And finally that we have computed acceleration response spectra which are significantly reduced from those that you would calculate using these current different load specifications.

DR. ZUDANS: So you apply this same source at every one of the events, and you took the acoustic characteristics of the entire containment instead of just 4-T, and you added all of the individual number of modes for each of the vent. And that is what you got, what you have shown us now?

DR. FITCH: That's correct. Yes.

DR. ZUDANS: Is there a report where this is discussed. It sounds like it is an extremely interesting piece of work.

DR. FITCH: Who can speak for the report at this time? Gordon?

DR. ASHLEY: There is a report in preparation that will be sent to the NRC, probably the middle or the end of November, and it will be made available to you.

DR. ZUDANS: It is very good work.

DR. PLESSET: Fine. Thank you, Mr. Fitch. We have another presentation before we have our break. Also secheduled for 30 minutes.

(Laughter.)

POOR ORIGINAL

MR. DAVIS: I am Mac Davis from General Electric. I will try to get us back on schedule.

(Slide.)

What I will be discussing with you today on the CAORSO test is briefly going over the test configurations, what the objectives of the tests were, just an instrumentation summary of the test matrix, and the results of the phase I test, some priliminary results of phase II, and then our conclusions from the test to date.

(Slide.)

This we have seen a couple of times today which is a cross-section of the Mark II plant.

(Slide.)

This slide will show you the arrangement of the quenchers in the CAORSO plant. I have indicated that when I talk about the multivalve phase II test a little bit later, what the groupings were for the various multiple tests. Unfortunately, my slide is color-coded and your copies aren't coded at all, but you can see the are the two quenchers that we used in the two vent test, the purple and the green. It is shown here.

(Slide.)

The objectives of the test were to confirm the load definition that we had provided in the DFFR and also to provide a data base for future load reduction after the earlier users, if there were any, of the quencher. And the things that we looked at and tried to provide data for wee the pool boundary pressures. We looked at the response of the building with accelerometers. We looked at the discharge line clearing and reflood that occurs after an actuation of a quencher, response of the quencher itself due to the air bubble and air clearing loads.

We looked at cool thermal mixing from an extended blowdown and we — that's some qualitative data on what would be happening to submerged structures and some strain guages and some instrumentation on the liner itself and on the downcomer.

(Slide.)

Just kind of a total assessment of the instrumentation that we had in the test. It was rather extensive. There were approximately — this is about 185 or 190 sensors in total. For the suppresion pull itself in the quencher and safety relief valve line, we measured the pressure, temperature. We measured the strain in the pipe and the quencher itself, accelerometers on the quencher itself, and we were looking at the water level in the pipe.

On the containment structure, we had strain guages and accelerometers. We measured the vacuum breaker flow for one of the -- on one of the safety relief valve lines that we did multiple testing on, and we looked at the stem position which told us when the valve opened, and then we had nine other miscellaneous sensors located.

(Slide.)

Just a summary of the phase I test. And this by the way, this information has been reported, and it has been submitted recently to the NRC.

On the phase I testing -- this is a summary of the type of tests we ran. In the phase I, we ran single valve tests only, no multiple valve, and we ran first actuation tests, and we ran consecutive actuation tests to see what was occurring as a result of having various things going on in the piping system after the first actuation.

And during these tests we varied these parameters. The water leg. We varied the pipe temperature from cold, warm, to hot. We varied the vacuum breaker area to see what the effects of the vacuum breaker on the reflood transient, and then we changed the valve that was actuated, and we used four different valves in the test.

In phase II we repeated tests of the single valve tests, which also included some testing, varied the vessel pressure, to get an indication of what the change in boundary loads were as a function of vessel pressure. We had 11 multiple valve actuations, and we ran some leaky valve tests. We didn't really plan these tests, but we -- it had been requested many times by the NRC to run a leaky valve test, and you might say we were lucky that we had a leaky valve.

(Laughter.)

We ran one extended valve blowdown, and we also varied the number of vacuum breakers utililzed. We varied the pipe temperature, the vessel pressure, and then we changed the valve groupings.

(Slide.)

These are the phase I test results which are in a report that has been submitted. When we look at these comparisons, I have one column here that says "Maximum Test Values" for various conditions.

One is the first actuation. One is the consecutive actuation. And when we look at these, we have picked out all of the data points the maximum positive pressure and the maximum negative pressure which don't necessarily occur on the same test point. We have calculated using the DFFR methodology predictions for both the mean 90-90 values for the boundary pressure.

These are calculated based upon the nominal conditions for first actuation and the same for consecutive actuation, and you can see that there is a significant difference between test data and what our predictive values are using our DFFR methodology.

The predominant bubble frequencies were from 5 to mac BWH

11 hertz. We did observe in the tests pressure attenuation which seemed to be more rapid than what we have used as our methodology from the DFFR, and the attenuation with distance was a little bit less -- a little bit greater than what we used for our methodology.

I would like to point out that when you are looking at these values and trying to make some assessment of how conservative the predictions might be relative to the test data that there are quite a bit of efforts going on in looking at this data to better define what this conservatism might be, and Burnes & Rowe is doing a detailed evaluation of this data, primarily looking for how you apply it to Mark II plant, rather than just plain old data report.

They are addressing it for application in their particular plant.

DR. BUSH: Was time a parameter in your consecutive actuation?

MR. DAVIS: Yes, we varied the time between actuations. I don't remember the exact values, but we did vary the times between the actuations.

DR. BUSH: Did you reach -- is time a critical path?

MR. DAVIS: We believe we tested at the most critical time variation for the consecutive actuation.

DR. BUSH: So time was another variable in your matrix?

MR. DAVIS: That' true.

DR. PLESSET: The predictions on DFFR, was that for a quencher?

MR. DAVIS: For a X-Quencher.

DR. PLESSET: Like the test.

MR. DAVIS: Yes, and it is predicted using the parameters of the CAORSO plant itself.

(Slide.)

Other parameters that — test results are the discharge line pressure. Pressure in the line agreed with the predictions. We did confirm the effects of the vacuum breaker side on the reflood transient. That means how far the water level, how far it reentered the line after the valve was actuated. The dynamic stresses on the quencher were less than what we predicted. Our maximum measured building response from the suppression pool loads were less than .07G, which is a very low value.

The liner strains were below predictions, and the bending strains were less than what we predicted.

DR. BUSH: How are you defining "dynamic stresses"?

MR. DAVIS: The dynamic response of the quencher strain guages, strain guages on the quencher.

DR. BUSH: If the time interval is low, then I get a totally different response than if it is longer, which means it is quasi-static, and I want to know if these are truly dynamic stresses or not.

MR. DAVIS: These are dynamic stresses in that the puncture arm is oscillating at a high rate.

DR. ZUDANS: And you had this strain guage trace?

MR. DAVIS: Right.

(Slide.)

On the phase II results, and we are just in the process of putting this report together, the single valve test is very consistent with the phase I testing. Relative to the multiple valve boundary pressures, they are relative for four and eight valves, and our maximum test predictions, our maximum predictions are shown over here, using the DFFR methodology again. And the maximum values we got out of the test are shown here, and, again, you can see that there is a significant test between the predictions and the values.

Again, this data is also being looked at by WPPSS as part of a plant application. During the 13 minute blowdown, we looked at the distribution of temperature in the pool. It looks like there are approximately 10 degree temperature differences between LOCA, bulk and LOCA.

Relative to our conclusions on this, we feel that we met the objectives of the test. The data was consistent and repeatable. The predictions of what is going on in the plant compared well with the test data. The boundary pressures were well below what we predicted, and I think we have shown that the DFFR methodology is conservative, and there is some work going on which will show it is more conservative than you might subjectively get from these slides.

DR. CATTON: How important is it that you know the LOCA to bulk Delta-T? What role does it play?

MR. DAVIS: When we look at the pool temperature limit as defined by the NRC criteria of 200 degree LOCA, when you look at all the various transients you can have in a plant, you want to be sure that you stay at or below that 200 degree local temperature and the quencher, as far as the importance it plays. It gives you a value to compare your calculated bulk cooled temperature, which is the way we do our analysis with the local temperature and the NRC criteria of 200 degrees.

The 200 degrees is something we had accepted, although we feel --

DR. CATTON: I understand that. Now what I am wondering, then, is how did you locate the thermocouples that measured this -- what do you mean by local temperatures? Is it maximum temperature?

MR. DAVIS: In that area of the pool that is feeding the steam jet as it comes out of the quencher itself.

DR. CATTON: How do you know where to put the thermocouple relative to the quencher. If you put it too close, you are going to measure saturation.

MR. DAVIS: We had some on the quencher. We had some at various locations from the quencher and around the pool. I think it would be best if I tell you that we are putting the report together that evaluates this data, relative to the bulk to LOCA pool temperature without trying to jump ahead as to what the conclusions are.

DR. CATTON: I can see where you could get whatever local to bulk LOCA Delta-T by picking a thermocouple, so there has to be some volume that you average. I would be interested in seeing the mechanics you go through, particularly the rationalization.

MR. DAVIS: That's all I have. Before we take a break, if we could, Ray Muzzy has the answer to Dr. Catton's question.

MR. MUZZY: The question was concerning the high speed channels of the 4-T CO test. We are recording the data on analog tape, and we are digitizing the tape at the rate of 1000 samples per second.

DR. CATTON: Thank you. That is quite adequate.

DR. PLESSET: Very good. We will take a 13 minute break until 3:15.

(Laughter.)

(Recess.)

BWH

DR. PLESSET: Let's reconvene and go to the next presentation on the GKM test. Would you proceed.

MR. ROTH: I am from Pennsylvania Power & Light Company. I am here to discuss tests that we will be running in the GKM facility in Manheim, Germany. You heard this morning from Ray Muzzy, of GE, a discussion of the decision that was made to run a full-scale condensation oscillation test. At the time that decision was made, PPL made a further decision to proceed with their own test on a prototypical basis at Manheim in the GKM facility. That was made in January of '79.

At that time, we were actively pursuing meeting a May of '80 fuel load date, and our review indicated that the GKM test would provide us with data earlier than they proposed for two tests. And in addition, the GKM test facility could be modified to more exactly represent the Susquehanna single cell and, hopefully, give us data which would be more prototypical of the Susquehanna configuration of containment and would, hopefully, expedite our evaluation of that data and licensing review that would go along with that to meet the May of '80 fuel load date.

Since the decision was made, a lot of things have happened, and presently we are working toward a December of '80 fuel load date. And the urgency of the test is sort of not as bad as it was. The decision was made early this year, and the primary reason was scheduling.

The next slide is the schedule of the test.

(Slide.)

Presently, we have just — we are just beginning shakedown. Hopefully, next week we will begin shakedown of the facility and actual testing will begin the first part of October.

(Slide.)

One of the benefits we saw from the GKM facility was the ability to represent more exactly the parameters of the Susquehanna single cell. This slide lays out the parameters that we did match in the GKM facility, and we will go through them all. They are all there. The drywell volume, wetwell air space volume, the unit cell, the vent, submergence clearance to the bottom, flexibility of the walls. They were all modified to match the single cell at Susquehanna.

(Slide.)

We did have to do a lot of modifications to the GKM tank, and this shows what we did at the facility. If you are familiar with the tests that were run at GKM by KWU, they included a flexible wall to simulate the flexibility of the German BWRs. That was removed. We added a new drywell in the tank. We included a new inner cylinder in order to get the right unit cell area. We stiffened the tank

BWH

foundation for structural reasons. We included a prototypical vent and bracing system for the Susquehanna configuration.

We added a viewing port in order to get some high-speed photograhy of the occurrences at the end of the vent, and we added a couple of submerged structure targets -- quencher arms and wide-flanged beam -- to get additional data on submerged structure loads.

The next slide is one you may have seen before.

(Slide.)

It is the GKM tank. What it looked like prior to the modifications we made. This is what it looks like when it was used by KWU for their test. I have indicated up here in green on the flimsy where we had to cut the facility in order to make modifications.

The next slide shows what it looks like today.

(Slide.)

Again, the green line sort of corresponds with the green line on the previous slide. The dimensions are all millimeters here. The old tank essentially is now the wetwell boundary for our modified tank. We added a new drywell. In order to get the correct unit cell, like I said, we included an inner cylinder in here. The dimension of the cylinder is about 100 millimeters thick. That was necessitated by getting the correct flexibility of the wall to match the Susquehanna flexibility.

We included down here a viewing port and hope to get some good movies, high-speed photography, at the end of the vent.

The foundation was stiffened, and we included in there the vent stiffness up here, and the bracing elevation was matched. Down here we show the target quencher arm was to be located beneath the vent exit at the right elevation corresponding to that in the Susquehanna plant.

In addition, there is another target beam situated about this elevation. It is not shown in this slide, but is shown in a subsequent slide.

(Slide.)

DR. BUSH: Did I understand you to say that the 100-millimeter wall was to simulate the concrete, the reinforced concrete?

MR. ROTH: Yes. That is shown -- that is shown on this acditional slide.

(Slide.)

The instrumentation, we have about 60 channels of instrumentation, measuring pressures and temperatures in the drywell in the vent, the wetwell air space, and the boundary of the pool. Also plan to measure the water level in the vent, the air content in the vent, by a continuous system, and I hope we have a sample, a grub sample, and measure displacements on the vessel, indication of any movement, measuring the strain on the vent, and the bracing and the quencher arm and the forces on the target beam and movies of the vent exit.

(Slide.)

The next two slides show a little bit more of the instrumentation, the layout of the instrumentation. It is self-explanatory, really. This gives you an overall representation.

The next slide goes into a little more detail down in the wetwell portion of the tank, a little more exactly.

(Slide.)

This shows a little bit better the inner cylinder that was added in here in order to get the right area. You can see in this one indication of the target beam.

DR. HANAUER: Are there strain gauges on the downcomer?

MR. ROTH: Yes. They are not shown on that slide, but there are strain gauges. They may have been added after this slide was made, but I can assure there are.

(Slide.)

The next slide shows a cross-section at the bracing elevation. Included in here, two braces, 90 degrees to each other. This brace, this configuration here, the connection to the vent, is prototypical of the connection we have in the Susquehanna. This brace portion extends through the inner cylinder and is actually attached to the outer cylinder. The outer cylinder is the old tank, and this represents the new inner cylinder, and we will be measuring the strain on that brace from the collapse of the steam bubble somewhere around the vent here (indicating).

The next slide clarifies that a little bit more. You see here the same representation from the previous slide blown up, and this is a cross section through that. This is a connection of the vent to the brace. The brace passes through the inner cylinder and is attached to the outer cylinder here. The strains are read in this section here, and we put a flexible coupling to protect the strain gauges (indicating).

(Slide.)

The last slide is the test matrix as it exists today, basically doing 10 tests, individual tests, and then a repeat test at each matrix point. It shows 20 tests, but it is actually 10 variations. We are varying the steam mass flux pool temperature during somewhat the air content. and everything else that we feel is prototypical of Susquehanna, so we don't intend to vary submergence or anything else.

DR. PLESSET: Any questions?

(No response.)

DR. PLESSET: Thank you.

DR. MARK: This will be run with compressed air or steam?

MR. ROTH: Steam.

DR. MARK: I noticed in the last chart, "Air 100 percent."

MR. ROTH: That means air content in the drywell prior to the tests. One of the concerns is that there may be differences in the air content.

DR. ZUDANS: A little observation. You have the quencher arm sticking out one side, and you also have an I-beam on the other side. You are going to measure lateral loads on this downcomer? You have strain gauges in two directions. Are you going to be able to decipher where they come from, whether from fluid interaction with these asymmetric pieces of structure that are there or because of some condensation process?

MR. ROTH: Generally, the lateral load, the peak lateral load in the bracing, is due to the collapse of the steam bubble.

DR. ZUDANS: Generally. But you do have asymmetric protrusions that will affect the flow of water and impinge on the sides.

MR. ROTH: The quencher arm is located directly under the -- it is relatively symmetric, although, of course, it doesn't extend all the way across the pool. You

BWH

are right. The target beam is asymmetric in the pool. I don't know if that would influence the measurement. Really, the maximum load we are going to see is due to the collapse of the bubble.

DR. CATTON: Where is the pressure transducer, the one you are going to measure the pressure with?

MR. ROTH: The pressure where?

DR. CATTON: Dr. Zudans questioned the arm. If you are measuring it at the bottom, the arm is between you and the bottom of the downcomer?

MR. ROTH: I think that is offset somewhat.

Is it directly under?

DR. ASHLEY: It is at the bottom center. We are looking at a quarter standing wave in this tank and not much fluid motion after the jet clearing is through the chugging CO regime. It is basically a standing pressure wave.

DR. CATTON: The pressure transducer is underneath the arm.

DR. ASHLEY: There is one bottom dead center and a string of them up the side of the tank also. I believe that the arm is instrumented.

MR. ROTH: I think we have sufficient pressure gauges throughout the pool. We won't be relying just on the one pressure gauge. This is the one you are concerned about (indicating).

DR. CATTON: It is masked from the vent.

DR. ZUDANS: It doesn't matter where the pressure gauge is located. My concern is that it may not have significant impact on the results. It may not have any at all. The only thing is that you are not going to get the clean answer to the question that you are raising. Condensation loads on the downcomer, they will be affected by the fact that there is perturbations of flow due to elements that are handing out there in a nonsymmetrical fashion.

Now, even if you measure all of the pressures on all of the surfaces, they still have the same inability. Is it important? I don't know.

MR. ROTH: We will have to think about that.

DR. ASHLEY: The load on the vent or the downcomer is essentially caused by a local condensation phenomena. The bubble sneaks up on the side and then collapses or collapses in some asymmetric manner and water rushes in from the side and creates the load. It is not created by a pressure field in the water itself. That is a smaller event. This the best thinking of both the German people and people at General Electric, I believe.

DR. ZUDANS: You precondition in your own mind that that is what you will be looking for. My question is very simple: Is it possible to, in fact, have a clean test like that?

DR. ASHLEY: You will know the pressure field throughout the water portion. You will see if there are asymmetries introduced because of the protrusions.

DR. ZUDANS: I have nothing more.

DR. BEDROSIAN: Our presentation will serve the purpose of presenting a summary of our efforts in defining a chugging load and methodology for application for WPPSS No. 2 for the specific purpose of applying it to WPPSS-2.

(Slide.)

This might give you some background of what has happened. We basically developed the chugging load definition and the methodology for the Mark II containment for specific application to WPPSS-2. And we had some meetings with the NRC staff in late '78. We had some other meetings with the NRC staff and consultants in the early 1979. And we also submitted to the NRC a summary in 1979 and a technical report in June of this year.

This presentation is concerned with the phenomena which occurs in the tail end of the LOCA with the chugging, which occurred in the Mark II plants. And we think the conditions in this cross-sectional view of the Mark II plant are like this at that time.

The steam -- at that time, the flow of steam is established from the pressurized drywell through the vents, and at the vent exit there is an interface between the vents and the water surrounding it from the suppression pool, and that is the steam condensers. Because of the reduced flow rates of steam, the surface is not stable, and it collapses.

(Slide.)

This is an impressionistic view of what is happening at the vent, and shows the interface of the water in the pool. And as we said, the phenomenon is basically representative of this interface, and the interface collapses, and the net effect is to induce a forcing signal which shows the event in the suppression pool.

(Slide.)

The next slide represents, in summary, the problem. It explains our understanding and also indicates our approach in answering the two questions we were asked. The first was to define a chugging load definition and then look at the available test data and then apply it to the Mark II containments, the chugging load conditions.

If a forcing signal is imparted to the vent above the vent exit, it excites the pressure in the vent and the pressure wave in the pool. These reflect and interact, and in the end of the -- traveling through the pool, reach the pool boundary, at which time we label this as a "pressure wave." When applied to this flexible boundary, the boundary will reflect, and then it will interact with the water contained behind it, and that will give rise to an additional pressure perturbation, which we labeled "FSI," in the common terminology.

This is the part that is the result of the interaction between the containment boundary and the containment water. This is really what one has available from the records of boundary pressures from the tests.

In terms of the two questions we had to answer, first, we had to solve the first problem, which means, given a set of pressures at the boundary of a test facility and its associated geometry, we had to express the forcing function which, at this point, becomes independent of the geometry of the test facility. And it may be transported into a Mark II and used for design conditions of the Mark II containment, assuming that the conditions in the test are reflective of conditions expected in the Mark II during chugging loads.

(Slide.)

The answer to the first question started with the data developed in GE's 4T test facility. This test was representative of the Mark II conditions during a LOCA event, including long-term effects such as chugging. And furthermore, the facility approximated a unit cell in the Mark II geometry, the vent size. And for these conditions -- or because of these conditions which were duplicated in the test facility, we considered that the load extracted from these test results would be a load which could be extrapolated for Mark II containment.

(Slide.)

This is a picture of the 4T facility. It was described before. The only difference between the 4T facility and the Mark II is the length of the vent for the downcomer vent. And since our load definition was extracted independent of this vent length, we made it portable to a Mark II without further assumptions. But we had to address that.

(Slide.)

In answer to the first question of how to define a single vent design load specification, since the test data was available from a single vent test facility, the 4T facility, we followed, in short, the three steps summarized on this chart.

(Slide.0

First, we analyzed the 4T boundary pressures, and tried to identify certain characteristics. And there we saw the chugging phenomena. And then, in order to identify the main 4T system components which are excited by the chugging phenomena, saw that the next step we could develop would be a realistic model of the 4T facility, and, with this help, to extract the forcing function at the vent end or exit.

BWH

(Slide.)

In the first step, the first phase we analyzed, we tried to identify the characteristics of the chugging load, and despite the variety of time histories and recorded, we identified in all cases the same set of discrete frequencies. We observed random trends, both in terms of peak amplitude and frequency trends in the phases. And we also have been able to identify the forcing nature of the chugging load.

We then went further to analyze the traces and tried to identify the main components of the 4T facility that were excited during the chugging phenomena. And we found that these frequencies would be identified to the vent acoustic frequencies and to the water tank, including support frequency.

The vent acoustic frequencies would be in the range of five, a couple of harmonics, evidently, in the recorded cases, the first and second, probably, and the water tank support frequency, the main and sometimes the second frequency which were also evident in the test.

(Slide.)

We were able to identify these frequencies, assuming that the steam in the vent is a linear acoustic fluid and that the water in the pool is likewise linear acoustic fluid, that the boundary structure is linearly

BWH

elastic. That gave us a chance to go into the second step —

(Slide.)

— And develop an analytical model of the 4T system, a linear model, which is shown in this figure.

(Slide.)

It is made of a vent with steam inside, the steam being modeled as one-dimensional acoustic. The water in the suppression pool surrounding the vent modeled as an axial (inaudible). And then also, the supports represented by (inaudible).

With this model at hand, we were then able to find a chugging load exit, which is its source, and this load being now independent of the geometry of the 40 tank would be portable and transferrable to the Mark II, since the thermodynamic conditions during the 4T test were similar in application of conditions expected in the Mark II plant during the chugging effects because we identified some random trends in the data. We performed a statistical analysis of the data and we were able to determine a design level load at the required probability of non-exceedence and confidence level.

Based on that, we could develop the design load specification.

That also included some observed characteristics of steam and water properties, as well as the expected variations in steam and water properties during long-term LOCA effects in a Mark II containment.

In somewhat detail, what we did is explained in this picture.

(Slide.)

We started with the 4T traces and we had the numbers supplied by General Electric. And we used as a measure of the traces the response spectra on these traces.

Any equivalent measuring units could have been used such as the amplitude spectra.

After obtaining the response spectra associated with each of the available traces, we performed a statistical analysis at each frequency and obtained a design level response spectrum at the required probability of non-exceedence and confidence level.

Then with the model that 4T had developed previously and what we learned about the inclusive nature of the chugging load, and roughly about the expected duration of the load, we applied this load at the vent and coupled vent pool tank support system.

It excited the system and computed the response of the system at locations comparable to where the data was recorded in the 4T tank.

At the bottom center, we then obtained these responses, the response spectra of these responses and computed the response spectra and compared the resulting envelope with the design level envelope obtained from the statistical interpretation of the 4T data.

At the time, this envelope was representative, we identified an acceptable conservative load.

ash

The design load we obtained has this configuration. It is impulsive in nature. It is time dependent. The one shown in this particular viewgraph response to a probability of non-exceedence of 50 percent and a confidence level of about — that would correspond to the distribution of mean plus two standard deviations.

It looks like this (indicating).

This is the load at vent exit that would be applied over the interface between the steam and the vent —

DR. CATTON: Is that 50 milliseconds wide?

DR. BEDROSIAN: Yes.

DR. CATTON: That seems awful wide.

DR. BEDROSIAN: We investigated the traces and found out that in view of the (inaudible) of the load, that the variation was between 50 and 60 milliseconds. We picked up this value — maybe it was representative of most of the cases we looked at.

DR. CATTON: What does it do to your conclusions if you decrease that?

DR. BEDROSIAN: No.

DR. CATTON: It doesn't really matter.

DR. BEDROSIAN: For the impulsive load, what really matters is the (inaudible).

DR. CATTON: Not the frequency.

DR. BEDROSIAN: No.

DR. ZUDANS: Where did you apply this load?

DR. BEDROSIAN: Over the steam/water interface at vent exist, over the entire interface.

DR. ZUDANS: In other words, it doesn't have to be flat; it could have been curved?

DR. BEDROSIAN: It is a hemispherical or cylindrical -

DR. ZUDANS: So a 50-millisecond duration is short compared to —

DR. BEDROSIAN: What?

DR. ZUDANS: The 50 milliseconds is very short compared to natural periods involved in the response. It really doesn't matter how short it is.

DR. BEDROSIAN: Indeed, for T tank, we used short impulse loads and longer impulse loads. And the responses we obtained were (inaudible).

DR. CATTON: Your amplitude also appears to be low.

DR. BEDROSIAN: It is proportional with the total energy imparted in the system. If you use a shorter impulse, you might have to use a larger amplitude.

It is not the amplitude which --

DR. CATTON: I cannot mentally integrate your curve with the data.

DR. BEDROSIAN: I am suggesting that if we use a shorter duration, you may need larger amplitude.

In answer to the second question, which was to

gsh

transfer this source load to a Mark II system, which is a multivent geometry, we basically followed the process pictured in this picture.

(Slide.)

We started with the traces, the statistical interpretation, the source load in the single vent 4T test facility and plotted that load at all of the vent exits in a multivent Mark II geometry and excited this geometry with the steam in the vent and water in the suppression pool and the containing elastic structure and obtained the responses of the complex.

To define the loading conditions for the Mark II, we, based on engineering judgment and what we expected to happen at the tail end of the LOCA, we devised what we labelled to be a mainly symmetrical loading condition. This loading condition assigns the source load at the same intensity concurrently at all vent ends at the same time.

And additionally, because the design was likely to see some unbalance as a result of the response and because we expect that towards the end of the LOCA, there may be some non-conformities within this system, we assigned it three stronger sources at three radially located vent exits. These stronger sources we estimated based on engineering judgment, and were conservatively to account for the expected non-symmetries.

(Slide.)

In addition, we devised a mainly non-symmetrical loading condition and this assumes variation in the intensity of the loads at the vent exits as shown in this picture on the average, which means it is the center of the system of the downcomers at mean intensity and linearily varying between the two extremes along the large diameter from mean plus or minus one standard deviation.

The idea was to try to, again, give the designers the tools which enabled them to account for some horizontal response in addition to the main vertical response, and to account in some sense for the probable nominal non-symmetries at the tail end of the LOCA in a Mark II plant.

We note at that time probably the conditions in the drywell and in the pool are quite uniform. And both systems will see probably nominal nonsymmetries.

And we felt that this would account for this.

(Slide.)

We analyzed the coupling system composed of vents coupled with the pool and the support beams and surrounding structures.

I would like to show you some of the typical results we obtained. This is the response of the reactor building at reactor pressure vessel support level. It is the horizontal response and is expressed in terms of slow

POOR ORIGINAL

response spectra, which means it is the response of a single system if it would be located at that location and it is an acceleration response spectrum.

The units are in Gs and it is given for this set of (inaudible). And it appears to be reasonably and is expected limiting.

(Slide.)

I would like to show you some of the responses we calculated on the boundary of the welded portion of our containment structure. And I would like to note that our containment structure is made of steel. It is a steel shell. And this is, again, it is expressed in terms of response spectra. It is an acceleration response spectra and it is at the location where the containment structure sees the maximum response.

This is about at half pool depth. Some of these responses may appear to be more significant.

I might explain why such larger volumes occur. The way we see it is if you impart to a system a load within an acoustic fluid which is practically non-compressible, the boundary will see that flow.

In our case, the containment bounardy is a thin shell and it is physically separated by the rest of the reactor building by a physical gap.

So the boundary will see this load in the thin shell.

It will penetrate not too far into the shell. And as we go away from the parameter and into the adjacent structural components, we will see less and less of the expectation.

In addition, I would like to note that the rather large values are recorded in this location and that can be explained because of some conservative assumptions we had to make in this Mark II analysis, short of having full scale multivent data.

Those include the way we assign in-phase the forcing signals to all vent exits and did not account for the phasing of the signals between events because of (inaudible), and the fact that the representation as an acoustic fluid of the pool itself transfers the load, acting on the boundary — that is an acoustic fluid which has no damping.

This is why if you look at this picture, one of the coupled containment system frequencies, because of the undamped pool representation and the rather small damping assigned to the containment steel sheel boundary, the response will have a number of cycles.

And in the flow response spectrum, they will amount to (inaudible).

DR. ZUDANS: I have two questions. One question: is this a special case or application of what Mr. Jim Fitch presented, or is it a completely independently derived source function?

DR. BEDROSIAN: It was developed independently. It was finalized in late 1978 before, I think, the other effort.

DR. ZUDANS: We heard a presentation that heard very much like what you have said. We didn't see the forcing function Jim Fitch indicated, but that is beside the point.

How did you get the — I guess in the beginning, you explained how are you going to solve the overall problem. Did you assume that there are point sources distributed throughout the fluid and ignored the physical presence of downcomers as obstructions?

DR. BEDROSIAN: The downcomers would not have presented obstructions.

DR. ZUDANS: You would have homogenous fluid —

DR. BEDROSIAN: They were represented as rigid boundaries in our Mark II containment analyses. The downcomers were present as rigid boundaries.

DR. ZUDANS: You would have extremely complicated geometry.

DR. BEDROSIAN: It is complicated.

DR. ZUDANS: Did you go around different rigid boundaries?

DR. BEDROSIAN: I think we are helped by the distribution of the vents in the WPPSS geometry. It could be done in a similar manner with any other containments.

All of the vents are radially located.

So we either performed the analysis for the radially located and then (inaudible). This was performed in the frequency domain, so it was an uneconomical task.

DR. ZUDANS: It was not a waive propogation type of analysis?

DR. BEDROSIAN: It was a combination of wave propogation in the vents and the suppression pool, that part of the combined system. And it was a linearily elastic dynamic analysis for the remainder of the structure.

DR. ZUDANS: The wave propogation from the end of the downcomer to the wall of containment should see rigid structures.

DR. BEDROSIAN: Yes.

DR. ZUDANS: These are submerged, rigid pieces sitting --

DR. BEDROSIAN: Our analysis accounted for the downcomers as rigid boundaries.

DR. PLESSET: The wavelengths are pretty long, I think, aren't they? They are quite long and I think that you have to keep that in mind.

That applies to your comment about the Pennsylvania Power and Light thing. The wavelengths are long.

Do you agree with that?

MR. ROTH: Yes, we are hoping that is the case.

gsh

DR. PLESSET: I think you can estimate what the wave lengths are going to be.

DR. ROTH: Yes.

(Laughter.)

What are they, 50 feet? 100 feet? Something like that?

DR. ROTH: About 100 feet.

DR. BEDROSIAN: 80 feet.

DR. PLESSET: These are very long wavelength effects.

DR. BEDROSIAN: We don't think the reflection between the vents —

DR. ZUDANS: That was not, in fact, directly accounted for.

DR. BEDROSIAN: It was counted in our analysis.

DR. ZUDANS: It is strictly a three-dimensional analysis.

DR. BEDROSIAN: Yes.

DR. ZUDANS: Is it the same forcing function that Jim Fitch said or not?

DR. BEDROSIAN: I have not seen his forcing function.

DR. FITCH: It is not the same. Certain aspects of it would be the same; namely, the triangular part. The larger triangle is, although ours is a somewhat shorter duration. But probably the most significant difference you would see in looking at it is that since we don't have the event pipes model, our forcing function includes the vent response.

So that tacked onto the triangular portion is a sinusoidal signal.

DR. ZUDANS: It is amazing that you would come up with the same idea independently.

(Laughter.)

DR. PLESSET: It is acoustics and that is well known.

(Laughter.)

DR. PLESSET: Sobon?

DR. SOBON: Apparently, during the break there was some discussions between some of your consultants regarding the multivent test program with CREARE. Perhaps a comment or two might be appropriate at this time to address some of that.

DR. PLESSET: Who will do that?

DR. PATEL: Since the presentation was so interesting to the members, we decided to give it a second try.

(Laughter.)

DR. PLESSET: Everything is interesting to us.

DR. PATEL: There was a question which was raised by, I believe, Dr. Catton, and I think there were a couple of points which I failed to clarify.

First of all, I have a hand-sketched figure here. That is the single vent geometry and this is the corresponding

POOR ORIGINAL

multi-vent geometry.

Now since the geometries were essentially preserved, if all of the vents basically went in phase, the pressure that — for example, you would observe at that point exactly what you would have observed — that you would observe at that point (indicating.)

DR. PLESSET: One would be greater, the loads would be greater?

DR. PATEL: Yes. I would just like to address the question. Since it is lower than one, it shows that the vents were not chugging out of phase, that within a given chug, the vents were chugging just slightly out of phase or enough out of phase that the pool pressure here was lower than the pool pressure there, as expected, because of the vent's chugging in a larger pool.

This is an important point for the methodology which is being used where we take the single vent 4T data and apply it to the Mark II plant.

Here they are taking all of the vents in phase and the data that I presented therefore shows that this assumption is certainly fairly conservative in giving the pressures at the pool boundaries.

DR. PLESSET: Thank you. I can see why Mr. Sobon wanted this clarified.

I hope this will all be clearly described in your report.

DR. PATEL: It will be.

DR. PLESSET: It is now Cliff Anderson's turn to give some comment from NRC.

DR. ANDERSON: The staff and our consultants have been reviewing these programs, essentially every one of these programs that were presented for the long-term program. We are keeping on top of this. The programs are not complete at this point and we have not completed our evaluations.

The process of the meetings that we have had -- and we have had something like two meetings for each -- on each one of these topics with the Mark II owners, and also plant-unique meetings discussing the Susquehanna program and Dr. Bedrosian's improved chugging load specifications. We have identified some areas that we wanted to see addressed. And what I wanted to do now is just touch on some of those significant comments that we have already made to the Mark II owners for three of the areas: condensation oscillation tests, and the creari tests, and the improved chug specification.

I am not going to try to separate out the two condensation oscillation programs, and I will try to separate out the improved chugging specifications. I will lump some of these together.

(Slide.)

The first one is the condensation oscillation test, again including both the 4T CO test and the KWU test being conducted for Susquehanna.

The first comment is, we did observe, the staff has observed, as a result of the tests conducted in the FFTS facility that the highest loads were observed under conditions of high total mass flux — it should be total mass flux — and under conditions of low air content. Recognizing this, we want to make sure that the test matrix for both of these tests would bracket the values for the plants. They are conservative values for total mass flux and air content.

We have reviewed the test matrix for the 4T CO tests and they have provided a comparison of the anticipated values of total mass flux and air content against the calculated values for the Mark II plants, and we have convinced ourselves that they have addressed this to our satisfaction.

The second comment is with regard to the potential for data scatter, and we want to make sure that once they have identified what the limiting conditions are as they go through these tests, that they should then reserve enough open slots to run replicate tests, so we can get a better definition of the load. The next, recognizing the importance of air content, we feel it is important that they do have proper type of instrumentation and measuring techniques to know what the air content is in the test. We have convinced ourselves, in looking at the type of things that are being done in both of these test facilities, that they are giving considerable amount of attention to this, and they should be able to address this one properly.

The last comment: In the case of the Susquehanna tests, they are making an attempt to measure lateral loads. We had not heard of any intention to measure lateral loads in the case of the 4T CO test. Our concern is that in the case of the FFTS facility, the highest lateral loads were observed in the conducting of the CO test, and we have conveyed the concern to the Mark II owners that they should give this attention, should have proper instrumentation to measure lateral loads in that facility, to confirm the conservative nature of our current specifications.

The next, the comments with regard to the Creari multi-vent test.

(Slide.)

The function of these tests in the Mark II program has changed back and forth some over the conduct of these tests. The original purpose was simply to show that a multiplier, multi-vent multiplier of less than one was

mte

In other words, using the single-vent full-scale full-scale 4T data was adequate. And there was some thought about trying to quantify that multiplier and use it in conjunction with the multi-vent hydrodynamic model.

That effort does not appear to be -- they are not doing that at that time -- at this time -- so, with the understanding that the primary purpose of these tests is to show a multi-vent multiplier of less than one, our review of this data at this point does indicate that this is correct. However, we feel there is some value in taking a pretty good hard look at a lot of this data, if there is a lot of data there, and being able to separate out some of these multi-vent effects.

There are competing things that are happening there, including -- there are FSI effects that are unique to those facilities. It is our understanding that there will be an effort -- I am not sure if it is NMSS -- will be looking at and doing studies of this facility to be able to separate out the different effects that are occurring, so that we -- there is a better handle on the margin associated with multi-vent effects.

And then finally, we believe that, again, there are a lot of things that could be done with vent data, and that data should be studied carefully for determination of how it could be used in supporting some of the assumptions in other long-term program efforts. In particular, the one we are talking about here is the improved chugging load specifications, and I will talk about that in the next slide.

(Slide.)

And this is with regard to the improved chug specifications. The first comment here, investigate 4T high-frequency response. We did observe that there was some high-frequency response in the 4T measurements, and also, when one uses these 4T measurements to come up with a source, there was observed some high-frequency response. The cause of that is not completely apparent at this time. It could be anything from some part of it due to instrumentation in the case of one of the approaches, where there might be some numerical questions involved with the way that they come up with the amplified response spectra that is used to establish the source.

We feel they should look at this to understand what is causing this, and if it is real, then it should be included as a part of the source specification.

The next one is, as you heard, two different approaches with a lot of similarities. One thing we might note is that one of the models that is to be applied to the Mark II plant is somewhat simpler than the other, in that the Bechtel approach does use a closed solution of the

Nauvier-Stokes equation with a number of dials. That is to be checked by some detailed Nastran calculation.

Our concern here is that these assumptions would be verified, and they are doing something along this line, both in the study of the 4T facility and in the calculations of the Mark II plant.

The next comment here is, the concern has always been that one would be able to come up with a source that is free of 4T signature. Both of these methodologies rely pretty heavily on data from the 4T facility. The total methodology is not just the analytical model. It also includes taking a look at the 137 chugs from the 4T facility.

We think there is merit in applying this methodology to do some calculations of other tests.

And then, finally -- this is the one that relates back to the comment that I made on the Creari facility. We believe that they should take a look at the available multi-vent test data that they have. This is with regard to how they establish that source.

Our concern here, again, as I mentioned before, is that there is a potential for a number of large chugs occurring at the same time and your not having a mixture of large chugs, small chugs, et cetera. We believe by looking at the multi-vent test data, one can get somewhat of an

mte

idea as to whether a number of large chugs occur at the same time. As a part of our evaluation of these efforts, there is a research program with Livermore. They have developed a code, the PELE IC code, that is comparable to K-FIX. They are doing calculations of the same type that you heard described here, where you heard Dr. Bedrosian's approach and Jim Fitch's approach.

We are using that program to help us in the — to help us assess any of these models and the assumptions that are implicit in there, and also in evaluating some of the different sources that you have heard here. And possibly we will be doing some more work with it and looking at some other test data that is available.

That concludes our comments.

DR. PLESSET: Thank you, Cliff. Let me see if any of the Committee members or consultants have comments on your comments.

(No response.)

DR. PLESSET: I guess not. Let me ask you a question for clarification. I am pretty sure I know the answer. The Japanese data is not generally available to the Mark II owners group, is that correct?

DR. ANDERSON: Right.

DR. PLESSET: And will not be, presumably?

DR. ANDERSON: We don't know. As far as we know right now, it will not be.

DR. PLESSET: They will be inscrutable.

(Laughter.)

DR. SOBON: We have been working to try and obtain the data. The point is, though, that it is the timeliness and the form in which it would be provided. At the moment it appears as though the reports would have to be finalized, and that there would be more or less some reports which are of some value, but of course the raw data is much better. And that we think is not going to happen, at least as the scheme of things is moving now, until after we likely would get data from the 4T test facility.

DR. PLESSET: They showed me the facility. There is some question about whether they would or not. But they finally agreed. I don't know why they are very protective of it. It is an impressive facility, I will say that.

Now, let me ask you one other question. Did the GKM data — that will not be generally available?

DR. ANDERSON: That is our understanding. GKS or GKM?

DR. PLESSET: GKSS is what I should have said.

DR. ANDERSON: As far as we see now, that is in the same classification as the Japanese test. And yes, it is not generally available.

DR. SOBON: The detailed data will not be available.

DR. KUDRICK: Summary reports and evaluations will be available.

DR. PLESSET: And the time scale for that, would that be helpful, or will it be a little bit slow?

DR. KUDRICK: I am not aware of the current schedule for those summary reports. I would imagine that before the end of the year, they would be available.

DR. ANDERSON: If you see the test schedule going over period of a year and a half, starting now, and while we will be getting some test reports as we go through this program -- the Mark II owners' long-term program is to be completed in 1980. So I would say generally it is not going to be available on a time frame consistent with what we are trying to do right now.

DR. PLESSET: Thank you.

DR. ZUDANS: You mentioned a third method is being developed.

DR. ANDERSON: It is not exactly a method, but a way of checking some of these methods.

DR. ZUDANS: The method that Livermore is doing, they --

DR. ANDERSON: PELE IC -- it is equivalent to the K-FIX, and it is a very rigorous treatment of this. It is more rigorous than the Nastran calculations. There are

mte

some potentials that we may do some less rigorous calculations that are comparable. But no, it is not 3-D. It would be for 4T, which is —

DR. ZUDANS: Is it aimed at generating the forcing function, taking boundary conditions away?

DR. ANDERSON: We are not trying to take the library and then work out of that source. We are trying to take — first of all, the sources that have been provided, and do some sensitivity study with those sources and see what happens on the boundary of the 4T facility. That is one thing.

Another thing is to look at the assumptions involved in the modeling of the two approaches and check out those with an independent model.

DR. PLESSET: Thank you.

Now we have to go into closed session because of proprietary material. But in order not to have you wait and come back to the open session, which would just consist of some general discussion, I am suggesting that we adjourn at the end of the closed session, so that those who are not going to be here for the closed session could leave now and not have to come back. They might not want to come back anyway.

(Laughter.)

What I propose is that we now go into closed

PWHmte

session, and that session will adjourn — we will adjourn at the end of that closed session. We we will take just a minute or two to do that. I don't think we need to break.

(Whereupon, at 4:30 p.m., the proceeding was adjourned.)

T-1
CR6841

Introduction Slides

1. Purpose of Meeting
2. Mark II Meeting Agenda
3. Mark II Facility Schedules
4. Typical Mark II Containment
5. Chronology of Primary LOCA Loads
6. LOCA Sequence of Events
7. Alternate Lead Plant Loads
8. Downcomer Design
9. Mark II Program Completion Estimate
10. Plant Unique Programs
11. Primary LTP Tasks
12. Related Foreign Tests

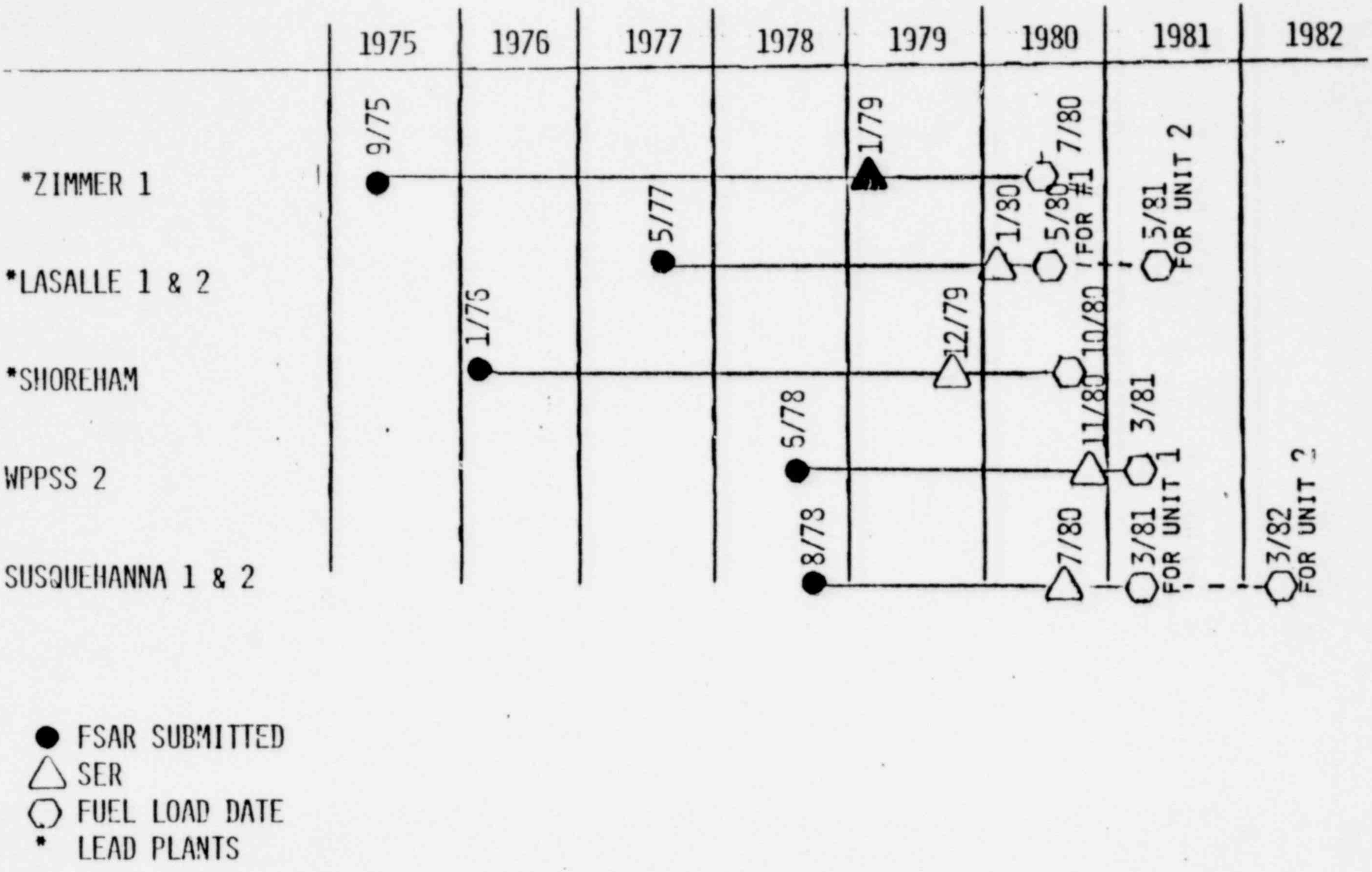
PURPOSE OF MEETING

- ALTERNATE LPP LOADS
- LEAD PLANT DOWNCOMER SUPPORT
- LONG TERM PROGRAM STATUS
- RELATED FOREIGN TESTS

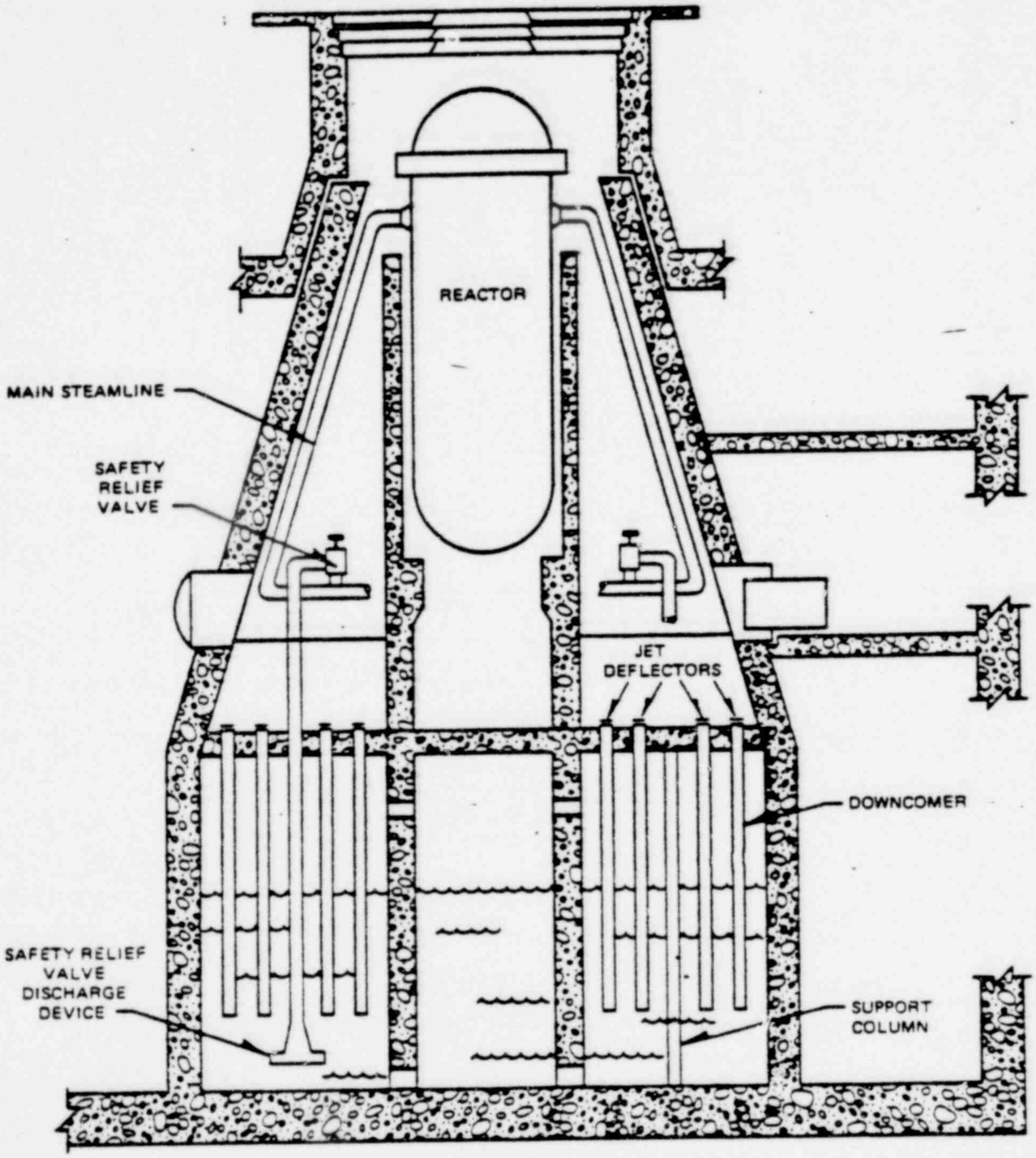
MARK II ACRS
MEETING AGENDA
SEPTEMBER 13, 1979

I.	INTRODUCTION	15 MIN.
II.	LEAD PLANT LOADS	
	A. LOCA LOADS	30 MIN.
	B. SRV LOADS	15 MIN.
	C. SUBMERGED STRUCTURE LOADS	20 MIN.
	D. DOWNCOMER SUPPORT	60 MIN.
III.	LONG TERM PROGRAM	
	A. GENERIC SUPPORTING PROGRAM	150 MIN.
	B. RELATED PLANT UNIQUE PROGRAMS	35 MIN.
	C. STAFF COMMENTS ON LTP	15 MIN.
	D. RELATED FOREIGN TESTS	30 MIN.
IV.	SUMMARY STATEMENTS	10 MIN.

MARK II FACILITY SCHEDULES



1031 146



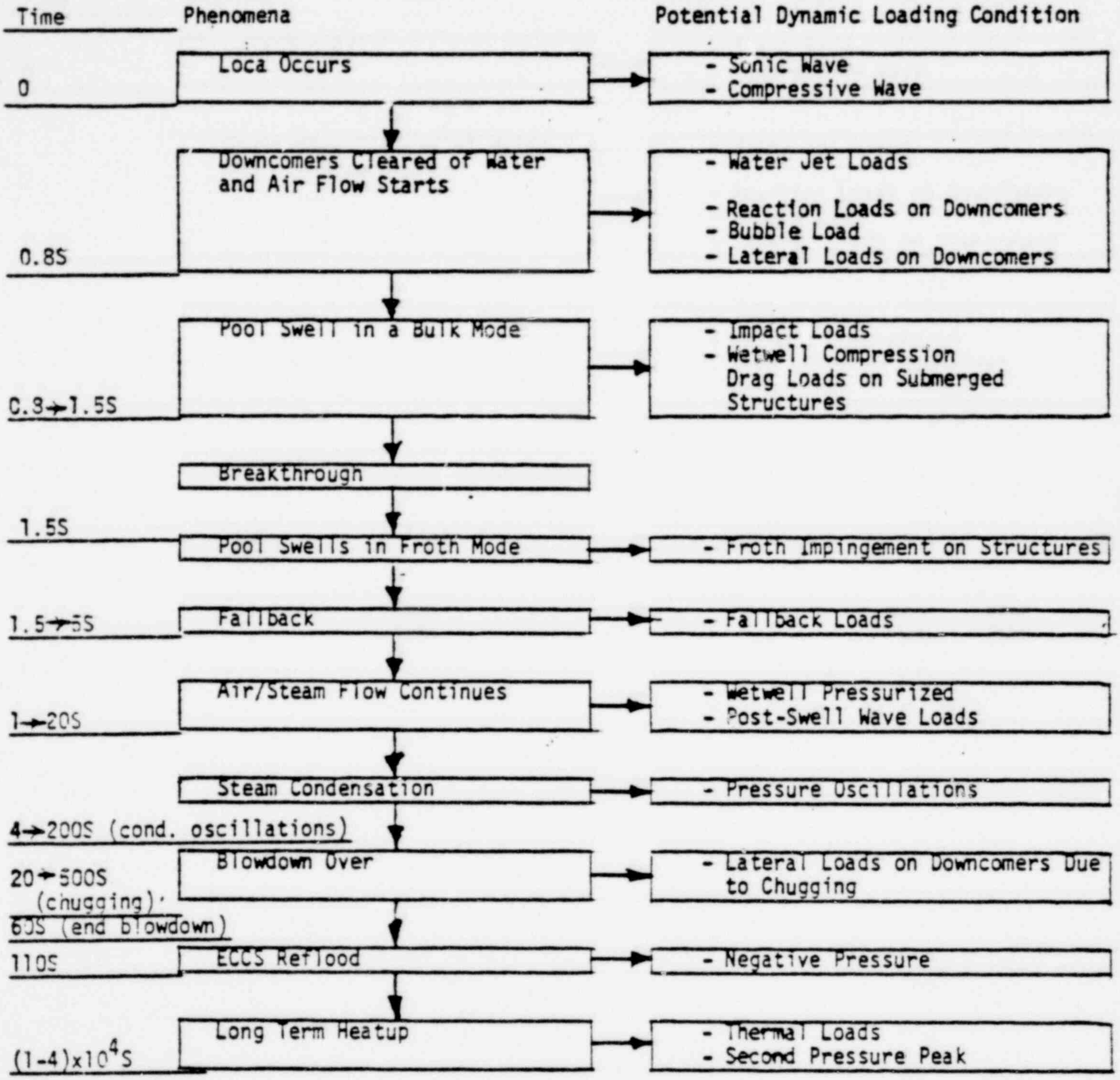
Typical Mark II Pressure Suppression Containment

POOR ORIGINAL

CHRONOLOGY OF PRIMARY LOCA LOADS

- VENT CLEARING
    - JET LOADS ON BASE MAT
- AIR BUBBLE FORMATION
    - DRAG LOADS ON SUBMERGED COMPONENTS
- POOL SWELL
    - AIR BUBBLE PRESSURE LOAD ON SUBMERGED BOUNDARY
    - DRAG LOADS ON SUBMERGED COMPONENTS
    - IMPACT LOADS ON WETWELL COMPONENTS
    - WETWELL AIR COMPRESSION LOADS ON BOUNDARY
    - UPWARD DIAPHRAGM LOADS
- POOL FALLBACK
    - DRAG LOADS ON SUBMERGED COMPONENTS
- STEAM BLOWDOWN AND CONDENSATION
    - DOWNCOMER LATERAL LOADS
    - PRESSURE LOADS ON SUBMERGED BOUNDARY
    - DRAG LOADS ON SUBMERGED COMPONENTS

LOCA Sequence of Events



Peak drywell and wetwell pressure 50S

Maximum diaphragm Δ P down 0.7S

Maximum diaphragm Δ P up 2.0S

ALTERNATE LEAD PLANT LOADS

LOCA

- SUBMERGED BOUNDARY
- POOL SWELL ELEVATION & WETWELL AIR COMPRESSION
- ASYMMETRIC POOL SWELL

SRV

- ALL VALVE LOAD CASE 5

SUBMERGED STRUCTURE DRAG

- MODIFIED DRAG COEFFICIENTS (UNSTEADY FLOW AND INTERFERENCE EFFECTS)
- LIFT DUE TO VORTEX SHEDDING
- STRUCTURAL NODALIZATION

DOWNCOMER DESIGN

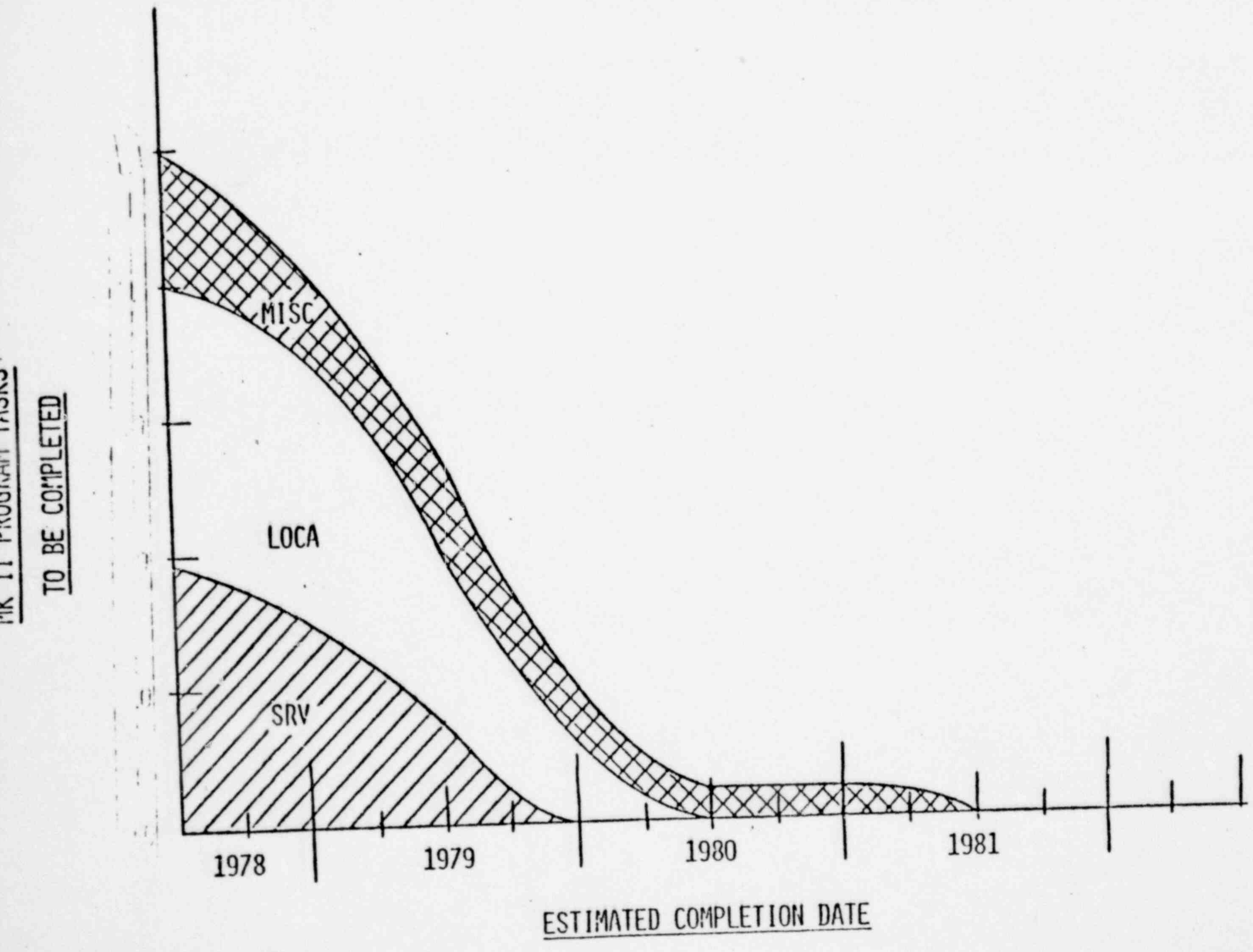
UNBRACED DOWNCOMER DESIGN

SUBMERGED STRUCTURE DRAG LOADS (CO AND SRV AIR BUBBLE)

REFINED SUBMERGED DRAG LOADS

REDESIGN DOWNCOMER SUPPORTS

LATERAL LOAD CHANGES


MK II PROGRAM TASKS
TO BE COMPLETED
MISC
LOCA
SRV
1978
1979
1980
1981
ESTIMATED COMPLETION DATE

PLANT UNIQUE PROGRAMS

	BAILLY	HANFORD	LIMERICK	NINE MILE PT.	SUSQUEHANNA
LOCA					
- VENT CLEARING	X			X	
- POOL SWELL		X			
- C. O. & CHUGGING		X			X
SRV					
- TEMPERATURE LIMIT		X			
- AIR CLEARING				X	
- TIE DOWN	X			X	
SUBMERGED STRUCTURES					
- JET					
- AIR BUBBLE	X	X	X		X
- STEAM COND.		X	X	X	X

PRIMARY LTP
TASKS

GENERIC

- 4T CO TESTS
- CREARE MULTIVENT TESTS
- IMPROVED CHUG LOAD
- CAORSO SRV TESTS

PLANT UNIQUE

- GKM II CO TESTS
- WPPS-2 IMPROVED CHUG LOAD

RELATED FOREIGN TESTS

JAERI MULTIVENT FULL SCALE TESTS

- 1/18 SECTOR, 7 VENTS, MARK II PROTOTYPICAL
- PRELIMINARY RESULTS

  POOL SWELL
  CO AND CHUG LOADS

GKSS TESTS

- 3 LARGE VENTS
- 3 SHAKEDOWN TESTS COMPLETE

T.21
3

Alternate LOCA Load Slides

1. Alternate LOCA Load Summary
2. Original Vent Clearing Load
3. Revised Vent Clearing Load
4. Original DFFR Pool Swell Load
5. NRC Acceptance Criteria Pool Swell Load
6. Alternate Methodology Pool Swell Loads
7. Pool Swell Criteria Evaluation
8. Comparison of Phase I Measured and Calculated Pool Swell Height
9. Comparison of Phase II Measured and Calculated Pool Swell Height
10. Original Asymmetric Pool Boundary Load.
11. Revised Asymmetric Pool Boundary Load

ALTERNATE LOCA LOAD
SUMMARY

POOL BOUNDARY LOADS

POOL SWELL ELEVATION AND WETWELL AIR COMPRESSION

ASYMMETRIC POOL SWELL

ORIGINAL
VENT CLEARING LOAD

ORIGIN

WATER CLEARING INDUCED PRESSURE ON POOL BOUNDARY

ORIGINAL MARK II SPECIFICATION

BASEMAT - 33 PSI OVERPRESSURE

BASIS

JET IMPINGEMENT ON BASEMAT
MAXIMUM VENT CLEARING VELOCITY
TOTAL MOMENTUM TRANSFER

NUREG 0487 CRITERIA

INCLUDE 33 PSI OVERPRESSURE AT WALLS

REVISED VENT CLEARING LOAD

ALTERNATE SPECIFICATION

- BASEMAT AND WETWELL WALLS BELOW VENTS

  24 PSI OVER LOCAL HYDROSTATIC PRESSURE

- LINEARLY ATTENUATE TO ZERO AT POOL SURFACE

BASIS

MARK II OWNERS

- 20 PSI IS 4T BOUND OF 20 TESTS
- INCREASE BY 4 PSI FOR MAXIMUM MK II DRYWELL PRESSURIZATION

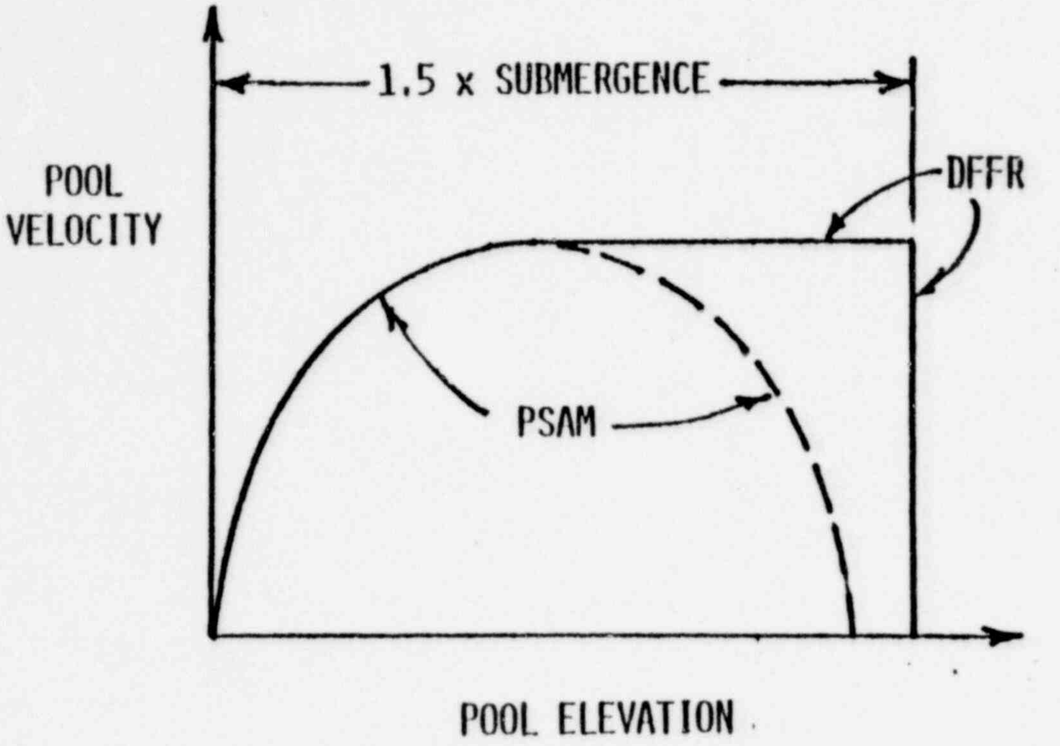
NRC

- LEAST SQUARES FIT OF 4T AND MARVIKEN DATA

$$\Delta P \text{ vs } \frac{\dot{M}HL}{A_p/A_V}$$

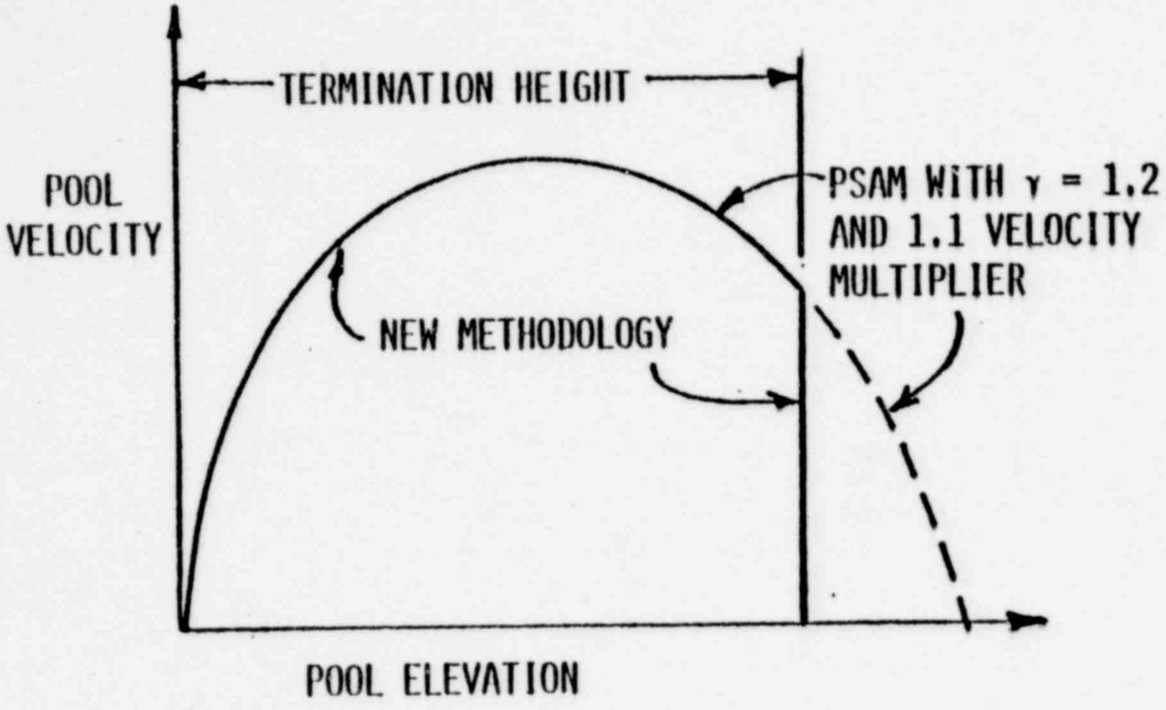
- 99-99 NON-EXCEEDANCE
  CONFIDENCE LIMIT

ORIGINAL DFFR METHODOLOGY



- MAXIMUM POOL SWELL UNDERPREDICTED IN TWO CASES
- LARGE UNCERTAINTY IN POOL SWELL ELEVATION

MARK II OWNERS GROUP ALTERNATE METHODOLOGY



- TERMINATION HEIGHT DEFINED BY ΔPUP SPECIFICATION AS DETERMINED FROM NRC ACCEPTANCE CRITERIA
- TERMINATION HEIGHT NO LESS THAN 1.5 x SUBMERGENCE
- MAXIMUM WETWELL AIRSPACE PRESSURE BASED ON TERMINATION HEIGHT

POOL SWELL CRITERIA
EVALUATION

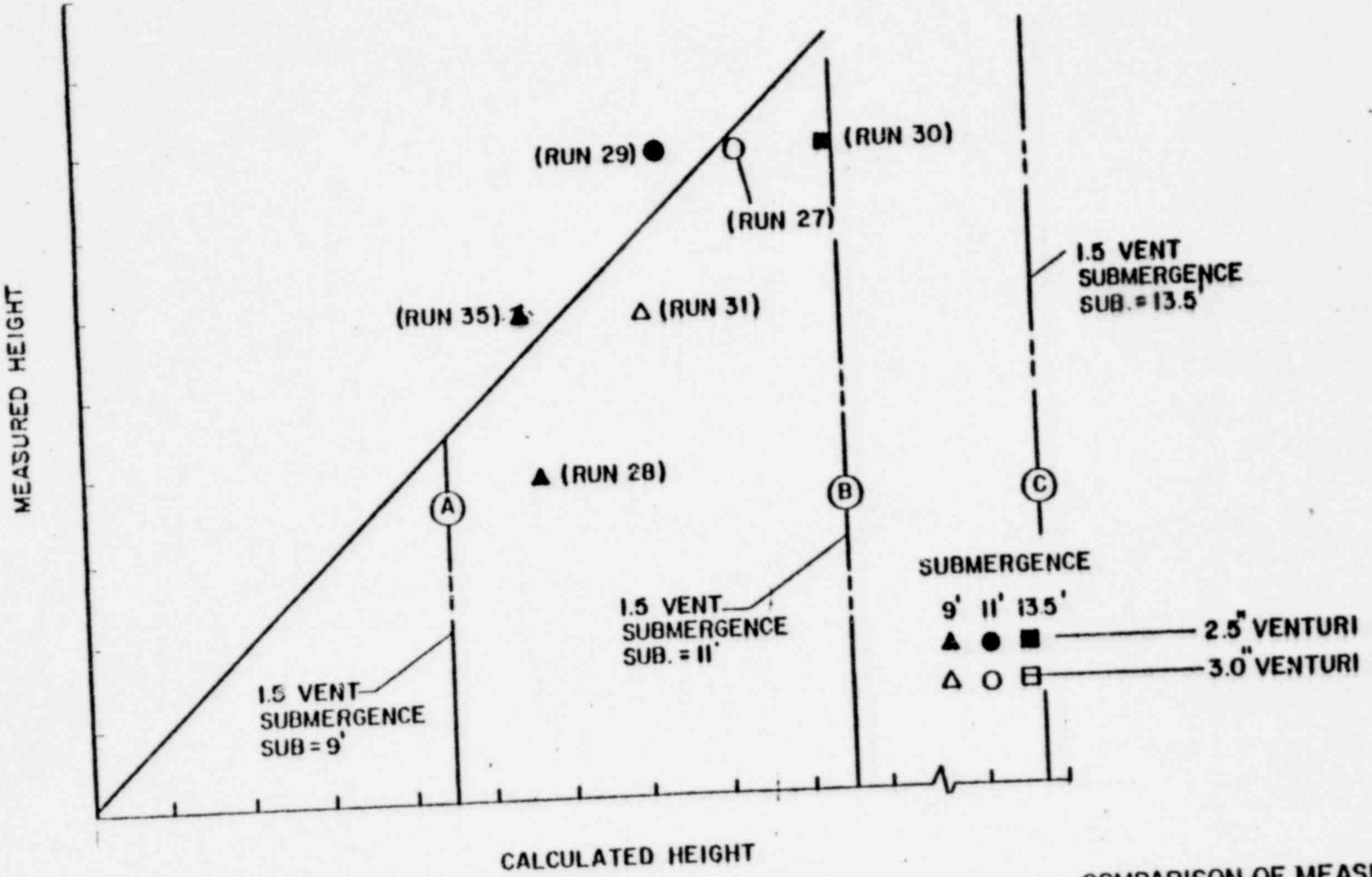
- CALCULATED SWELL HEIGHTS FOR SELECTED PHASE I AND II TESTS SHOW METHODOLOGY CONSERVATISM

- RUN 35 EXCEEDS CALCULATION BY 6 INCHES

- METHODOLOGY CONSERVATISMS
    - 4 T CONSERVATISMS
    - NEDM-10320 CONSERVATISMS
    - LEVEL SENSOR FROTH DETECTION
    - DBA SATURATED LIQUID BREAK

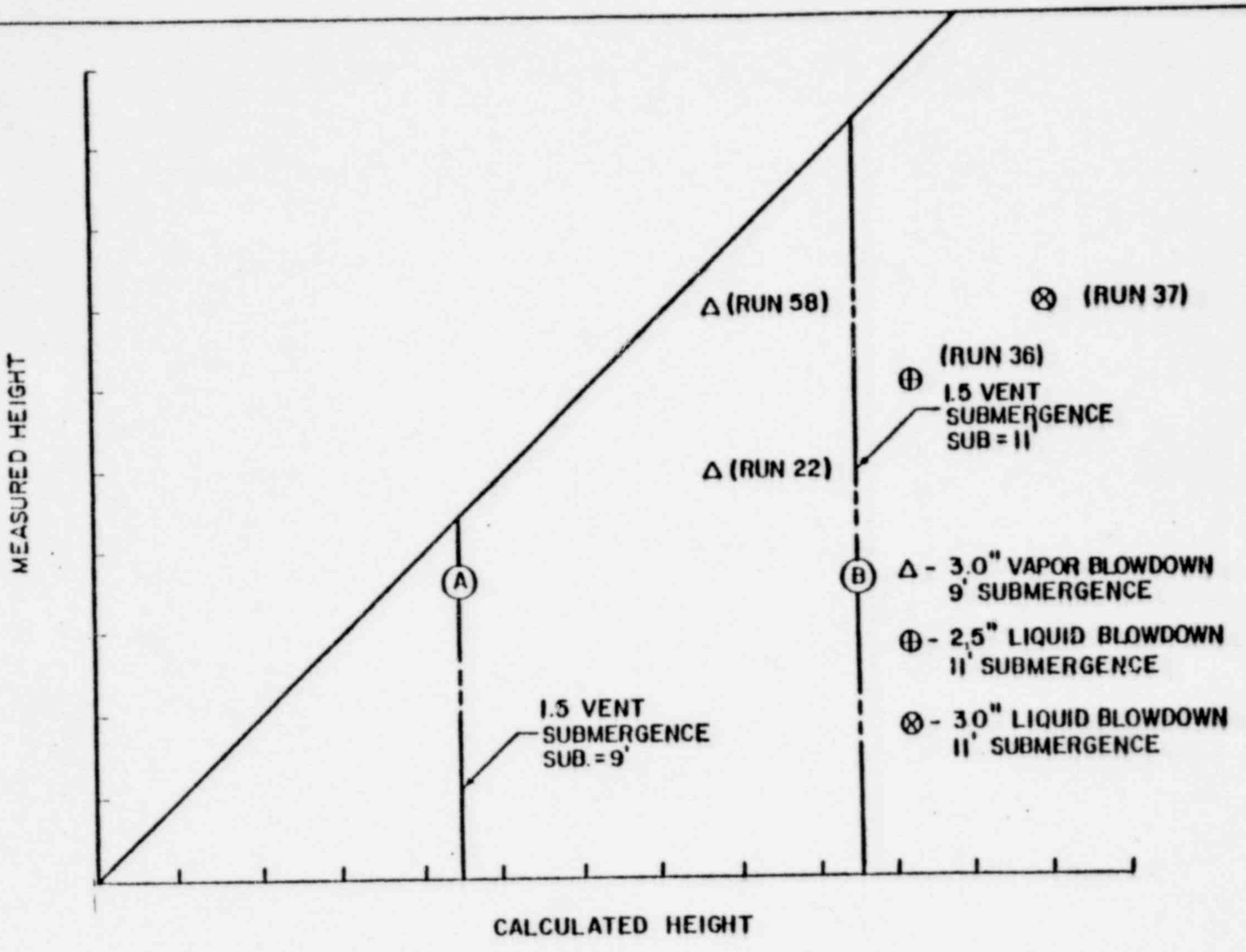
- METHODOLOGY LIMITATION -

  INCLUDE CONSERVATISMS OF NEDM-10320 FOR DRYWELL PRESSURE RESPONSE



COMPARISON OF MEASURED AND CALCULATED SWELL HEIGHT 4T PHASE I

1031 163



COMPARISON OF MEASURED AND
CALCULATED SWELL HEIGHT
4T PHASE II

ORIGINAL
ASYMMETRIC POOL
BOUNDARY LOAD

ORIGIN

CIRCUMFERENTIAL VARIATIONS IN VENT AIR/STEAM MIXTURE RESULT IN ASYMMETRIC BUBBLE PRESSURE LOAD ON BOUNDARY

NUREG 0487 CRITERIA

- ALL AIR VENTED ON ONE HALF OF CONTAINMENT AND STEAM ON OTHER HALF
- MAXIMUM PSAM VENT CLEARING AIR BUBBLE PRESSURE ONE HALF OF CONTAINMENT AND ZERO PRESSURE OTHER HALF

REVISED ASYMMETRIC POOL BOUNDARY LOAD

REVISED SPECIFICATION

20% MAXIMUM VENT CLEARING AIR BUBBLE PRESSURE ONE HALF OF CONTAINMENT AND ZERO PRESSURE OTHER HALF

BASIS

- CALCULATIONS

  MODEL BUBBLE PRESSURE DIFFERENCE IN 2 VENTS INITIALLY FILLED WITH AIR

  - NEAR VENT IMMEDIATELY SUPPLIED WITH HOMOGENEOUS STEAM/AIR MIXTURE
  - FAR VENT SUPPLIED WITH AIR FOR 0.4 SECONDS
  - AIR/STEAM FRONT VELOCITY FROM BATTELLE TESTS
  - OTHER CONSERVATIVE ASSUMPTIONS-LOW BREAK ELEVATION, SHORT VENT CLEARING TIME AND INSTANT STEAM CONDENSATION
  - CALCULATE ΔP = 8 PSI WITH 65% STEAM MIXTURE IN ONE VENT AND ALL AIR IN OTHER VENT
- ARGUMENTS FOR LOW STEAM/AIR VARIATIONS IN THE DRYWELL
  - HIGHLY TURBULENT FLOW
  - DRYWELL STRUCTURE AID IN MIXING
  - MARVIKEN AND BATTELLE TEST DATA INDICATE GOOD MIXING

OT-3

MARK II LEAD PLANT

SRV LOADS ACCEPTANCE CRITERIA
NUREG-0487 OPEN ITEM

BACKGROUND: MARK II LEAD PLANTS ACCEPT NUREG-0487 SRV LOADS CRITERIA EXCEPT LOAD CASE 5.

LOAD CASE 5 (NUREG-0487):

(1) ALL VALVES DISCHARGE SIMULTANEOUSLY ASSUMING ALL BUBBLES OSCILLATE IN-PHASE;

(2) PRESSURE AMPLITUDES OF EACH BUBBLE SHALL BE PREDICTED BY RAMSHEAD MODELS DESCRIBED IN DFFR REV. 2;

(3) PRESSURE AMPLITUDES DUE TO MULTIPLE BUBBLES SHALL BE ADDED BY ABSOLUTE SUM;

(4) A RANGE OF BUBBLE FREQUENCY OF 4 TO 12 Hz SHALL BE EVALUATED FOR STRUCTURAL, PIPING AND EQUIPMENT RESPONSE.

MARK II LEAD PLANTS DESIGN BASIS AND PROPOSED ALTERNATIVES

I. CURRENT DESIGN BASIS

- RAMSHEAD LOADS AND BUBBLE FREQUENCY PREDICTED BY DFFR MODEL;
- BUBBLE PHASING DUE TO SRV SET-POINT, SRV LINE LENGTH;
- VARIOUS ALL SRVS CASES HAVE BEEN EVALUATED;
- DESIGN CASE SELECTED ON THE BASIS OF STRUCTURAL CHARACTERISTIC,

II. PROPOSED ALTERNATIVES

- RE-EVALUATE CURRENT DESIGN BY USING T-QUENCHER LOADS AND BUBBLE FREQUENCY AS A RESULT OF KWU TESTS;
- ALL BUBBLES IN-PHASE;
- MEET THE INTENT OF NUREG CRITERIA.

STAFF'S EVALUATION OF LEAD
PLANT PROPOSED ALTERNATIVE

- PROPOSED ALTERNATIVE COMPLIES WITH NUREG CRITERIA EXCEPT THE FOLLOWING.

NUREG	PROPOSED ALTERNATIVE	STAFF'S EVALUATION
PRESSURE AMPLITUDE BASED ON RAMSHEAD	PRESSURE AMPLITUDE BASED ON T-QUENCHER	ACCEPTABLE. ALL LEAD PLANTS HAVE COMMITTED TO T-QUENCHER. PRESSURE AMPLITUDE USED FOR RE-EVALUATION OF PLANT DESIGN IS SUPPORTED BY KWU T-QUENCHER TESTS.
FREQUENCY RANGE 4 - 12 Hz	3 - 9 Hz	ACCEPTABLE. PROPOSED FREQUENCY RANGE SUPPORTED BY KWU T-QUENCHER TESTS

STATUS OF LEAD PLANTS
EVALUATION BY USING T-QUENCHER
LOAD

MAJOR STRUCTURES	PIPING	EQUIPMENT
SUBSTANTIAL MARGIN HAS BEEN DEMONSTRATED	- EVALUATION ON CRITICAL PIPING SYSTEMS HAS BEEN COMPLETED. RESULTS SHOW CURRENT DESIGN ADEQUATE. - SHOREHAM HAS DOCUMENTED THE EVALUATION RESULT - LASALLE AND ZIMMER HAD PRESENTED THEIR EVALUATION RESULTS ON JULY MEETING. EVALUATION RESULTS WILL BE DOCUMENTED BY 3RD Q OF 1979.	- EVALUATION IS UNDERWAY. - PRELIMINARY ASSESSMENT SHOWS CURRENT DESIGN ADEQUATE - COMPLETION DATE TO BE ESTABLISHED.

T-5

MARK II LEAD PLANTS

SUBMERGED STRUCTURE DRAG LOADS

- INITIAL LOAD SPECIFICATION IN NEDO-21061 SEPT 1975 (DFFR)
  - EXPANDED TO INCLUDE INERTIAL DRAG
  - EXPANDED TO INCLUDE LOCAL FLOW FIELD EFFECTS
- DESIGN ASSESSMENT REPORT (DAR) 1ST QRTR 1976
  - STRUCTURAL ASSESSMENT FOR DFFR LOADS
  - SRV RAMSHEAD DESIGN BASIS
- DESIGN ASSESSMENT CLOSURE REPORT 3RD QRTR 1978
  - STRUCTURAL ASSESSMENT FOR DFFR - 2 LOADS
  - METHODS FOR PREDICTING LOADS JUSTIFIED
  - RESULTS OF LOADS ON STRUCTURES PROVIDED IN DAR AMENDMENT
- ASSESSMENT CRITERIA SATISFIED ON ALL STRUCTURES

MARK II LEAD PLANTS

SUBMERGED STRUCTURE DRAG LOADS (CONT'D)

- RECENT RESULTS AND DEVELOPMENTS
  - NRC ACCEPTANCE CRITERIA (SEPT 1978)
  - KWU T-QUENCHER FOR SRV DISCHARGE (DEC 1978)
  - LOCA/STEAM CONDENSATION DRAG (JUNE 1978)

- NRC ACCEPTANCE CRITERIA ADDRESSED
  - UNSTEADY FLOW EFFECTS ON DRAG AND LIFT
  - INTERFERENCE EFFECTS
  - NON-UNIFORM FLOW FIELD

- KWU T-QUENCHER FOR SRV DISCHARGES
  - RELOCATION OF SRV LINES
  - BUBBLE PRESSURE DECREASES
  - BUBBLE FREQUENCY DECREASES

- LOCA/STEAM CONDENSATION DRAG
  - WATER JET/VENT CLEARING
  - CHUGGING
  - CONDENSATION OSCILLATION
    - ∘ LOW FREQUENCY
    - ∘ LOW MAGNITUDE

MARK II LEAD PLANTS

SUBMERGED STRUCTURE DRAG LOADS (CONT'D)

- CURRENT STATUS SUMMARY
  - LOAD DEFINITION CRITERIA
    - (1) DESIGN ASSESSMENT CLOSURE REPORT METHODS
    - (2) NRC ACCEPTANCE CRITERIA
  - DOWNCOMER RESTRAINT SYSTEM

- FUTURE PROJECTIONS
  - REALISTIC LOAD DEFINITION
    - (1) LOWER BUBBLE PRESSURE
    - (2) NARROW FREQUENCY RANGE
    - (3) ENERGY DISSIPATION AND ATTENTUATION
  - DOWNCOMER RESTRAINT MAY NOT BE REQUIRED

4
T-42 6

LOCA/SRV SUBMERGED STRUCTURE LOADS

- ORIGIN OF LOADS
- HISTORY OF LOAD SPECIFICATION - LEAD PLANTS
- INITIAL OWNERS METHODOLOGY
- NRC ACCEPTANCE CRITERIA
- OWNERS' RESPONSE
- NRC SUPPLEMENT TO ACCEPTANCE CRITERIA

- TECHNICAL BASIS FOR:
  - OWNERS' METHODOLOGY
  - NRC ACCEPTANCE CRITERIA
  - ACCEPTANCE CRITERIA SUPPLEMENT

INITIAL MARK II OWNERS' LOAD SPECIFICATION

- WATER JET LOADS
  - QUASI ONE-DIMENSIONAL MODEL
  - NEGLIGIBLE INDUCED FLOW TRANSIENTS
  - STANDARD DRAG ONLY (STRUCTURES WITHIN JET)
  - MOMENTUM BALANCE (STRUCTURES INTERSECTING THE JET)

- AIR BUBBLE LOADS
  - SPHERICAL SOURCE AND IMAGES
  - ONE-DIMENSIONAL FLOW AFTER COALESCENCE (LOCA)
  - EQUIVALENT UNIFORM FLOW AT GEOMETRIC CENTER OF STRUCTURE
  - ACCELERATION (LOCA) & STANDARD (LOCA/SRV) DRAG
  - NO INTERFERENCE OR BLOCKAGE EFFECTS

- STEAM CONDENSATION LOADS
  - NO GENERIC BASIS PRESENTED

NRC ACCEPTANCE CRITERIA

- DFFR Methodology & Applications Memorandum Subject to Modifications/Additions In:
    - LOCA/Ramshead SRV Jet Loads
    - SRV Quencher Jet Loads

    - LOCA Air Bubble Loads
    - SRV/Ramshead Air Bubble Loads
    - SRV/Quencher Air Bubble Loads

    - Steam Condensation Loads

WATER JET LOADS

- NRC ACCEPTANCE CRITERIA
  - LOCA JET LOADS
    - MODIFY ONE-DIMENSIONAL MODEL
    - INDUCED FLOW AT JET FRONT
    - INCLUDE ACCELERATION DRAG
  - SRV-QUENCHER JET LOADS
    - SPHERE OF INFLUENCE (QUENCHER ARM)

- OWNERS' RESPONSE
  - LOCA JET LOADS
    - ACCEPT NRC CRITERIA OR
    - PLANT UNIQUE (RING VORTEX MODEL)
  - SRV-QUENCHER JET LOADS
    - MODIFY SPHERE OF INFLUENCE TO 5FT CYLINDER

- NRC SUPPLEMENT
  - OWNERS' RESPONSE ACCEPTABLE

NRC CRITERIA - MARK II OWNERS RESPONSE

LOCA AIR BUBBLE DRAG LOADS

NRC MODIFICATION	OWNERS RESPONSE
•• BUBBLE ASYMMETRY (10%)	ACCEPTABLE
•• STANDARD DRAG COEFFICIENT BASED ON ACCELERATING FLOWS	DIFFERENT DATA BASE (UNIFORM ACCELERATION & IMPULSIVE)
•• EQUIVALENT UNIFORM FLOW (AT MAX. VELOCITY NOT GEOMETRIC CENTER)	SENSIVITY ANALYSIS OF USING GEOMETRIC CENTER
•• INTERFERENCE EFFECTS (DETAILED ANALYSIS OR CONSERVATIVE MULTIPLIERS)	DETAILED ANALYSIS BEING PERFORMED
•• BLOCKAGE EFFECTS USE STANDARD "WINDTUNNEL" CORRECTION	ACCEPTABLE

SRV-QUENCHER AIR BUBBLE LOADS

•• AS ABOVE -MODIFIED SOURCE	EFFECTS ABOVE EXCEPT DRAG COEFF. BASED ON OSCILLATING FLOW SOURCE-QUENCHER CORRELATION

CONDENSATION LOADS

•• PLANT UNIQUE	EFFECTS ABOVE WITH OSCILLATORY FLOW 4T DATA BASE

CONCERNS ADDRESSED

I. MODIFIED DRAG COEFFICIENTS

   A. UNSTEADY FLOW

      1. UNSTEADY ACCELERATING FLOW - SARPKAYA AND GARRISON

      2. OSCILLATORY FLOW - SARPKAYA

   B. INTERFERENCE EFFECTS

      1. STANDARD DRAG

         A. DALTON AND SZABO

         B. HORI

         C. ZDRAVKOVICH

      2. ACCELERATION DRAG

         A. DALTON AND HELFINSTEIN

         B. SARPKAYA

         C. YAMAMOTO

         D. YAMOMOTO AND NATH

II. LIFT DUE TO VORTEX SHEDDING

   A. OCCURS ONLY AFTER SEPARATION

   B. PREDICTED BY DURATION OF FLUID FLOW

   C. POTENTIAL TRANSVERSE PERIODIC LOAD

   D. LIFT COEFFICIENT AND VORTEX SHEDDING FREQUENCY

      1. DEN HARTOG

      2. ROBERSON AND CROWE

      3. SARPKAYA

      4. SARPKAYA AND GARRISON

III. STRUCTURAL NODALIZATION

   A. NODAL LENGTH (L) SUCH THAT $1.0 \leq L/D \leq 1.5$

   B. SENSITIVITY STUDY SHOWS APPLICABILITY

NRC SUPPLEMENT TO ACCEPTANCE CRITERIA

- LOCA/SRV JET LOADS
  - NO CHANGES
  - ALTERNATIVES - PLANT UNIQUE

- LOCA/SRV AIR BUBBLE LOADS
  - DRAFT REPORT ACCEPTABLE FOR CYLINDRICAL STRUCTURES WITH MINOR MODIFICATIONS
  - FOR STRUCTURES WITH SHARP EDGES:
    - DRAG COEFFICIENT FROM RELEVANT DATA (PLATES)
    - LIFT COEFFICIENT FROM DATA, THEORY OR $C_L=1.6$
  - QUENCHER SOURCE STRENGTH - EVALUATION

- CONDENSATION LOADS
  - APPROACH ACCEPTABLE
  - SOURCE STRENGTH - EVALUATION

T-7

MARK II CONTAINMENT PROGRAM

TASK STRUCTURE SUMMARY

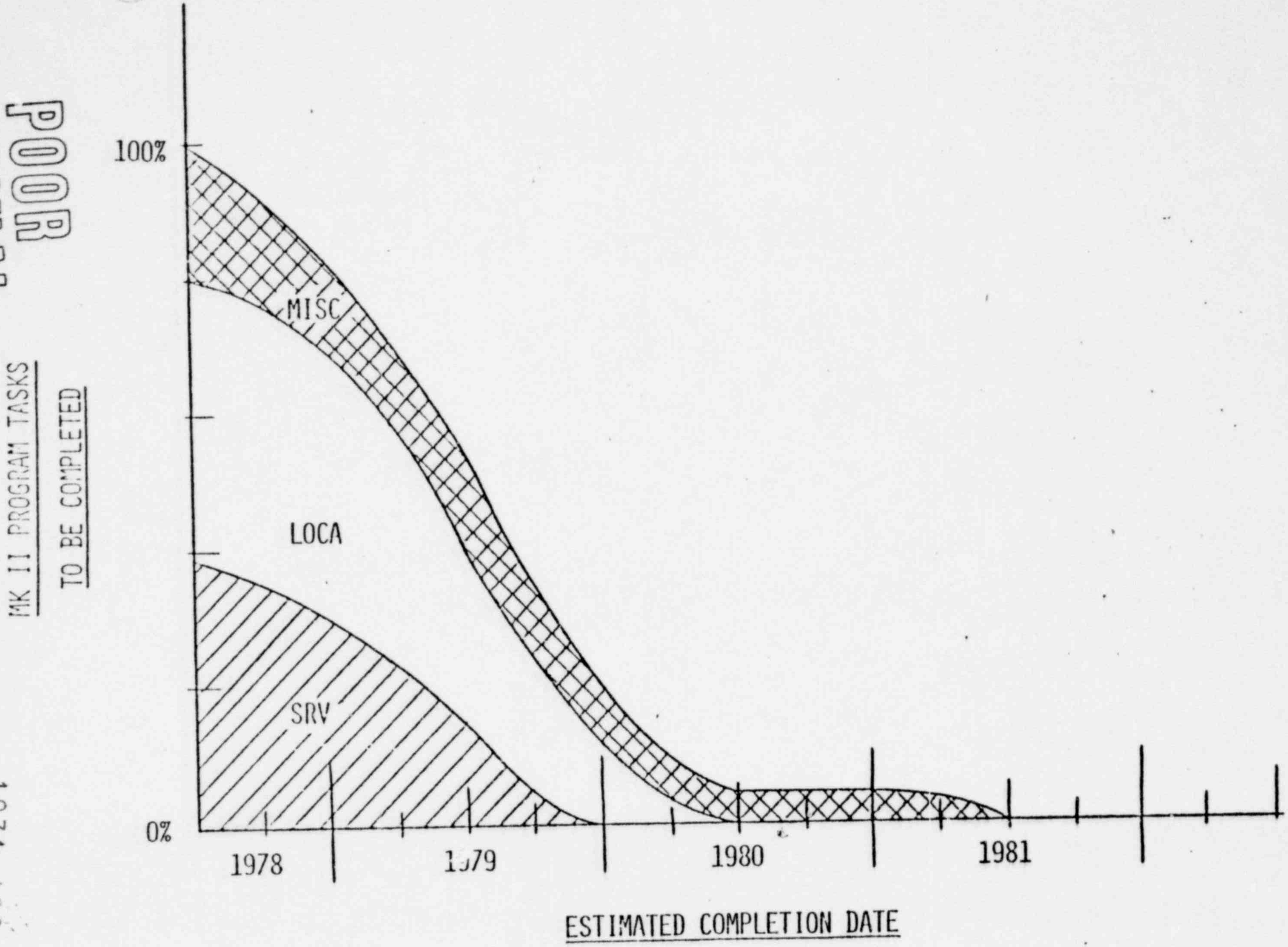
TOTAL NUMBER OF TASKS ——— ≈ 101

MARK II PLANT APPLICATION	% OF TOTAL TASKS
LEAD PLANT SER	8
NON-LEAD PLANT	32
COMBINATION OF PLANT CATEGORIES	34
CONFIRMATORY	12
INFORMATIONAL	14
TOTAL	100%

JULY 1979 COMPLETION STATUS:
(BASED ON COST WEIGHTING)

- OVERALL PROGRAM 70%

ARS/DH
9/79


MK II PROGRAM TASKS
TO BE COMPLETED
100%
0%
MISC
LOCA
SRV
1978
1979
1980
1981
ESTIMATED COMPLETION DATE

ARS
9 /79

MARK II GENERI PROGRAM SCHEDULE

LOCA RELATED ACTIVITIES

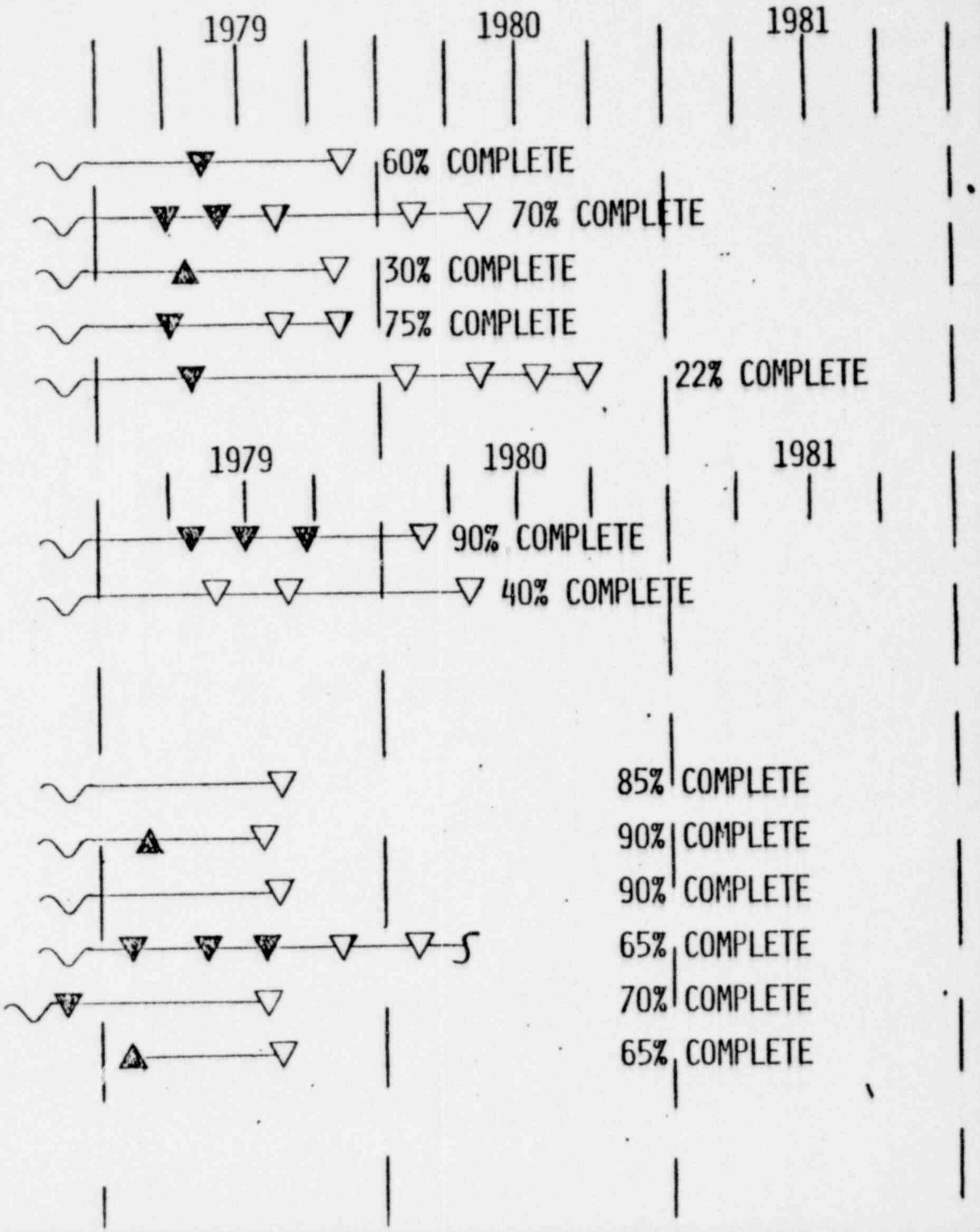
A.5.5	(CHU) RING VORTEX MODEL	60% COMPLETE
A.11	SUBSCALE MULTIVENT TESTING	70% COMPLETE
A.13	EXTENSION-LATERAL LOADS ANAL.	30% COMPLETE
A.16	IMPROVED CHUG LOADS	75% COMPLETE
A.17	CONDENSATION OSCILLATION TESTS	22% COMPLETE

SRV RELATED ACTIVITIES

B.5	CAORSO QUENCHER TESTS	90% COMPLETE
B.14	QUENCHER EMPIRICAL MODEL UPDATE	40% COMPLETE

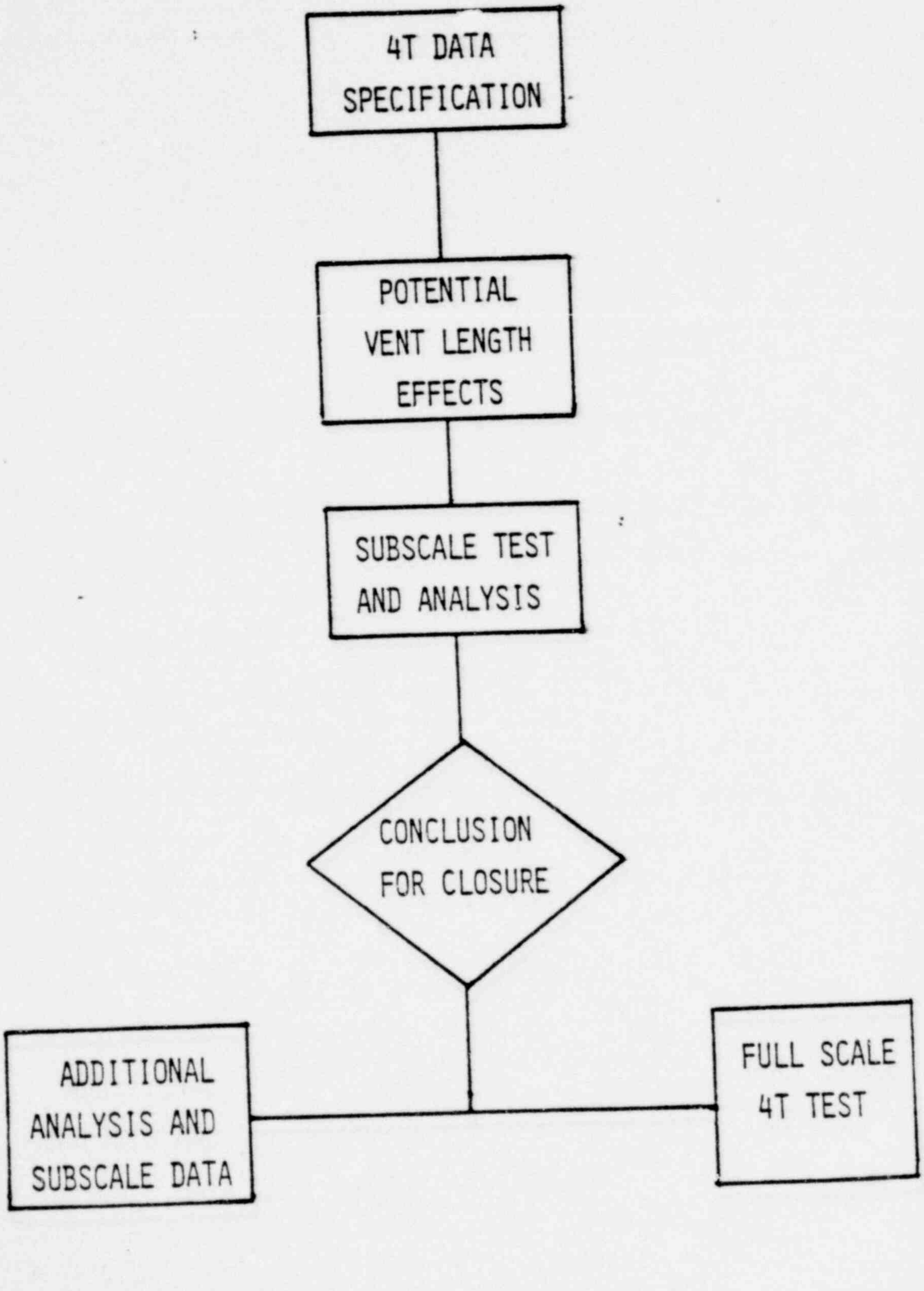
MISCELLANEOUS ACTIVITIES

A.5	QUENCHER AIR BUBBLE	85% COMPLETE
C.5.3	N/K SRSS SUPPLEMENT	90% COMPLETE
C.6	NRC ROUND 2 QUESTIONS	90% COMPLETE
C.9	WORLD TEST MONITORING	65% COMPLETE
C.12	NRC QUESTIONS 20.43 /20.59	70% COMPLETE
C.15	NRC SUBMERGED STRUCTURES CRITERIA	65% COMPLETE



T-7

CONDENSATION OSCILLATION



RJM
9/79

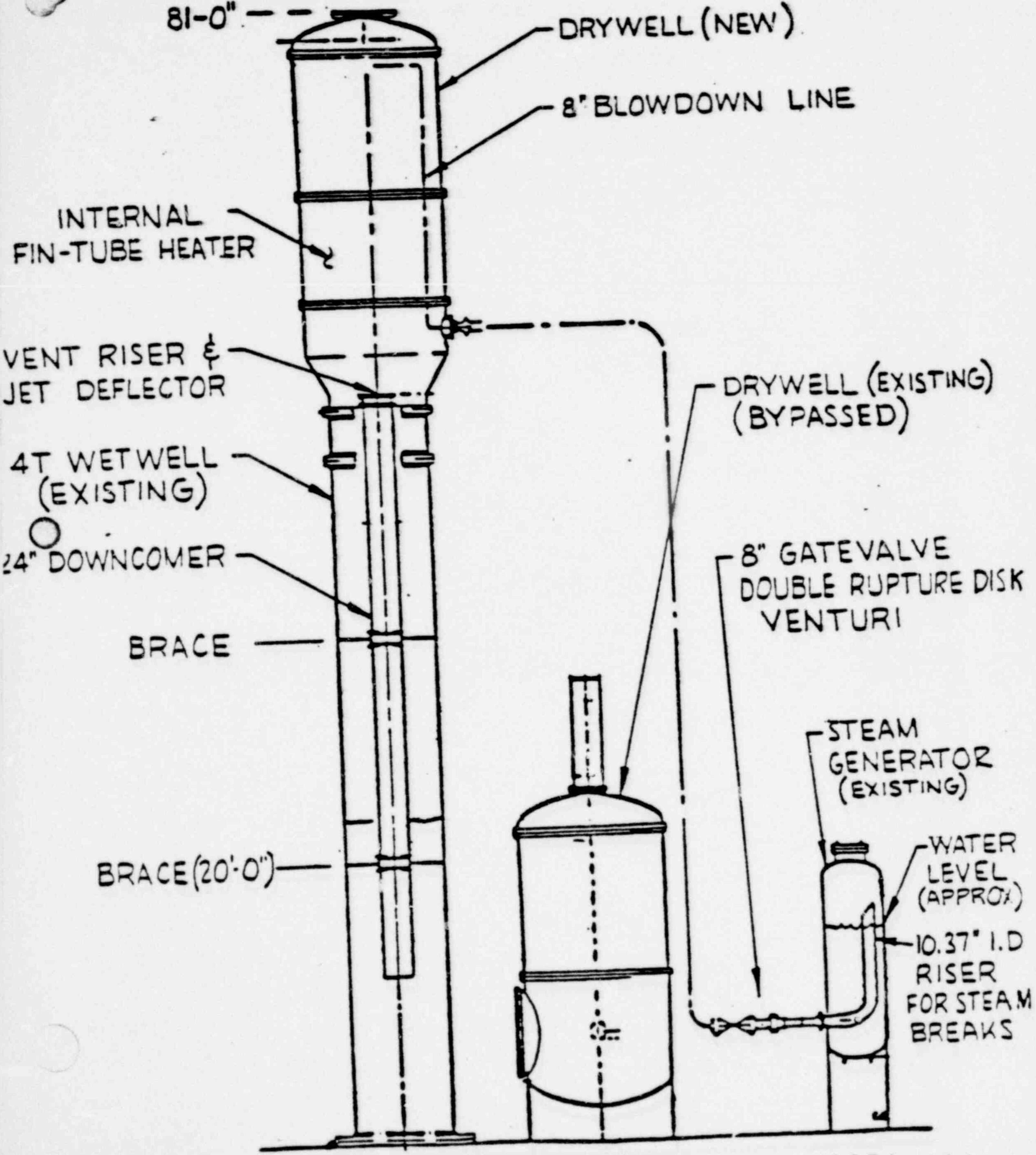
4T CO TEST PROGRAM

OBJECTIVES

- CONFIRM ADEQUACY OF EXISTING C.O. SPECIFICATION
  - EXISTING FACILITY (4T)
  - PROTOTYPICAL CONFIGURATION
  - VARYING TEST CONDITIONS

RJM 9/79

TEST CONFIGURATION FOR MARK II CONDENSATION OSCILLATION TESTS



4T CO INSTRUMENTATION (PRELIMINARY)

LOCATION	INSTRUMENT TYPE	MEASUREMENT	NO.
Wetwell & Suppression Pool	Flush Mount Press. xdcr	Pool Boundary Press.	11
		Wetwell airspace press.	1
	Accelerometers	Fac. Response	6
	Strain gages	Fac. Comp. Response	3
	Thermocouples	Pool temperature	11
		Freespace temperature	1
	Cavity Press. xdcr	Liquid Level	1
Downcomer	Flush Mount Press. xdcr	Vent acoustics	5
	Cavity ΔP xdcr	Vent flow	1
	Cavity press. xdcr	Vent flow	1
	Level probe	Chug initiation	1
	Accelerometers	Chug initiation	2
	Thermocouples	Vent flow & temp.	1
Drywell	Flush Mount Press. xdcr	Acoustics	1
	Cavity press. xdcr	Static press.	1
	Capacitance Probe	Liquid retention	1
	Thermocouples	Drywell temperature	1
Blowdown Line	Cavity press. xdcr	Blowdown flow	1
	Thermocouples	Blowdown line exit temp.	1
Steam Vessel	Cavity ΔP xdcr	Liquid blowdown flow	8
	Cavity press. xdcr	Vessel pressure	1
Vacuum Breaker	Micro Switch	Valve opening	1

Other Instrumentation

- Air Content

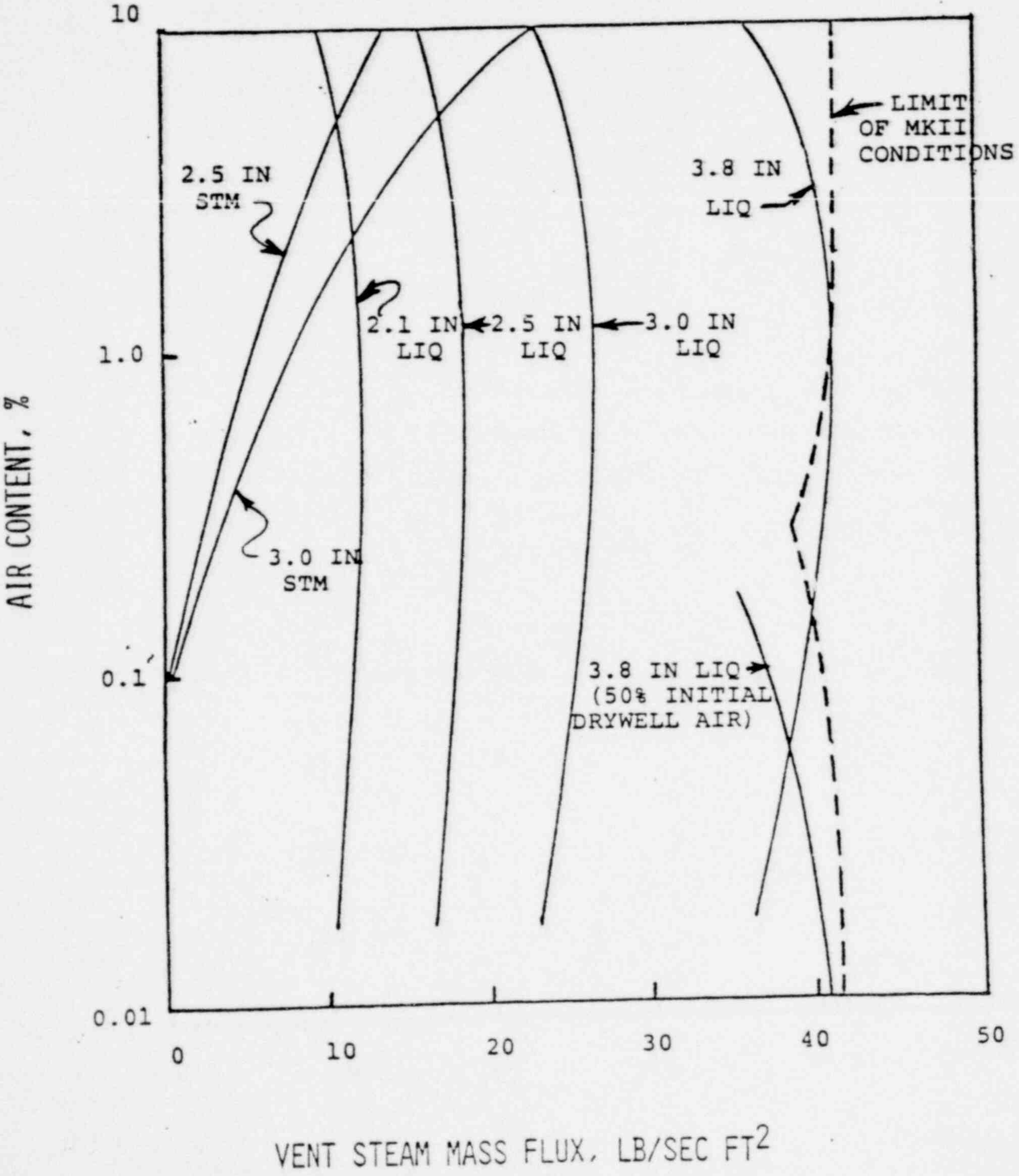
RJM
9/79

4T CO TEST MATRIX (PRELIMINARY)

	No.	Break Type	Break Size(in)	Pool Temp.(^{o}F)	Vent Submer.(ft.)	Vent Riser
Phase I	1	Steam	3.0	70	11	No
	2	Liquid	3.0	70	11	No
	3	Liquid	3.8	70	11	No
	4	Liquid	3.8	70	11	Yes
Phase II	5	Liquid	3.8	80	11	No
	6	Liquid	3.8	80	11	No
	17	Steam	3.0	70	9	No
	18	Steam	3.0	70	13.5	No
	16	Steam	3.0	70	11	Yes
	7	Liquid	3.8	90	11	No
	8	Liquid	3.8	110	11	No
	9	Liquid	3.0	110	11	No
	10	Liquid	3.0	70	9	No
	11	Liquid	3.0	70	13.5	No
	12	Liquid	2.5	110	11	No
	14	Liquid	2.1	70	11	No
	13	Liquid	2.1	110	11	No
	15	Liquid	3.0	70	11	Yes
	20	Steam	2.5	70	11	No
	19	Steam	3.0	70	13.5	No
	21	Steam	2.5	70	11	No
	22	◄		Repeat		►
	23	◄		Repeat		►

RJM
9/79

4T C.O. TEST MATRIX
AND
MARK II C.O. CONDITIONS
FOR BLOWDOWNS



RJM 9/79

4T CO DATA INTERPRETATION

ELEMENTS	USAGE
● VENT PRESSURE HISTORIES	- DETERMINATION OF STANDING WAVE PRESENCE
● POOL WALL PRESSURES	- ESTABLISH CO AMPLITUDE vs FREQUENCY CONTENT
	- INTERPRETATION FOR MARK II APPLICATION
	- COMPARE TO DFFR

RJM 9/79

4T C.O. PROGRAM SCHEDULE

• FUNCTIONAL SPECIFICATION	COMPLETE
• TEST PLAN	COMPLETE
• COMPLETE FACILITY MODIFICATION	SEPT 79
• SHAKEDOWN TEST	SEPT/OCT 79
• PHASE I TEST	OCT/NOV 79
• PHASE II TEST	DEC 79/MAR 80
• DATA REDUCTION	NOV 79/JUNE 80
• FINAL TEST REPORT	3Q 80

RJM 9/79

T-8

ACRS PRESENTATION

September 13, 1979

Los Angeles, California

MULTIVENT TEST PROGRAM

Performed for GE and Mark II Owners

by

Creare Incorporated

Hanover, New Hampshire

B. R. Patel
Creare Inc.

MULTIVENT TEST PROGRAM

OVERALL OBJECTIVES

- OBTAIN A SINGLE-VENT/MULTIVENT CHUGGING DATA BASE TO ESTABLISH TRENDS IN POOL WALL LOADS WITH NUMBER OF VENTS
- DEMONSTRATE THAT THE MULTIVENT TRENDS OBSERVED IN SUBSCALE TESTS ARE VALID BY
    - COMPARING SINGLE VENT DATA AT FOUR SUBSCALES
    - COMPARING MULTIVENT DATA AT TWO SUBSCALES

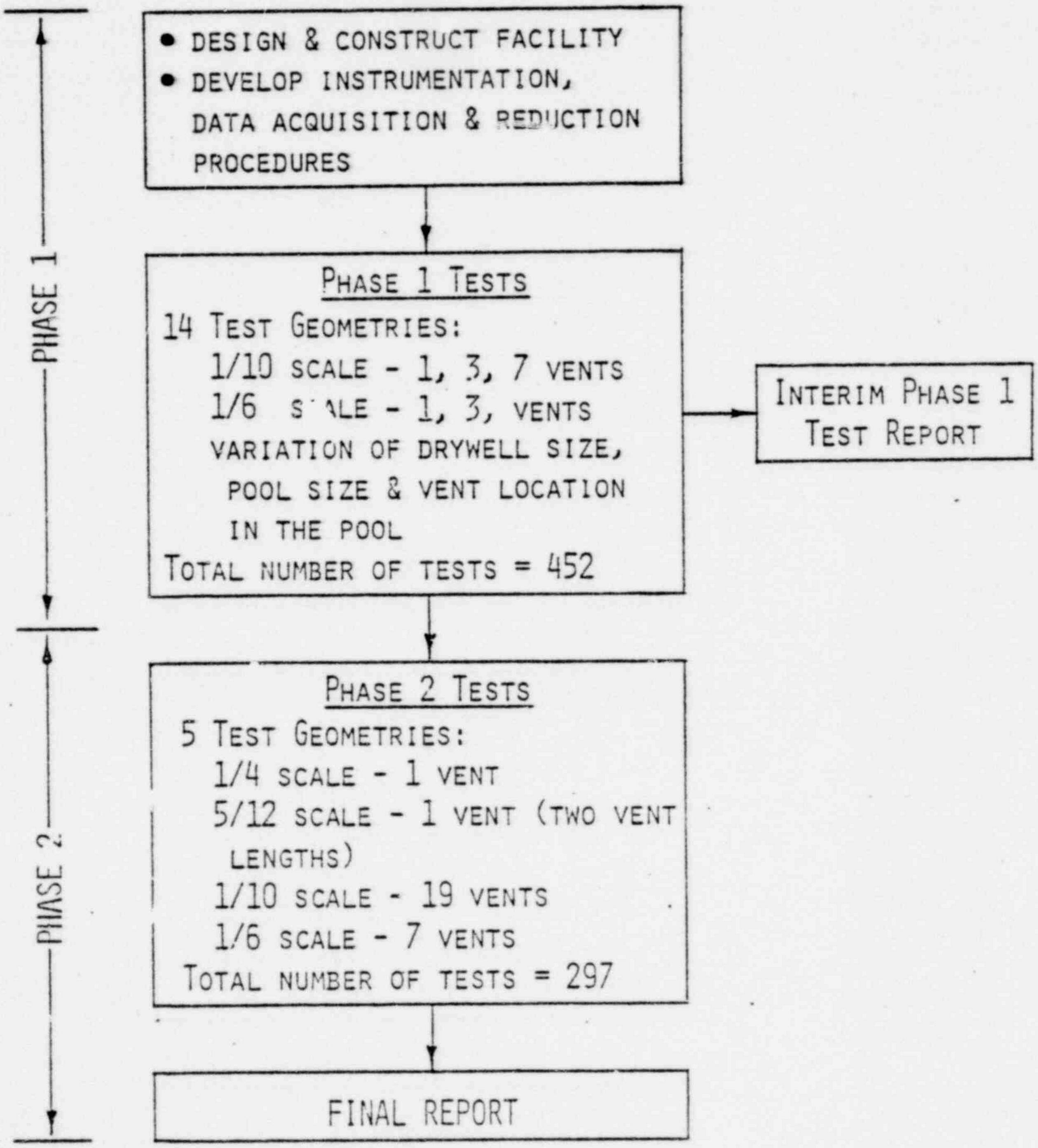
MULTIVENT TEST PROGRAM

- SINGLE VENT TESTS:
  1/10, 1/6, 1/4, 5/12 SCALES
- MULTIVENT TESTS:
  1/10 SCALE 3, 7, 19 VENTS
  1/6 SCALE 3, 7 VENTS
- ADDITIONAL TESTS TO EVALUATE EFFECTS OF:
  DRYWELL SIZE
  POOL SIZE
  VENT LOCATION IN THE POOL

TOTAL NUMBER OF TESTS: 749

MULTIVENT TEST PROGRAM

PROGRAM OVERVIEW



- DESIGN & CONSTRUCT FACILITY
- DEVELOP INSTRUMENTATION, DATA ACQUISITION & REDUCTION PROCEDURES

PHASE 1 TESTS

14 TEST GEOMETRIES:
1/10 SCALE - 1, 3, 7 VENTS
1/6 SCALE - 1, 3, VENTS
VARIATION OF DRYWELL SIZE, POOL SIZE & VENT LOCATION IN THE POOL
TOTAL NUMBER OF TESTS = 452

INTERIM PHASE 1 TEST REPORT

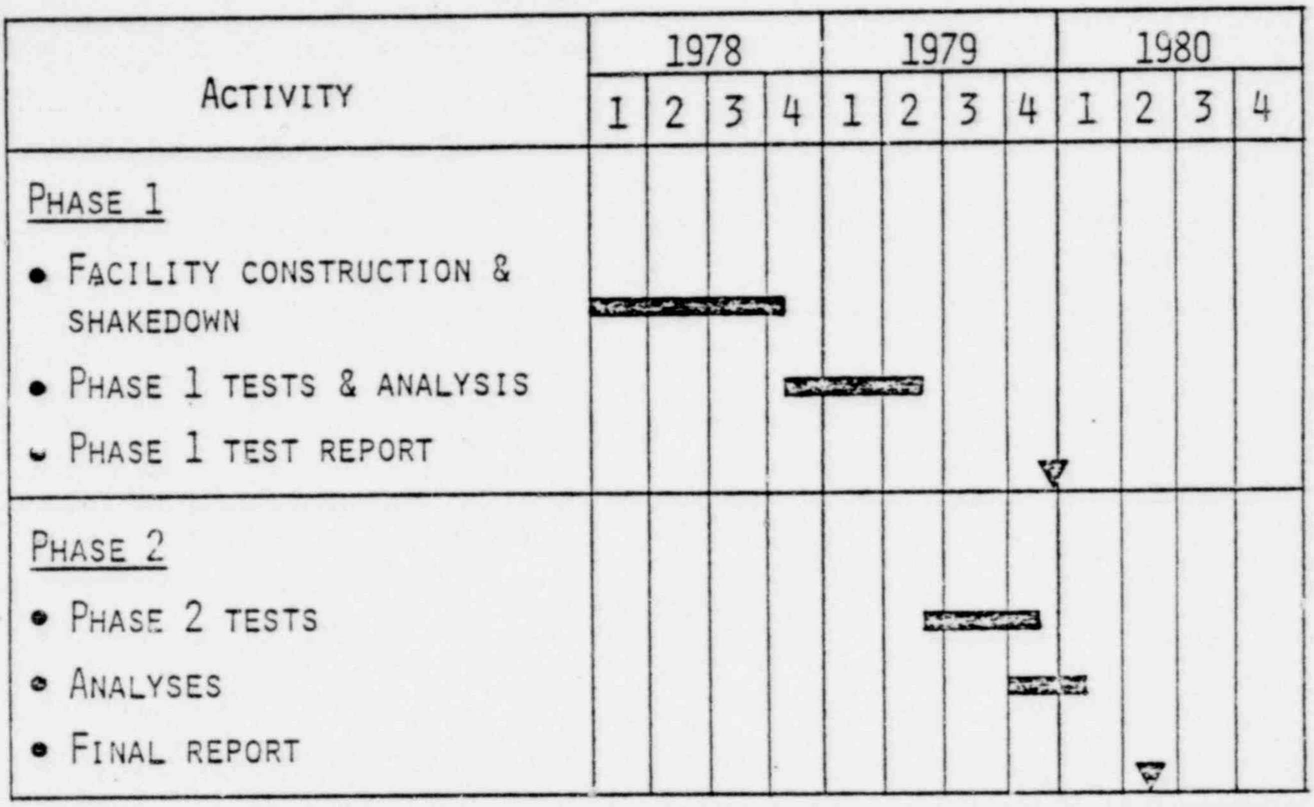
PHASE 2 TESTS

5 TEST GEOMETRIES:
1/4 SCALE - 1 VENT
5/12 SCALE - 1 VENT (TWO VENT LENGTHS)
1/10 SCALE - 19 VENTS
1/6 SCALE - 7 VENTS
TOTAL NUMBER OF TESTS = 297

FINAL REPORT

MULTIVENT TEST PROGRAM

SCHEDULE



MULTIVENT TEST PROGRAM

TEST MATRIX

SUBMERGENCE AND CLEARANCE (FT): SCALED BY THE SCALE FACTOR

WETWELL DIAMETER (FT): SCALED TO KEEP POOL TO VENT AREA RATIO CONSTANT*

DRYWELL VOLUME (FT^3): SCALED BY THE CUBE OF THE SCALE FACTOR*

WETWELL AIRSPACE PRESSURE (PSIA): SUB-AMBIENT TO 45

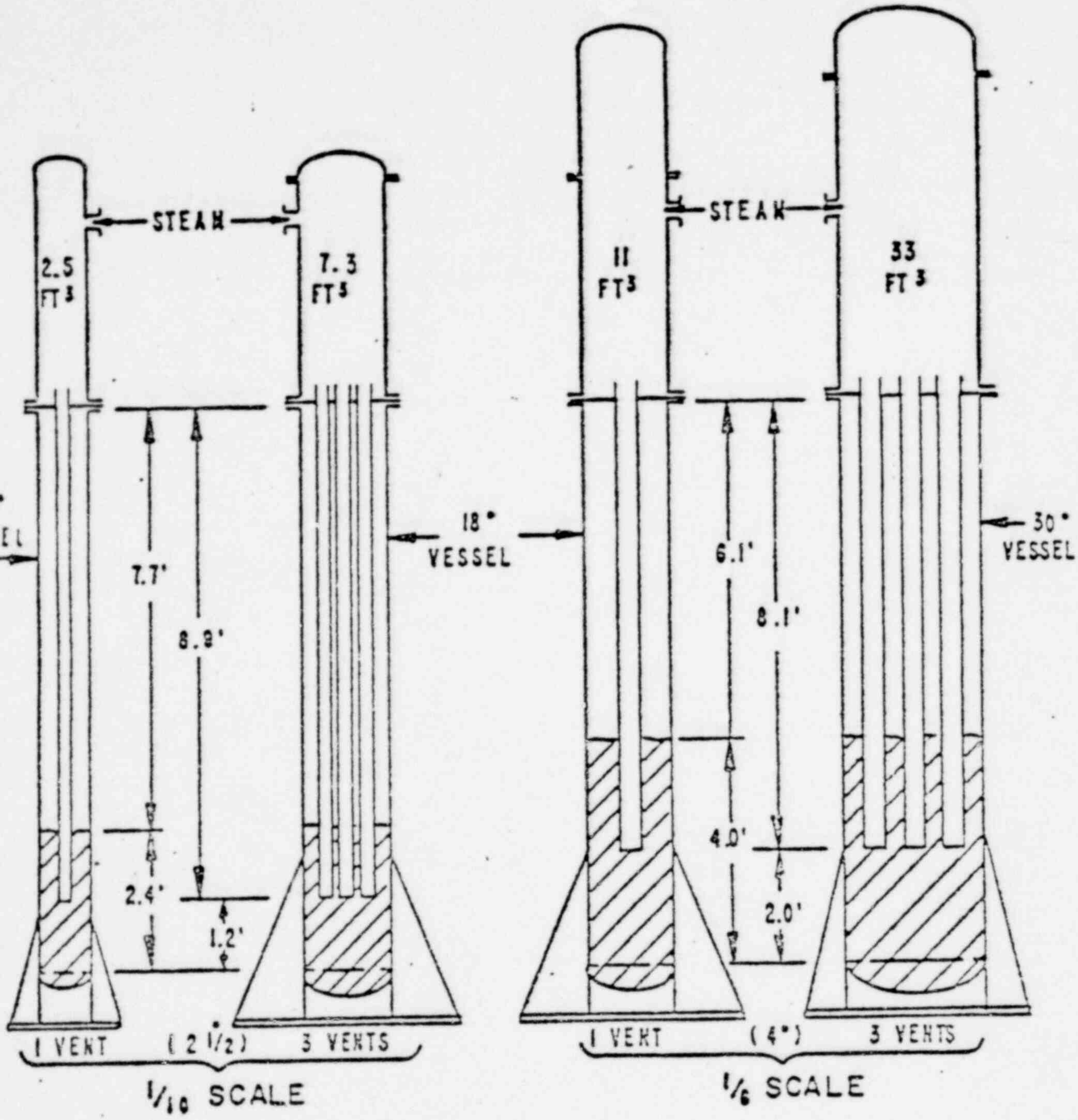
STEAM MASS FLUX (LBM/SEC FT^2): 0.1 TO 16

POOL TEMPERATURE (°F): 90 TO 200

STEAM AIR-CONTENT (%): 0 TO 0.5

---

*EXCEPT WHERE VARIED ON PURPOSE.



POOR ORIGINAL

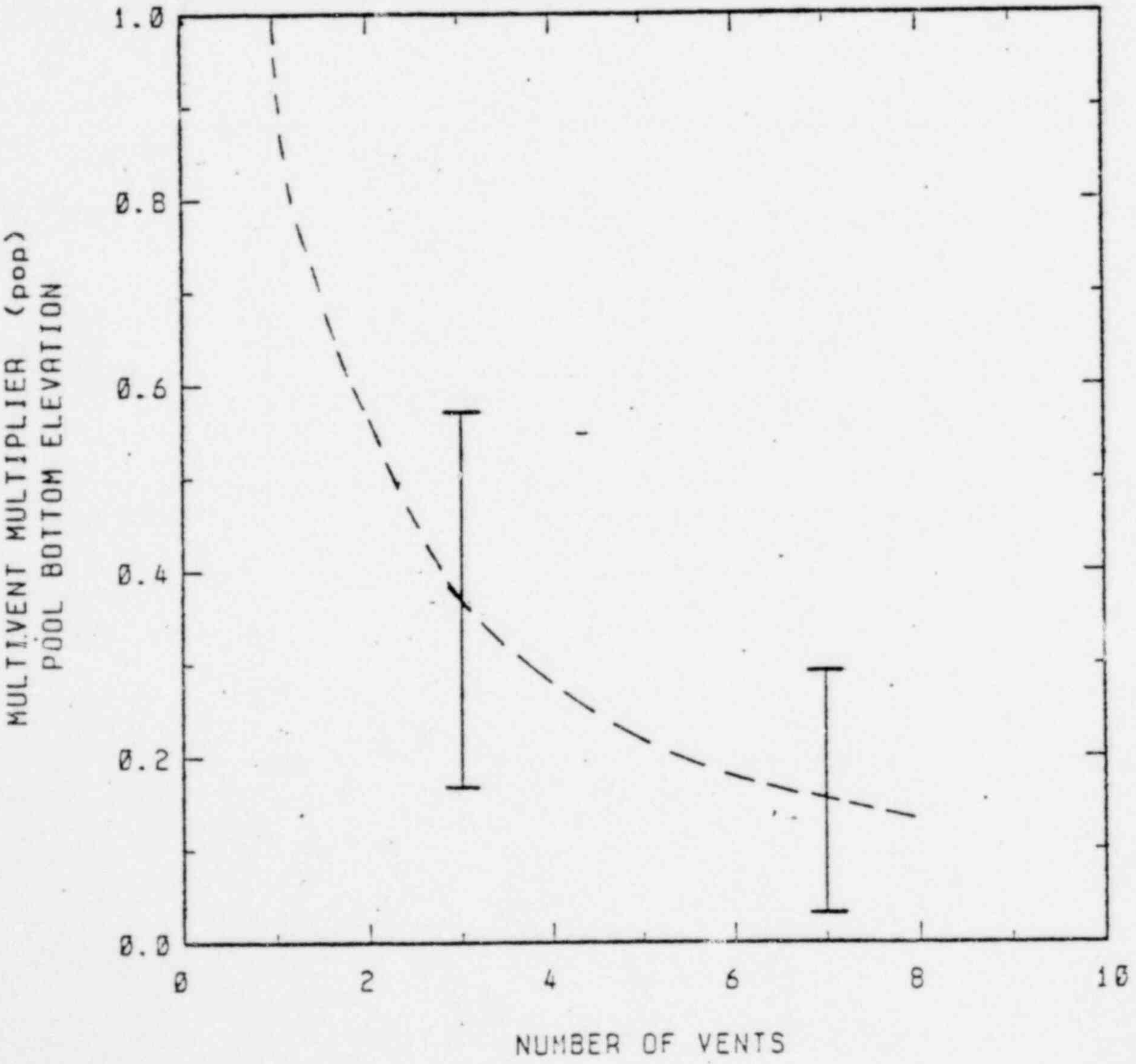
MULTIVENT TEST PROGRAM

PHASE 1

PRELIMINARY DATA

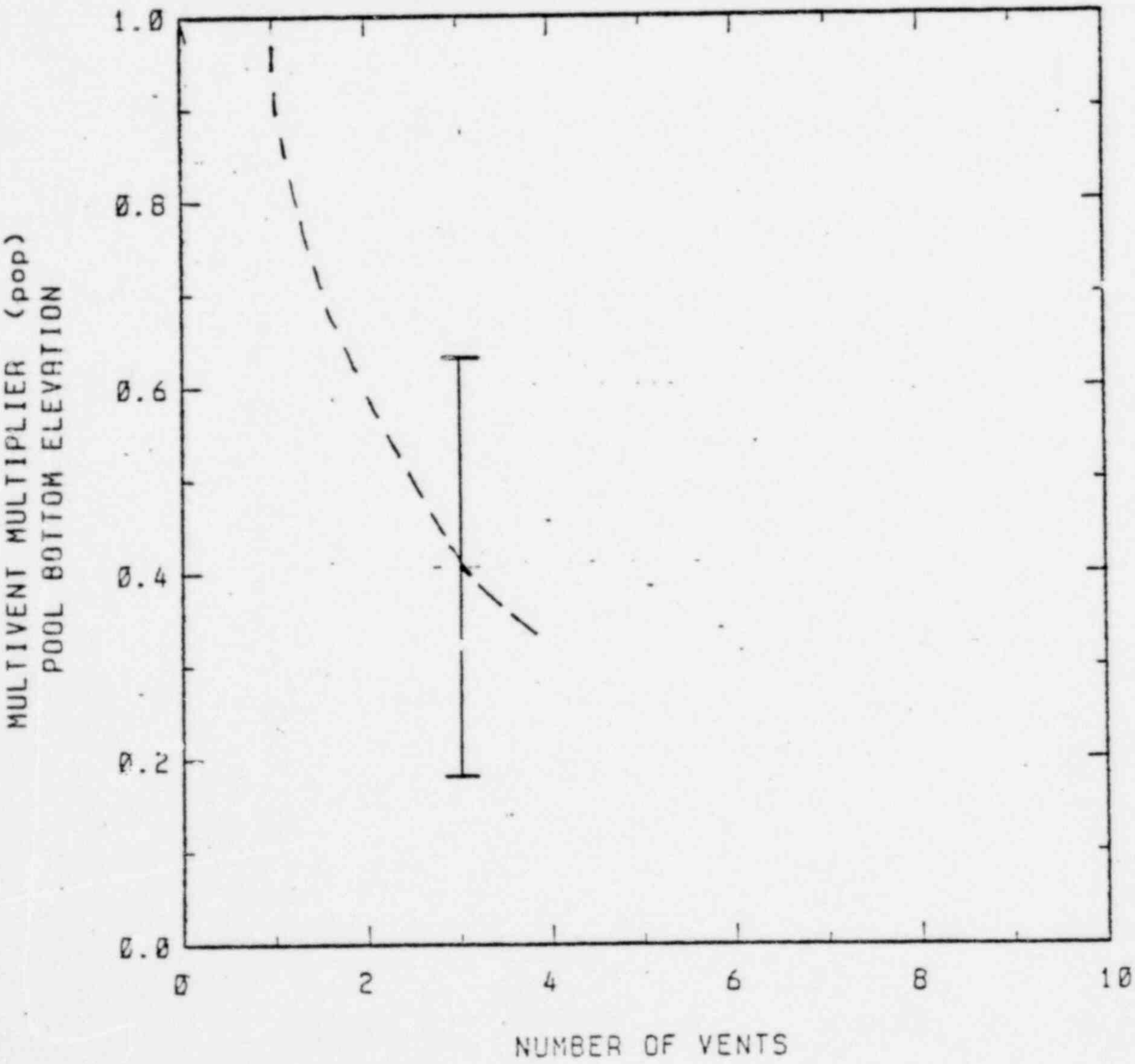
PRELIMINARY UNCHECKED DATA

WETWELL AIRSPACE PRESSURE (psia)	45.0±0.0
STEAM MASS FLUX (lb/sec ft²)	2 to 8
POOL TEMPERATURE (deg F)	90 to 200
AIR CONTENT (%)	0.0 to 0.5
GEOMETRIES A.K.P : 1/10 SCALE	MULTI-VENT



PRELIMINARY UNCHECKED DATA

WETWELL AIRSPACE PRESSURE (psia)	45.0±0.0
STEAM MASS FLUX (lb/sec ft²)	2 to 8
POOL TEMPERATURE (deg F)	90 to 200
AIR CONTENT (%)	0.0 to 0.5
GEOMETRIES J.M : 1/6 SCALE	MULTI-VENT



1031 201

1/10 & 1/6 SCALE MULTIVENT DATA

CONCLUSIONS

- OVERALL CHARACTERISTICS OF MULTIVENT CHUGGING ARE SIMILAR TO SINGLE VENT CHUGGING
- MULTIVENT POOL WALL PRESSURES ARE LOWER THAN SINGLE VENT POOL WALL PRESSURES, I.E., THE MULTIVENT MULTIPLIER IS LESS THAN UNITY
- THE MULTIVENT MULTIPLIER DECREASES WITH INCREASING NUMBER OF VENTS

J-9-11

IMPROVED CHUG LOAD

MARK II PROGRAM - TASK A.16

SEPTEMBER 13, 1979

IMPROVED CHUG LOAD

BACKGROUND

• 4T Chugging Tests	(1975,76)
• Application Memorandum	(1977)
• Bounding Loads Report	(1977)
• Multivent Hydrodynamic Model	(1978)
• 4T FSI Study	(Anamet, 1978)
• NRC FSI Concerns	
• Large Mark II Responses	
• Task A.16 of the Mark II Program	

IMPROVED CHUG LOAD

APPROACH

- Further Study of 4T Data
- 4T Model
- Vent Exit Forcing Functions
- Mark II Containment Model
- Mark II Response

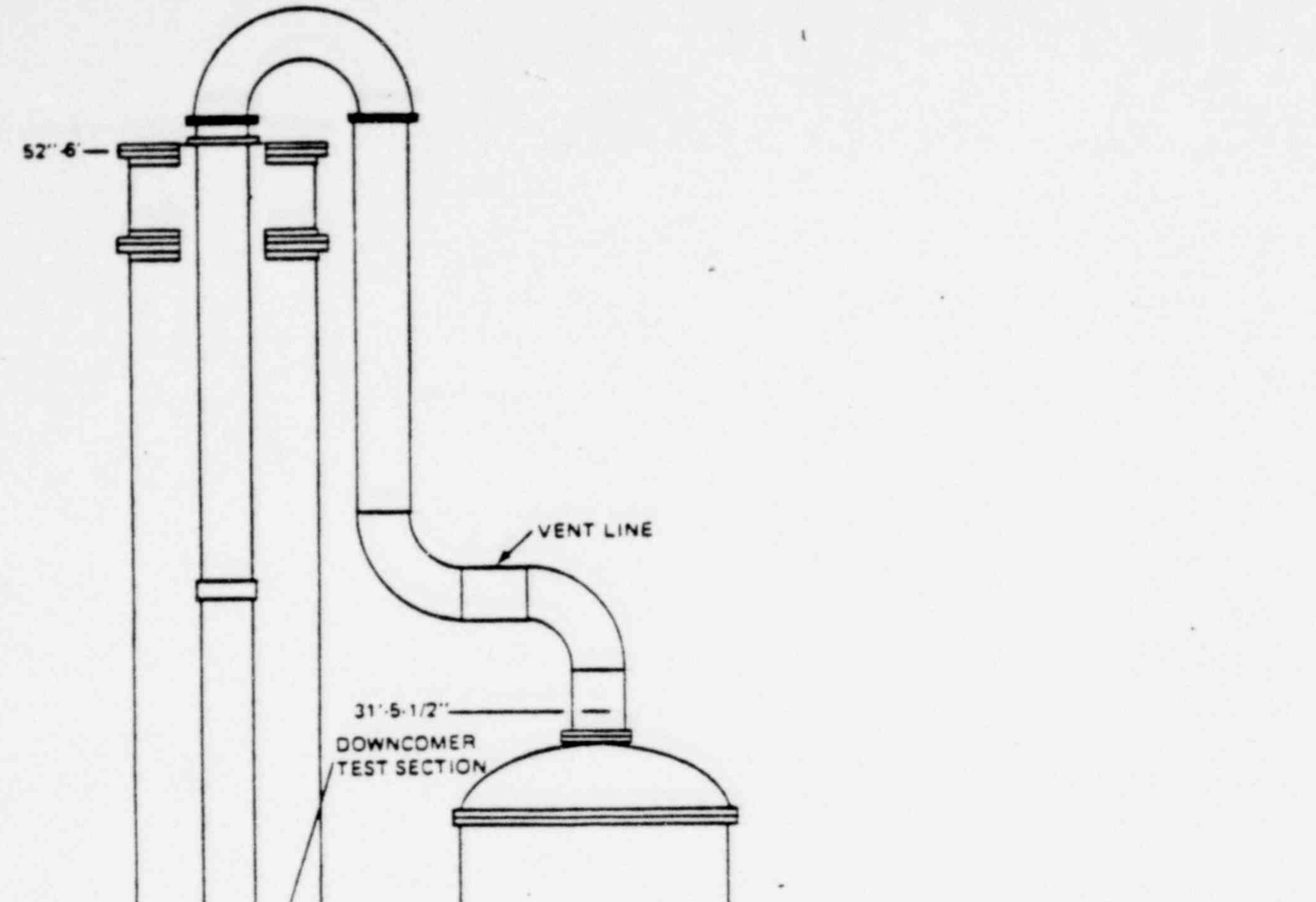


Figure 3-1 4T Test Facility Schematic

G1002820 11

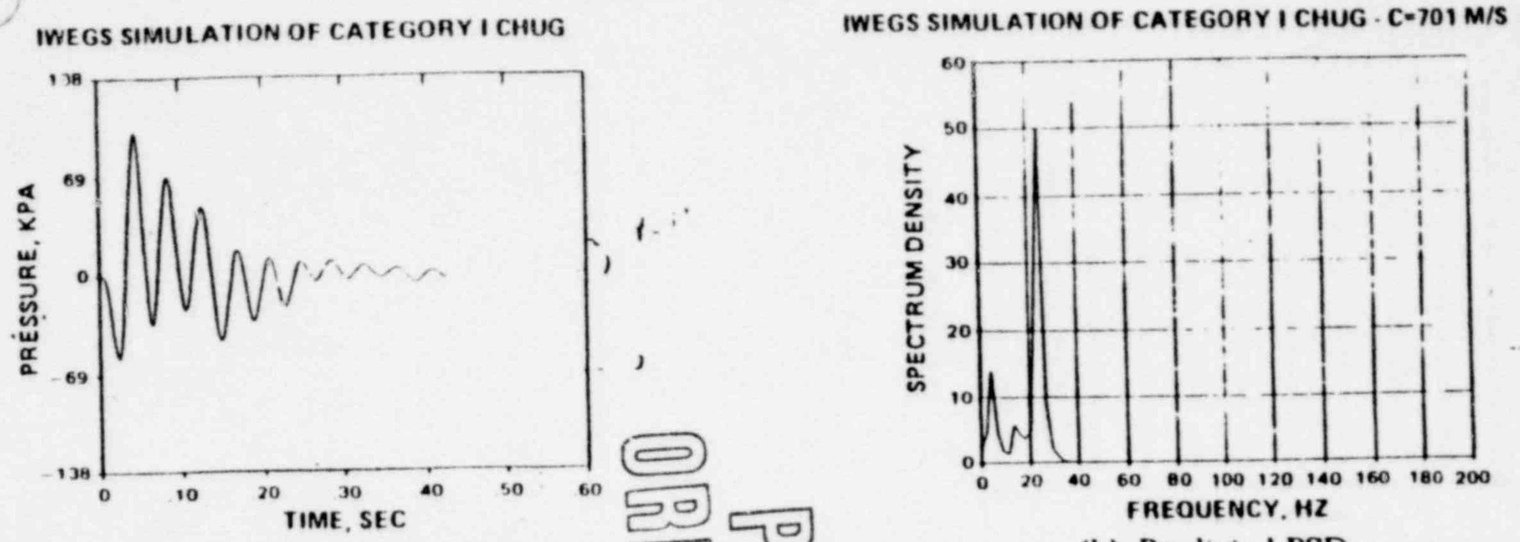
IMPROVED CHUG LOAD

4T DATA

- 137 Chugs
- Four Categories
- Dominant Frequencies
  - Vent pipe
  - Tank/pool

4T MODEL

- Linear Acoustic Fluid
- Point Source Chug Excitation
- Vent Decoupled From Tank/Pool
- Sonic Speed Adjustment for 4T FSI
- Chug Simulation

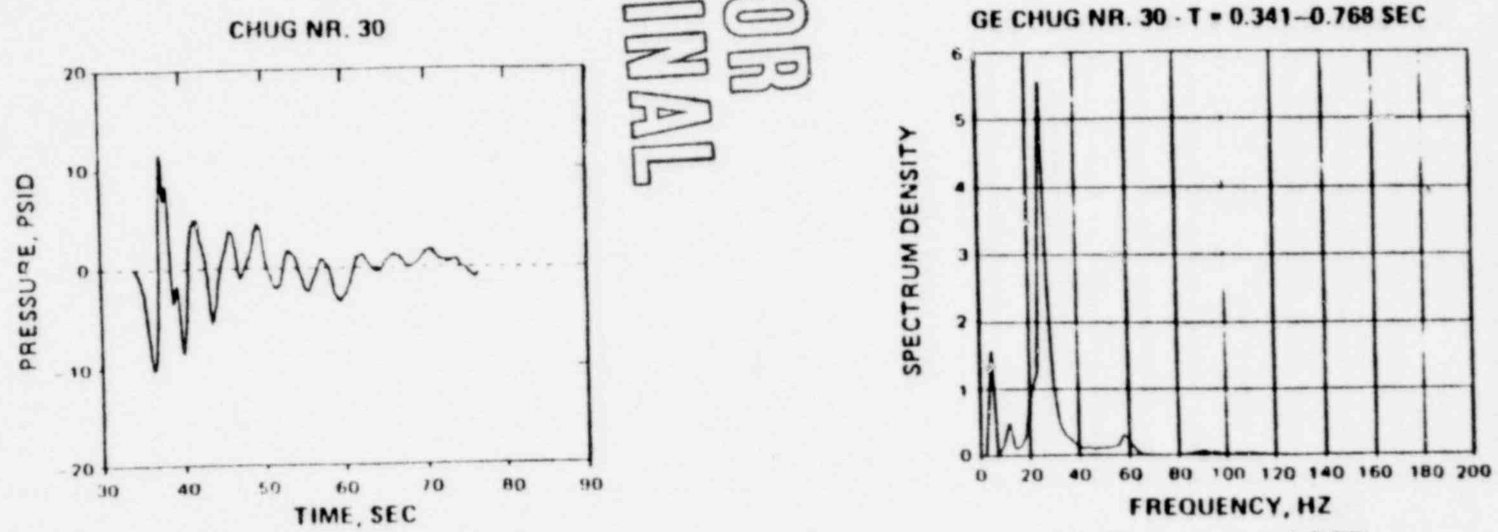

IWEGS SIMULATION OF CATEGORY I CHUG

IWEGS SIMULATION OF CATEGORY I CHUG - C=701 M/S

(a) Predicted Pressure Time-History

(b) Predicted PSD

POOR ORIGINAL


CHUG NR. 30

GE CHUG NR. 30 - T = 0.341–0.768 SEC

(c) Experimental Pressure Time-History

(d) Experimental PSD

Figure 5-5 Comparison of IWEGS Results with Chug #30

⑤

IMPROVED CHUG LOAD

4T MODEL VERIFICATION

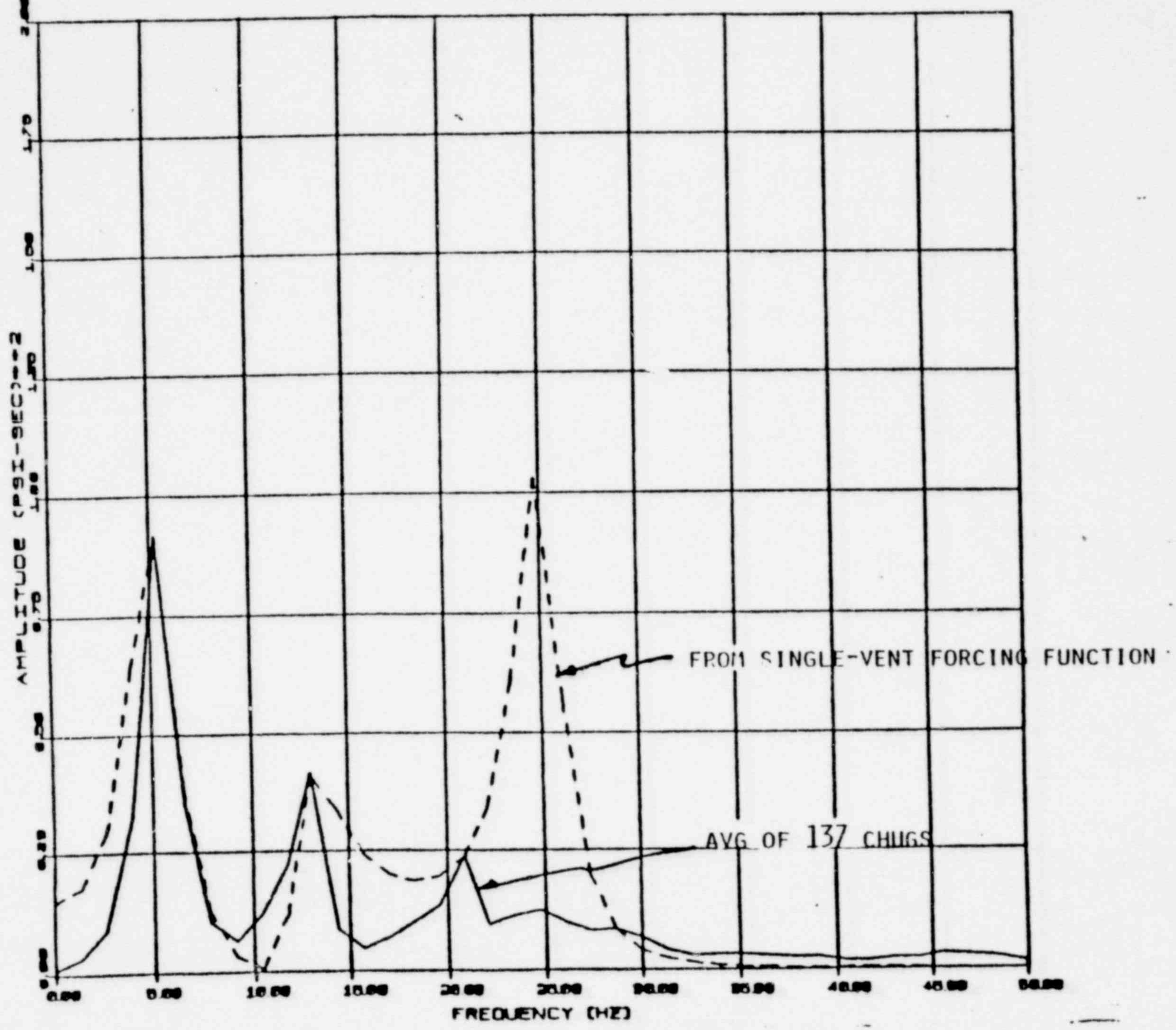
- KFIX Studies
- Bell Jar Tests
- Baseplate Excitation Tests
- Standard Analysis of Pipe Flexibility Effects
- NASTRAN Studies

IMPROVED CHUG LOAD

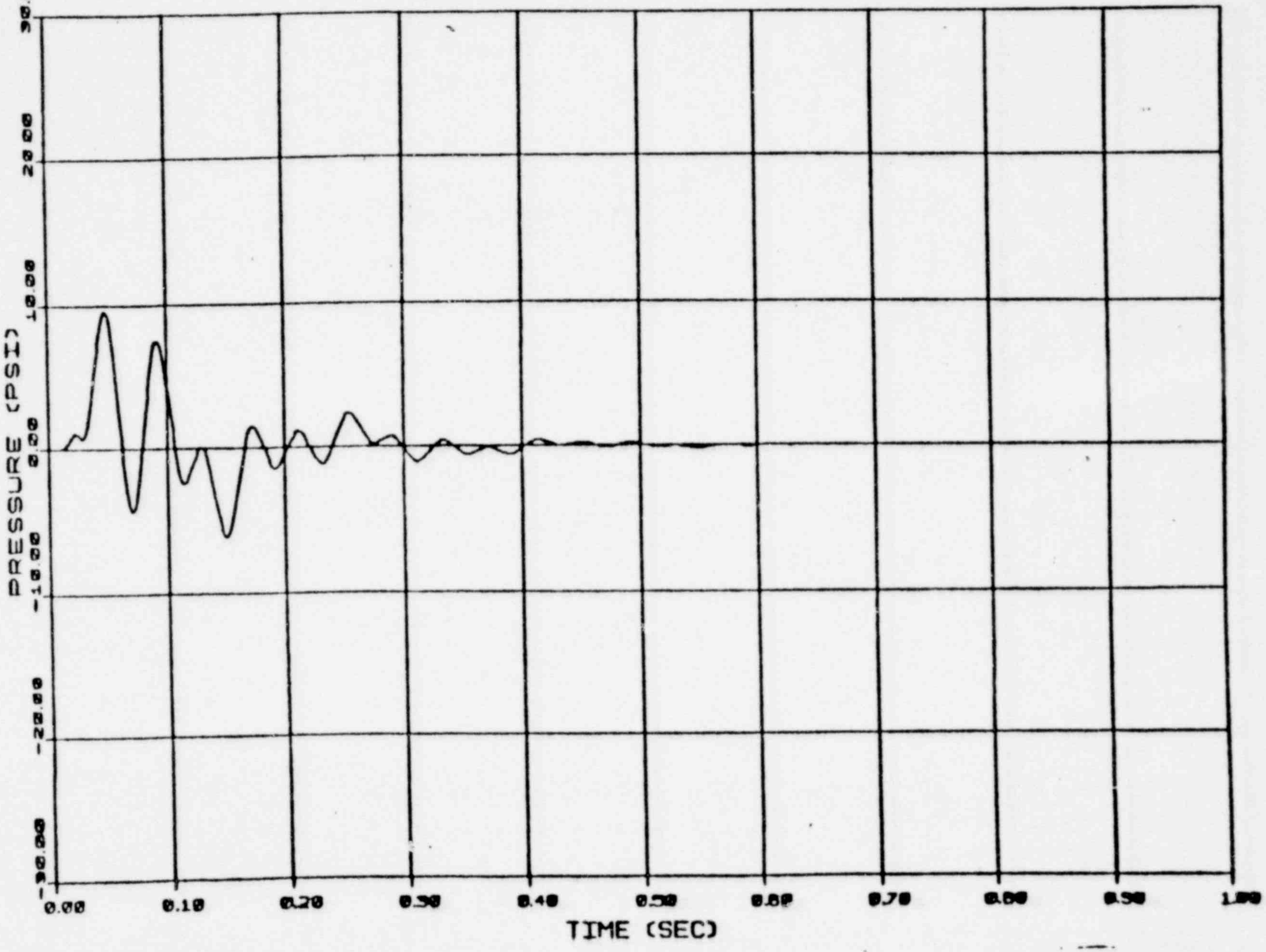
DEVELOPMENT OF VENT EXIT FORCING FUNCTIONS

- ESTABLISHED SINGLE VENT CRITERIA
  - TOTAL SIGNAL POWER OF 4T RESPONSE
  - POWER BY FREQUENCY
  - PEAK PRESSURE
- COMPUTED WITH 4T MODEL
- ACCOUNTED FOR DIFFERENCE BETWEEN 4T AND MARK II
- ASSIGNED TO MARK II VENTS IN PHASE (LOAD CASE 1)
- ASSIGNED TO MARK II VENTS IN PHASE WITH CIRCUMFERENTIAL MULTIPLIER (LOAD CASE 2)

PSD COMPARISON



4T BCP RESPONSE TO SINGLE-VENT FORCING FUNCTION



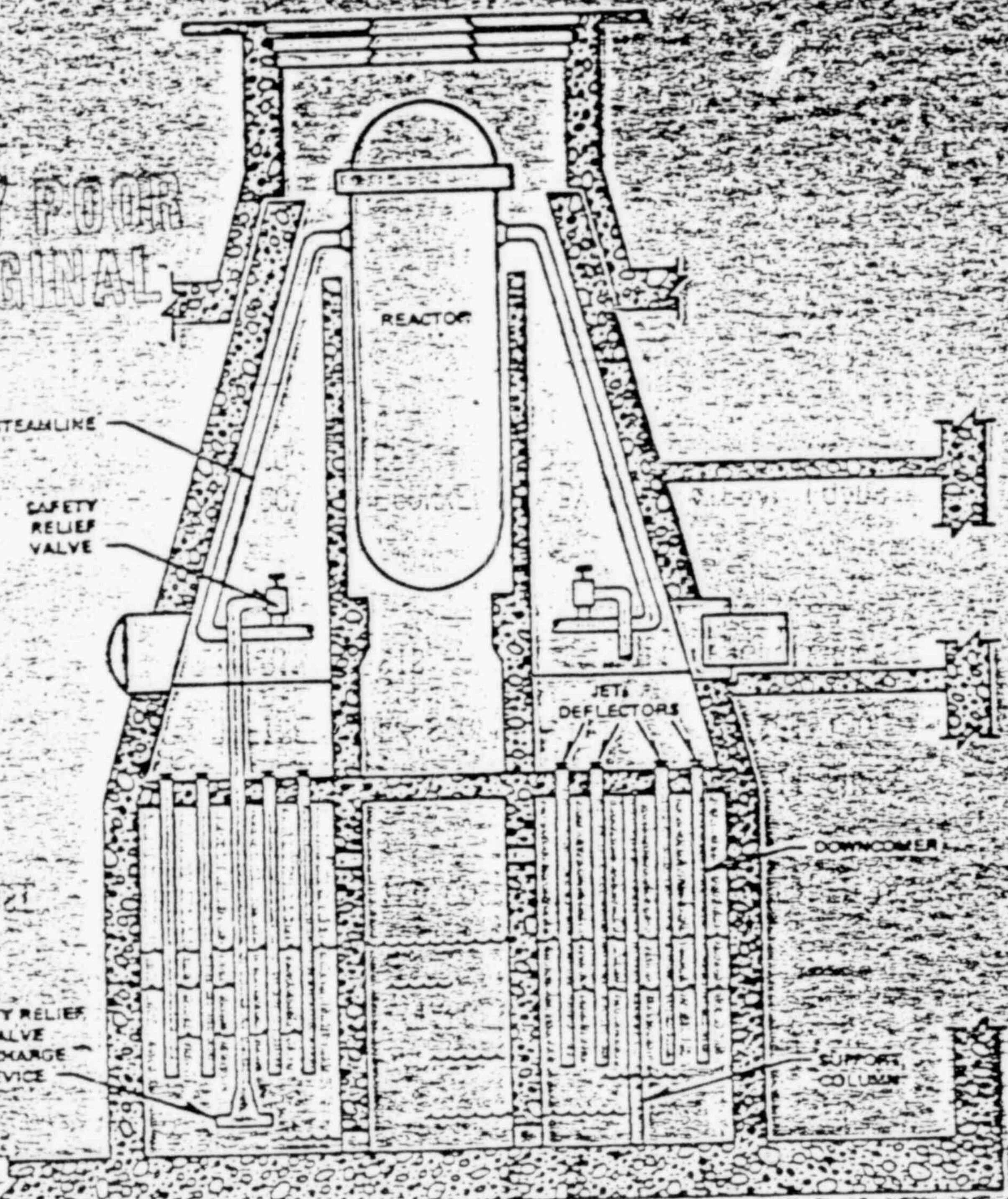
IMPROVED CHUG LOAD

MARK II CONTAINMENT MODEL

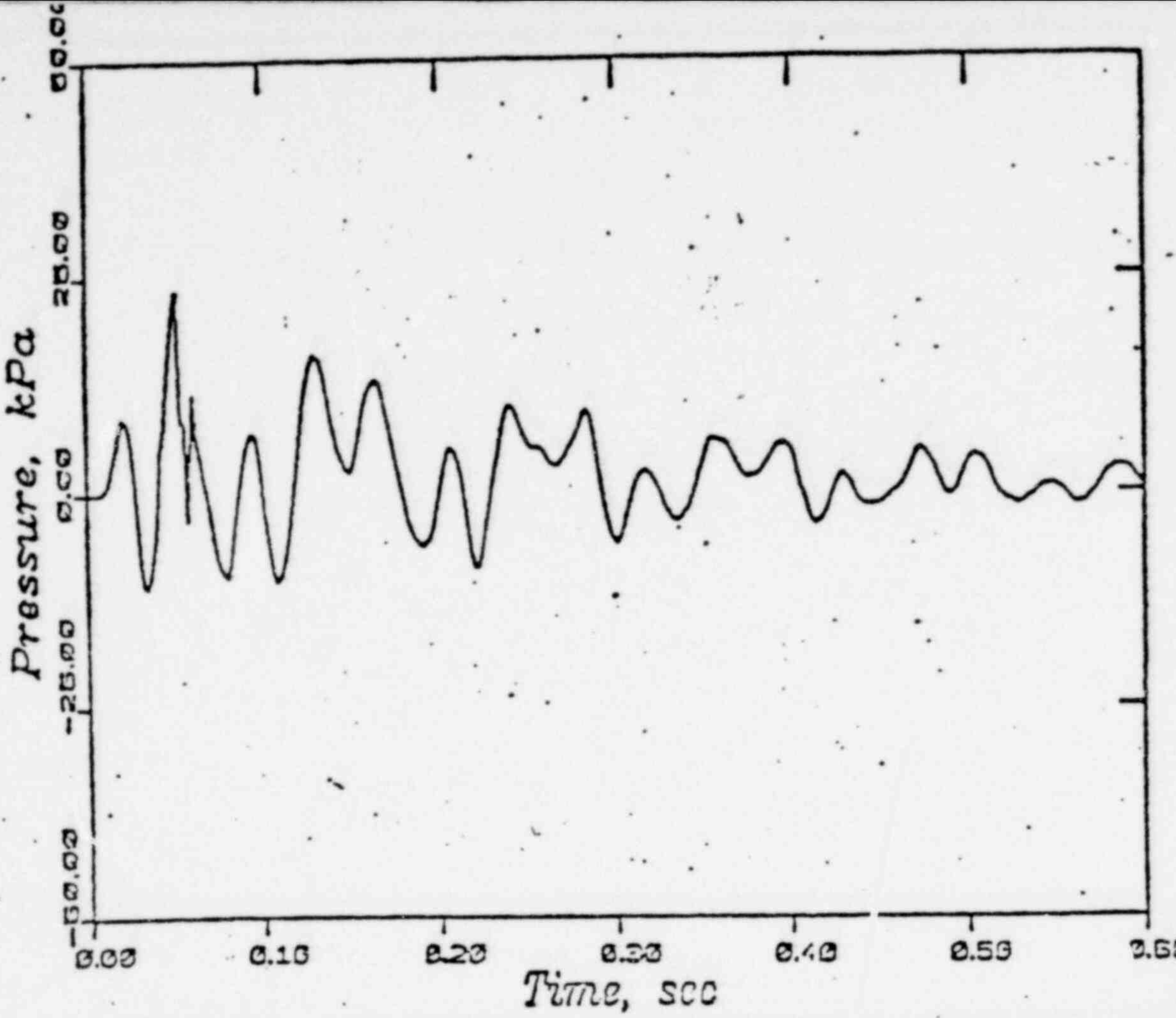
- ACTUAL MARK II GEOMETRY
- LINEAR ACOUSTIC FLUID
- CHUG EXCITATIONS AS POINT SOURCES

MARK II RESPONSE

- RIGID WALL LOADS
- ACCELERATION RESPONSE SPECTRA

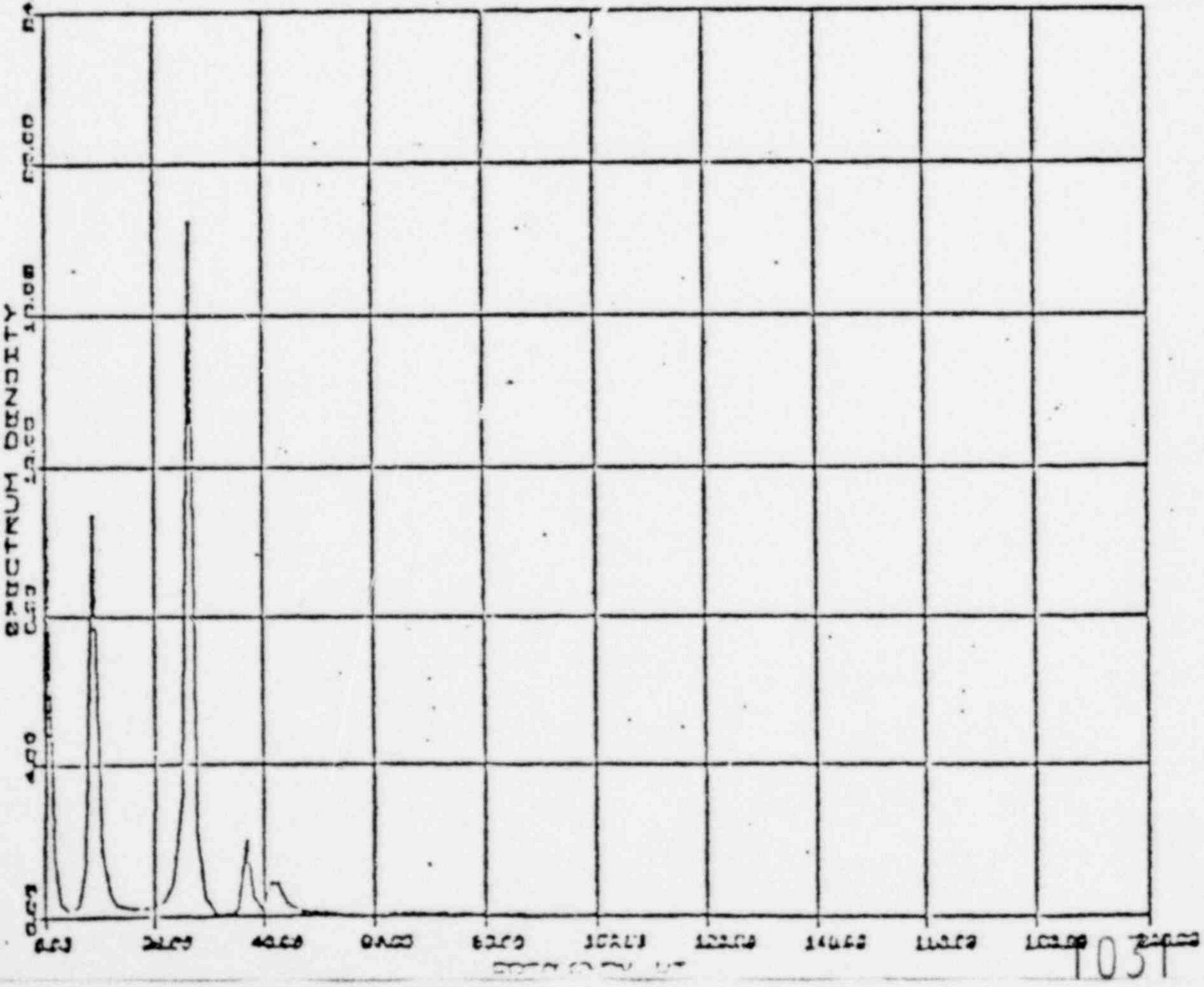


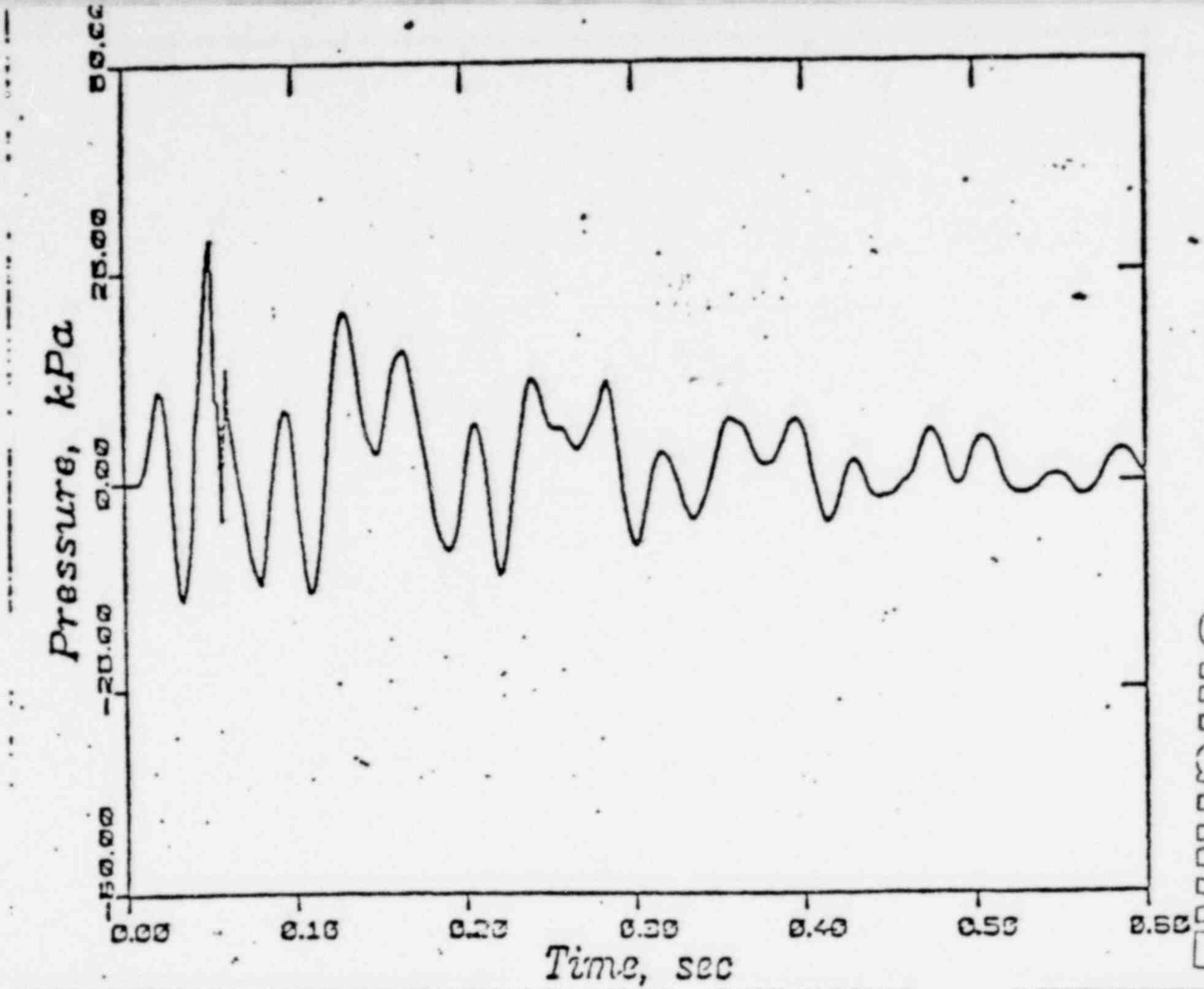
Typical Mark II Pressure Suppression Containment



POOR ORIGINAL

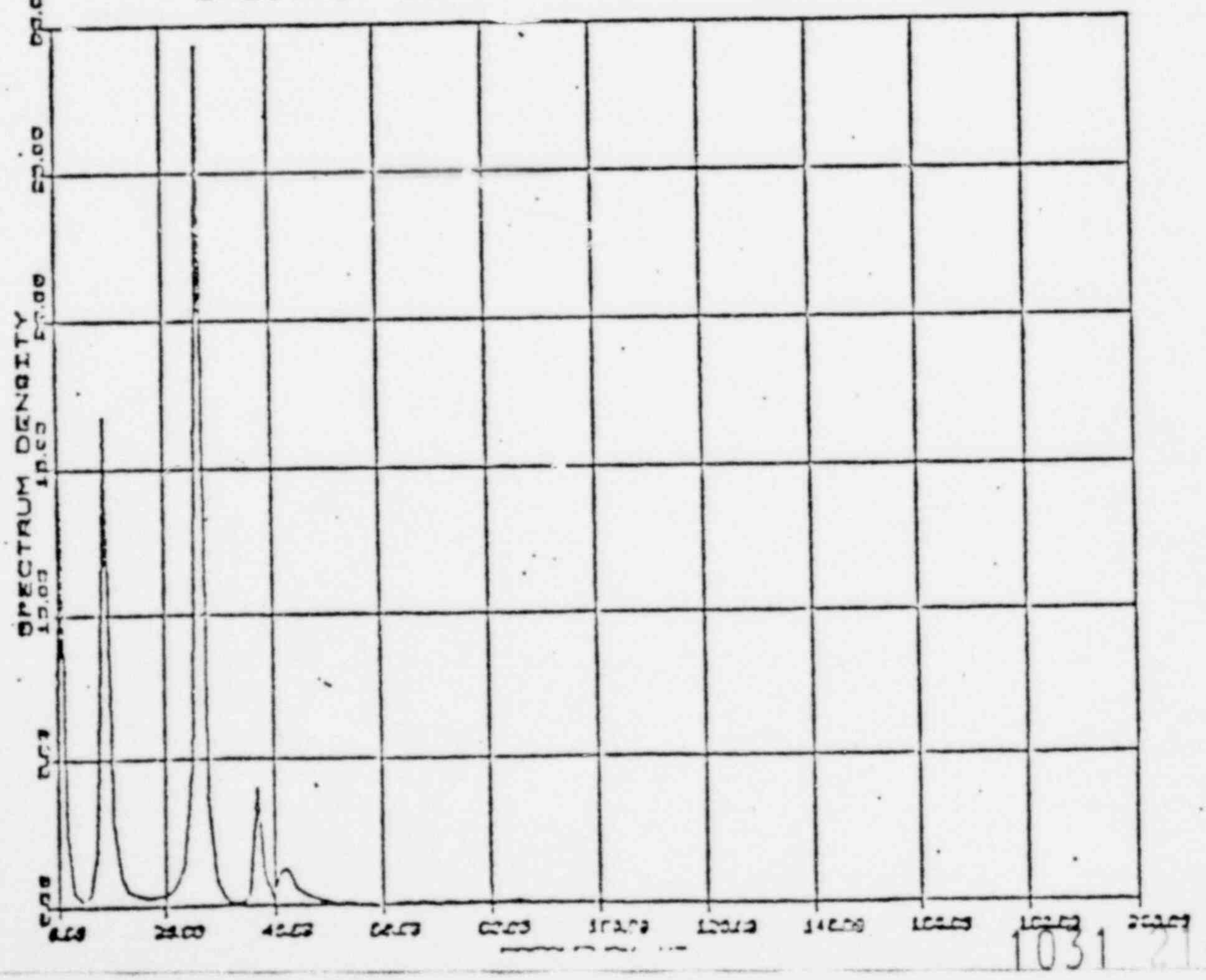
SPECTRUM DENSITY - SYMMETRIC



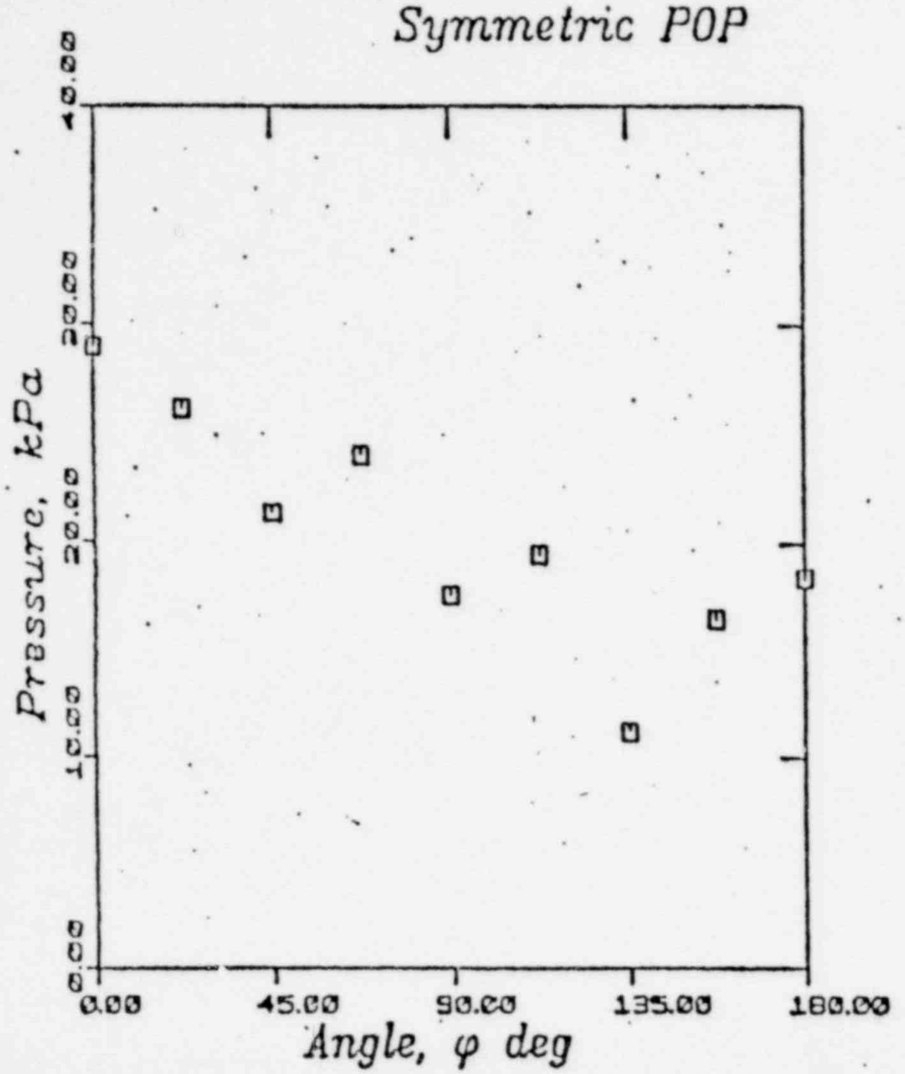


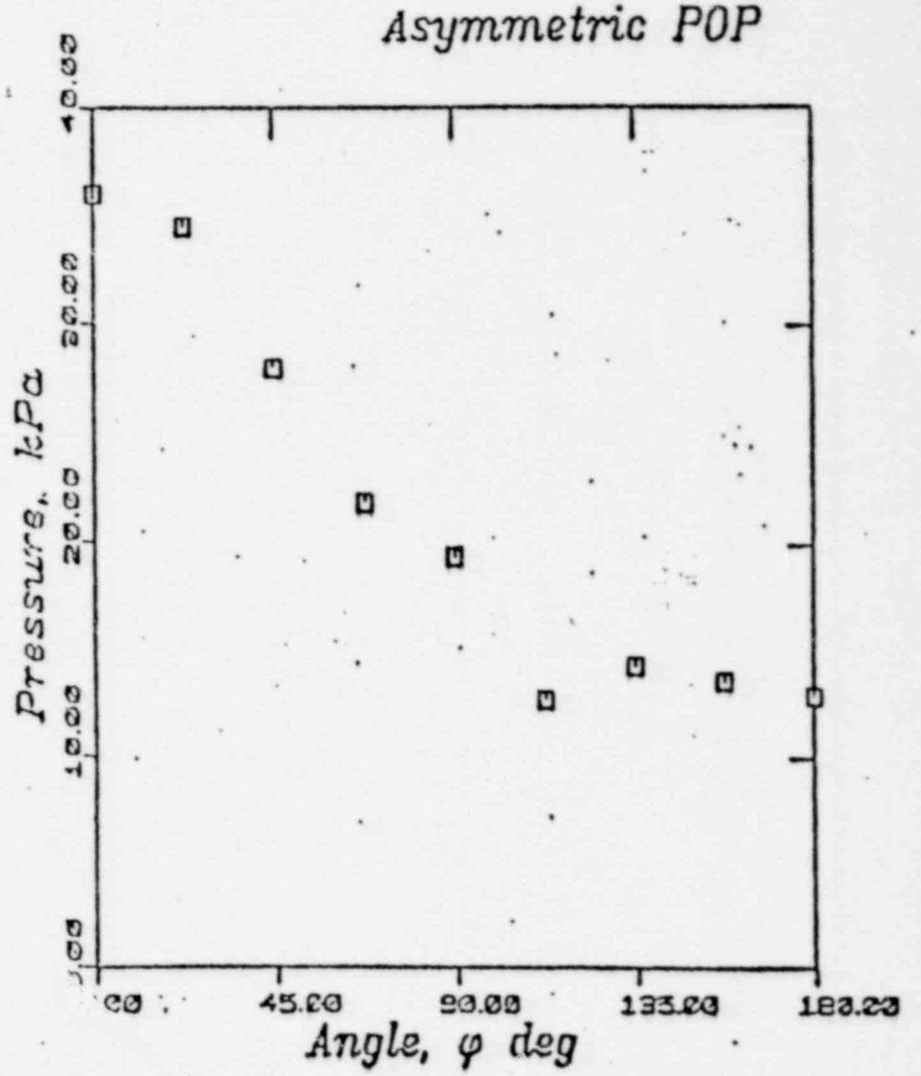
POOR ORIGINAL

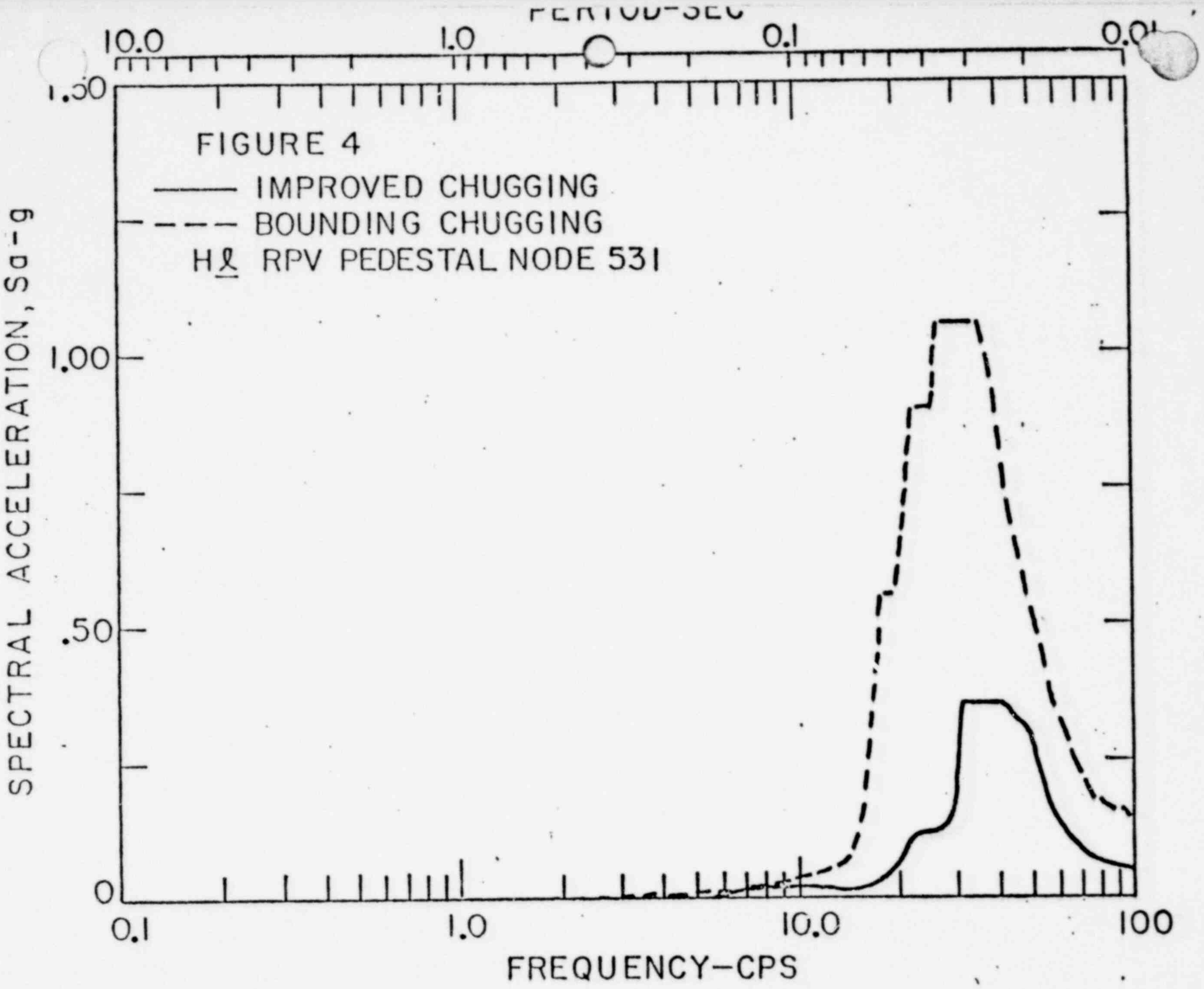
SPECTRUM DENSITY - ASYMMETRIC



1031 216


Symmetric POP
Pressure, kPa
40.00
30.00
20.00
10.00
0.00
0.00
45.00
90.00
135.00
180.00
Angle, φ deg


Asymmetric POP
Pressure, kPa
40.00
30.00
20.00
10.00
.00
.00
45.00
90.00
135.00
180.00
Angle, φ deg


PERIOD-SEC
10.0
1.0
0.1
0.01
FIGURE 4
IMPROVED CHUGGING
BOUNDING CHUGGING
HL RPV PEDESTAL NODE 531
SPECTRAL ACCELERATION, Sa-g
1.50
1.00
.50
0
0.1
1.0
10.0
100
FREQUENCY-CPS

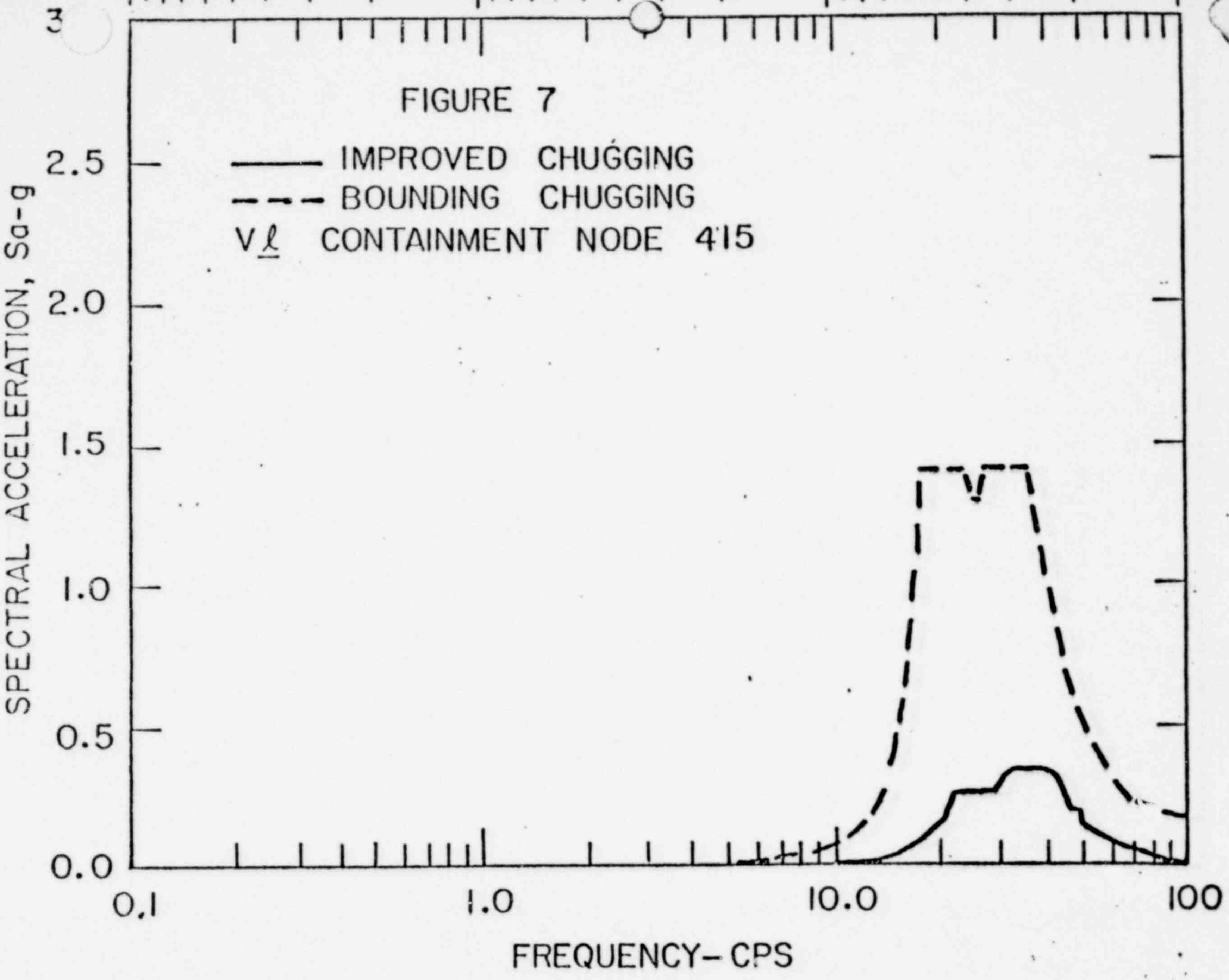


FIGURE 7

IMPROVED CHUGGING
BOUNDING CHUGGING
Vℓ CONTAINMENT NODE 415
SPECTRAL ACCELERATION, Sa-g
3
2.5
2.0
1.5
1.0
0.5
0.0
0.1
1.0
10.0
100
FREQUENCY- CPS

IMPROVED CHUG LOAD

SUMMARY

- Developed Forcing Functions Which Bound Mark II Chugging
- Provided Basis For Modeling Differences Between 4T Experiment and Actual Mark II
- Computed Mark II Acceleration Response Spectra Which Are Significantly Reduced From Results Using Current DFFR Load Specification

J-11

CAORSO

SAFETY/RELIEF VALVE DISCHARGE

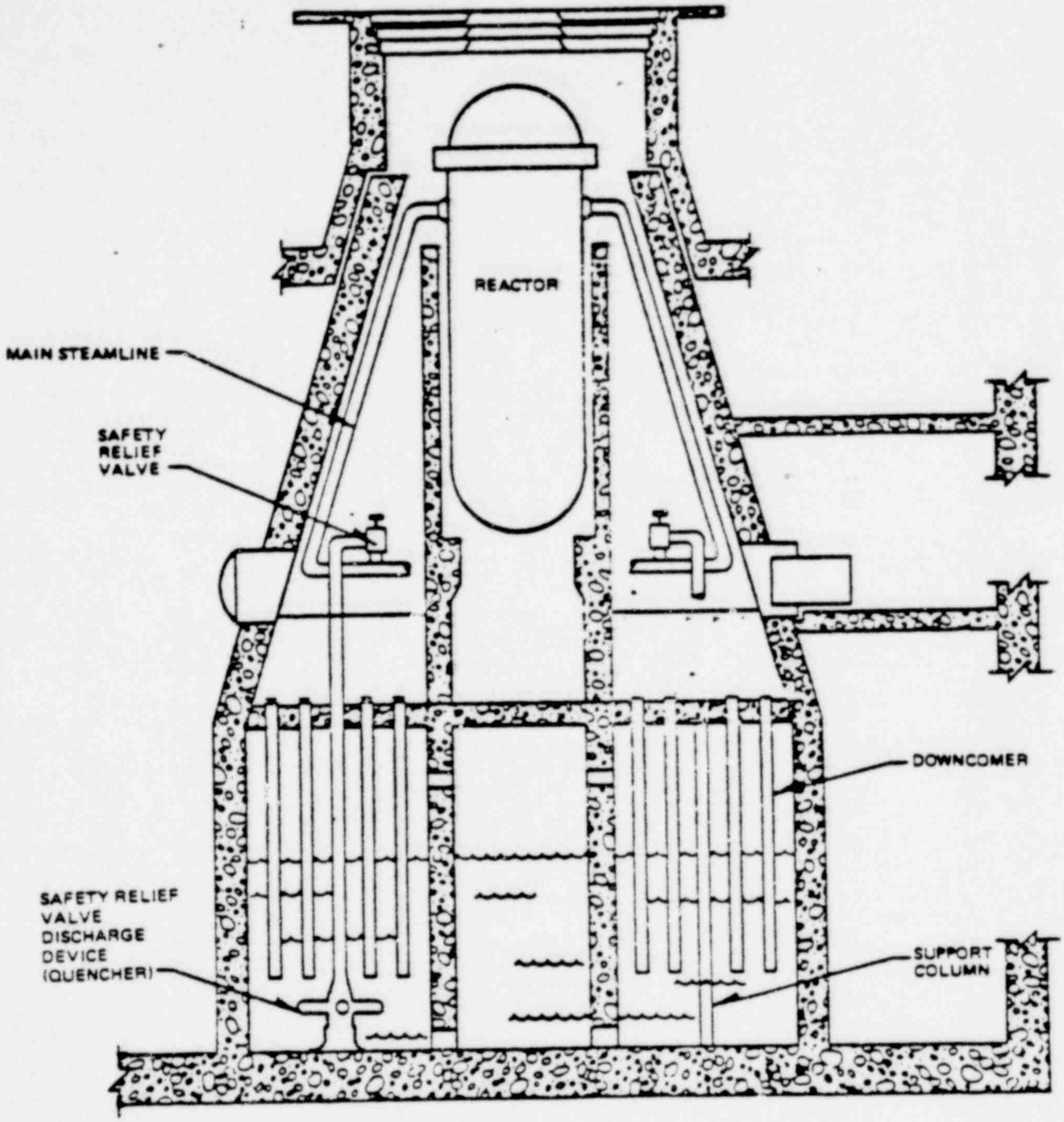
TEST PROGRAM

WMD 9/79

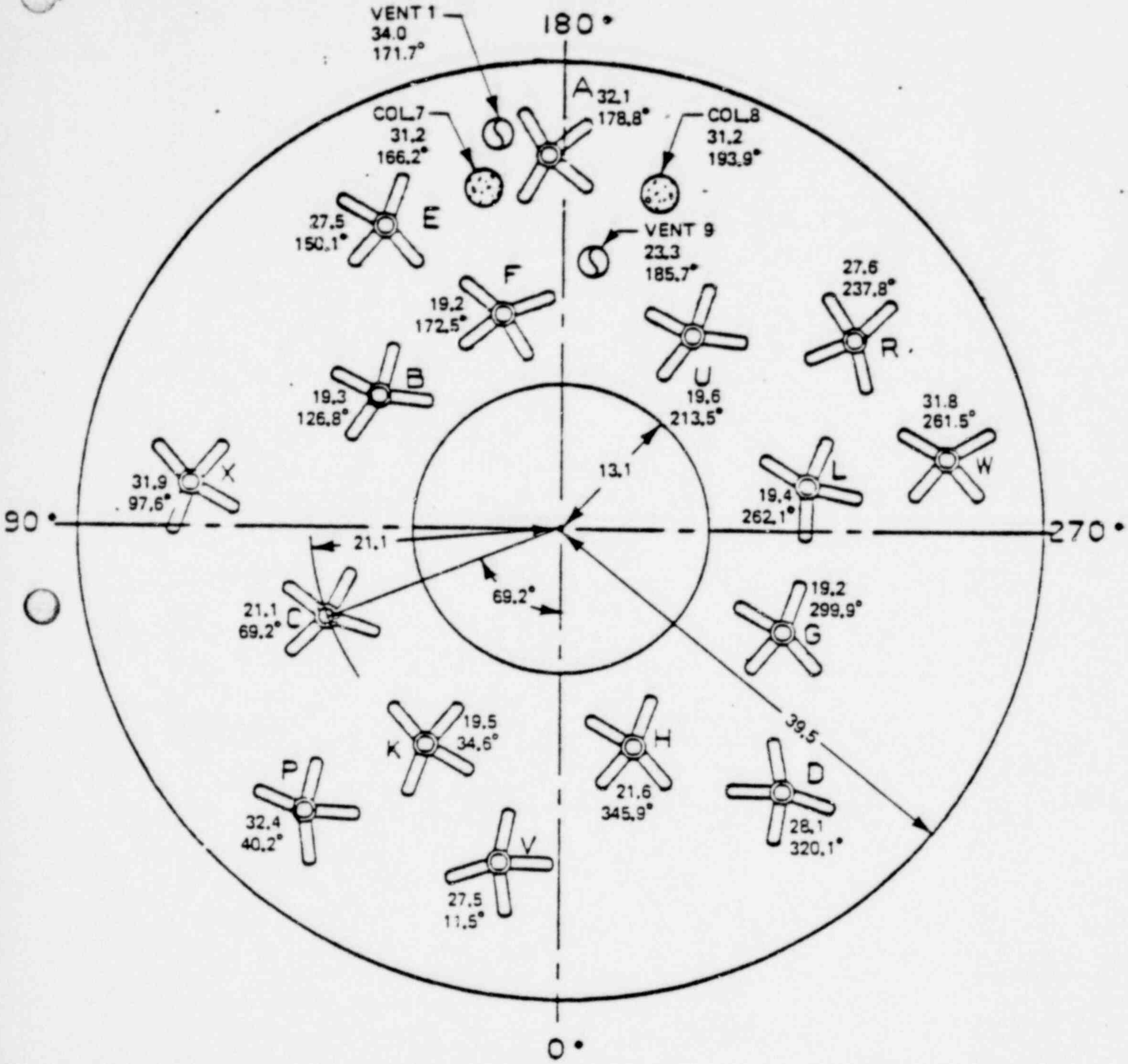
CAORSO TEST

- TEST CONFIGURATION
- TEST OBJECTIVES
- INSTRUMENTATION
- TEST SUMMARY
- PHASE I RESULTS
- PHASE II RESULTS (PRELIMINARY)
- CONCLUSIONS

WJD 9/79



Mark II Pressure Suppression Containment



NOTE:
1. ALL MEASUREMENTS ARE IN FEET
UNLESS OTHERWISE NOTED.

Caorso Quencher Locations

TEST OBJECTIVES

- CONFIRM CROSS QUENCHER LOAD DEFINITION
- PROVIDE DATA BASE FOR FUTURE LOAD REDUCTION
- PROVIDE DATA FOR EVALUATION OF
  - POOL BOUNDRY PRESSURES
  - BUILDING DYNAMIC RESPONSE
  - DISCHARGE LINE CLEARING AND REFLOOD
  - QUENCHER STRUCTURAL RESPONSE
  - POOL THERMAL MIXING
  - SUBMERGED STRUCTURES
  - LINER & DOWNCOMER STRUCTURAL RESPONSE

INSTRUMENTATION

SENSOR	NUMBER
• SUPPRESSION POOL	
PRESSURE	40
TEMPERATURE	10
• QUENCHER & SRVDL	
PRESSURE	11
TEMPERATURE	17
STRAIN GAGE	40
ACCELEROMETER	6
WATER LEVEL	13
• CONTAINMENT STRUCTURE	
ACCELEROMETER	17
STRAIN GAGE	18
• VACUUM BREAKER FLOW	2
• SRV STEM POSITION	4
• MISCELLANEOUS	9

TEST SUMMARY

CONDITION	NUMBER OF VALVE ACTUATIONS
PHASE I	
SINGLE VALVE FIRST ACTUATION	23
SINGLE VALVE CONSECUTIVE ACTUATION	29
VARIED: WATER LEG	
PIPE TEMPERATURE	
VACUUM BREAKER AREA	
VALVE ACTUATED	
PHASE II	
SINGLE VALVE FIRST ACTUATION	11
SINGLE VALVE CONSECUTIVE ACTUATION	16
MULTIPLE VALVE ACTUATION	11
LEAKY VALVE FIRST ACTUATION	5
LEAKY VALVE CONSECUTIVE ACTUATION	8
SINGLE VALVE EXTENDED BLOW DOWN	1
VARIED: NUMBER OF VACUUM BREAKERS	
PIPE TEMPERATURE	
VESSEL PRESSURE	
VALVE GROUPINGS	

WMD 9/79

PHASE I RESULTS*

- POOL BOUNDARY PRESSURES

	MAX TEST	DFFR PREDICTIONS MEAN	DFFR PREDICTIONS 90-90
• 1st ACTUATION			
POSITIVE	4.8	8.5	12.2
NEGATIVE	4.3	6.2	7.9
• CONSECUTIVE ACTUATION			
POSITIVE	8.0	14.8	23.2
NEGATIVE	5.7	8.9	11.4

- PREDOMINANT BUBBLE FREQUENCIES 5-11 Hz
- OBSERVED PRESSURE-TIME ATTENUATION MORE RAPID THAN DFFR METHODOLOGY
- OBSERVED PRESSURE DISTANCE ATTENUATION MORE THAN DFFR METHODOLOGY

* A DETAILED EVALUATION OF CAORSO DATA IS BEING PERFORMED BY B&R INC. TO IMPROVE SRV LOAD DEFINITION FOR APPLICATION ON WPPSS-NP #2

WMD 9/79

- OTHER PARAMETERS
  - DISCHARGE LINE PRESSURE AGREED WELL WITH PREDICTIONS
  - CONFIRMED EFFECTS OF VACUUM BREAKER SIZE ON REFLOOD TRANSIENT
  - DYNAMIC STRESSES ON QUENCHER LESS THAN PREDICTED
  - MAXIMUM MEASURED BUILDING RESPONSE FROM SUPPRESSION POOL LOADS WERE BELOW .07G
  - LINER STRAINS WELL BELOW PREDICTIONS
  - BENDING STRAINS ON DOWNCOMER WELL BELOW PREDICTIONS

WMD 9/79

PHASE II RESULTS*

PRELIMINARY

- SINGLE VALVE RETESTS CONSISTENT WITH PHASE I
- MULTIPLE VALVE POOL BOUNDARY PRESSURES

	MAX TEST	DFFR PREDICTIONS MEAN	 90-90
• 4 VALVES			
POSITIVE	6.5	9.9	14.0
NEGATIVE	4.8	6.9	8.5
• 8 VALVES			
POSITIVE	5.5	11.7	17.0
NEGATIVE	4.8	7.7	10.0

- THIRTEEN MINUTE BLOWDOWN RESULTED IN LOCAL TO BULK $\Delta T \approx 10^{\circ}$

WMD 9/79

CONCLUSIONS

- TEST OBJECTIVES MET
- DATA CONSISTENT AND REPEATABLE
- COMPARES WELL WITH PIPE PREDICTIONS
- WELL BELOW PREDICTIONS FOR POOL BOUNDARY PRESSURES
- DFFR METHODOLOGY CONSERVATIVE *

WMD 9/79

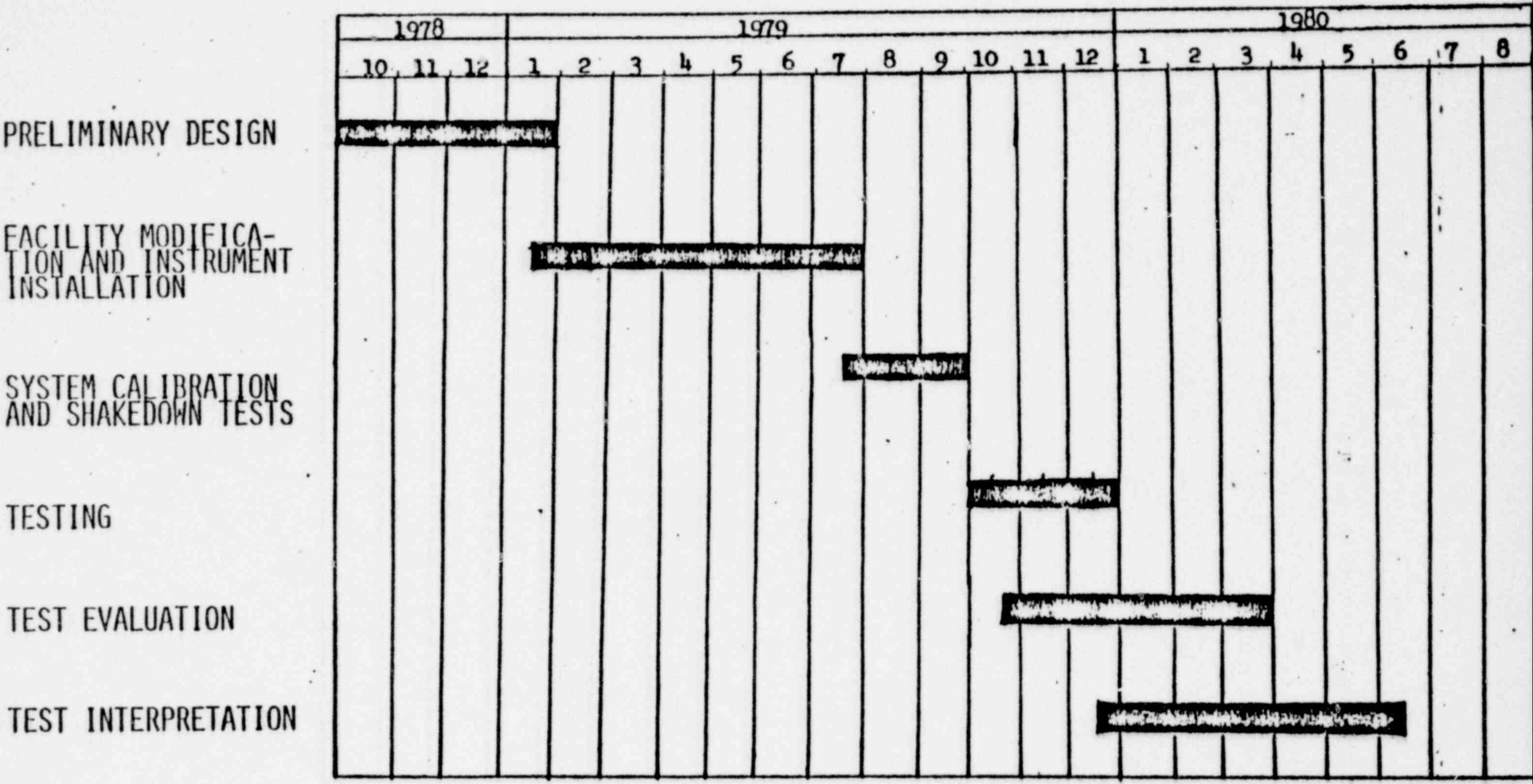
T-12

PP&L GKM-IIM TEST PROGRAM

- DECISION TO PROCEED WITH GKM-IIM MADE IN JANUARY, 1979
- SUSQUEHANNA SES FUEL LOAD WAS THEN SCHEDULED FOR MAY, 1980
- THE GKM-IIM TESTS PROVIDED DATA EARLIER THAN THE 4T TEST AND THE MORE PROTOTYPICAL NATURE OF THE FACILITY ALLOWED FOR A MORE EXPEDITIOUS EVALUATION OF DATA AND SUBSEQUENT LICENSING REVIEW

DFR 9/79

GKM-II-M SCHEDULE



Comparision of Fixed Parameters

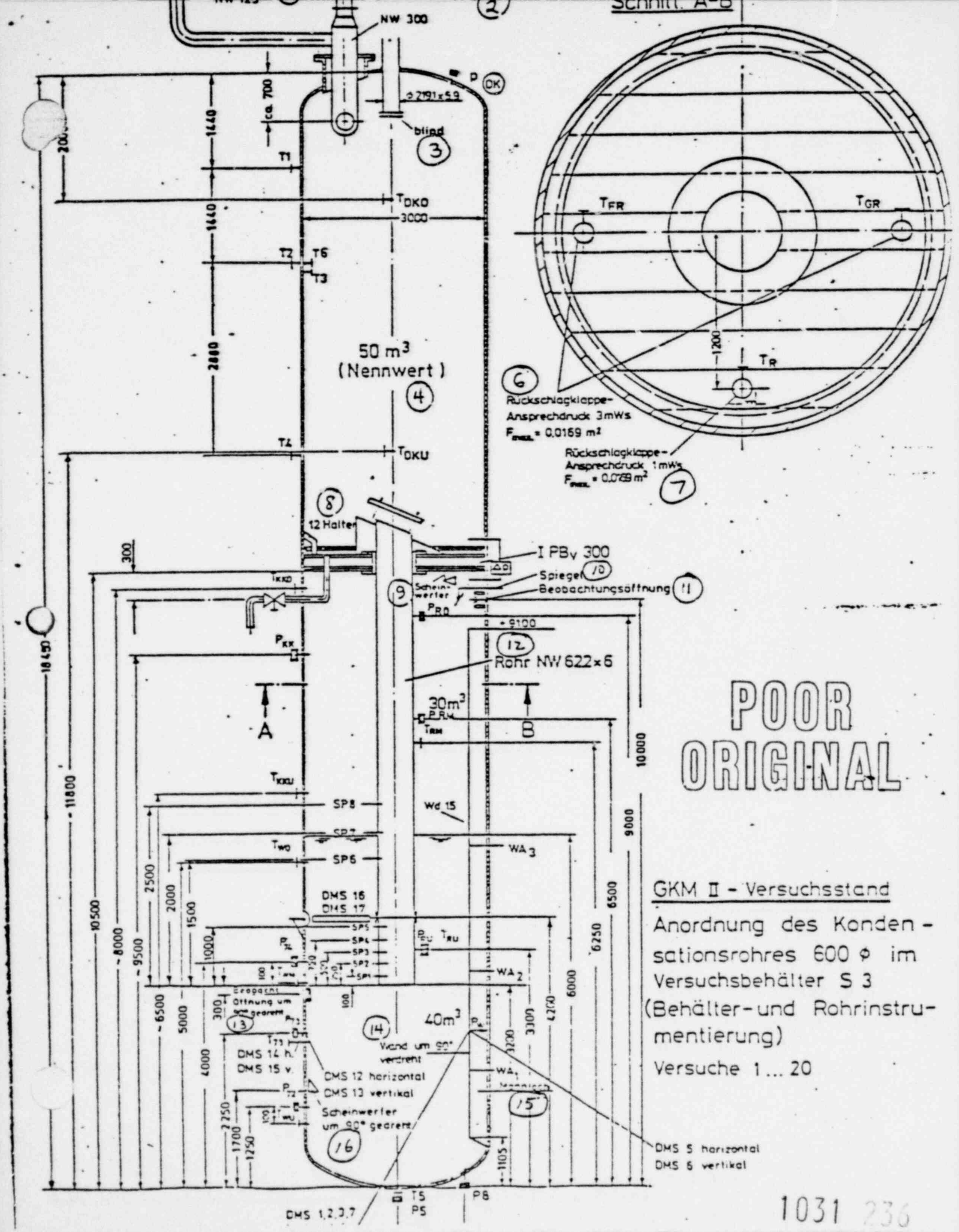
	SSES Single Cell	GKM II M Test Vessel (Design Values)
Drywell Free Volume, m^3	75	75
(including Vent Pipe)	78	78
Wetwell Free Air Volume, m^3 (normal water level)	50	50
Drywell/Wetwell Air Volume Ratio	1.5	1.5
Free Pool Area, m^2		
Small Cell at Containment Wall	3.66	3.66
Mean Value	5.64	
Vent Pipe Dimensions		
Length, m	13.86	13.86
Outer Diameter, mm	610	610
Wall Thicknes, mm	9,5	9,5
Vent Pipe Submergence, m (high water level)	3.66	3.66
Vent Pipe Clearence, m	3.66	3.66
Distance between Bracing and Vent Opening, m	2.44	2.44
Volume Flexibility of Wet Containment Walls, dm^3/bar	0.6	0.6

PP&L GKM-IIM TEST PROGRAM

FACILITY MODIFICATIONS INCLUDED:

- REMOVAL OF FLEXIBLE WALL
- ADDITION OF NEW DRYWELL
- ADDITION OF INNER CYLINDER
- STIFFENING OF TANK FOUNDATION
- ADDITION OF PROTOTYPICAL VENT AND BRACING SYSTEM
- ADDITION OF VIEWING PORT
- ADDITION OF SUBMERGED STRUCTURES TARGETS
    - QUENCHER ARM
    - WIDE-FLANGE BEAM

DFR 9/79



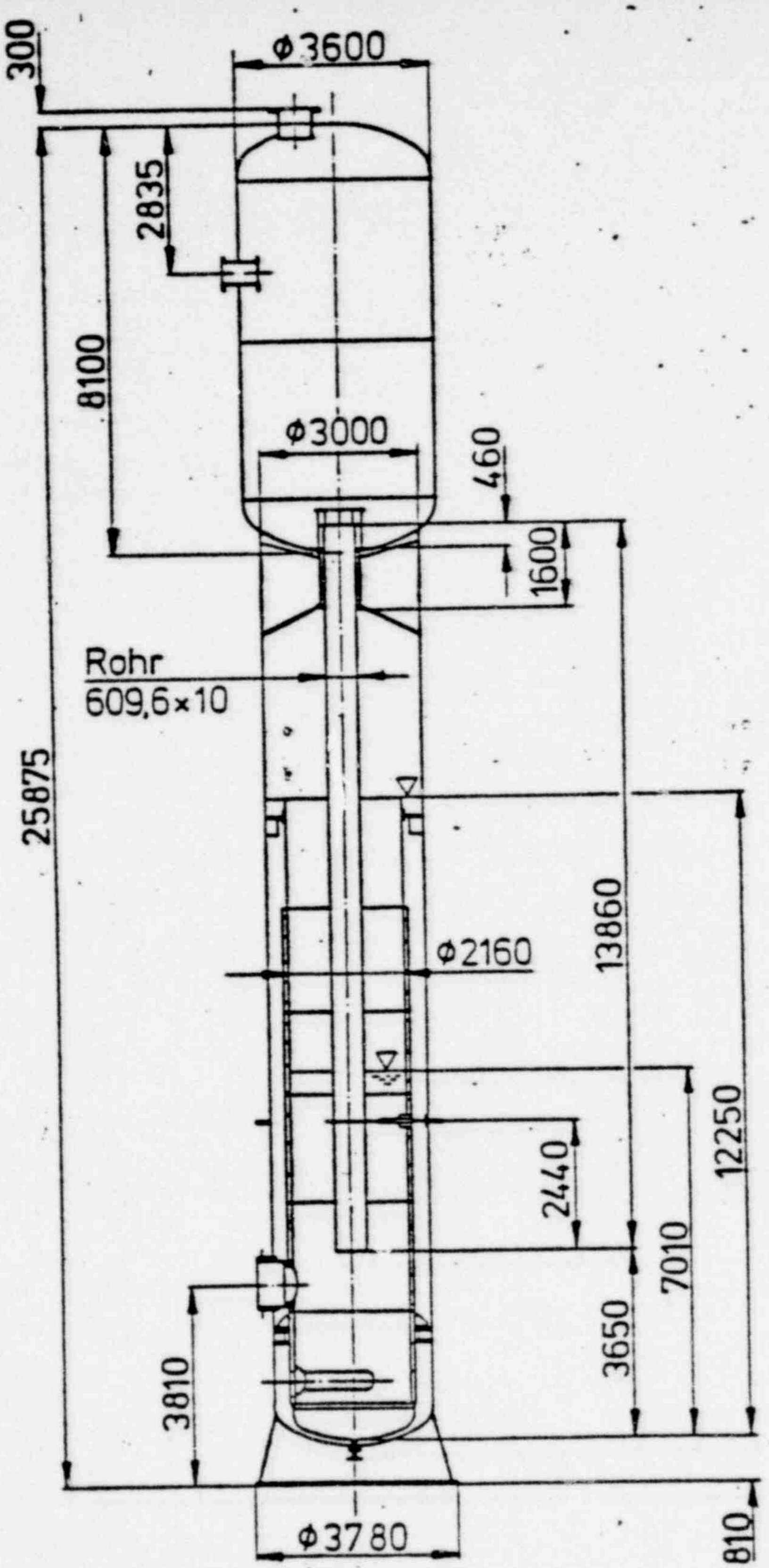
POOR ORIGINAL

GKM II - Versuchsstand

Anordnung des Kondensationsrohres 600 ϕ im Versuchsbehälter S 3 (Behälter- und Rohrinstrumentierung)

Versuche 1 ... 20

1031 236



GKM II-M-Condensation Tests

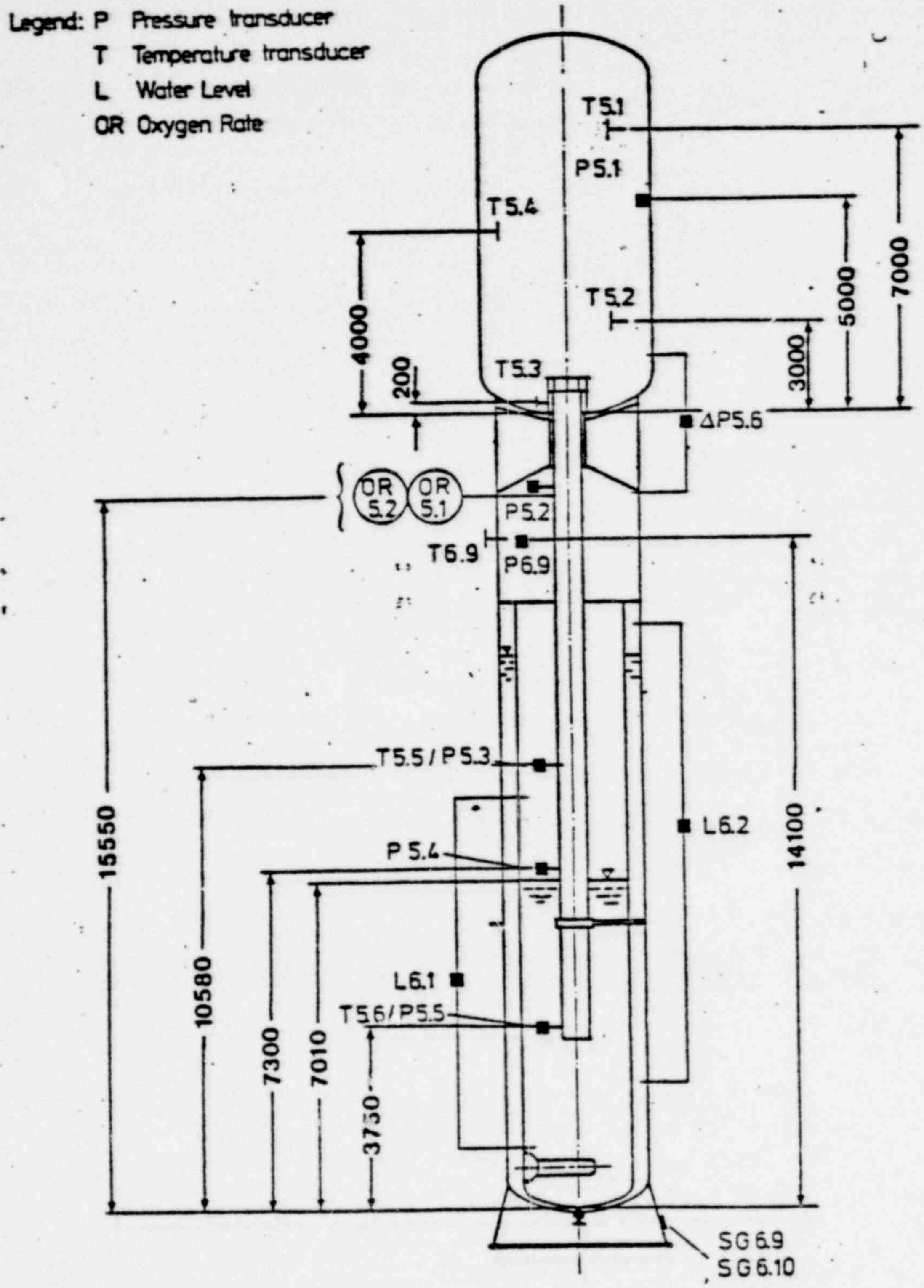
Test Tank

PP&L GKM-IIM TEST PROGRAM

APPROXIMATELY 60 CHANNELS OF TEST INSTRUMENTATION

MEASUREMENT VALUE INCLUDE:

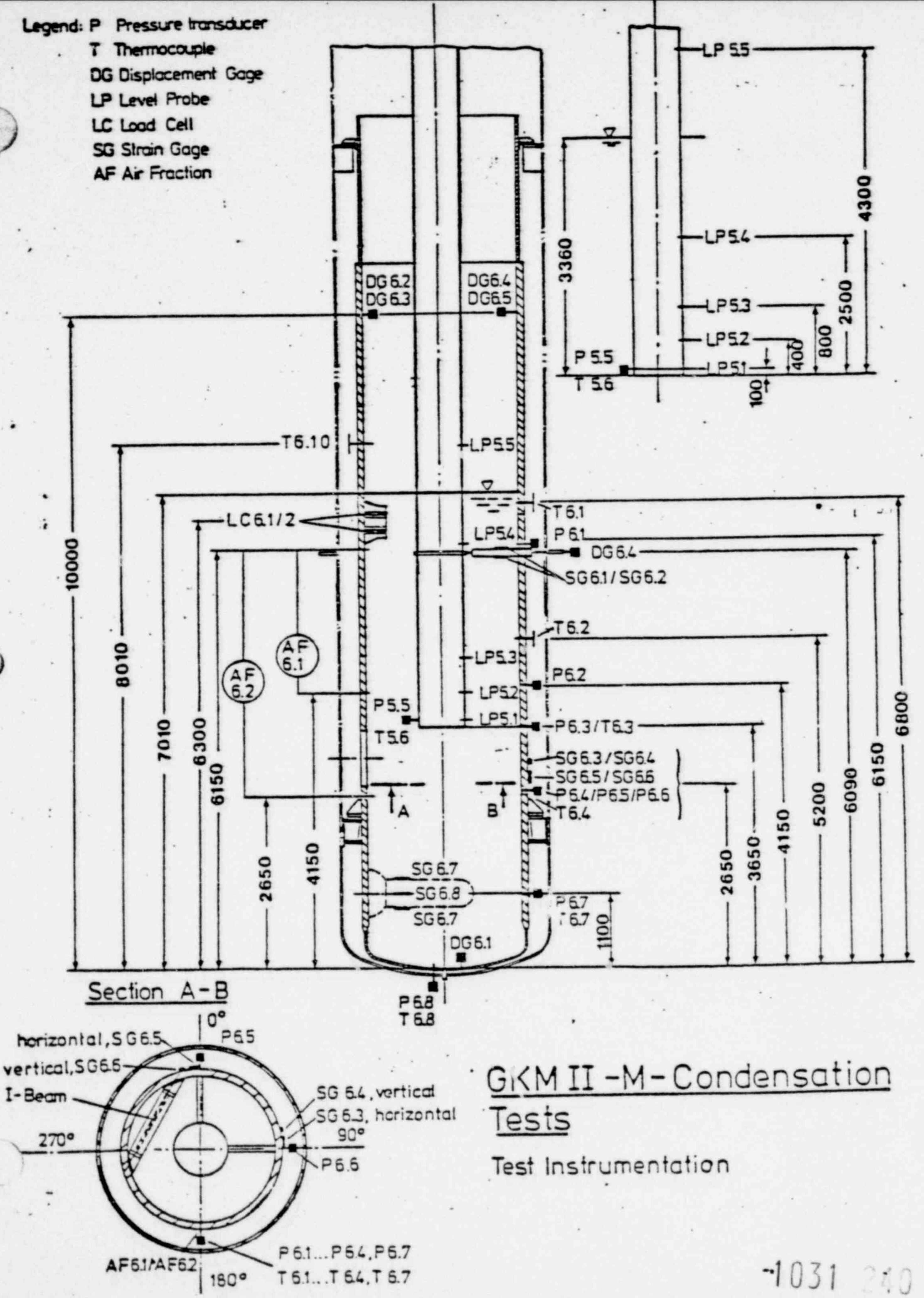
- PRESSURES AND TEMPERATURES IN THE DRYWELL, VENT AND WETWELL AIRSPACE AND WETTED BOUNDARY
- WATER LEVEL IN THE VENT
- AIR CONTENT IN THE VENT
- DISPLACEMENT AND STRAIN ON THE VESSEL
- STRAIN ON THE VENT BRACING AND QUENCHER ARM
- FORCE ON THE TARGET BEAM
- MOVIES OF VENT EXIT



GKM II-M-Condensation Tests

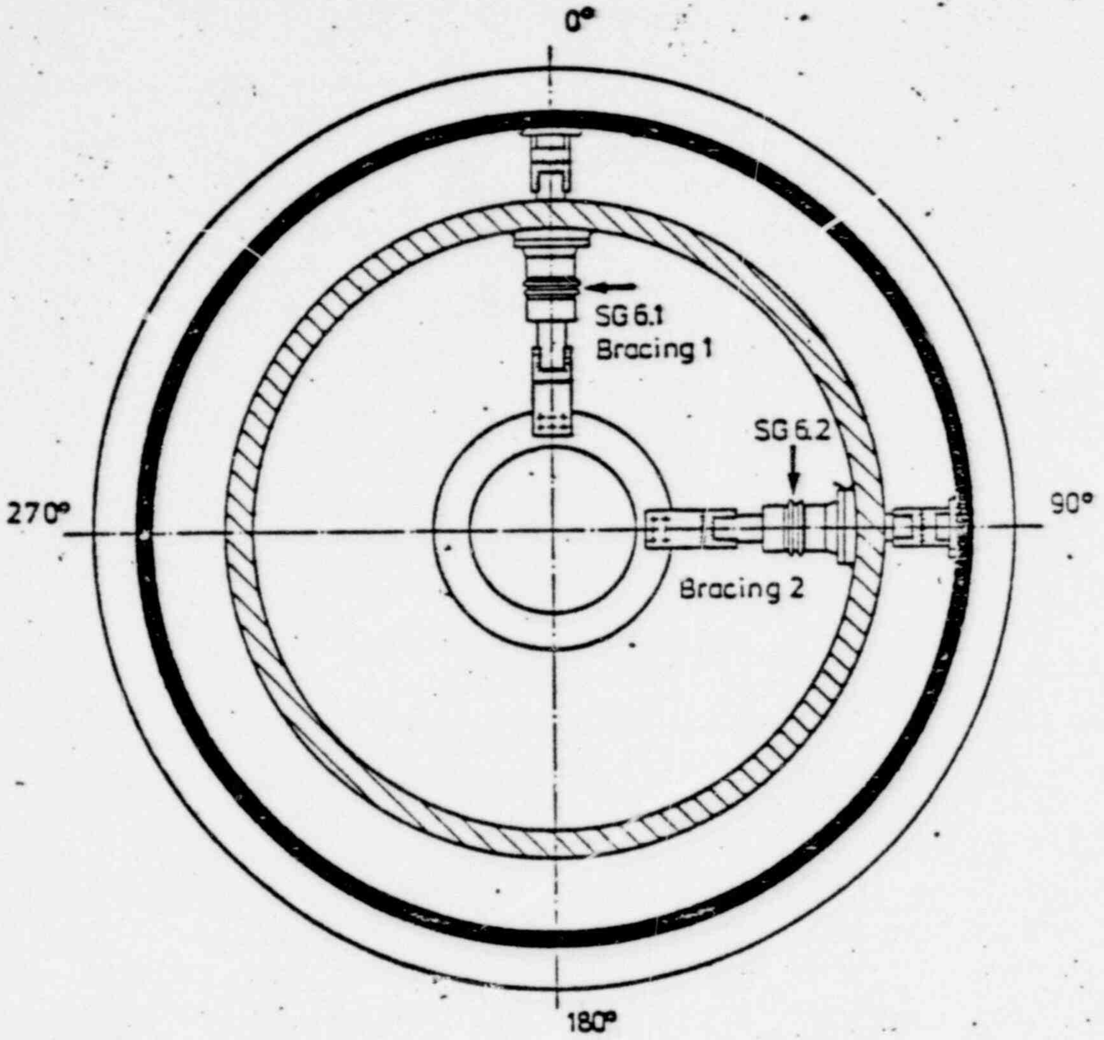
Test Instrumentation

Legend: P Pressure transducer
T Thermocouple
DG Displacement Gage
LP Level Probe
LC Load Cell
SG Strain Gage
AF Air Fraction



GKM II -M- Condensation Tests

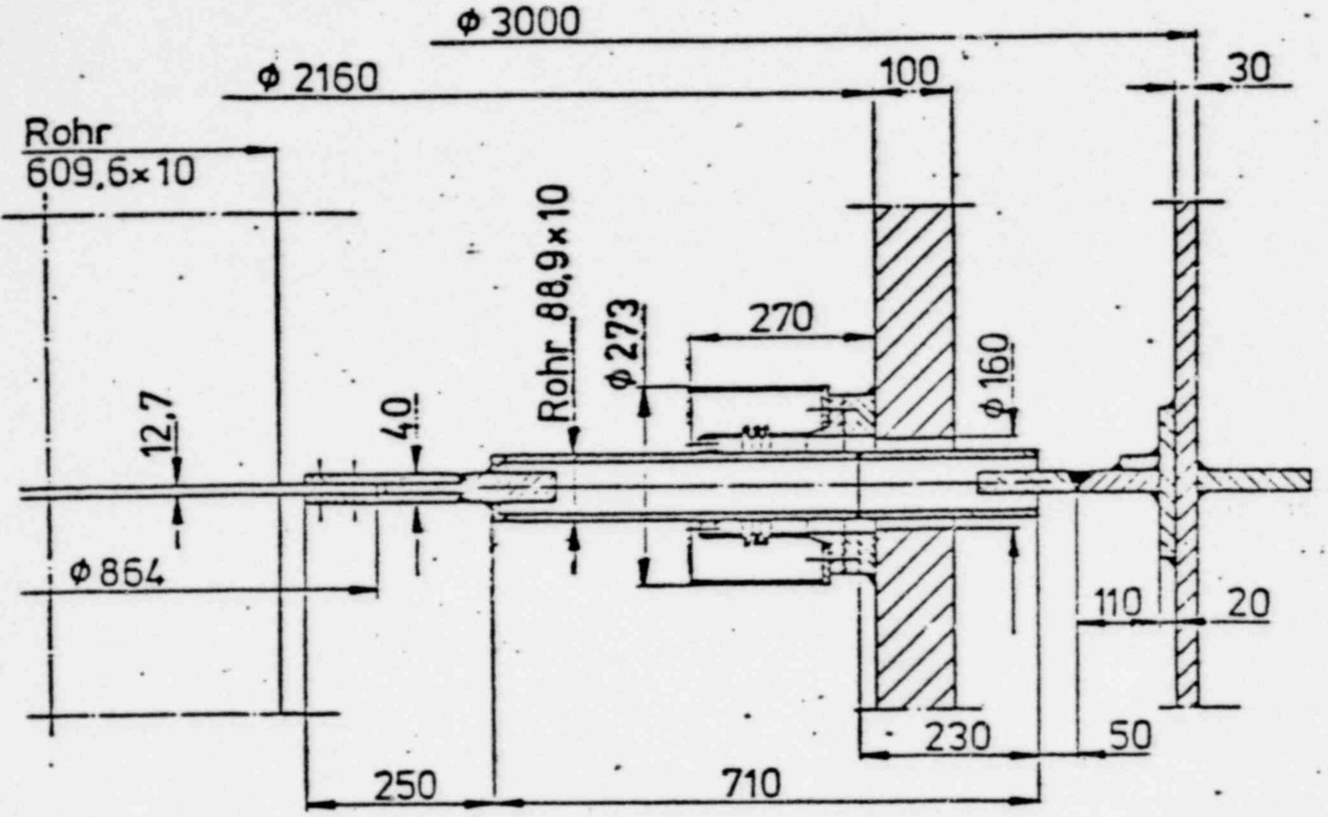
Test Instrumentation

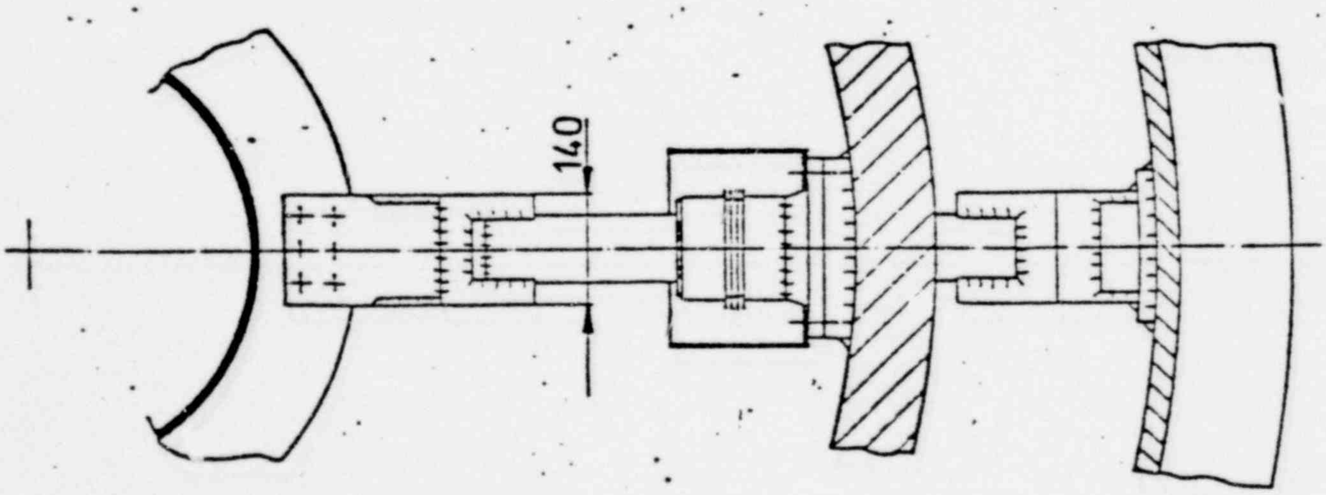


GKM II-M-Condensation Tests

Bracing Configuration

1031 241


ϕ 3000
ϕ 2160
100
30
Rohr
609,6×10
Rohr 88,9×10
ϕ 273
270
ϕ 160
12,7
40
ϕ 864
110
20
230
50
250
710


140

GKM II M Test Matrix

Test Number		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
Break Size (mm)	Ø 210 (RCL)	*	*																		
	Ø 190 (MSL)			*	*	*	*	*	*	*	*										
	Ø 110 (1/3 MSL)											*	*								
	Ø 80 (1/6 MSL)													*	*	*	*	*	*	*	*
Pool Temperature	24°C (75 F)			*	*									*	*						
	32°C (90 F)	*	*			*	*	*	*			*	*			*	*	*	*		
	55°C (130 F)									*	*									*	*
Drywell Air Content	100 %	*	*	*	*	*	*			*	*	*	*	*	*	*	*			*	*
	85 %							*	*									*	*		
Repeat Test			*		*		*		*		*		*		*		*		*		*

-12,13

PRESENTATION TO ACRS SUBCOMMITTEE MEETING
OF SEPTEMBER 13, 1979

CHUGGING LOADS - IMPROVED DEFINITION AND APPLICATION METHODOLOGY TO MARK II CONTAINMENTS

DEVELOPED BY BURNS AND ROE, INC.

FOR

APPLICATION ON WPPSS - WNP #2

B.B,
9/13/79

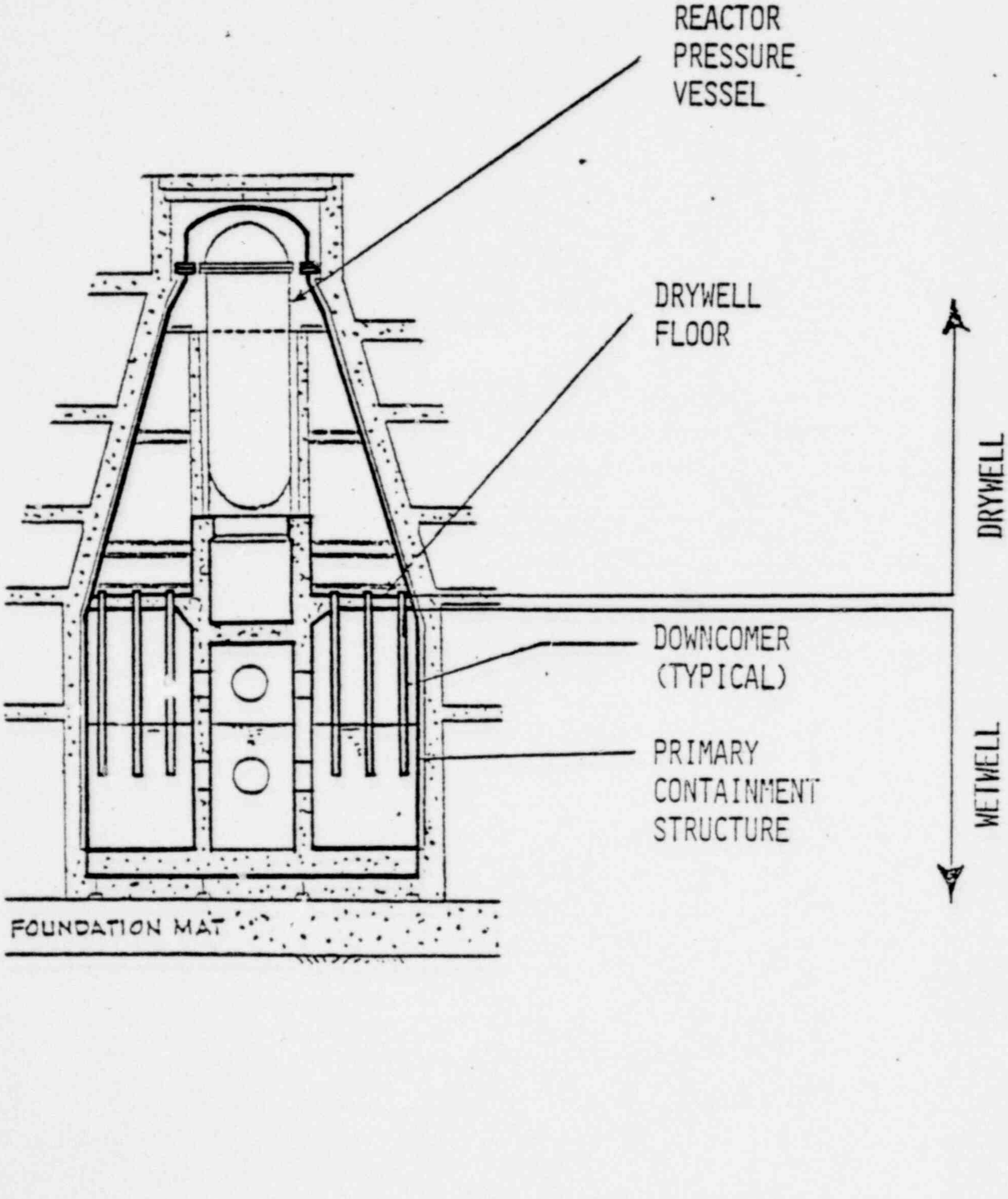
BACKGROUND

- MEETINGS WITH NRC STAFF IN LATE 1978
- MEETINGS WITH NRC STAFF AND CONSULTANTS IN EARLY 1979
- SUMMARY REPORT SUBMITTED TO NRC IN APRIL 1979
- TECHNICAL REPORT SUBMITTED TO NRC IN JUNE 1979

B.B.
9/13/79

MARK II CONTAINMENT:

CROSS-SECTIONAL VIEW



E.B.
9/13/79

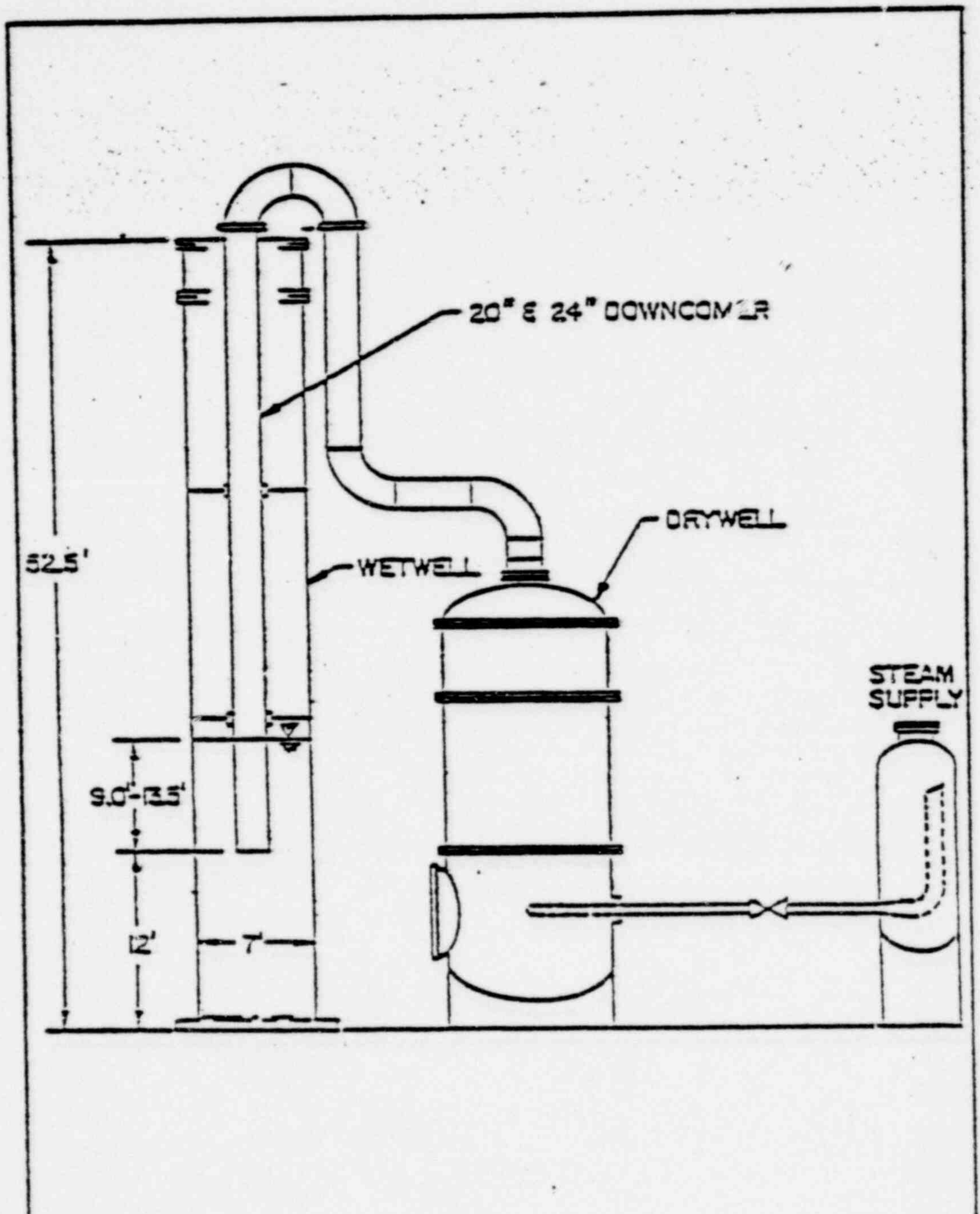
EXPERIMENTAL DATA BASE DEVELOPED IN GE'S 4T FACILITY

FOR MARK II PLANTS.

- 4T TESTS ARE REPRESENTATIVE OF MARK II PLANT CONDITIONS DURING LOCA;
- 4T FACILITY APPROXIMATES A "UNIT-CELL" OF A MARK II CONTAINMENT.

B.B.
9/13/79

SCHEMATIC OF THE 4-T TEST FACILITY

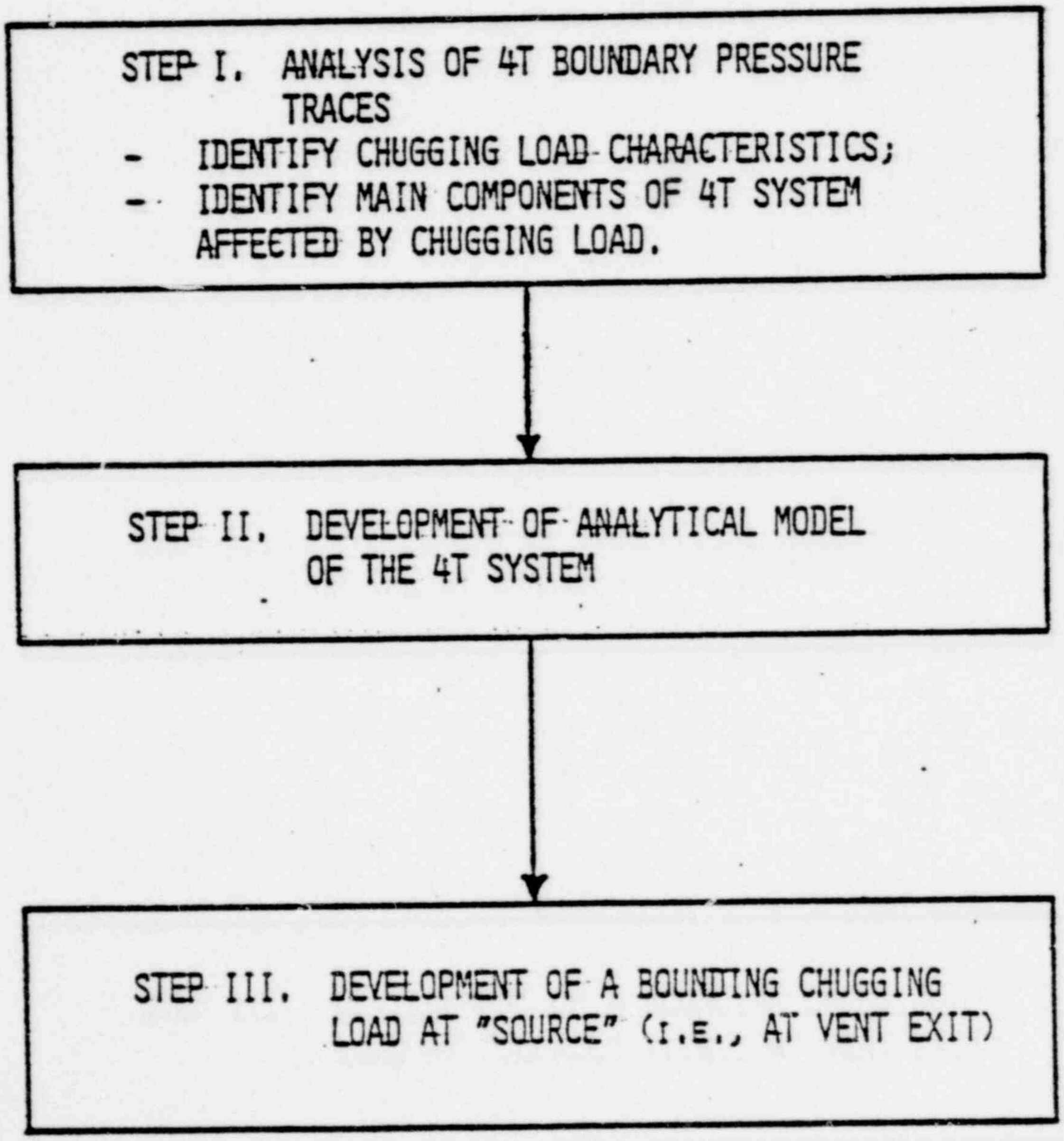


B.B.
9/13/79

SINGLE VENT DESIGN LOAD
SPECIFICATION

B.B.
9/13/79

DEVELOPMENT OF IMPROVED CHUGGING LOAD - FLOW CHART



STEP I. ANALYSIS OF 4T BOUNDARY PRESSURE TRACES

- IDENTIFY CHUGGING LOAD CHARACTERISTICS;
- IDENTIFY MAIN COMPONENTS OF 4T SYSTEM AFFECTED BY CHUGGING LOAD.

STEP II. DEVELOPMENT OF ANALYTICAL MODEL OF THE 4T SYSTEM

STEP III. DEVELOPMENT OF A BOUNDING CHUGGING LOAD AT "SOURCE" (I.E., AT VENT EXIT)

B.B.
9/13/79

STEP I. ANALYSIS OF 4T BOUNDARY PRESSURE TRACES

A. IDENTIFY CHUGGING LOAD CHARACTERISTICS

- APPARENT WIDE VARIETY OF PRESSURE TRACES;
- SAME DISCRETE SET OF MAIN FREQUENCIES IDENTIFIED IN ALL TRACES (APPROX.);
- RANDOM TRENDS OBSERVED;
- IMPULSIVE NATURE OF CHUGGING LOAD.

B.B.
9/13/79

STEP I. ANALYSIS OF 4T BOUNDARY PRESSURE TRACES (CONT'D)

B. IDENTIFY MAIN COMPONENTS OF 4T SYSTEM AFFECTED BY CHUGGING LOAD.

- VENT FREQUENCIES;
- WATER-TANK-SUPPORT FREQUENCY.

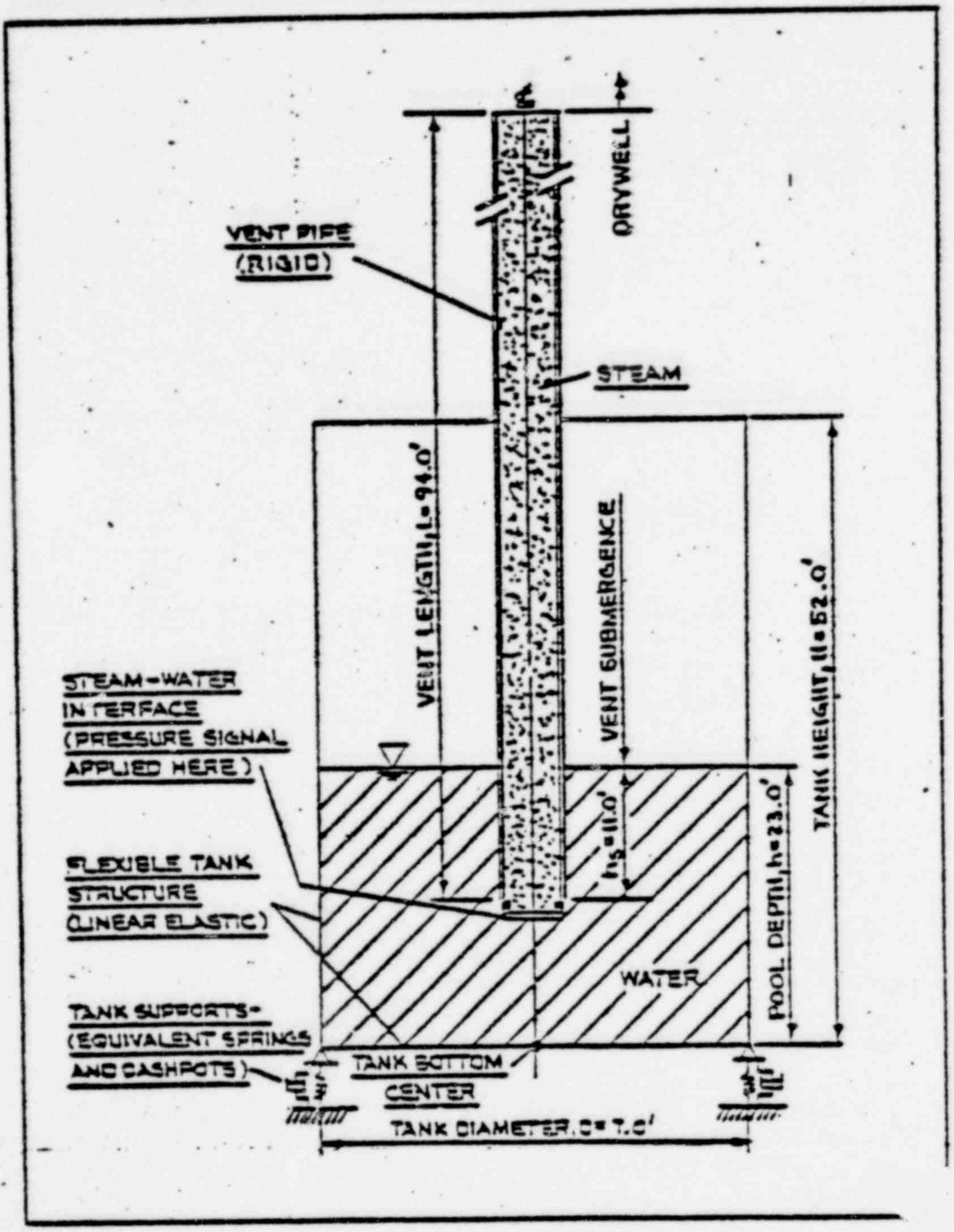
B.B.
9/13/79

STEP II. DEVELOPMENT OF ANALYTICAL MODEL OF 4T SYSTEM

B.B.
9/13/79

MATHEMATICAL MODEL OF 4T TANK

(REDUCED MODEL)



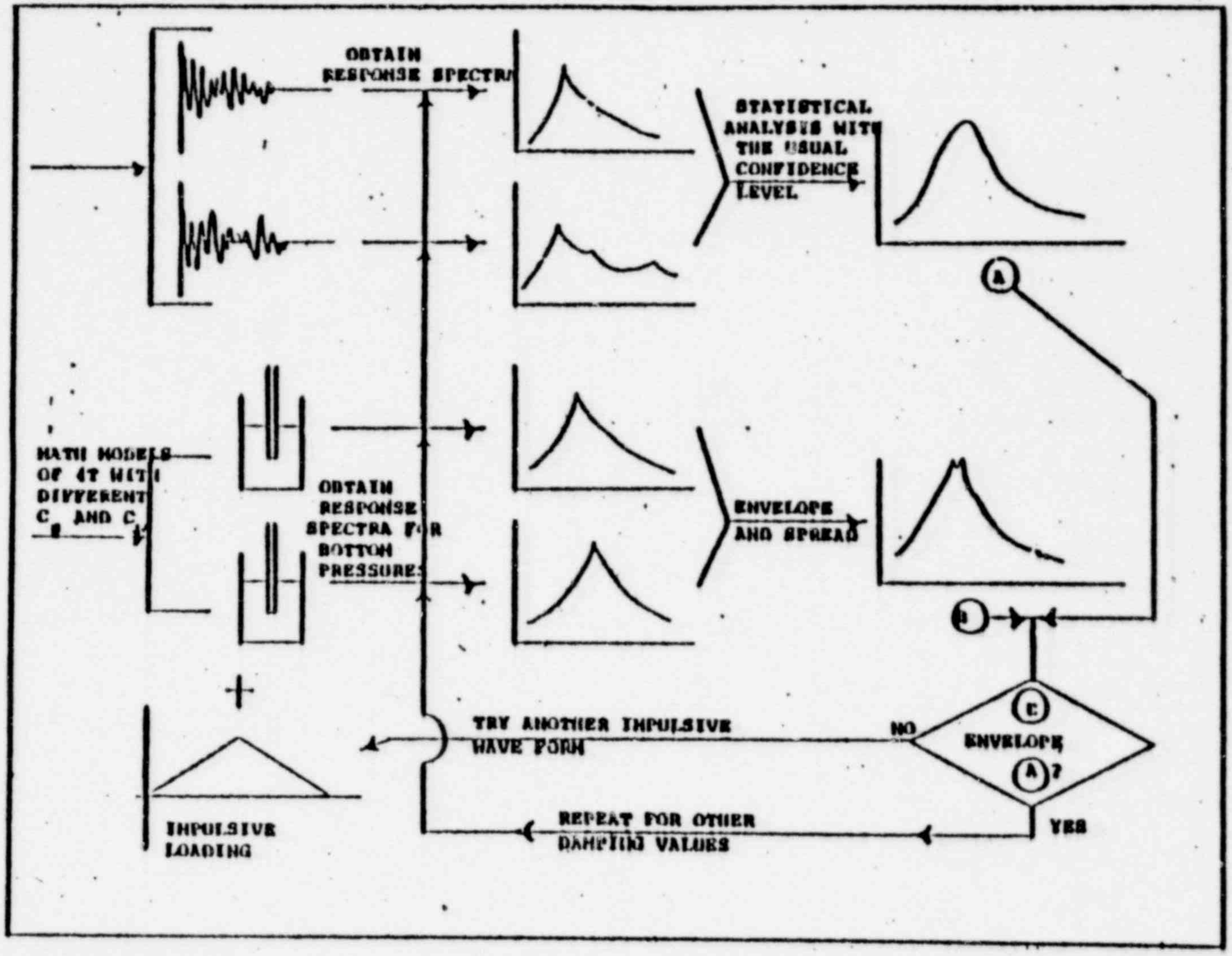
POOR ORIGINAL

B.B.
9/13/79

STEP III. DEVELOPMENT OF BOUNDING CHUGGING LOAD AT "SOURCE" (I.E., AT VENT EXIT)

- PERFORMED STATISTICAL ANALYSIS OF DATA;
- DEFINED THE DESIGN LEVEL LOAD;
- DEVELOPED THE DESIGN LOAD SPECIFICATION.

DEVELOPMENT OF A BOUNDING CHUGGING LOAD AT "SOURCE" - FLOW CHART

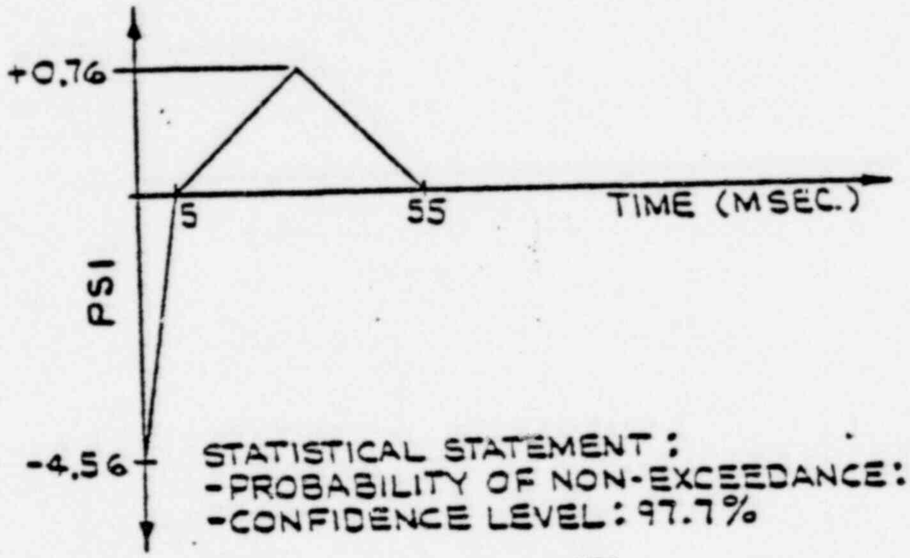


POOR ORIGINAL

B.B.
9/13/79

1031 256

DESIGN LOAD AT SOURCE



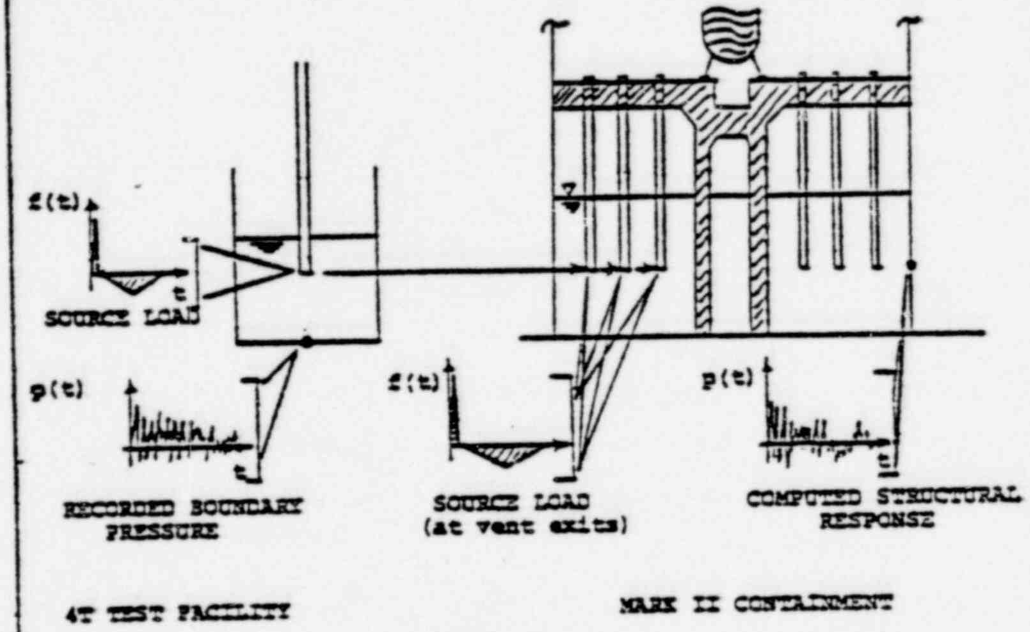
STATISTICAL STATEMENT:
- PROBABILITY OF NON-EXCEEDANCE: 50%
- CONFIDENCE LEVEL: 97.7%

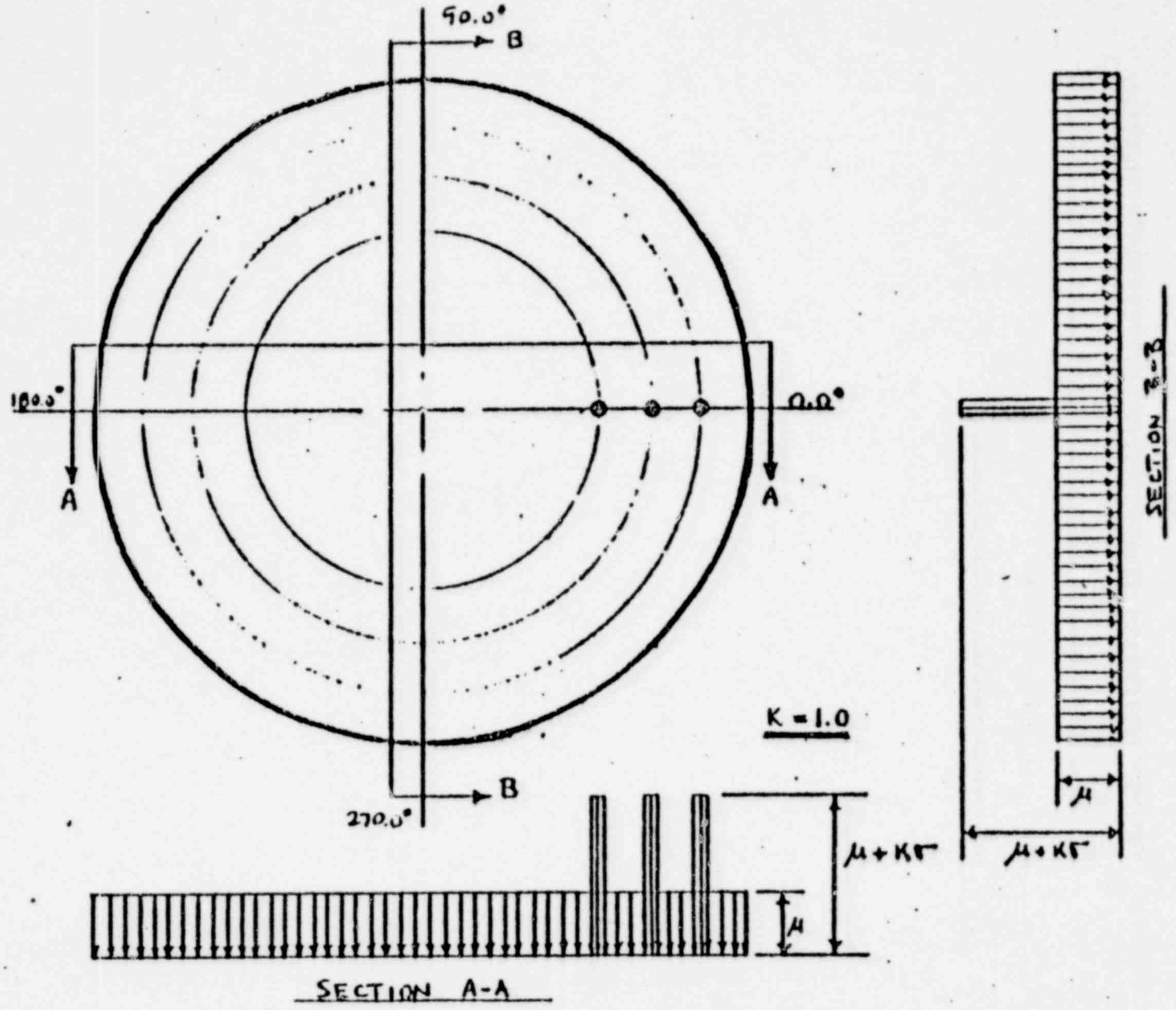
B.B.
9/13/79

CHUGGING LOAD - APPLICATION METHODOLOGY TO MARK II CONTAINMENTS

B.B.
9/13/79

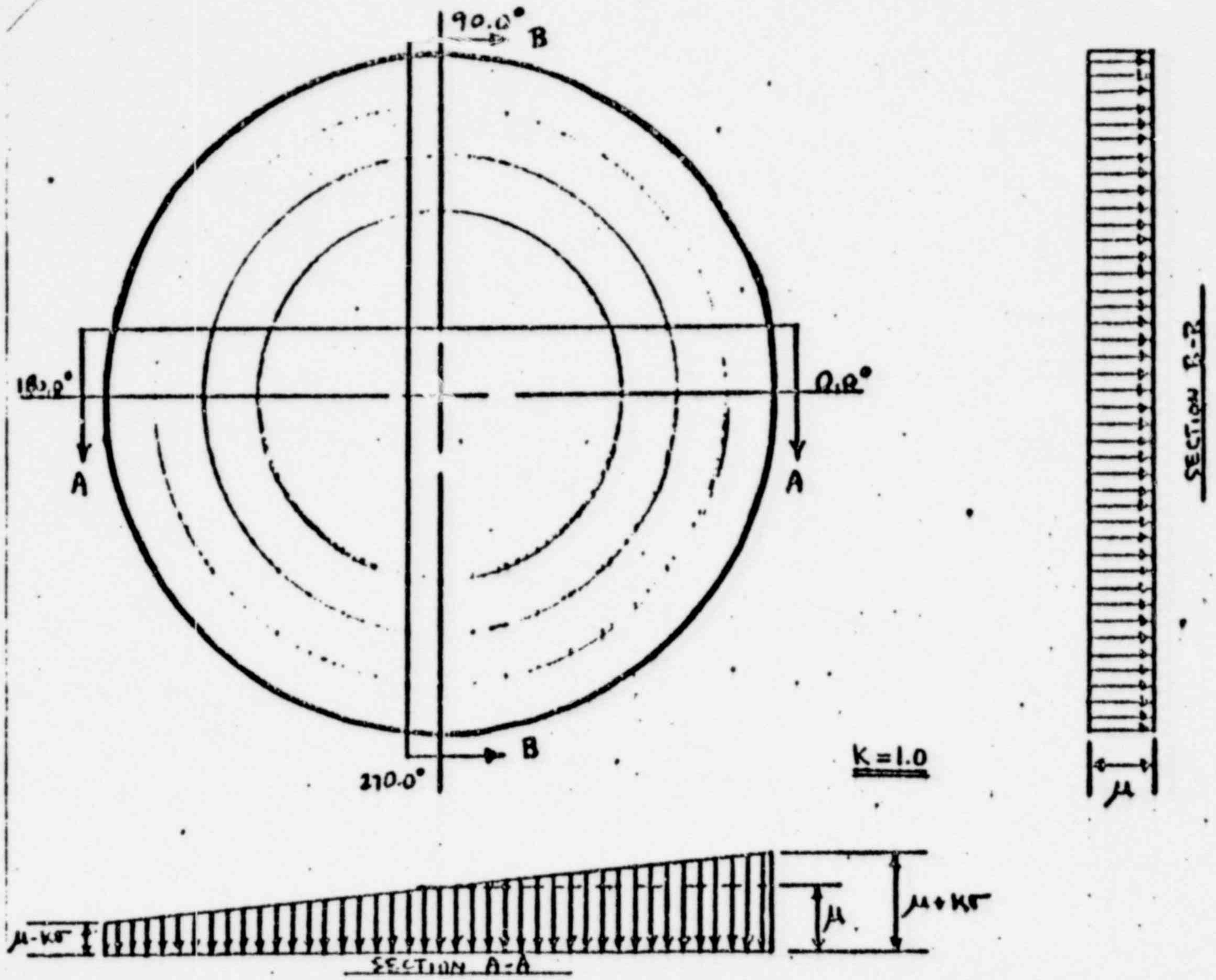
APPLICATION METHODOLOGY TO MARK II CONTAINMENTS - DIAGRAM





I - MAINLY SYMMETRICAL LOADING CONDITION

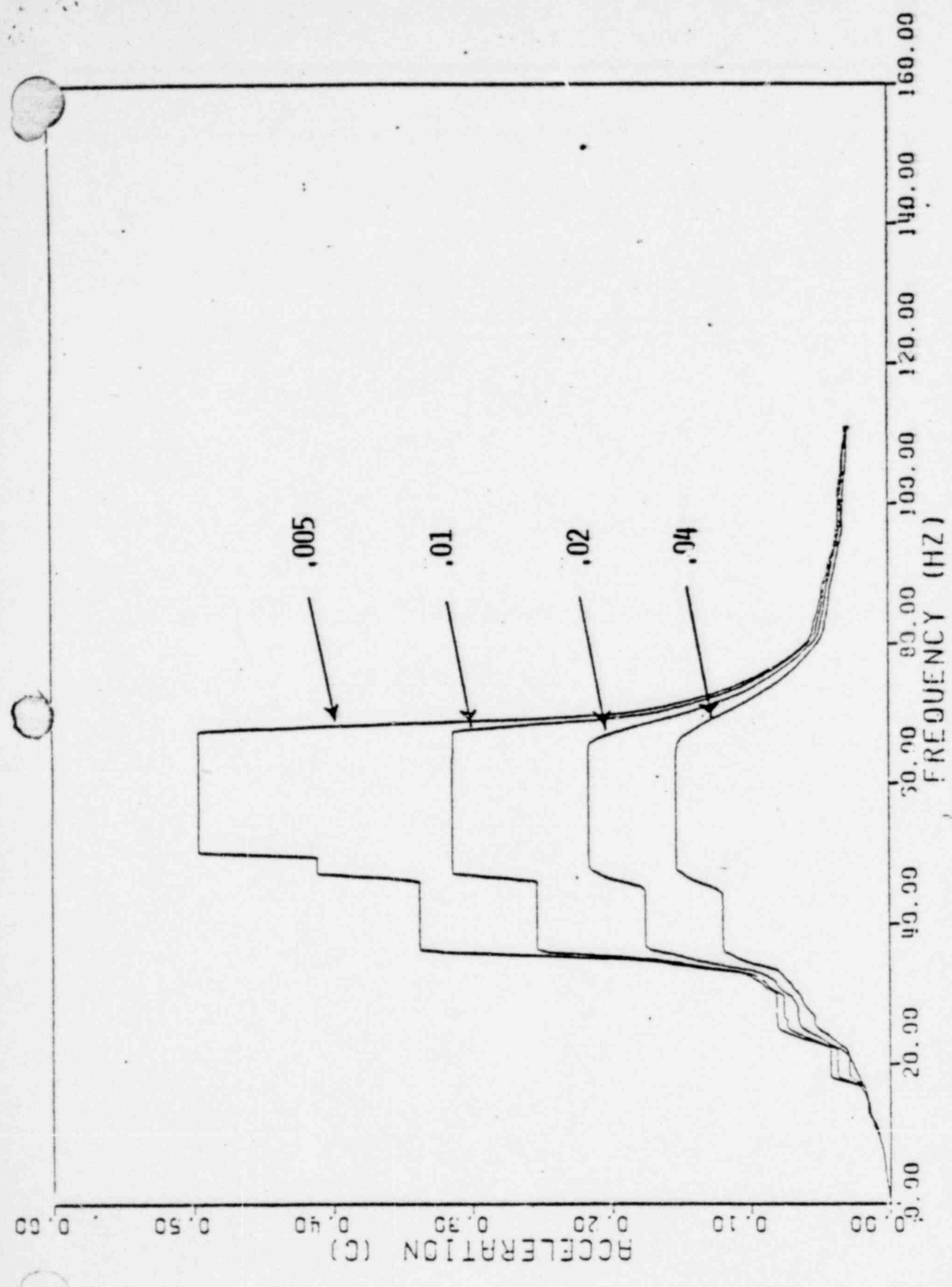
B.B.
9/13/79



II - MAINLY NON-SYMMETRICAL LOADING CONDITION

B.B.
9/13/79

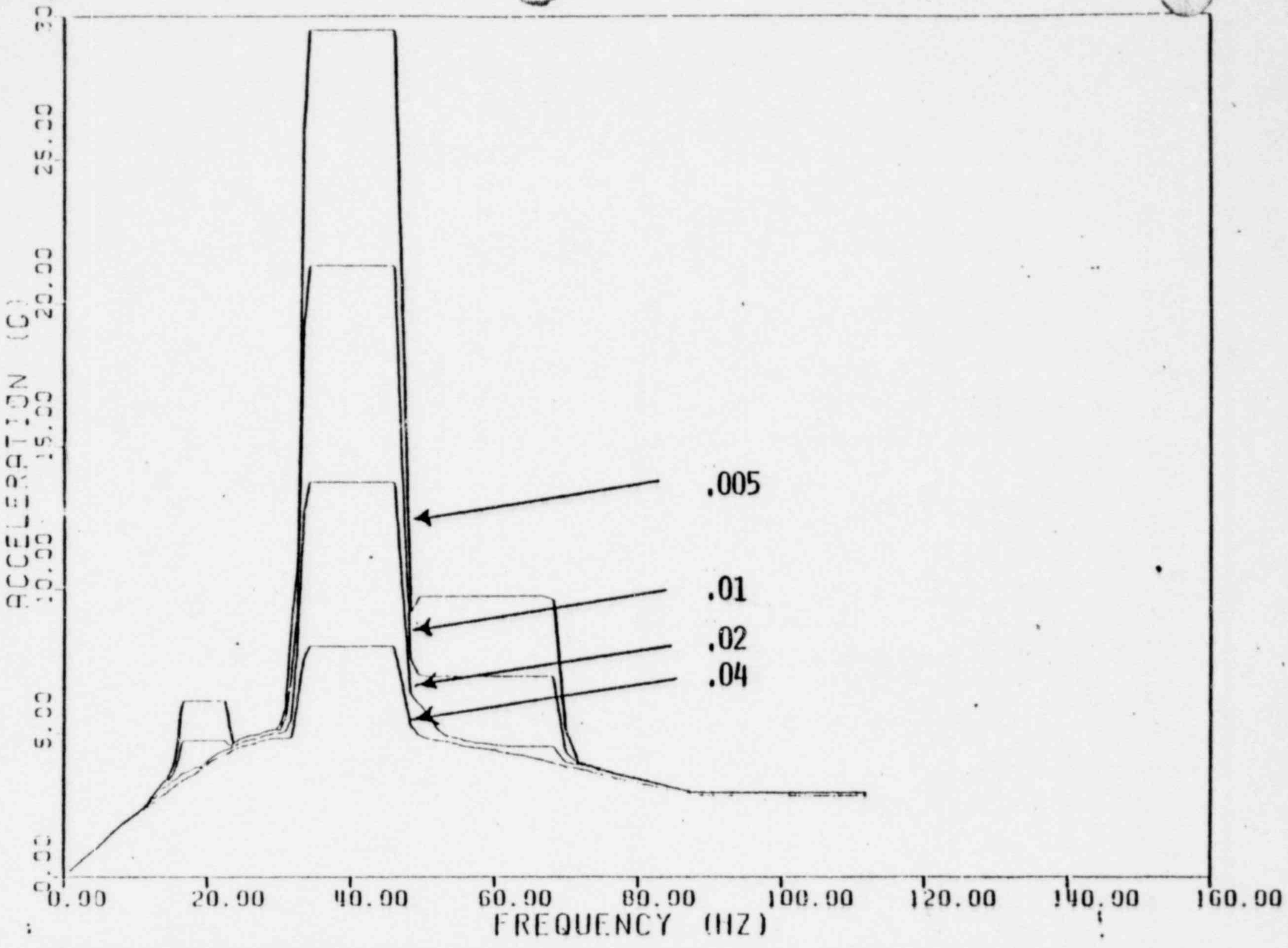
POOR
ORIGINAL



JPPSS REACTOR BLDG. NEARLY SYMMETRIC CHUGGING
MASS NO. 59 EL. 520 FT. HORIZ. TRANSLATION
RPV SUPPORT

B.B.
9/13/79

POOR
ORIGINAL



WPPSS REACTOR BLDG. NEARLY SYMMETRIC CHUGG,NG
MASS NO. 33 EL. 459 FT. HORIZ. TRANSLATION
CONTAINMENT VESSEL

B.B.
9/13/79

POOR ORIGINAL

F-14

NRC Comments
Mark II Generic
Long Term Program

1. CO Tests
2. Creare Tests
3. Improved Chug Load

NRC COMMENTS
CO TESTS

- ACHIEVE COMBINATION OF HIGH VENT STEAM MASS FLUX AND AIR CONTENT TO BOUND ANTICIPATED PLANT VALUES
- REPLICATE TESTS AT LIMITING CONDITIONS
- ACCURATE MEASUREMENT OF VENT AIR/STEAM MIXTURE
- MEASURE VENT LATERAL LOADS

NRC Comments
Creare Tests

- Tests appear to confirm multivent multiplier less than one
- Further study of FSI effects
- Detailed study of results

NRC COMMENTS
IMPROVED CHUG SPECIFICATION

- INVESTIGATE 4T HIGH FREQUENCY RESPONSE

- VERIFY SIMPLIFIED ACOUSTIC MODEL ASSUMPTIONS

- METHODOLOGY SHOULD BE CONFIRMED BY APPLICATION TO RELATED STEAM TESTS

- MULTIVENT TEST DATA SHOULD BE STUDIED TO VERIFY STATISTICAL TREATMENT OF 4T DATA.