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NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

IN THE MATTER OF:

WORKING GROUP NO. 4

Place - Washington, D. C.

Date - Thursday, 25 March 1976

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UNITED STATES NUCLEAR REGULATORY COMMISSION'S

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

Thursday, 25 March 1976

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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

WORKING GROUP NO. 4

Room 1046
1717 H Street, N. W.
Washington, D. C.

Thursday, 25 March 1976

The ACRS Working Group No. 4 met, pursuant to notice, at 9:00 a.m., Dr. Herbert S. Isbin, chairman of the Working Group, presiding.

BEFORE:

Dr. Herbert S. Isbin, Chairman
Dr. Spencer H. Bush, Member
Dr. Max W. Carbon, Member
Dr. Milton S. Plesset, Member

PROCEEDINGS

DR. ISBIN: Good morning. The meeting will now come to order.

This is a public meeting of the ACRS Working Group No. 4 to review nuclear reactor safety matters raised by Messrs. Bridenbaugh, Hubbard, Minor and Pollard in their recent testimony before the Joint Committee on Atomic Energy. On February 17, 1976, Chairman William A. Anders, U.S. Nuclear Regulatory Commission, wrote to the Advisory Committee on Reactor Safeguards as follows:

> "As you know, an NRC Staff member and three GE electrical engineers, who recently resigned, have raised concerns regarding the safety of nuclear plants. The Commission requests the Advisory Committee on Reactor Safeguards to review the statements these individuals have made to ascertain:
>
> 1. Whether they raise issues affecting the safety of nuclear facilities of which the ACRS has not been aware.
> 2. Whether they present new information concerning generic or specific issues which indicates a need for regulatory action, and
> 3. Whether their statements present any other basis for altering Commission regulatory

requirements or research priorities.

. "The Commission wishes to be informed at an early date concerning the Committee's plans for conducting this study with an estimated date of completion."

In response to the Commission's request the ACRS has established five working groups, Working Group No. 1 has already held its Subcommittee meeting and is examining the concerns regarding structures and containment, components and material failure inspection, and er ement, two QA requirements, Fort St. Vrain.

Working Group No. 2 will examine fire protection, electrical systems, human errors, simulator and control rooms.

Working Group No. 3 will examine regulatory procedures and philosophy, reliability analysis, reactivity problems.

Our group, Working Group No. 4, will examine thermal and hydraulic problems, flow induced vibration, pump flywheel missiles.

Working Group No. 5 has already conducted its Subcommittee meeting as is involved with spent fuel storage, personnel exposure and protection, decontamination and waste disposal, decommissioning.

The categories noted for the working groups are of broad designation, but they do cover specific concerns raised and serve to partition the subject areas. In this way, the Committee has sought to make its reviews more effective and efficient.

Some overlap in topics among working groups is to be anticipated and we have provided for such arrangements in our agenda today to enable some of our participants to augment their presentations in a more meaningful manner.

Our working group assumes for the most part that the testimony presented to the Joint Committee on Atomic Energy by the four engineers who recently resigned their position has been read, that the testimony of GE and that from the Nuclear Regulatory Commission have been examined and that the ACRS itself is knowledgeable in safety concerns that it has dealt with on a generic basis, as well as in case by case application.

Thus the purpose of today's meeting is not to redevelop anew the safety concerns, but to reexamine issues to see whether facets have been missed or misunderstood, whether there is a need for change in how issues are being treated and whether the progress being made in the development and confirmatory research is adequate.

The presentations may involve some recapitulations to improve our understanding and at least in one issue, some new features of core spray which is a new topic for the ACRS. We will have a more definitive development of the subject material.

This working group also wants to use this occasion to ask the General Electric Company to comment on the testimony presented by the NRC Staff to the Joint Committee on Atomic Energy and thus we will cover the assignments for all working groups. Although most of the agenda topics are related to boiling water reactors, our agenda does include several topics relating to pressurized water reactors.

In addition, several items not covered in previous Working Groups 1 and 5 will be included in today's discussion. Our participants include representatives from the Nuclear Regulatory commission, the General Electric Company, the Mark I and Mark II containment owner groups and their consultants, EPRI, Westinghouse, Combustion Engineering, Professor Leahy from RPI and the Atomic Safety and Licensing Appeals Board.

ACRS members present are Bush, Carbon, Plesset, and Isbin. And our consultants are Etherington and Catton.

The meeting is being conducted in accordance with the provisions of the Federal Advisory Committee Act.

In attendance at the meeting today is R. Muller, the designated federal employee.

The rules for public participation have been announced as part of the notice of this meeting previously published in the Federal Register on March 15th, 1976.

Copies of the Federal Register notice are available for those in attendance today.

A transcript is being kept and will be available to the public on or after March 31st, 1976 in the Public Document Room at 1717 H Street N.W., in Washington, D. C.

Since a transcript is being kept, I would ask that each speaker first identify himself and speak clearly so that everyone here is able to follow what is being said.

We have received no requests for oral statements. If there are others present who wish to participate and depending upon our ability to stay within the schedule, you may have the opportunity to present a short statement.

For each presentation, I and my colleagues will try to restrain ourselves and not ask any questions until the presentations have been completed. And should our restraint fail, my instructions to each of the speakers is that he continue his presentation and not stop to answer any questions until he has finished.

It is important to preserve the continuity of each presentation and we do want to try to adhere to the schedule that we have established.

We are now ready to proceed with the meeting.

First, are there any introductory statements that the Staff or GE would like to present?

MR. STELLO: No. I think it is best we get started with the ambitious agenda. It will be necessary to move quickly.

DR. ISBIN: That being the case, I will call on Mr. Ross from the General Electric Company.

MR. G. ROSS: My name is Gail Ross. I am the Manager of Operating Plant Licensing for General Electric Company and I have here with me today Mr. Steve Stark, Senior Engineer for Mark I Containment Application; Dr. Fred Moody, Senior Engineer for Systems, Methods and Engineering; and Mr. Ron Engel, Manager of Special Projects, Licensing; Mr. Pat Marriott, Manager of ECCS Analysis; and Mr. Bert Sobon, Acting Manager of International Reactor Licensing.

We sincerely welcome the opportunity to appear before the ACRS Subcommittee Working Group 4 to respond to the allegations made by the three engineers who resigned General Electric Company February 2nd.

The first item I have been asked to comment on is the NRC Staff's responses to the testimony of Bridenbaugh, Hubbard and Minor, as presented February 18, 1976 before the Joint Committee on Atomic Energy.

General Electric has performed an in-depth review of the Staff's comments of the 727-page allegation. While General Electric may address some of the responses in a slightly different manner, there are no material differences between these responses generated by the Staff and those that would have been prepared by General Electric Company.

I believe it is very important to note that the exhaustive efforts to discredit the uclear industry has not come up with a single new issue, and that in itself represents a significant test that the present way we are doing licensing is adequate.

I would like to state that in another way from your point of view.

General Electric isn't trying to hide anything from the NRC or ACRS.

At this time I would like to ask Mr. Ron Engel to start with comments on flow-induced vibration and control rod design responses.

One of the subjects in that set of responses is core spray. That will be covered by Mr. Marriott at a little later time.

(Slide.)

MR. ENGEL: I would like to say --

DR. ISBIN: Your name again?

MR. ENGEL: My name is Ron Engel, Manager of Special Projects Licensing for General Electric Company.

I would like to state that I fully concur with Mr. Ross' : atements in that we have reviewed in detail the allegations of the three ex-GE engineers.

We think that it provides a significant indication of the concern that both the NRC and General

Electric Company have with respect to plant safety.

I would like to give you people a brief overview of the way we look at the concerns expressed on flow-induced vibrations and on control rod drive design.

I will start off first with flow-induced vibrations.

We have two areas here that have been identified as concerns: the feedwater sprager and in-core vibrations.

First with respect to the feedwater spargers, we have inspected twenty plants. Six of these plants had cracks in the feedwater sparger.

In no case had the feedwater sparger failed in a gross manner. They were all still in place.

The allegations of the three ex-GE engineers said you cannot detect a cracked feedwater sparger while a plant is in operation.

It is true you cannot detect cracking, but if you had a gross failure of the feedwater sparger or a significant opening in the feedwater sparger, you would get power asymmetry because of the differences in sub-cooling throughout the core.

This is a measurable quantity and it has been demonstrated before that it can be seen if you have differences in feedwater inlet enthalpy.

We have designed and tested a fix which consists of either welding or putting in a very tight fit on the feedwater sparger to limit the amount of bypass flow around the thermal sleeve in the feedwater sparger.

This has been demonstrated to effectively reduce the vibration levels. It has -- spargers of this type have been operated for a year, inspected, and there is no indications of any problems.

We have also, as a part of this program, done an extensive safety analysis on the implications of failed feedwater spargers.

Three areas of concern were identified. These were: a change in feedwater subcooling which could lead to power asymmetries.

It has been demonstrated that the normal operating mode of the plant takes into account this, and there is no safety issue.

The potential for flow blockage on the jet pumps has been evaluated and it has been shown to be less significant than other transients.

The potential for blocking of the fuel inlet orifices has been evaluated, and it would take an almost impossible size piece to find its way into the area below the -- into the plenum area below the fuel.

It would have to have the right weight, size, and other features to find its way there, and the density of that piece would be too great to be lifted up by the low velocity water and block a fuel assembly.

The potential for a piece impacing on the core spray system has been evaluated and we find, again, that the core spray break detection system would adequately cover any concern with the possibility of that system failing.

With respect to in-core vibrations, we have identified the cause of the problems and on all domestic plants interim corrective action has been taken.

I think this demonstrates again the concern of the nuclear industry for potential problems.

Once the potential concern was identified, plants that had the potential or indications that they could have worn through channels immediately reduced power to a level consistent with a thorough safety analysis.

They then shut down and plugged in a normal manner, plugged the bypass flow holes which had been identified as the cause of the problem and testing out of reactor demonstrated this would substantially eliminate in-core vibrations.

One of the allegations that has been posed by the ex-GE engineers is that it is impossible or nearly impossible without high risk to the public to drill irradiated fuel.

We have demonstrated that fuel can be drilled out of reactor but this is not a nece sary part of the fix.

We can, and have demonstrat that it is an acceptable solution to install pre-drilled reload fuel assemblies and it is an economic decision as to whether or not the irradiated fuel is drilled.

You can either implement the fix in part by putting reload fuel assemblies in, pre-drilled, or you can do the drilling on the irradiated fuel.

In the testimony of the ex-GE engineers I would like to point out that they mention LPRM seal failures. They say that that is due to in-core vibration.

The LPRM failures that have been identified occurred on all product lines, BWR-2, 3 and 4.

BWR-2s and 3s do not exhibit significant in-core vibrations.

We have identified that the cause of the seal failure is irradiation embrittlement of the seal which causes it to crack and leak.

Another statement made in their testimony was that we unexpectedly identified rounding of the channels during our testing at Moss Landing.

That, again, was not true.

We, in 1973, reported to the Commission the existence of a channel deflection phenomena, a creep related phenomena, which is operational history dependent.

We have accounted for the additional bypass flow due to the rounding of the channel in previous safety analyses.

It is not a new concern and it is one that does not have an impact on the safety of the plant.

Next I would like to go on to the CRD design charges.

First was end of cycle scram reactivity.

This, I think, is again a plus for the industry.

(Slide.)

What happened was that we discovered that the scram curve that we were using in our analyses was not conservatively based on operational data accumulated from plants.

With this discovery we incorporated into all licensing applications the new analysis techniques.

It has always been the philosophy of the General Electric Company to take into account the most limiting points in the cycle.

We have proposed fixes which enable plants to get up to full power and increase their operating margins.

These have not been accepted by the Staff in certain cases.

However, this is not an indicat:on that plants are operating in an unsafe manner, since the MCPR and pressure margins that have been identified previously are still maintained.

In some cases this does result in a derated end of cycle.

However, this is an economic, not a safety concern.

As an aside, I would -- in the end of cycle scram reactivity, they talked about a patch that was implemented on the Gregliano Plant.

Again the testimony of the ex-GE engineers is misleading in that the Gregliano fix involves in essence a time delay on scram from flux, from the in-core monitors.

This fix was conceived of during the final design stages when the final transient analyses evaluated were done on Gregliano.

It was evaluated. It showed that there was less than a 5 percent increase in thermal flux due to the time delay, less than a 2 psi increase in vessel pressure, and it was tested during the startup phase and then implemented.

So I think the categorization of it being a patch is not correct.

The n t concern had to do with control rod lifetime, talking about leaching of boron from the control rod drive blades.

The control rod drive blades that failed during -- not blades. The rods that failed during plant operation at Dresden 1 were special test rods located in high flux positions. They were not production rods.

We have not seen any boron loss from our production rods, and because of the very inherent design of the control rod drive, boron loss would only be significant if you had failures of many rods in a blade.

There are from 44 to 84 rods in each control rod blade and it would take a significant number of these to have any safety significance.

In addition, we do shutdown margin tests prior to each criticality to determine if there has been a significant control loss due to the blades.

We think that this is an adequate demonstration that the control rods are capable of providing their design function.

Next was the recent discovery of cracking in the control rod drive collet. I think it is important to realize, with respect to this, that we have not seen to date any failure of a drive to operate as a result of the cracking of the collet tube.

The cause is understood. We have had meetings with the Commission to describe what the cause is and the significance of it. We have tested drives up to six times their expected lifetime in San Jose, and those drives have always filled their design function.

They are still capable of being scrammed. They are also capable of normal maneuvering.

In addition, if you were to have postulated complete severance of the collet tube, this failure would be discovered during the normal surveillance testing which requires the blade to be exercised weekly, and it would take -- and by very design the control rod system we have always designed that the reactors should be made subcritical with the most highly worth rod stuck out, so we still meet that design function.

But I think it is still important that we have gone to six times the expected lifetime of the blades and still not seen any failure.

Finally, with respect to the rod drop accident, we believe that the overall probability of the control rod drop accident is small, even without the operator aids that have been installed on various product lines. I think it is important here to remember that the rod drop is only significant at low power levels.

The event does not use the rod block monitor as described in the ex-GE engineers testimony. The RBM is designed to operate only above 30 percent power level. It is there to provide protection from the control rod withdrawal there, not the rod drop accident.

The rod worth minimizer and RSCS have been designed and installed on a number of plants. The operational history has been quite good.

These are not patches, as is alleged in the testimony. These are design systems which are operationally -- are working operationally.

In addition, they say that there should be added concern in the rod drop accident, because of collet tube and channel failures. It is important here to remember that the important function is to maintain the driveline integrity. Neither of the other concerns have anything to do with drive integrity. Therefore, we see it does not have any significant impact on the overall already insignificant activity of the control rod drop accident.

In conclusion, I would like to say that GE agrees completely with the staff assessment in these areas.

DR. ISBIN: Thank you, Ron.

Are there any questions?

MR. ENGEL: Okay. At this time, I would like to introduce Mr. Bert Sobon to give you a brief overview on the containment concerns.

MR. SOBON: I don't have any viewgraphs. I will make my presentation from the table.

My name is Robert Sobon. I am from General Electric.

I have reviewed the staff's March 2 response to the testimony given before the Joint Committee on Atomic Energy on February 28 by the three former General Electric employees. My specific area of review was what could generally be categorized as the area dealing with the dynamic loads that might be imposed upon the containment and its main components.

I believe that the staff responses given, again, in the March 2 response accurately reflect both the history and the present situation relative to each of the contentions riased by the former employees. General Electric also concurs with the conclusions that were stated in those responses.

We, in the Vermont Yankee ACRS subcommittee and full committee meetings, addressed several of the containment status items relative to Mark I and some of the non-Vermont Yankee items also covered by this testimony, so I will not go into that again.

I do think, though, that it is important to point out the considerations that are given in reaching conclusions on safety. In reaching these conclusions, it is important to note that considerations are given to the inherent conservatisms that are built into nuclear power plants. This is referred to as "safety margin."

Margin of safety is a qualitative consideration of risk that includes such factors as the probability of the events that you are designing for, potential consequences of those events, possibilities for human error, the design margins that are built into the codes and standards used for construction of the plants, the material properties that are used, Your material properties are generally better than the values stated in the material property handbooks.

There are calculational conservatisms that are built into the design, and you become smarter, if you will, from plant operating experience and inspections that are conducted regularly.

Plants are designed to accommodate, then, postulated equipment failures, operator mistakes, design errors and failures. In other words, the plants are designed to withstand certain low probability events to insure that the health and safety of the public is protected."

Therefore, when new information becomes available as a result of the continued effort to improve the quality of plant design, concerns about the adequacy of previous designs can be evaluated in a timely fashion without undue risk to the health and safety of the public.

It should be noted that where temporary quick and relatively easy changes can be made to the plant design for the mode of operation to obtain increased safety margin during this detailed evaluation, they have been made. An example of this basic philosophy is reflected in the effort being given to addressing the pressure suppression containment capabilities.

With the information from the more sophisticated testing that was done to support the design and model confirmation for Mark III containment, it was appropriate that the previous suppression containment types be reevaluated to assure that the so-called new loading conditions could be accommodated.

For this, the utilities with Mark I and II containments formed owners' groups and retained GE, along with other consultants, to perform this reevaluation. Today in the audience there are members from both Mark I and Mark II utilities.

To complete this evaluation, each group chose to report the results in two phases. The Mark I effort was to conduct small-scale tests to define loads that would permit a rapid assessment of the structural capability and thus demonstrate that the plant operation could continue while further testing and more sophisticated and detailed structural analysis is performed.

Mark II effort consisted of using all the available information to develop a dynamic forcing function report which would allow plant-unique load determination for plant-unique structural evaluations.

This is being followed by selected -- by confirmatory information to verify the load determination efforts.

Again, you have heard the effort of the Mark I evaluation as part of the Vermont Yankee subcommittee and full committee meetings on March 3 and 5 of this year. Mark II applicants are in the final stages of submitting their analysis to the staff.

As a result of the short-term evaluation, Mark Is are operating with the dry well to wet well delta P to increased margins. Several Mark II applicants have incorporated structural modifications to increase their capability to withstand postulated loads determined from this dynamic forcing function report.

This, the , is an example which, hopefully, demonstrates that prompt attention is given to assessing and assuming and assuring the safety of operating plans and the capability of plants under design and/or construction.

That is my presentation.

DR. ISBIN: Thank you, Bert.

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Are there any questions?

Some of these subjects will be covered in a little more detail during the meeting. I think that would be the more appropriate time to ask questions of you, as well as of the staff, when those topics are introduced.

We will go along, then, to the next item, which will be the core spray.

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MR. MARRIOTT: Good morning. Pat Marriott, General Electric, manager of ECCS engineering.

(Slide.)

MR. MARRIOTT: I would like to talk this morning about this concern which is, as you say, new to the ACRS, although we have been discussing the potential concern with the staff for some time. I would like to address it first by referring to the allegation in the testimony of Bridenbaugh, et al.

First of all, Bridenbaugh and company assert that the present test program for core spray is inadequate for demonstrating good cooling. In particular, they make reference to the fact that we use what they call cold tests to determine the distribution of core spray cooling over the core. This cold test is certainly true but it is a limited part of the story and I will go into detail on that in a moment.

Their second contention is that if there were inadequate core spray flow, a core meltdown could result.

And their third contention refers to what they make sounds like very mysterious European tests, which indicate that steam upflow could prevent delivery of cooling to the fuel rod. I will address these contentions point-by-point in a later part of my presentation, but I think it would be useful first to describe what I think is the set of European tests to which they refer in their test.

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Assaya Atom, which is a Swedish manufacturer of boiling water reactors with whom we have a technical exchange agreement, conducted in 1974 a series of tests on their own core spray system. It varies substantially in concept from ours. They have an overhead sparger adjustment in which the nozzles are vertically downward into cells. They tested their system using a spray cell system, in a steam environment at pressure. They test single spray nozzles in the vertical orientation, which is characteristic of their reactor. The trajectory of the nozzle between its placement and the collecting apparatus is of the order of two feet.

As I say, it is conducted in a pressure vessel in a steam environment with a range of spray water temperatures and system pressures, which simulate the conditions expected in the reactor under post loss-of-coolant conditions. The nozzles, which are used in the Assaya reactor are very fine droplet, high velocity nozzles. They are centrifugal atomizing time.

We use some nozzles of a design similar to this in our reactors. Details later. But the nozzle which they use is significantly enough different from that used by us that their results were not directly applicable, but they did reach conclusions which we believed had some relevance to the GE BWR. Namely, that in steam, under certain conditions, the spray cone can change as a function of water temperature and

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as a function of system pressure. We considered this observation, which was brought to our attention in May of 1974, to be significant enought that we undertook a series of tests in their facility, using nozzles of the types used in BWRs. We found some results which were similar to theirs on some nozzle types. We found some of our nozzle types, which were practically affected, and I will go into that later.

We quickly told the staff about it, and initiated a analytical and experimental program to address the effects more precisely. Given that general conclusion with regard to the Assaya test, what does it mean to the spray distribution of the boiling water reactors built by GE?

(Slide.)

MR. MARRIOTT: Well, as you know, the General Electric core spray system consists of two-ring spargers, which surround the periphery of the core, slightly above the top of the core, and for a typical reactor, there are alternated around the spargers course low velocity nozzles and high velocity atomizing typ nozzles.

As I say, they are alternated around the core and in the particular typical configuration I have shown here, there are 65 nozzles of each type on each of the two core spray spargers.

Now, because of this close placement of the nozzles in proximity to each other, the core spray flow into any

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given fuel assembly in the GE BWR is the result of the super-position of the flows from a good many nozzles. And so it is possible to conclude intuitively that the BWR spray distribution should not be particularly sensitive to variations in the cone angles of the nozzles. As an example of how one might reach that sort of intuitive conclusion, let me point out that we have used a number of nozzle types in BWR designs. We have used very narrow pattern nozzles, pipe elbows, as a matter of fact, we have used very wide cone angles and we have designed successful core spray systems using both types with the same number of nozzles on each sparger.

That points to the conclusion again that it is the super-position of the flows from many nozzles which is responsible for the flow into any given fuel assembly.

Furthermore, the BWR typically has two independent full capacity core spray systems and we conduct our core spray heat transfer test using the minimum specified flow from one system, so that the existence of two provides some additional margin.

(Slide.)

MR. MARRIOTT: I mentioned that on becoming aware of the Assaya test we put into place an experimental and analytical program. Let me describe this in a small amount of detail. First of all, before I begin this part, let me

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say one thing about the staff's test on this point. The staff has chosen in one place in the test, to be very concise in an area where I feel a little more detail is due with respect to the effects observed in our tests on the various nozzle types, so let me expand a little bit on what the staff said.

I think their statement is excellent on the point, but it does need some clarification and expansion. We did indeed in tests of our nozzles at Assaya and subsequently at our own facility, find some effects with fine droplet atomizing types similar to the Assaya test. That is, we found that elevated pressure in steam, the cone from the nozzle could contract.

We have used a number of types of atomizing nozzles and we have observed significant contractions in some, practically no contraction in others. It is simply impossible to generalize.

On the other hand, in testing the open elbow nozzle, which is the work horse nozzle in many of the BWRs, we observed practically no effect to the steam environment. This is probaby not surprising because that is a very low velocity with very large droplet size so it is not as subject to condensation effects as the high velocity types.

Finally, in one of our nozzle types, the so-called VNC, we observed a shift of the pattern 7 to 10 degrees off the center line, so in summary, we have conducted tests on our own nozzles. We have, in fact, in the past few months conducted quantitative tests in order to precisely measure the amount of contraction experienced by each nozzle and we have found effects on our nozzles ranging from quite notable to practically none at all. We have also conducted tests in our full-scale air test facility in order to test sensitivity of the core spray distribution to large changes in the nozzle angles. We did this simply by reconstructing a sparger from one of our earlier plant designs and modifying the nozzles in such a way to make the cone angles very much narrower than they were in the original designed tests. What we found, I won't go into the details of the results, but what we found was indeed the BWR spray distribution can tolerate very significant narrowing of cone angles without making big changes in the overall core spray distribution, which confirms what I said earlier, about the super-position of flows for many nozzles, being the effect which really controls the distribution into any given fuel bundle.

We have a continuing program underway, I mentioned a moment ago, that we have done tests to quantify the performance of the various nozzles. That is an ongoing program in that some of the data re[illegible]on is not yet complete.

We have, in addition, experiments planned to attempt to quantify the interaction between pairs and triplets of adjacent nozzles. We have a test planned to measure the

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distribution with steam environment effects simulated. That is, to actually measure the amount which the cone angles change in actual steam environment tests, and then simulate these effects in air tests in order to get a rather precise measurement of what happens in the steam environment; and finally, programs to determine the interaction, if any, with liquid over the core.

You are aware of the counter current flow limiting which exists in the fuel bundles. We have programs underway to assess whether that has any effect on the spray distribution. I am sure that we will find that on balance it is a very positive effect.

Finally, we have an analytical program underway to dome up with a predictive model for the core spray distribution. That is a rather complicated phenomenon. The approach which we have chosen is to begin by predicting single droplet trajectories in steam, extending that into a model which will predict the performance of single nozzles, use empirical results to determine the interaction between nozzles and, finally, develop a global model to predict the overall distribution.

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(Slide.)

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I don't want to end this presentation without talking about the treatment in our evaluation model of a very significant related effect.

We have talked in various ACRS Subcommittee meetings about the countercurrent flow limiting model, which is currently in use, and for the record, let me summarize it.

The core sprays inject water over the core, which reaches the lower plenum in part by passing through the fuel bundles, in part by passing into the bypass region and into the lower plenum through the leakage augmentation paths.

The counter current flow limiting model, which is currently in use, assumes that the entire core behaves as a single, average power channel, and it uses the results from single channel counter core current flow results to determine what the bundle l ne is.

Counterflow limiting at the top of the fuel assembly restricts the down flow then and any liquid which is not permitted to pass to the lower plenum in our model is simply thown away.

It's simply ignored in the calculation.

The use of this model has resulted in very significant delays in the calculated reflooding time for boiling water reactors,

Unrealistic delays, we believe, because we have substantial and growing body of experimental evidence that

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introduction of subcooled liquid into the fuel bundles breaks down the countercurrent flow limiting phenomenon.

I don't recall if I mentioned it a month ago.

If I didn't, I will say it now.

The current model uses test results which are based on saturated steam, input to the top of the fuel bundle.

Saturated liquid. I beg your pardon. The liquid over the core is injected by the core sprays, most assuredly will not be saturated.

It comes from the suppression pool and possesses a great deal of subcooling.

We now have a lot of experimental evidence that introduction of subcooling into the bundles breaks down the countercurrent flow limiting.

The moment you get liquid into the bundle, it quenches flow inside, which means more restriction and more water can get flow.

It's a positive feedback effect, which causes unimpeded flow.

Even if breakdown occurred only in the peripheral fuel assemblies, those closest to the core spray spargers, where the subcooled water is coming in, it would not be possible for any accumulation of liquid over the core to occur.

Which says two things. It say, first of all, that

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the current evaluation model is indeed very conservative and it says in the second place, that the calcualted reflooding delays which are coming out of our current model are very unrealistic.

Now, I have made this point only because the calculated reflooding delay in today's models is so much more significant than any variations in spray heat transfer, which one would wish to postulate because of the consideration of the cone contractions.

(Slide.)

Now, let me talk point by point about the specific items in the Bridenbaugh testimony.

There first point was that we used cold tests and by that they mean tests in atmospheric room temperature conditions in air, to measure this core spray distribution.

That certainly is true.

We have a full-scale core spray distribution test facility, which does just exactly that.

The effect of steam updraft is simulated in that facility with fans, which put air through the simulated core mock-up at a rate that simulates the effect of steam updraft.

So, yes, indeed, we do use atmospheric air tests to evaluate the core spray distribution.

But they go on to condense that there are no "actual thermal tests," on the point and that is not at all

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true.

We conduct hot tests under simulated reactor conditions to evaluate a great many things which are directly on point.

We conduct tests of single core spray nozzles in steam over the range of pressure and spray water conditions which are characteristic of the BWR under post-LOCA conditions.

We conduct full-scale full power tests to determine the amount of spray penetration into the fuel assemblies.

Full-scale, full power tests to measure the updraft due to vaporization in the bundle and to measure the heat transfer coefficients due to spray convection and due to reflooding.

So the contention that there are no actual thermal tests is not at all true.

The third contention is that the core spray has to be effective in "seconds" in order to prevent a meltdown.

Now, one could mean any of a number of things by "seconds."

But this statement is certainly nonsense.

There are a number of phenomena involved as you know in the BWR loss of coolant accident.

The BWR is completely self-cooling, using entirely natural phenomena for 30 or 40 seconds after a postulated accident which gives the emergency core cooling systems plenty of time to come on.

More importantly, in all but the very earliest BWRs, the bottom plenum refloods and even if there were no heat transfer at all, from the core sprays, there would be a 1,000 degrees Fahrenheit margin to core melt, even for the design basis LOCA, due to reflooding, with no credit for spray heat transfer whatever.

The final point which isn't really relevant, but since it was mentioned I will address it.

"Steam blasting" will prevent spray delivery to fuel rods.

That's not true. I think what they are probably referring to is countercu ent flow limiting phenomena, which as you know we have evaluated in full-scale full power tests and have found that steam updraft, while it does delay delivery of the spray water to the lower plenum, certainly does not prevent it.

What's more, the cooling which you get from the updrafting steam is very significant in and of itself and we take no credit for that in the calculations.

(Slide.)

So, to summarize, we have evaluated the tests from Assaya Atom on cone spray angles.

We brought it to the Staff's attention and we have been working with them since then.

We have conducted experimental programs and our own nozzles and our own systems to determine the effect on spray distribution and a continuing program is underway.

We found that our overall distribution is not spray system to cone angle changes for the reasons I have said.

It's super position in flows that really governs the distribution and, furthermore, we have two full-capacity systems in each reactor.

The third point, the peak temperature is insensitive to spray transfer in the first place.

We have very conservative treatment of related effects in the evaluation model, contercurrent flow limiting, the fact that we now inventory away if it's not permitted to pass to the lower pleanum.

We ignore counter core flowing limiting breakdown resulting in a much delayed calculated REFLOOD time, much more significant than variations in spray heat tranfer.

So in conclusion, on this concern, the effects of a steam environment are significant on some nozzle types, used in GE BWRs.

The design of the BWR, ECCS, however, minimizes the sensitivity of the peak clad temperature to variations in spray heat transfer and the whole effect is very conservatively treated in the evaluation model.

That's all I have prepared to say. I will be happy

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to answer any questions.

DR. CATTON: How does the steam affect the cone angle physically?

MR. MARRIOTT: In two ways. Number 1 is by condensation.

Condensation, of course, adds mass to the droplets and therefore affects their trajectory.

More important --

DR. CATTON: Does it increase or decrease the cone angle?

Added mass will increase the movement of the droplets.

MR. MARRIOTT: It would decrease but in a vertical field it's clear to see.

By making the droplet heavier it would make the gravitational effects more important.

DR. CATTON: Isn't your spray momentum enough that the gravitational effects are small?

MR. MARRIOTT: No.

DR. CATTON: There is quite a distance between the spray head and where it is supposed to impact.

MR. MARRIOTT: There is a horizontal difference.

The spargers are just above the top of the core and they lost water over the top of the core so gravity effects are quite significant.

The more important effect, though, of the steam environment is that, as condensat occurs on the droplets inside of the cone, it causes a net inflow of steam which causes an inward drag on the droplets and actually pulls them inward.

DR. CATTON: So these cone angles that you talk about are relative to the axis of the spray?

MR. MARRIOTT: That's correct. That is a good question.

I should have made that clear.

DR. CATTON: So what about relative to the central part of the core?

You are talking about a single cone spraying out across the core.

If you change the cone angle a little bit, and you have got a whole lot of them, I wouldn't expect much of a net effect.

MR. MARRIOTT: There isn't much net effect. That's right.

DR. CATTON: If you draw down the overall spray pattern, that might change things.

MR. MARRIOTT: It might, indeed. Let me explain.

DR. CATTON: You talked about cone angles but not the integrated effect.

MR. MARRIOTT: I guess I need a little elaboration

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on the effect.

DR. ISBIN: You mean the net results.

DR. CATTON: Yes. Talk about a single spray and its cone angle, I think that is different than talking about a multiplicity of cones around the periphery and the net effect on the spray distribution over the core.

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MR. MARRIOTT: Right. Let me reiterate something I said earlier on that point.

We took in our full scale core spray test facility a sparger from one of the BWR 4 designs and modified it in such a way that all the cones contracted, very severely, as a matter of fact, much more severely than we have measured them to do in a steam environment to find out what the overall effect would be.

DR. CATTON: Okay. You are picking the spray head that you know to be the worst.

MR. MARRIOTT: In fact we didn't do quite that. We simply — we took a BWR design which uses a very wide cone nozzle and modified that nozzle, modified all of those nozzles in such a way that they had a very narrow cone. Simply to find out what the sensitivity was to big changes in the cone angle.

We weren't attempting to simulate what the nozzles would actually do in steam. In fact we kind of overdid it. We made the nozzles in the tests a fair amount narrower than we have measured the nozzles performance to be in steam.

So we evaluated then what the overall effect would be. It wasn't surprising that the distribution was not as uniform with the narrow cones as it was with the wide cones. Why wasn't it surprising?

Well, because we optimized the design in those tests to get the most uniform distribution we can, so anything you do to it brings it off optimum.

But the important results were, one, there were no areas of the core which received zero flow and second, the distribution while less uniform was not all that much less uniform. The minimum measured flow went down by about 30 percent.

DR. CATTON: So what you are saying, with these cones, very narrow cone angle, you didn't run into any problems relative to delivery to the core.

MR. MARRIOTT: That's right.

DR. ISBIN: On this point, let me be more explicit and address my question also to the Staff.

On page 2-28 of the Staff's testimony they refer to a review of all operating BWR plants, modifications in the LOCA evaluation models, were not needed. This was on the basis of the review of the core spray results.

My question is, who made this review? Was it both GE and the Staff independently or was it a single review? Let me ask the question first of Vic Stello.

MR. STELLO: The statement I think is still true today. I believe the reviews were made by the General Electric Company and the Staff jointly. The view in looking at the BWR plants, except Oyster Creek and Nine Mile Point which didn't have flooding capability and the ones remaining that did have the ability, are concerned with the c untercurrent flooding distribution and the results that would be obtained with countercurrent flooding indicated to us that the real concern was countercurrent flooding.

In that case the BWRs for which flooding was included there was a rather substantial penalty imposed on the way in which the calculation was done. So that case, that remains true today.

I share the same view as Pat had, that when we have better data, we prove that we have indeed imposed considerable conservatism in the way we are calculating performance in those plants today.

I believe the nozzles are different in the two other plants, for which there is no appreciable change in cone angle in Oyster Creek and Nine Mile. Basically open elbows were shown to be insensitive in these tests. That still is true today.

MR. MARRIOTT: That's correct.

MR. STELLO: This is a quick summary of what we did and I might ask Pat if he will speak for what the General Electric Company did in its review.

MR. MARRIOTT: Well, Vic, I have nothing really to add to that. What you say is very true. We have indeed systematically tested practically all of the nozzles now which have ever been used in BWRs and steam environments, to be sure that there are no surprises and we have run tests in the full scale facility at ________________ as appropriate when cone angles did change to quantify the effects of those cone angle changes and our conclusion still holds that it is inappropriate to make any changes to the evaluation models to account for this effect.

DR. ISBIN: Now, will this also include Dresden 1?

MR. MARRIOTT: Yes.

DR. ISBIN: Are there any questions particularly with reference t Dresden 1 which might be affected by the changes in the spray angles?

MR. MARRIOTT: I should mention that. We have been evaluating not only Dresden 1 but all of the so-called BWR-1s, the very early boiling water reactors, along with the rest of the BWRs, with respect to this phenomenon.

We were not prepared at our most recent meeting with the Staff which Paul Bonner attended to discuss the BWR-1s, so he hadn't had the benefit of a presentation on that. But first the core spray distribution systems in BWR-1s vary in detail from plant to plant.

I don't propose to talk about them plant by plant today, but we have tested nozzles of the types used in the BWR-1 reactors. The data reduction is not yet complete. When it is complete we will assess the effect on the BWR-1s as we have for the other reactors.

If there is a potential concern indicated we will notify the customers and take appropriate actions through the normal licensing channel. The systems, however, are roughly similar to those used in the later reactors. That is, they are ring sparger systems with a number of nozzles, so it is quite likely that my general comments with regard to super position and so forth hold true with respect to the BWR-1s as well as they do to the later reactors.

DR. ISBIN: What is the position of the Staff with reference to BWR-1s?

MR. STELLO: If General Electric Company is doing some new work I assume they will also make sure that the Staff is informed of any results that they have. At the moment we have several of the older reactors, and by that let me just say earlier that Oyster Creek under review includes Big Rock, Humboldt, and so far we found no reason to change anything we said thus far, but I would have to leave the future open to what the future holds.

When we finish the review we surely will keep the ACRS informed as to the results. I have no reason to speculate there is any serious problem.

DR. ISBIN: One more question in this area, Pat.

The ACRS, particularly through its subcommittees involved with reactors and emergency core cooling systems, has been meeting with you.

At our last meeting I recall your making a statement that you would include both the pluses and minuses in the evaluations of emergency core cooling systems. In retrospect, how did it come about that the ACRS Subcommittee was not informed on this particular topic? Was it the Committee members and our consultants weren't astute enough to ask the questions, or was it perhaps your thinking that this was a trivial problem or what? But what do we learn from this experience to improve the communications?

MR. MARRIOTT: Okay. That is a good question. Clearly the ACRS cannot be astute enough to ask questions about something that they don't know about. I would not call this a trivial concern, either. Indeed we have devoted a fair amount of manpower to studying it, but we have not interpreted it as a safety problem.

Clearly, the effects on BWR design are significant and we are going to benefit greatly I think in the design of the subsequent BWRs from what we have learned on this, but inasmuch as it didn't represent to us a matter of burning safety sigificance, we have simply not brought it up.

We have notified the Staff and we have been working with them in an orderly manner to understand the effects more precisely, but neither we nor the Staff have seen fit to, for example, take action to account for it in the models. Take action to impose more restrictive limits and so forth, because we don't believe it to be a significant concern.

MR. STELLO: Herb, I think the Committee and the Subcommittee and the Staff, in that we can be blamed I suppose to the Staff for not bringing this to the Committee's attention in a forceful manner.

The Staff has taken steps some time ago to be sure the Committee is fully informed with respect to our inspections and our meetings with the vendors. I made sure the meetings that were referred to were sent to the ACRS and they in fact were sent.

However, I think perhaps in retrospect what we should have done is called this more forcefully to your attention and we were negligent in doing so and I apologize for not doing so.

However, I think what we were preoccupied with was the new phenomenon which we felt was a much more serious concern for which the penalties were much more significant. That was countercurrent flooding. In our view a model where you could stack up water over the core of several feet, up as high as 10 feet, that the spray distribution was not the concern that we needed to focus on.

The true concern at that time was countercurrent flooding and how to directly account for that phenomenon. As I recall, that is the phenomenon we forcefully brought to the

Committee's attention and perhaps in focussing and concentrating on that particular aspect of it we were negligent for not saying, oh, by the way, there is another matter that has come up and didn't bring that to your attention as forcefully as we can.

We will try very hard in the future to make sure that we interview the system to make people more aware of these problems. Although I think you have to agree that perhaps it is useful for the Staff to act as kind of a filter mechanism and bring really important issues to the Committee's attention and hold back some that -- although we can make them available to you, but don't make as big an issue out of the lesser important issues.

DR. ISBIN: Thank you. Thank you, Pat.

MR. MARRIOTT: Thank you very much. It was a pleasure.

DR. ISBIN: Our next item starts with the Staff.

MR. D. ROSS: My name is Denny Ross.

With respect to agenda item 3 we wanted to take item B first and talk about the BWR pump overspeed and I would like to note also, sometime this and the EPRI representative would be available to discuss EPRI's research on this. When you get there, I would suggest we find a place for him at that time.

DR. ISBIN: All right.

MR. KLECKER: My name is Ray Klecker. I am with the NRC Division of Operating Reactors.

My subject today is the reactor coolant pump overspeed and flywheel missiles.

You might note from the title that the first part of this, pump overspeed, will pertain to boiling water reactors as well as pressurized water reactors, but that the flywheel missile part of it will pertain to pressurized water reactors only since the boiling water reactors do not have flywheels.

I would like to start with reading the allegation by Mr. Pollard.

Actually his allegations were contained on several sheets. I have taken the liberty of excerpting from that, and I believe I have covered a few of his main points.

One, there is the existence of a generic issue and, two, that the NRC is proceeding with licensing facilities while the issue remains unresolved.

His allegation, or at least the excerpt of his allegation, reads as follows:

As a result of the reactor coolant system pipe rupture and the blowdown of reactor coolant to the reactor coolant pump, the pump impeller may act as a hydraulic turbine causing the pump, motor and flywheel to overspeed and become potential sources of missiles.

The potential for missiles from pump overspeed remains an unresolved safety problem for Indian Point 2 as well as other plants.

This particular issue was under review in depth by the Staff some three years ago, and at that time we had a series of meetings with all of the LWR, that is the light water reactor vendors, and subsequent to those meetings we prepared a Staff report which was presented to the ACRS.

The date of the report is August 3, 1973, and I believe that the date of our presentation was August 8th or 9th of that year. I am not sure of the exact date.

Mr. Pollard himself was a participant in these meetings and also had an opportunity to contribute to our report to the ACRS.

Since the subject was discussed with the ACRS at that time in some detail, I am planning only to go into it briefly today.

However, if the subcommittee wishes, I will go into it at any depth or any aspect of it, at your desire.

I have additional slides here which can be used for that purpose, if you want to take the time to use them.

I might just at this time put a slide on here which gives the conclusion as indicated in our report of August 3, 1973. It states as follows:

We believe that because of the small likelihood for the occurrence of a pump overspeed event that could seriously increase the consequences resulting from a loss-of-coolant accident, the action being taken by the Staff to assess this problem in a generic fashion outside the context of individual application reviews is an acceptable course to follow.

Our conclusion today is essentially the same as it was at that time. And I have a slide here which very briefly gives the bases for that particular conclusion.

(Slide.)

First, flywheels are simple devices. That is the stresses and stress intensities can be calculated to the degree of accuracy required.

The geometry of a flywheel is essentially a flat plate.

Of course, it is machined to be a flywheel, but the surfaces are all available for inspection prior to assembly.

It can be built without welding and, as a consequence of these items, it is easy or relatively easy to control the quality of the flywheel.

Number two, the material properties are known and specimens from each or the same plate as each reactor flywheel are tested to determine its specific properties, and I might point out that generally the materials used for flywheels today at least are very tough materials of essentially the same grade as used in reactor vessels and in many cases it is exactly the same material.

Number three, the Staff has had a Regulatory guide which is now numbered 1.14 -- it was originally Safety Guide 14 -- which addresses the design and inspection of flywheels.

Within that Regulatory guide we request from the applicants and ultimately from the various vendors topical reports addressing the subject.

In these topical reports we would ask for design bases and the vendors' critcal -- the vendors' calculations of the critical failure speeds.

I might point out there are about three potential ways in which a flywheel could fail if it was overspeeded to some unlimited degree.

One is it could fail ductilly. That the material reaches the yield strength and yields.

Number two, it could fail in a non-ductive manner. That is if it had a flaw in it to begin with. it could fail ductilly.

Here we request vendor to do a fracture mechanics analysis.

The third way in which it might possibly fail is as the flywheel overspeeds the interbore region will tend to expand first and reach yield and, as a consequence, the flywheel may lose an undershaft, to some extent to become unbalanced.

These three areas we are asking the vendors to address in their reports and, of course, the Staff will review them.

Next item, flywheels are spin-tested at 125 percent speed.

This is a requirement of the Regulatory guide and, further, we ask for in-service inspection as well as the pre-service inspection that I mentioned a little earlier.

On the in-service inspection we understand that flaws less than -- or up to one-half inch or greater can be detected, such that even if flaws were to develop in service, we have a confidence that they would never exceed, say, a half inch in depth.

Flaw growth rates have been calculated and found to be extremely slow in service, so that even between periods of inspection, we would not expect flaws to exceed, say, a half inch.

This at normal operating speeds is really no problem because the critical crack size is the order of -- let's say several inches or more.

The fourth item, the only potential mechanism for significant overspeed is the loss-of-coolant accident.

I have underlined the word "significant" because I am sure all of you can imagine turbine transients can drive a flywheel or the pump itself and, as a consequence, the flywheel, to some degree of overspeed.

However, the magnitude of those overspeeds are quite within the design capability of the flywheel and the motor itself, so they are really not of concern.

The only overspeed of real concern is the overspeed as a consequence of a LOCA.

Number 5, we say the specific LOCA probability is low. By this I mean that the only LOCA, loss-of-coolant accident that will result in a very serious overspeed, is the complete severance of a pipe and the pipe offsetting such that we have essentially unimpeded blowdown to the containment environment.

If the pipes do not separate or if we have only crack type flaws in the pipe, even fairly large ones, then the overspeed problem diminishes very rapidly.

We have put a probability here of somewhat between 10 to the minus 6 and 10 to the minus 5 per facility year for this type of rupture.

In addition to that, the BWR plants in particular that I am familiar with have a restraint system. By this I mean it is a system to limit the offset of pipes, should there be a rupture in the primary coolant system.

Now, these restraints are not rigid against the pipe such that there can be some opening of the break area.

However, it is difficult to envision a full double-ended break.

The third item of probability is that missiles from the flywheel would cause additional damage to the plant over and above what was caused already by the LOCA and, as a result, the consequences would be more severe than what we now analyze.

We have placed a probability of something like 10 to the minus 3 to 10 to the minus 2 on that.

The overall probability then is a range of somewhere between 10 to the minus 11 and 10 to the minus 8 per facility year.

Now, we noted in our report to the ACRS that even if we were off by a factor of 100, that is, we would reach a probability of something like 10 to the minus 6 per facility year, we felt that we could still proceed with the licensing of plants because this probability was low enough for an interim period.

I might make one observation on the side here. That is, subsequent to our preparing these numbers, the Rasmussen Safety Study Group came out with what is known as

WASH-1400 in which they also addressed the same subject.

Our approach was somewhat different from theirs.

However, I believe that we arrive at essentially the same conclusions.

Their net result for this particular sequence of events is two times 10 to the minus 6 per facility year and I think in view of the uncertainties involved for the numbers, that is pretty good agreement with what we have presented earlier.

The sixth item on the slide is present analytical calculations are conservative. By that I mean all of the vendors and the Staff in our calculations have used more or less idealized analytical procedures because we do not have sufficient test information to treat certain of the phenomena we expect to occur.

In each case we have been, I believe, overly conservative and these EPRI tests that Mr. Ross alluded to there, I believe, ultimately should demonstrate the degree of conservatism we now feel that are in the calculations.

I have to admit that we are still speculating at this time. We have no positive proof but it is just intuition and knowledge of other aspects of pump performance and that leads us to believe tha two-phase flow through the pump is going to be less efficient in driving that pump as a turbine and, as a consequence, the mechanical engineering in the flywheel is expected to be less.

The seventh item I have on my slide is that electrical braking can limit overspeed.

Now, this, again, can be argued with to some extent.

If you will recall in Mr. Pollard's allegations he specifically stated that there were people who disagree with this approach.

I think the reasoning behind this disagreement is that the electrical braking, as it is now installed in plants, does not meet the IEEE criteria.

That is the switch gear and controls are not Seismic Category I. It is not single-failure-proof and the pump motors are not qualified for the LOCA environment.

In Mr. Pollard's discussion, or at least earlier in the paper that he wrote, prior to our presentation to the ACRS in 1973, he did discuss these issues, and at that time he pointed out that the pump motor could probably be expected to survive at least for the 20 seconds during blow-down, so that that wasn't the main issue, but what he was most concerned with is that the switch gear, of course, was not single-failure-proof.

We have considered this matter previously in our reviews with the vendors and among ourselves and we find that it would be extremely difficult to make this heavy switch gear comply with all of the criteria that I think Mr. Pollard would like to see.

For instance, the main breakers for the coolant pumps could not readily be put in parallel without jeopardizing the normal protection of the pump motors themselves.

That doesn't mean that certain parts of electrical braking schemes could not be made to comply; but, again, this was discussed with the ACRS earlier.

Despite all the limitations of electrical braking which we in our paper to the ACRS acknowledged, we still feel that the electrical braking can go to a reduction in overspeed that would be contained in the event of a major loss-of-coolant accident.

If electrical braking does work, the problem is probably moot, simply because the overspeed that would be reached would be the order of perhaps 5 to 10 percent which is well within the design capability of the particular pump motor and the flywheel.

So, in conclusion, we do consider the pump overspeed issue to be resolved on an interim basis, so that we can proceed with licensing of facilities.

We do believe it prudent, however, to obtain two-phase blowdown information, test results and so forth, to better understand the phenomenon and to determine the efficiency of actually converting to hydraulic energy to mechanical energy.

That more or less completes my presentation.

I will be happy to answer any questions.

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DR. ISBIN: Do I understand EPRI will cover some of the experimental parts?

MR. D. ROSS: Yes. Tom Fernandez is here. Perhaps, you would like to go directly to his statement.

DR. ISBIN: One aspect he might want to consider and answer later, Ray, your report of, what, 2-1/2 years ago?

MR. KLECKER: Yes.

DR. ISBIN: Was a very good report and the ACRS members did indeed study that report. I think what we are trying to do in this particular meeting is to give an accountability of what we have been doing, to point out, however, that perhaps we have not tackled some problems as vigorously as they might have been tackled.

One item in your report, as I recall gave a schedule for testing. You are far behind that schedule now. I think some comments should be made on the speed in which items are resolved. The experimental data base obtained. This would be the time to do it.

MR. STELLO: I agree it would be tie time. I noticed Dr. Kouts was here a moment ago and he is gone. I will try to answer it. I think I have to share your observation that it would have been more desirable for the program to have proceeded on a schedule that gives us results sooner than we somehow have gotten on track with. I think that the priority for which programs are funded and the schedules that are set and the money allocated for each program is one that causes the schedules to vary the way that they do.

In retrospect, I don't know that there would have been a way to rearrange the priorities with the available budget to have caused this program to have given us the data sooner then it seems to. I think it is the classical story of limited resources. We just did not have sufficient funds to start the program moving vigorously enough to get us the information.

However, I think things now look better. When EPRI gets up, the program is in place and it is moving, although I guess I just have to agree, I would have liked to have seen it move faster.

Maybe, if you want to ask the question again with Dr. Kouts back, he might want to add something to what I have said.

DR. ISBIN: No. Let's go on with EPRI. What was the last name?

MR. FERNANDEZ: Fernandez.

DR. ISBIN: Would you want to come up?

MR. FERNANDEZ: I am a program manager at EPRI.

I regret to say that due to travel schedule and the short notice about this meeting that my remarks will be presented in an informal fashion. I will try to do the best

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I can. And I will try to address the two comments that you just made, Dr. Isbin.

EPRI currently is sponsoring four research projects in the area of coolant pump behavior under LOCA conditions. These four projects, the funding support for them amounts to approximately $1-1/2 million, which represents a significant commitment of our safety budget in this area.

And the projects, as currently laid out, should be completing their schedules between late 1976 to the early to middle part of 1977. So, we are trying to proceed with all due haste, in this direction. The project includes both large scale pump model tests, as well as small scale pump model tests. It also includes both fundamental analyses, as well as what you might call engineering model development, verification and application.

Now, if you like, I can go through a brief hop, skip and a jump through the four projects we are sponsoring or I could entertain questions.

DR. ISBIN: Let's take the quick jump.

MR. FERNANDEZ: Okay. The first and probably larger project is being sponsored by Combustion Engineering and EPRI at Combustion Engineering. It predominantly involves testing a one-quarter scale model pump under both single and two-phase conditions. There is a phase of testing that includes steady state tests to characterize the pump performance under single and Phase 2 -- single and Phase 2 conditions and that will be followed by some transient blowdown tests, with this pump, to obtain information on the behavior under transient conditions. The status of the project right now is that shakedown tests on the loop and the pump are in progress and we hope soon to be into the Phase 1 testing.

A second project, which is in direct support of the CE project, is being conducted by Creare in New Hampshire. The Creare project will perform scale model tests with a 1/5 scale of the 1/4 scale, CE pump. Therefore, it would be essentially a 1/20 scale model of the large pump.

In addition, it will perform tests on a 1/20 scale of a B&W pump. The test loop is a mock-up for the CE pump test. They have a mock-up of the CE test loop, so that we will be investigating the nature of that loop as well as the pump. And the same will be true for the small scale tests on the B&W system. Tests will be performed with an air-water system. Later on I think there will be another loop that will be constructed that will be able to go to higher pressures and test under steam water conditions.

That project also includes some phenomena of ecological analyses and some model development, as well as a review of the state of the art on multi-phase behavior in pumpts.

The third project that is being performed by rm5

Babcock and Wilcox. It essentially utilizes their test data obtained on a 1/3 scale pump, with air water conditions. It includes approximately, I think, 500 steady state data points and about 250 to 300 transient data points. Don't quote me on those numbers, but they are approximately right. And essentially what they will be doing is constructing homologous curves for thepump, both head and torque curves, feeding those into a pump model and then later on taking that model and using it within the system calculation to assess the pump behavior under transient LOCA conditions.

The fourth project is being conducted at MIT. It is a small project. It is essentially addressing analyses of the pump under two-phase conditions.

DR. ISBIN: All right. Fine. Thank you.

DR. PLESSET: Who is setting the scaling logs? Is the MIT group going to do that? Also who is going to compare the significance of air-water tests with steam water tests and the effects there?

MR.FERNANDEZ: The scaling question is being addressed primarily by Creare. We are obtaining data on different model tests. I should mention that CE and B&W will also be looking at this question, both questions, Professor Plesset. The scaling, as well as the relative behavior of air-water versus steam-water.

A part of the CE pump testing -- I don't

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want to say that. The CE-EPRI pump tests will be followed by some KWU-CE pump tests, and in that program they will be testing both 1/4, as well as a 1/5 scale model pump, so that we are trying to obtain data on -- we will be obtaining data on 1/2, 1/3, 1/5, and 1/20 scale model pumps.

MR. ETHERINGTON: You evidently expect to show the pumps will not overspeed to the point of disruption.

MR. FERNANDEZ: We will be addressing the pump overspeed question in the CE pump test program.

DR. ISBIN: The staff's position is, I believe, you expect from the tests to show that the pumps will not overspeed to the extent they were calculated in Ray Klecker's report. This has to be verified. This is on the basis also of a Westinghouse report, WCAP-8163, which the staff is reviewing. The staff does note that they expected additional data to confirm the calculation of this report by December of '76. Does this tie in with, or is this another set of data?

MR. FERNANDEZ: It probably ties in with the schedule for the CE pump test program and we hope to maintain that schedule as close as possible. It is going to be a difficult testing program. We are going to try to bring it to a conclusion as soon as possible, around December of '76.

MR. DOCHERTY: The data you were referring to, the data that you were referring to in reference to the

Westinghouse WCAP is an independent study data. It is being developed from pump tests that are occurring in France, conducted by Framatone.

DR. PLESSET: Where? Where are those tests?

MR. DOCHERTY: In France.

DR. PLESSET: It is a big country.

MR. DOCHERTY: I believe somewhere near Marseilles. I can get the specifics if you wish.

DR. ISBIN: Thank you.

MR. KOUTS: Herbert Kouts.

Dr. Isbin, we did have a pump test program in 1973 and in 1974. Because of limitations on resources we had to decide where to place the emphasis in our program, so after we discussed matters with EPRI and foudn where they were trying this, we decided as a matter of emphasis we would put our resources in a plenum fill experiment and they would take care of the problems, and this is the cut we have had since that time.

DR. ISBIN: Thank you.

We are pretty close to schedule. I think we will move on.

Thank you. It is suggested that we break until 11:00 o'clock.

(Recess taken.)

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DR. ISBIN: We will resume the meeting now.

(Slide.)

MR. KNIGHT: I am Jim Knight, from the Regulatory Staff.

Our next topic concerns the allegations of Messrs. Bridenbaugh and Hubbard shown on this first slide.

The emphasis lines supplied by me.

The essence of this allegation is that the postulation of a pipe rupture in the vicintity of the reactor vessel nozzle referred to here as a nozzle break could yield incalculable results due to large lateral motions or even tip-over of the reactor vessel.

The allegation states thes gross vessel motions would be due to instantaneous pressure wave that would build up between the vessel outside surface and the biological shield.

We believe this is more accurately characterized as nonasymmetrical delta Ps that arise as a result of steam flow into the cavity.

And, finally, the allegation implies that these are new concerns and that the NRC Staff does not require evaluation of the phenomenon associated with pipe rupture at the vessel nozzle.

(Slide.)

The NRC Staff response made three primary points.

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First, despite the very low probability of a full pipe rupture in the reactor coolant lines, protection against breaks postulated to occur at the juncture to the vessel nozzle has been a design requirement for all light water reactors for many years.

Secondly, the external pressure differential effects referred to as instantaneous pressure waves is only one of the three loading phenomena that must be evaluated.

Reaction forces, and internal differential pressures must also be considered where appropriate.

And finally, that the natural resistance to motion stemming from the high inner shaft massive somponents coupled with the resistance from support systems, piping and seismic restraints result in small vessel motions, yielding results calculable by common techniques in fluid and structural mechanics.

(Slide.)

Just to put it very quickly in context, a very simple picture of a pressurized water reactor vessel.

The pressurized water reactor vessel, this happens to be a Westinghouse vessel, the vessel sitting down within the biological shield.

Not shown in detail here, this type of vessel would be supported by a nozzle support, sitting right up in

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this region.

We take a closer look.

(Slide.)

The vessel taken out of the cavity, with the location of the supports, depending upon tne vendor they either be as shown here resting directly on the concrete.

They may rest on a shield tank, or may have columns going down to a concrete support near the base of the reactor.

However, they are all nozzle supports, all current pressurized water reactor nozzles are supported.

This is a similar view of a boiling water reactor.

(Slide.)

This is the full 360 degree support.

Not shown in great detail also would be lateral restraints, primarily seismic restraints, which typically would be in the vicinity of the upper portion of the shield wall.

(Slide.)

To give some further insight of the Staff review of this matter, Mr. Vincent Noonan of the Regulatory Staff will run through the situation on the more difficult loading case, that of a pressurized water reactor.

(Slide.)

MR. NOONAN: I would like to start my presentation by giving you a rundown of the typical pressurized water reactor support, the ones we used in our analysis and also to tell you about the conservatisms in our analysis compared to the acutal support.

What we see at the top, we are looking into the reactor nozzle, from the reactor head.

This part is supported by socket plates to some cap screws and some dole pins.

In this particular version there is a threaded ball point that sits in the lower sliding block.

On the side here, before any load can be reacted there is a gap into the hold-down pins which are carried by the shear key and the vertical load carried out by the vertical cap screws.

This is part of a redundant support system because in the analysis done to date, these hold-down gyp plates in the end have been removed and we have looked in analysis where it's free to move this way or this way without any restraint.

That's a small version of the vessel. Only half-inch.

The large support would be provided by the large piping systems.

An idea of the type of loads.

(Slide.)

There are three types of loads. The first one is called the asymmetric internal pressure loading on the vessel due to a cold leg nozzle break.

It can be noted during the first 25 to 30 milliseconds this type of load is very, very transitory.

In fact, if you look at the load, it's four times with relaxation occurring within 25 milliseconds.

In fact, you can get a complete load reversal.

After that time, from 25 milliseconds out to a half-second we see a typical expedential decay, a classical textbook type of decay of the system.

A third type of load-second type of load is what we call the asymmetric extern. . pressure loading.

Again, this load is very, very short time duration, occurring in about 60 milliseconds coming in a steady type load around 200 millisecon1s.

Once we reach this plateau, this is well within the limits of the support by itself.

(Slide.)

The final load that we consider in the analysis, we refer to it as the jet reaction force, again we see the peak occuring within one millisecond.

Very rapid drop-off.

A stabilization of approximately 900 kips of force.

The oscillations out here are due to the pipe dynamics.

The pipes are constantly in motion while this force

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is being applied.

(Slide.)

To give you a brief rundown on the analysis and results of our analysis, looking at the pressurized support, horizontal load is applied to the nozzle, reacted to the socket plate, threaded ball, sliding block, eventually down to the concrete support sector.

Due to the transitory nature of load, all of it remains within the elastic limits of the load, except for the cap screws shown here.

There are six cap screws. And the analysis shows only two of them go in in less than six milliseconds' time.

I might note, the definition of plastic in our analysis is nine-tenths of yield.

(Slide.)

Finally, to show you the final benefit of conservatism used in the analysis, we assume a one millisecond break time and a 144 square inch break area.

The analysis on the pressurized water reactors have shown this is indeed conservative.

The piping system analyzed here took from six milliseconds to get to 40 inch break time, and average out around about 40 square inches.

In the analysis, again we are using 144 square inches.

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This is less than one-third of the value of the values for the analysis.

In summation, I might add that because of the original conservatisms used in the design, we find that due to the -- in spite of the high load we now experience in this new loading that the support itself is well within the limits, very capable of taking this load, and that we see close motions of less that two-tenths of an inch of the vessel.

MR. KNIGHT: To summarize the Staff's response then, we feel very deeply, I feel we have a strong basis for saying that the spectra of catastrophe that is portrayed in the allegation simply has no basis in fact.

DR. ISBIN: Just to be sure, your presentation is covering really A, B, and C?

MR. KNIGHT: Yes.

DR. ISBIN: So this is meant to be complete.

MR. KNIGHT: Yes, sir.

DR. ISBIN: All right.

Are there any questions?

DR. CATTON: One, on the assymmetric external pressure loading due to the cold leg break, is this due to the flow into the annulus, between the vessel and the wall?

MR. KNIGHT: Yes. That is a point I should have made rather quickly.

DR. CATTON: The forces seem to be so smooth, whereas the flow rates seem to be different.

MR. KNIGHT: Perhaps some of the follow action from the containment system could go deeper into it but I think that is primarily from the fact that the analysis, it's a multiload analysis and hence it tends to smooth out at peak values.

I think you can actually see in reality peaks below this.

It's an envelope.

DR. CATTON: Okay. So essentially it's integrated, over part of the vessel wall.

MR. KNIGHT: Yes.

DR. ISBIN: Vincent, these analyses were made by who? The Staff is critically reviewing analyses, but whose analyses are you presenting?

MR. NOONAN: The analyses are based on Westinghouse and Stone & Webster for the North Anna case.

DR. ISBIN: Now the question of vessel support and the subcooled blowdown load, was raised in the spring of last year.

MR. NOONAN: Last year. That's right.

DR. ISBIN: The Staff has been looking at the question as posed for a specific reactor, but has now concluded that this is a generic problem?

MR. NOONAN: That's correct, sir.

MR. KNIGHT: If I may address that, I believe very early in the game we came to the conclusion that it should and must indeed be reviewed on a generic basis.

And the Staff review proceeded initially to look at the case, North Anna case, in greater detail, while simultaneously looking at all other vendor support systems and our appraisal was based first on looking at the original design bases for these supports.

The original design loads used for the supports, in the realization that phenomenelogically the loads are of a similar magnitude.

In doing so, we found indeed the lower design loads were used for Westinghouse plants because they used a far more sophisticated analytical technique.

Others, rather than investing time and money in the more sophisticated technique, took a much higher original design load and designed within elastic limits within that, a typical engineering approach.

For purposes of immediate comparison we had the support or the vendor's approach that gave us the lower design loads.

We looked at the responsive systems designed to those lower loads first to see if there was indeed a major problem.

We concluded from these analyses, from original, more simple analyses and the in-depth analyses, that these were not the case and we therefore have the confidence that those supports designed to much higher original loads, even if subjected to an incremental load, still do not put you in a situation where you have an immediate cause for concern.

DR. ISBIN: But just to follow through on the chronology, you identified potential problems, the consequences were not yet evaluated, but you came to the conclusion that this could be a generic problem, but wasn't it until December perhaps, that letters went out to other Applicants to review their vessel support systems?

Or was it earlier and in a specific case where one Applicant was applying or going from 80 percent power to 100 percent power? They had not even completed their analysis.

I'm just trying to have you ascertain whether the substance of what I'm saying is in place or not, and you can modify it as you think appropriate.

MR. KNIGHT: Very good. Of course, what is missing in your scenario is the fact that within a very short time, after learning, if you will, on the North Anna docket that this is a possible problem, the Staff had made its own immediate assessment of the magnitude of the problem, and had not simply put on blinders and let things go on until when the eventual letter went tout to all the vendors.

By the time the letter went out to all the vendors we feel we were in command of the knowledge necessary to ascertain that there was not an immediate safety problem, in that you would get loads sufficient to cause gross vessel motion.

What was now needed was to ascertain by virtue of the letter that went out and others that are going out and will come out, that the design margins that are appropriate are still maintained.

I am differentiating between a situation that is an immediate safety problem and one where we want to restore design -- restore appropriate design modules that may have been infringed upon.

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DR. ISBIN: One final question in this regard.

Does the Staff have access to any independent evaluations for these loads?

MR. KNIGHT: We have two programs underway. One at AeroJet Nuclear and one at I believe Arnold Research.

MR. D. ROSS: In addition to doing some technical assistance work at Aerojet, there are two other locations where we are seeking independent aid. One is at Sandia, where we are asking them for specific assistance in reviewing the Westinghouse report on the multiflex code, and assistance in doing some independent calculations with the Sandia code. Its name, I believe, is CSQ, but I don't know what it stands for. It is a general multi-property mechanical code.

Also, Arnold Engineering Center in Tennessee where we started to work about a month ago on the effects of subcooled loads on fuel assemblies. All this work is at best a few months old and there is no progress to report at this date.

In addition to that, we are doing some work in-house with the WHAM code. Along this line, we are setting up models of each PWR type. That is, one per PWR vendor, to initiate blowdown and follow some of the pressures as a rather simple matter. It would not reproduce as is some of the hydroelastic results that Westinghouse might get.

Now, Dr. Kouts is here. He might like to comment cam2

further on further work or research in this area of a longer range.

The work I'm speaking of is Los Alamos, doing both analysis and experiments on subcooled loads and doing some — planning some experimental verifications.

MR. KOUTS: This work is very early. We don't have anything to report.

DR. CATTON: Can I ask one more question about this diagram you showed on the asymmetric external pressure loading. Who did these calculations? Did I hear you say Westinghouse?

MR. KNIGHT: What is shown here are calculations accomplished by Westinghouse. In the particular case, on the external, what we refer to as the external force or nonasymmetric external pressures, the Staff does an independent analysis to confirm the pressures that are calculated.

DR. CATTON: Do you know anything about how it was done?

MR. KNIGHT: I would like to refer to colleagues from the Containment Systems Branch who are ready to speak at great length on that matter.

DR. CATTON: I don't need any great length.

MR. KUDRICK: Jack Kudrick from the Staff.

We as a matter of course do independent evaluations on reactor cavity analyses. We normally use the RELAP-3 program as our basis for the nodalization and the detailed calculations.

DR. CATTON: This would be between the vessel and the concrete wall.

MR. KUDRICK: That's correct.

DR. CATTON: Most nodalization I have seen done with RELAP is very coarse. Here if it was too coarse you would tend to underpredict pressures.

I am curious now fine a nodalization did you use?

MR. KUDRICK: Nodalization sensitivity studies have been done with nodes ranging from 6 to a dozen nodes all the way up to 75 nodes, in this angular region.

DR. CATTON: Okay. Thank you.

DR. ISBIN: With reference to the nozzle break, was it the implication in the statement by the 3 GE engineers that the nozzle itself might rupture from the vessel?

MR. KNIGHT: No, sir. I don't believe that to be the case at all. With a minor bit of facility, if I can get back to the slide showing the allegation, they specifically discuss past experience with primary piping systems, cracks are most likely to occur at the vessel safe end, which is the most susceptible point for an instantaneous pipeline break.

(Slide.)

There is no issue with the postulate that is used which is a pipe rupture at the nozzle.

DR. ISBIN: But two questions.

What is the Staff position with reference to a disruptive pressure vessel break in which the nozzle comes out? That is one question.

Second, if you have a nozzle break which is the equivalent of the diameter of the pipe, has that been specifically included in your analyses?

MR. KNIGHT: To address your first question, does the Staff require evaluation of a failure whether the nozzle is blown out of the vessel; the answer is no. The credibility or probability of occurrence is we feel well established in the failure of reactor pressure vessels to be far below the level required for evaluation.

I am not sure I get the full impact of your second question. If there were a nozzle break of the same flow area as the pipe —

DR. ISBIN: Yes.

MR. KNIGHT: The analyses are not particularly sensitive to — in my own view, rather than breaking right at the pipe safe end weld, they break up a little bit toward the safe end — we are talking about relative inches, and the analysis would not be sensitive to that type of change.

DR. ISBIN: Harold, maybe you can ask the question better than I.

MR. ETHERINGTON: I think you asked the question cam5

and I think the answer I agree with completely.

DR. ISBIN: All right. Thank you very much.

Professor Leahy.

DR. BUSH: Harold, were you intending to ask Bill Cooper what his opinion of a probability of this event was?

MR. ETHERINGTON: To my satisfaction.

DR. BUSH: Would you express an opinion on the possibility of a blowout, not in the safe end necessarily, but in the nozzle per se?

MR. COOPER: Bill Cooper, Teledyne.

I think if you are talking of break in the general vicinity of the nozzle, to adjacent pipe, it is most likely to occur in that safe end region and I think it is unimportant to differentiate where in that safe end region.

The other thing that we have studied with respect to in-service inspection results of cracks in the vicinity of the vessel region of the nozzle, it is extremely unlikely that any through crack propagating from those areas would have significant cross-section, as compared to this area which is of considerably lesser strength.

This generally results from the fact that the piping forces are treated quite differently by the codes. As one moves toward the vessel within the so-called reinforcement limit away from the vessel — as a rough rule of thumb, at allowable loads, the allowable stresses in the vessel area are about 2/3rds of those in the closely adjacent piping.

MR. ETHERINGTON: I think the question really was, what is the possibility of a nozzle popping out like a cork out of a bottle and leaving you a hole bigger than the type mentioned?

MR. COOPER: It is not the type of vessel failure I would expect with the through penetration type of welds that we use in these plants. I can't recall ever having seen one in any other non-nuclear applications, where that pop-out occurred, that you describe.

DR. LEAHY: I am Dick Leahy from RPI. I am going to try to address what I think you wanted.

It turns out one day when I was investigating the wonders of my mailbox I found an invitation to appear here, and I think I understand what you want me to talk about, and I will try to do so.

(Slide.)

First of all, for those of you who aren't familiar with me, before I became Chairman of the Nuclear Engineering Department at RPI I was responsible for General Electric's safety program, safety R&D program, so I worked quite closely with a number of people, including some people that have spoken here this morning and also people such as Dale Bridenbaugh.

I really think rather than address this particular subject as a steam binding problem, I would like to call it parallel channel effects.

It will include many things, including the postulated steam binding concern.

As a matter of history, when I was with the General Electric Company we looked at a number of experimental data which we were taking and others were taking, which made us believe that the flooding, the so-called counter-flow current limiting CCL flooding phenomenon which we found occurred at the top end of our bundles had some implications on the stacking up of the water in the upper plenum and possibly some implications on the ability of the REFLOOD from the lower plenum to get up into the core.

If indeed you could support all this water in the upper plenum you could postulate a situation in which you would for sure delay the REFLOOD and also based on currently legislated models could predict some damage to the core in terms of meltdown.

So once we identified this as a real concern, we launched out on an aggressive program which Pat has described some of it this morning.

Part of it was analytical. In fact, a large share of it was analytical because of the lack of data and the difficulty to acquire it.

Then we also, in my particular group, launched out on a program to plan an aggressive experimental program to address this particular concern.

Since then I have gone to Rensselaer Polytechnic Institute, and I am still quite concerned with the problem, and in particular the generic aspects of parallel channels concerned, so I believe the reason I am here today is because of a proposal that I sent in on this particular subject.

Is that correct?

DR. ISBIN: Yes.

DR. LEAHY: I have a full copy of that proposal which I would like to submit to you and your committee, which gives a more detailed description than the short preproposal letter introduced into the public document room.

I think this puts it in much more perspective.

As I am sure you can realize, in a few pages of a preproposal you can't do very much. You can just excite some interest. And I take it the interest was definitely excited.

The technical concern itself is basically what Pat described. It is the water stacks up in the upper plenum.

I do not have a nice slide to show it, but you will have to let me wave my hands a bit.

As the water stacks up in the upper plenum from the ECCS injection, the water which does penetrate down to the lower plenum and builds up has various paths to flow.

It can go either through the core or through a parallel path in the stand pipe diffusers which are two-thirds the length of the core.

It is a parallel type of problem.

Where is the hydraulic resistance to loads?

If the water is stacking up in the upper plenum, there is a large hydrostatic head which it has to overcome in order to penetrate up through the core, so the preferential path in that particular situation would be up through the stand pipe diffusers and out through the break, so it would really have no beneficial effect to the cooling of the core.

Now, my personal opinion is that what really would have happened is because you have a large number of parallel channels at different power levels, that some of these, particularly the lower-powered bundles, would tend to preferentially break down.

That is, the water would tend to flow down through these parallel paths into the lower plenum and essentially alleviate this concern.

Unfortunately we have no real hard data to base our conclusion on.

There is some simple geometry data that has now been taken at General Electric which tends to indicate this could very likely occur and indeed one of the mechanisms which could cause it would be the subcooling.

However, there are a number of others which I could discuss if you care for me to.

I think the likely scenario is not really a full core steam binding concern. It is just that the interaction that you would have between the parallel channels would allow you to break the liquid through, so rather than stacking up, running out the top of the steam separators it would penetrate to the lower plenum and allow reflood.

That is something I would like to show experimentally and also qualify the analytical models to do that.

I think one thing that has come out of this particular concern is when I sent it into the NRC I was called in to review it in detail with them and we did so and it became very evident to all of us that there is quite a difference between the legislated licensing models that we license our plants with right now, in the real world, based on engineering judgment.

A good example of this is in this particular concern.

The fact is the current model would s , you would not get credit for steam cooling. However, the only thing that really holds the water up in the upper plenum is steam coming up through the core.

Now, if you take credit for the steam cooling you find that the heat transfer coefficient that you get is at least as great as the spray heat transfer coefficient from the water coming down, so as a matter of fact, the big concern would really be how long do you delay reflooding.

It would not be possible to set up a situation in the real world in which you have adiabatic core and thus melt the core.

All right. The research that I have proposed is certainly not all-inclusive. I think it is appropriate for a university to be engaged in.

(Slide.)

I won't dwell on this too much, but let me show you an example of what we would do.

The analysis, of course, would complement the experiment.

Let me be very clear on the fact that I think that other people such as the General Electric Company should aggressively address this progam and, indeed, they are on a more prototypical basis, a large water experiment.

What I would propose is a small freon experiment in which you have some instrumented annuli.

These are heater rods in test sections.

This is a simulated bypass to mock up the interstitial region in the reactor.

Simulated upper plenum with the stand pipe to the steam separator.

Water to simulate the steam spray injection and ability to simulate flash-off or the sensible heat from the lower plenum walls.

So you have flow in the various channels,

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including the stand pipe diffuser, which is two-thirds the core, and measure with transient delta P cells what occurs and measure impedence void gauges, what the void is and, therefore, the counter-current flow situation and in effect determine what nappens in a parallel channel array.

We don't have the information right now as to what really occurs.

Depending upon which hat you want to wear, you can speculate bad things or good things.

I think this sort of thing would help answer these kinds of questions.

That is all I have to say.

I would be happy to answer any questions.

DR. ISBIN: Well, Dick, we appreciate your coming here. We, that is the ACRS, particularly the subcommittees, have met with you in the past at our ECCS meetings. We have visited San Jose and visited you and your staff in San Jose. We have come to respect the opinions that you have given and, therefore, I thought it was appropriate that we ask you to come to our meeting today.

With respect to a proposal which you have discussed, but in the transmittal of this proposal, you highlighted in a manner which indicated a very pressing need, a pressing problem.

It is possible that others can misunderstand what you had in mind as far as the severity of the implications, consequences, or the phenomenon involved.

We thought it best that you give it to us in your own words, a perspective on where this problem sits as to its real need.

We did note that your experimental program would take some three years to complete. If this were indeed a problem of pressing importance, perhaps it should be addressed in other ways.

Therefore, if you can be very frank with us, in your point of view, on perspective, this is important because one of the charges that we have is: are we pursuing these problems correctly? Should we be doing more?

We would like to be sure that there is no misunderstanding on anything that you may have submitted or said and, therefore, by having this direct meeting with you we get firsthand what your point of view is and the place to assign it.

DR. LEAHY: I understand. Let me say a word about how I view this problem if it wasn't clear.

I view this in essentially the same category as the PWR core bypass problem, the steam binding problem, that whole bag of PWR problems that people are concerned about.

Now, certainly we look at those problems and as engineers we say, well, this one probably won't occur and that one probably won't occur, but as a matter of fact there are fairly aggressive programs to address those problems and I think rightly so.

I think the same sort of thing should be done on this particular hypothetical concern, because I have always believed my whole engineering career has been devoted to smoking out concerns. I think the more you smoke out the safer your reactor is going to be. Sometimes you smoke out imaginary snakes, but in this particular case this concern is real enough to be taken seriously.

I do and I know that General Electric does.

DR. ISBIN: Now, in treating concerns, positions taken by the Staff have been to exact some rather conservative restrictions on the evaluation model. You mentioned the bypass in which all water is lost during this bypass period, so there is an artificial restriction embodied in the evaluation model.

Let me ask you this question -- for the concern that you are looking at now, should there be some restriction in the interim?

DR. LEAHY: Beyond what is now in the evaluation model?

DR. ISBIN: Yes.

DR. LEAHY: I think the evaluation model as I know it is sufficiently conservative, perhaps for some of the wrong reasons, but I think it is sufficiently conservative to handle this particular case. I think the real implication of this concern is that you delay reflood and the way they handle the water which gets to the lower plenum now by flooding all the various parallel regions does indeed delay reflood.

DR. ISBIN: If I understood, when you stack up water you throw it away.

MR. MARRIOTT: That's correct. We do not account mechanistically for the accumulation of water over the core. The model is simply to do that precisely and we assume it is loss from the system. We take no further credit for it.

DR. LEAHY: I believe when all the dust settles there will be a net gain for the PWRs in terms of safety margins. I think it will speed up the reflood compared to the way it is calculated today and I think this will lower the peak clad temperature, but I would hasten to say we need some firm basis before we make those kind of changes.

DR. ISBIN: One additional point, or question:

would you comment directly on the implication one might have gotten by looking at that short transmittal? You talk about it generally, but I think it is best that you state something one way or the other so there can be no misunderstanding of what was meant.

DR. LEAHY: I think as I said initially, it is very difficult in two pages to describe what everything means. You just describe the overview and what the likely implication is.

Now, in the formal proposal which I have given you here, it describes it in more detail and I think puts it in perspective. What I meant by that, if you live by today's rules, which as far as I am concerned are the law, you can indeed calculate using presently available techniques and information, as the concern, but as the state of the art, detrimental concern, as the state of the art advances as we sharpen our experimental data and so forth, I think we will improve greatly where we are now.

One example is exactly the inability at the present time to take credit for steam cooling. I can calculate, in fact gave to my class for a homework problem, a situation in which had I peaked the power towards the bottom of the bundle and made some assumptions on vapor superheat which were very reasonable, we could calculate a situation in which you could have local clad melting toward the bottom of the bundle.

Surely you wouldn't have that in the real world.

Surely the vapor going up, using any reasonable correlation would give you a heat transfer which would prevent that from happening, but what I meant in that letter is in fact there is a difference in the real world and the legislated world.

If you can believe the legislated world, you can calculate anyting.

One thing encouraging, the trend on both the Nuclear Regulatory Commission's part and the vendor's move, moving toward realistic calculations, because I think once you have realistic calculations in place you can add on any margin you see fit and know where you are at. Right now that is not the situation.

DR. ISBIN: Are there other questions?

DR. PLESSET: Just as a point of information, when you are looking at the stacking of water above steam or in your research program, have you considered the possible instability of such a configuration from a mechanical point of view?

DR. LEAHY: I think it is a beautiful example. It is a freshman physics problem.

If you have parallel channels about a large plenum up at the top, if you start getting liquid down one of those channels, that will increase the hydraulic resistance of that particular channel, tend to divert the vapor holding the water up to the other channel, and that will create an accelerating effect for the liquid to come down the other channel for these and other reasons, including the effect of subcooling on steam, I believe we will indeed get breakthrough of some of the parallel channels.

It turns out though that you have to get a lot of those parallel channels conducting the liquid down to the lower plenum before you can take away all the water and completely alleviate the concern.

DR. PLESSET: I see. I was thinking of going to more junior grade physics. I was thinking about what is classical Taylor grade instability.

DR. LEAHY: The pattern of the parallel channels doesn't have the classy wave patterns of the classical Taylor instability. Anything I have seen in two phases flows including the flows at the top of the bundle where you have liquid going down and ultimate vapor going up, I think [illegible] this particular case it would be more related to the [illegible] in the individual bundles rath[illegible] than just the inst[illegible]

I guess I would expec[illegible] the peripheral bundles where the subcooling seems to be the highest would be the likely candidates to conduct the water down to the lower plenum.

DR. PLESSET: I don't know about the actual BWR, but I think in your research program you are going to find maybe some surprises because of the instability.

DR. LEAHY: I am sure we will.

DR. PLESSET: Okay.

DR. CATTON: Just out of curiosity, most of the core models used are commonly used in the horizontal direction, one node all across the core?

DR. LEAHY: For the BWR they treat them separately, because you have the channel walls across them.

DR. CATTON: Do you treat more than one?

DR. LEAHY: Yes.

DR. CATTON: It seems you would get flow of water down one and steam up the other. What precludes that?

DR. LEAHY: Prior to my leaving General Electric Company, and Pat can comment on where it stands now, there was no model with pressure drop coupling. All the parallel channels have the same delta P impressed across them, so they are driven by the delta P, but there was no calculational model at that particular point in time which handled the various power type bundles.

The bundles were different power. There was a model which would handle the core as one channel, as you had described, plus a parallel channel with the interstitial region, the bypass region, plus the stand pipe defuser.

Of course, that is the worst of all worlds. That is the worst possible situation because then you can never break down a parallel channel. The whole thrust of the analytical program at GE was to develop this calculational possibility which interestingly enough doesn't exist in the open literature. And appraise it.

DR. CATTON: I have a feeling this is a relatively simple problem.

DR. LEAHY: It sounds so when you first start, but it is a tremendously interesting problem, because what happens is, you get flooding at the lower orifice and you are starting to stack up water at the lower orifice in some of the channels and you are in a countercurrent flow situation; also you are starting to pull a free surface in the water in the lower plenum.

The boundary conditions in the code as far as you how much liquid goes down and how much vapor goes up each channel is not straightforward. If you assume each one of those channels is in the flooding conditions you can't satisfy continuity so mother nature comes and bites you a little bit.

It requires some care, because you are in counter-current flow and pulling free surfaces in various regions. This is why I am interested in it. It makes a fine project for some of my students. It makes very fine PhD work.

Do you understand what I said? Because I can draw it.

DR. CATTON: I understand the simplicity, but not the complexity that you describe, but that's okay.

DR. ISBIN: Are there other questions or comments?

MR. D. ROSS: Dr. Isbin, before we leave this item I think you should hear from RSR with a few words with respect to some comments we heard this week from General Electric with respect to research.

MR. SCROGGINS: The only comments I had planned to indicate was an indication that the position of RSR with regard at least to the points raised by Dr. Leahy, I think were fairly well summarized in the transmittal letter in the public document room written by Dr. Kouts, which simply states that the -- well, the concern has been raised.

We do not believe from the engineering judgment that it is a highly plausible situation and therefore would not represent a real safety concern. However, we, in agreeing with Dr. Leahy, feel there is a need for additional data and correlations in models indeed to verify the situation and we are currently in a planning stage, looking at experiments of the type that Dr. Leahy has proposed as well as larger scale experimenting analysis and analysis to indeed verify our judgment that this is not a true safety concern.

Denny, I don't know specifically what you were referring to earlier. I guess as part of our planning stage we are talking with people about some of the kind of programs that were indicated by Dr. Leahy that he felt would be desirable and also indicated in a letter from Dr. Kouts to you that we are looking into both fundamental laboratory-type experiments of the type RPI proposed as well as larger scale more integral system experiments. They can look into specifically these parallel channel effects.

MR. MINNERS: Warren Minners from the STaff.

There are some very simple two-channel experiments with the countercurrent flooding model GE has performed. I don't know whether you would like to hear more about them, but I think they can certainly shed some light on this problem.

DR. ISBIN: Well, we are really trying to place the proposal in perspective. We have gotten some better indications from Dick as to what he is suggesting. I don't think that we would take the time now to look at it in detail. We will come back with it at some later time.

MR. MINNERS: If I could summarize the results, this seemed to indicate in this simple model, that breakthrough countercurrent flooding occurs based on the models, and based on these reactors is occurring.

MR. MARRIOTT: I would like to mention our experimental program, not in detail but we have currently in operation a two-channel quarter scale loop of the nature of what Dick Leahy has suggested and in fact the results as Warren has indicated have been extremely encouraging.

We are going beyond that this year to two-channel experiments with heated tubes in which precise measurements of the phenomenon will be made. We are going to a full scale, full power ECCS bundle to very precisely quantify what the effects of subcooling are, and as Mr. Scroggins pointed out a moment ago, we have come to RSR with a proposal for a large integral test facility.

So let me make it clear that all of these programs are not in place to resolve a BWR steam binding problem. They are to gain insight into the mechanisms which we fully believe from an engineering judgment standpoint indicate that our models are extremely conservative to permit us to take credit for some of the subcooling effects and reduce the operating restriction on our reactors, because of the overly conservative model which is in effect fixed.

We don't believe that there is a safety concern with regard to the steam binding which Dr. Leahy is discussing.

DR. ISBIN: You may have the last word if you would like.

DR. LEAHY: I don't think that is inconsistent with what I said. I still would -- I can say the same thing about some of the PWR concerns and I guess people do, you know, if you work for Westinghouse, you would say the same thing. I think they are in the same category.

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DR. ISBIN: Thank you very much for coming. We will now proceed to the last item for this morning's session. This is an item which concerns what is called the Reed Report.

The General Electric Company offered to discuss in detail the Reed Report with the subcommittee in a closed session since they considered the material to be proprietary. The Committee concluded that we would prefer to have a shorter overall presentation which may nearly repeat what has been said in the testimony to the Joint Committee and keep the session open. So with that brief introduction, who will make the presentation?

Mr. Ross.

MR. G. ROSS: I would like to provide you with some information concerning three important items of the Reed Report. These items are: one, the reason the report was generated; two, the makeup of the task force that generated the report; and three, the fact that the report has been reviewed by the NRC staff.

The General Electric Nuclear Reactor Study, also called the Reed Report, was undertaken in the fall of 1974 at the request of the General Electric Chairman Reginald H. Jones. The general purppose of the study was to chart a technical course whereby GE's boiling water reactor could improve its competitive position by achieving a superior

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availability across the entire range of design, development, manufacture, construction, and operation.

Stated another way, the principal purpose of the study was to provide a basis for assessing a level of corporate resources including engineering, and development facilities, technical personnel, and financial support requried to enable the BWR reactor product line to achieve the same technical and competitive success that our turbine generator enjoys.

The Reed task force included nine of the most experienced designers in the areas of the General Electric Company. However, only two of these were from the nuclear division and the remaining seven were from other parts of the General Electric Company. The task force had eleven meetings, each of two or three days duration. They utilized 10 sub-task forces, which made in-depth studies of the specific areas of nuclear fuel, mechanical systems, materials, processes and chemistry. Members of the task force and of the sub-task force met with scores of engineers and scientists involved in our nuclear operation.

The effort focused at gaining complete information from all levels of our organization, not merely senior management. The work of the task force was completed last summer when the report was delivered to Reginald Jones and to other corporate officers with responsibility of charting

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our course and resources in the nuclear business. The report is typical of the process of study and review through which our top management can obtain objective appraisals of our major business ventures by persons who are not involved in the day-to-day management of that individual business.

The task force made numerous recommendations intended to improve the availability of the BWR. These recommendations dealt with the overall design considerations as well as specific plant components and services. It also made recommendations concerning the development and test facilities and concerning questions of management and organization.

The report is a document of considerable sensitivity from a competitive standpoint because it candidly discusses the opportunity for improvement of our product line and our organization and recommends steps to strengthen our competitive position.

A point I would like to make is that this report is not a safety report. The study was not conducted as a safety review. The study group found no reason to believe that applicable safety requirements are not being met for operating BWR plants or will not be met by future BWR plants.

While the nuclear reactor study is not a safety study we are mindful of our obligation to report to the NRC potential safety problems. Thus, the work of the task force was carefully reviewed by the General Electric safety and rm4

licensing staff to determine whether anything reportable had been discovered which had not been previously disclosed to the NRC. This review concluded that there were no reportable deficiencies which had not previously been reported to the NRC.

I would like to read three statements from the Joint Committee hearings. The first one is on the 24th. It is by a member of the Joint Committee. This is Representative McCormack. He said concerning the report: "This issue was raised clearly and deliberately as a red herring by Messrs. Bridenbaugh, Minor and Hubbard to try to challenge the company, to force the company to release proprietary information and to try to draw us into a position publicly to force them to do so --"

He went on to say, "I think it is a serious mistake for us to fall into that trap."

The other statement from the same hearing, is from our vice president, George Stathakis, general manager of the nuclear energy division. He said, "I think there is also another serious mistake or potential mistake that we must look at. If we cannot prepare an internal document which criticizes the way we go about doing our job that is critical and then make recommendations for improvement all across the line so that we can be a better party in that business and br more competitive, then, I think we have a very terrible

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problem. We will get to the point where we cannot prepare any document."

The last one I would like to quote from is a statement by Mr. Bernard Rusche, director of the NRC. He said, "A copy of the letter prepared by Knuth and Minners reporting the results of their review and our conclusion that we believe there is no need for NRC to possess the report is also included. It was evide . from our review that the detailed critical study of the GE BWR was valuable to the company and that they have honored their obligations to inform NRC of all safety related information thus developed. And more importantly, all of the matters mentioned are being considered in our current safety reviews."

In conclusion, I would like to say this study represents a major corporate effort which forms the bases where millions of General Electric Company dollars are committed to improve our competitive position. This is why we request that the information contained in the report remain company private in accordance with the provisions of 10CFR2.790 because this document contains the candid findings and conclusions of a task force created to improve the availability and reliability of the General Electric boiling water reactor.

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Thank you.

DR. ISBIN: The staff also had a response?

MR. MINNERS: Warren Minners, of the staff.

I was with Dr. Knuth, one of the two persons who reviewed the Reed report at the request of Mr. Rusche. I agree with Mr. Ross' statement that the items in the report, the Commission was aware of those items. There was no new safety information in the report.

If you have any questions about it, I will be glad to answer.

DR. ISBIN: The committee and the subcommittee have been involved with the review of GESSAR. There are continuing aspects of GESSAR which the committee will be looking at.

Can you indicate whether in our discussions with the staff and with GE we included all 27 items which have been noted in the Reed report?

MR. MINNERS: On the GESSAR review?

DR. ISBIN: Yes.

MR. MINNERS: I don't really know the answer, but some of the safety-related items were things for specific plants other than GESSAR. I would doubt that they would have been discussed in the review.

DR. ISBIN: Can anyone from GE respond in this regard?

MR. G. ROSS: I would say that all the items are covered in the total, overall review. I wouldn't say that just GESSAR would cover those. There are many of the items covered by GESSAR, yes.

I don't know what else I could say.

MR. D. ROSS: Dr. Isbin, in order to get much further, we would have to start discussing specific items, I am afraid. I think the decision of General Electric is that perhaps that should be done in closed session. Perhaps we misunderstood.

DR. ISBIN: No. I appreciate your offer to discuss things in a closed session, but if possible, we would like to answer some general questions in an open session and expressly verify whether or not the ACRS in its conduct of review of GE plants, taking GESSAR in particular, whether there are any items which might pertain to GESSAR which were not included.

I restricted the question to tie it in with your answer.

MR. G. ROSS: I guess the examples of that, things not in GESSAR, would be Mark I - Mark II containment.

MR. MINNERS: It is a difficult question to answer, Dr. Isbin, because there may be come details which were discussed in the Reed report which were not discussed in GESSAR, because GESSAR was a construction experiment review.

When you get to more detailed review in an FSAR, those things would probably be discussed. But the subject heading was there, but the details that were reported in the Reed report may not have been specifically discussed in GESSAR.

DR. ISBIN: As I recall, in your report, Warren, you did not specifically state that there were 27 items. Where does the 27 items come from?

Who furnished that quantity?

MR. G. ROSS: That was a list we generated, the Safety and Licensing Group. We looked down through this and out of that, we said here's 27 items that has safety significance. Let's look at each one of those.

Number 1, have we told NRC about that; and, number 2, is it a reportable deficiency?

We went through each one of those, mindful of that, and we came up with the answer of no.

MR. MINNERS: The licensing group specifically went through the Reed report for the specific purpose of identifying those items. The licensing group generated the list of 27 items.

MR. G. ROSS: Mr. Minners and Don Knuth read the whole report. They didn't read just the 27 items. They assured themselves that the whole Reed report didn't contain safety significant items that they hadn't heard about.

DR. ISBIN: The conclusion that I am coming to, and correct me, is that in the opinion of the staff, all 27 items are known to the staff, and out of these items there is nothing in particular that demands special attention at this time.

I was trying to get your judgment on whether the ACRS also knows these 27 items, from its actions in the past. And your indication is that the only way for us to proceed is to use a closed session to verify it for ourselves?

MR. MINNERS: It is my opinion that all the items in the report are a matter of public record.

DR. ISBIN: But there is a reluctance on the part of GE just to list the items as such. Is that correct?

MR. G. ROSS: Well, sir, if you were one of the stockholders of General Electric Company and you knew they were going to commit millions of dollars to a certain research project, I don't think you would want to give that to our friends of Westinghouse, Combustion Engineering, and B&W over here today. I think that is the kind of things we are really talking about.

DR. ISBIN: But you are asking the question to the wrong party. My position generally is that all safety-related items ought to be in the open literature.

MR. G. ROSS: That is the point. They are.

MR. MINNERS: That is the point. All the safety-related items in the Reed report, to my knowledge, I think it is a pretty complete document, are on the public record. They are available in the public document room.

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There may be more items than that, that are safety-related items concerned with GE and not the vendor but all of these I am certain are in the public record. So your question of whether the ACRS knows all of them, I can't judge what is in your mind and what your knowledge is, but I presume if it's in the public record that the ACRS is aware of it also.

DR. ISBIN: Well, the statement still leaves us a little bit short and the Committee will have to decide how to proceed from here, unless you want to add something else, that you are puzzling over.

MR. STELLO: I am puzzled as to why you are puzzled. The safety items in there, it certainly reveals the strategy how they are going to spend their money. There is nothing new there. There is nothing you aren't aware of.

DR. ISBIN: You haven't told us that though. It's the first time you are saying it in that positive way. I don't want to force you to say anything but that is what I have been trying to find out, whether in our actions we have dealt with these items.

MR. STELLO: When you say "dealt," may I use one example. I wouldn't look at the General Electric Company, because they may frown. The question of the core spray tests were, I looked at the report. I recall it was mentioned. Let me ask you, would you say the ACRS knew of that particular test result? You may choose to say you didn't know, but yet the

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information was clearly available. It was not an item that the ACRS probably until today had discussed. It was clearly available. The Staff knew about it. If I may, may I change it and say, did the Staff know all of the items in there? Yes.

Did the ACRS go down and discuss each and every item that was in there at one time or another? Well, I don't know if I want to venture a guess on that. I would say, if I had to look for a market, it would be a very high percentage, 85 percent of the time I think you would agree with me, it has been discussed. You personally may disagree with 5 percent of the items. Maybe a different ACRS member would disagree with 5 percent but I suspect it would be a different 5 percent of the items. It's a substantial report. It's very hard to do, unless you personally read it and you are looking more for personal assurance. The Staff is aware of all of the items that are in there, and they have been identified previously on the public record.

The only question that I guess I feel hesitant to address is, have each of those items been addressed at an ACRS meeting. I think that is what you are asking me.

DR. ISBIN: Yes. Well, I think we are perhaps placing too great a burden on you.

I am going to suggest that we adjourn for lunch. We can reconsider the question right after lunch.

MR. STELLO: And we will consider whether or not we

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will be able to make a statement that says we believe the ACRS has in fact discussed each of those issues. We will try to come back with an answer that says that if we can.

DR. ISBIN: Thank you. We will reconvene at 1:15.

(Whereupon, the meeting was adjourned for luncheon at 12:20 p.m. to reconvene at 1:15 p.m. in the same room.)

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AFTERNOON SESSION

(1:15 p.m.)

DR. ISBIN: The meeting will come to order again. For our first item this afternoon we would like to call upon the Atomic Safety and Licensing Appeal Board for any comments they would like to furnish us regarding their own independent review of issues. One of the particular issues raised by the engineers who recently dealt with the integrity of the steam generator for the pressurized water reactors. This is an item, for example, which the Appeals Board on its own initiative has undertaken a review of some of the issues.

With that as a brief introduction, whom am I calling on? Rosenthal, Buck?

MR. ROSENTHAL: I am Alan Rosenthal, chairman of the Atomic Safety Licensing Appeal Panel. I have also been sitting on the Appeal Board which is assigned to the Prairie Island Units 1 and 2 operating license proceeding. It would be inappropriate for either Dr. Buck, the vice chairman of the panel, who is on my left, and I might say is also sitting on the Prairie Island Board, or myself to discuss the merits of the controversy over steam generator tube integrity. That controversy is still pending before our Board.

It was suggested to me by Mr. Fraley, however, that the Advisory Committee might be interested in the procedures which the Appeal Board has followed in pursuing this matter.

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Because I think among other things that the course that was followed here is fairly illustrative of the manner in which the Appeal Boards generally confront technical issues. The Appeal Boards by direction of the Commission are called upon not merely to review such issues presented by Licensing Board decisions as the parties to the proceeding may see fit to put before the Appeal Board.

In addition, we conduct what is known in the vernacular as a sua sponte review, review our own initiative, of any question, technical or legal, which we think upon our review of the Licensing Board decisions and the record of the Licensing Board proceedings, merits consideration. And that, I might say, is how we became involved in the steam generator tube integrity issue.

That issue had been raised by Intervenors in the Prairie Island Operating License proceeding, raised before the Licensing Board. The Licensing Board determined that the methods that the Applicant then was employing, proposed to continue employing with respect to treating secondary system water, namely, the so-called phosphate treatment, was satisfactory and rejected the claim of the Intervenors that there was any safety problem presented by reason of the possibility of thinning or cracking of the steam generator tubes.

The decision of the Licensing Board authorizing the issuance of Operating License was appealed to us, but the appeal did not encompass the steam generator tube integrity issue. In other words, the parties in effect chose to accept the Licensing Board's resolution of that issue adverse to the contentions which they had raised before the Licensing Board. On a preliminary review of the record, however, the then single scientific member of the Appeal Board, assigned to the case, came to the conclusion that in fact there was serious question as to whether the conclusions that the Licensing Board had reached was adequately supported by the record.

Accordingly, the parties were asked in conjunction with the oral argument that was scheduled on the issues that had been rasied by the Intervenor's appeal to address themselves to the steam generator tube integrity question. On the eve of argument, the Appeal Board was informed by the Applicant that it was converting from the phosphate water treatment method to the AVT or All Volatile Treatment method.

We explored the question with the counsel at the oral arguments and we decided immediately thereafter that there sh ld be further proceedings conducted by the Licensing Board on issue of steam generator tube integrity. We accordingly remanded the case to the Licensing Board for that purpose. This was in September of 1974.

In January of 1975 the Licensing Board conducted a further evidentiary hearing confined to the steam generator tube integrity issue. It lasted a little over one day. Subsequently the Licensing Board came down with a supplemental initial decision in which in essence it reaffirmed its prior determination, that time in the context of the All Volatile Treatment method, that there were no safety problems associated with cracking or thinning of steam generator tube walls and they specifically determined among other things that the decision as to whether to install condensate demineralizers was an economic decision and not a safety decision.

In other words, they indicated that there would be no safety problems presented by the use of the AVT method. This decision as well was accepted by the Intervenors. Nonetheless, again following our ordinary procedure of conducting a sua sponte or on-our-own-initiative review, we examined the initial decision in the light of the record that had been developed at the supplemental evidentiary hearing in January.

By this time I might say the composition of the Board had changed to the extent there was now no longer two lawyers and one scientist but one lawyer and two scientists, Dr. Buck having joined this Board, replacing a lawyer member who had left the panel. So it was at this juncture, myself as chairman of the Board, Dr. Buck and Dr. Johnson, who had been on the Board throughout.

Dr. Johnson and Dr. Buck, upon their own review of the supplemental initial decision, again measured against the record that had been adduced at the supplemental hearing, came to the conclusion that the issue still had not been satisfactorily resolved. This time the determination was made by our Board that we, rather than the Licensing Board, would conduct a further supplemental evidentiary hearing and one was scheduled for initially October. It was finally held in January and the parties were advised there were certain specific areas of inquiry and broadly speaking, Dr. Buck may want to elaborate upon this, but they were, first of all, whether the AVT water treatment method was efficacious so far as minimizing steam generator tube degradation, thinning or cracking.

Second, whether the eddy current testing procedures were sufficient or adequate insofar as the determination of any degradation that had occurred was concerned. And third, whether the established criteria for the plugging of degraded tubes were adequate. The hearing was held in January as I have indicated. The case is still under submission. We have just received the proposed findings of fact and conclusionsof law of the Applicant. Findings of fact and conclusions of law of the staff and the Minnesota Pollution Control Agency, which is the other party in the proceeding at present, are due in approximately a week to 10 days.

My guess is that our decision is at least another two months off. That is essentially again what we have been doing in this case, and I want to stress that the Appeal Board does not normally conduct evidentiary hearings itself. If it meri 6

determines that a further evidentiary hearing is required, its usualy practice is to remand it to the Licensing Board for conduct of those additional proceedings. Indeed as indicated in the course of my discussion of the history of the Prairie Island, that is what we did in the first round. We have, however, on prior occasions, rare though they may be, taken evidence ourselves.

We did this, for example, in the Vermont Yankee case or the question of whether the containments of the boiling water reactors should be inerted and we have done it perhaps one or two other occasions but normally our review of technical issues is made on the basis of the record that was developed before the Licensing Board. Again, if we think that record is inadequate we will remand. I don't know whether Dr. Buck would like to remand something to that.

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DR. BUCK: I don't think I have much to add except that I think you should remember the steam generator situation has come up before.

I was happy to be a member of the ECCS hearing board and the Intervenors in that particular hearing tried to bring the steam generator failure into that hearing and we rejected that on the basis these were separate criteria, not connected with the ECCS critiera by themselves and that on the basis of the criteria one could rely on the integrity of the steam generator tubes.

This we still believe was the correct situation at that time.

We were not concerned with the steam generator tube primarily.

However, when the question of the integrity was brought up again before us and we aere dissatisfied with the hearing we felt we had to go through with this to satisfy ourselves that the situation was not a critical one.

DR. ISBIN: In your deliberations, Dr. Buck, or Mr. Rosenthal, where you consider particularly an issue you saw as spontaneous you indicated and you have some concerns over facets of the review, meanwhile the reactor is operating in this particular case and we need not be specialized as to cases at hand, but now make some judgment during the initial stage of your review whether or not more immediate action might be required.

And if so, what would be your avenue of approach?

MR. ROSENTHAL: Well, we have a standard with respect to whether or not we will allow a reactor to continue operating, or if we are on a construction permit level, whether we will allow a plant to be built while we pursue further inquiry, or during the pendency of any remand to the licensing board.

That standard simply stated is whether in our judgment the continuation of operation, or the continuation of construction, as the case may be, during the pendency of the further inquiry, will present an imminent threat to the public health and safety.

It it is our conclusion that it will, we would have no hesitancy at all about suspending the effectiveness of the operating license or the permit.

In this instance, I might say, that the appeal board at each stage of this steam generator tube integrity inquiry had to consider that precise question, whether allowing the Prairie Island facilities to continue to operate while the matter was further pursued, might present a threat to the public health and safety.

We concluded at each stage that it would not.

It was for that reason and that reason alone that Prairie Island is still operating today.

DR. ISBIN: The Subcommittee has considered it important that your group make the statement.

You represent an independent review. It's another part of the process.

The Subcommittee and the full Committee is concerned only in an advisory way, but we are interested in the technical information which may well be generated at your hearings as elsewhere.

Do you have any suggestions regarding the input of the technical information and whether the means of making it fully available to others is being used effectively?

DR. BUCK: I believe it certainly can be. We had three days of hearings.

Of course, the transcripts are available of all of the answers and so forth on examinations.

Mostly questions by the board in this particular case.

DR. ISBIN: Are these official transcripts?

DR. BUCK: These are official transcripts. All of our hearings have official public transcripts, except an occasional in camera hearing.

In addition to that, we have asked for additional information from both the Staff and the licensee in this particular case and some of that has already come in.

Again, it's in the docket book, it's public record, and there are still some more papers to come in, findings of fact and that sort of thing.

So all of the technical information we get is available. Certainly it can be made avilable to ACRS without any problem whatsoever.

DR. ISBIN: Are there any questions?

DR. BUSH: The discussion to this time has been on steam generators which of course is an inherent problem in regard to the response of the emergency core cooling system in the event of a LOCA.

With regard to this particular series of working group open meeting that we are holding, the statement that was made during the allegations was more generalized, in that they discussed failures of heat exchangers, which as a generic class will include steam generators, could I ask if either the licensing board or the appeals board have ever investigated the safety significance of heat exchangers other than those primary units that are specifically noted as steam generators?

DR. BUCK: No, sir, we have not. Not as far as the appeal panel is concerned. I don't know of a licensing board that has done it, either.

DR. BUSH: That would be my suspicion but I wished for the record to clarify it.

As an individual question, are you looking at the

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systems, have you ever assessed any safety significance for those units in the secondary system or tertiary systems?

DR. BUCK: No, we have not.

DR. BUSH: Thank you.

DR. ISBIN: Well, thank you very much, Mr. Rosenthal and Dr. Buck, for coming.

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MR. ROSENTHAL: Thank you very much.

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DR. ISBIN: Do we return to you now, Denny?

MR. D. ROSS: Yes.

Jim and I had some comments we could make or, if the Committee had any specific questions, we could handle it either way.

DR. BUSH: Could I ask a question? Let me reiterate my question that I have just asked, the system predominantly with regard to the steam generator, on the basis of the regulatory evaluations, have you established the safety significance for those other heat exchangers in the circuits?

MR. KNIGHT: Yes. Of course, depending upon the location of the heat exchangers, whether in the reactor coolant pressure boundary or portions of it, within the pressure boundary. Certainly, yes.

DR. BUSH: Can you clarify?

MR. KNIGHT: We require that they be designed concerning that portion of the heat exchanger, be designed according to the other portions of the reactor cooling system.

DR. BUSH: That I understand. I am thinking in the context of what kind of design basis accident could the failure of X tubes in a steam generator,for instance, result? And I will satisfy any question of failures of shells, et cetera.

MR. STELLO: I think, perhaps, Dr. Bush, you are looking more in terms of a consequence or design basis

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oriented review. So let me start with a few examples. The emergency condensers are looked at with respect to tube failures, amount of activity that can be released as a result of the tube failures and they are bounded and evaluated on the basis of the failure of the pipe, as far as the condenser in terms of what its consequences might be to assure that proper devices are placed on the system to limite the radiological consequence of that event. The heat exchangers that are used for the fan coolers inside of containments are evaluated much on the same basis, as to what they might be and their protective devices, to assure that these units are properly protected. The Millstone intrusion incident, as far as in terms of the condenser tube failures are evaluated in terms of tracking what they might do to insure that proper monitoring equipment is placed in the system to detect the failures and isolate them before they can have untoward consequences, so the particular design basis is dependent upon the particular component that is being evaluated.

I will get to one which I think is perhaps the most difficult to summarize simply. But I will try. The heat exchanger that is used for decay heat removal.

One has to evaluate what the consequences of leakages might be in these theat exchangers and how one would cope with the consequences of the leakage in terms of, again, a design basis event where you can detect and isolate the

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unit before you exceed radiological limits. The only direct effect you have in terms of interaction with the primary system, of course, is related to the steam generator to the PWR .

I am not prepared to go item-by-item in heat exchanger but I am trying to give you an indication of the types of considerations that are included in our evaluations.

DR. BUSH: When I listen to your remarks, predominantly one of the control of release of activity in the event of another initiating phase, I thought -- can you specifically point out anywhere the failure per se could be an initiator of a significant event?

MR. STELLO: Radiological release?

DR. BUSH: Well, no, I am trying to think of it as being the initiator of a fairly severe accident as such. That is what I am trying to establish The radiological release is obviously a functiion of the amount of activity that may have gone from primary to secondary system or that is in the primary system if it is, say, a letdown unit or something of that nature that could be released. I am trying to see if the failure of a unit could cause an initiation in itself.

I don't know of any. I am trying to see if in your evaluation you have established any. I can't remember this as one, that was looked at primarily from the radiological

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release point of view. I am trying to find out if it can be considered as a stage in an accident initiation.

MR. STELLO: The emergency condenser example I gave you is by itself a loss of coolant accident. It is outside of containment, so you have to be able to put in protective devices in the limit prevent that leading to a core heat up so there has to be equipment to isolate and mitigate that accident in that context.

DR. BUSH: That one is where you have a failure of the pipe --

MR. STELLO: Which also a failure of the primary coolant capacity.

DR. BUSH: Which gets back to our pipe failure, the design basis accident.

MR. STELLO: But that is the largest failure. There are smaller ones which are the tubes themselves.

DR. BUSH: Well, that is the next step I was going to raise. That is if we follow the analogy of the steam generator, if the pipe fails, can its failure initiate failure of X tubes and what will be the consequences? Can it affect the tenor of the path of the accident, or is it primarily a release of activity?

MR. STELLO: Primarily a release of activity with the limit of the primary coolant being standby actuation of protection systems, which isolate the break from the

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primary coolant system. Valves are made to close, to isolate that break.

DR. BUSH: But from a radioactivity release point of view, let me put the break arbitrarily right next to the heat exchanger. Okay? If I break the pipe in the classical mode and spill the water, primary coolant. Okay. What is the difference between that a the failure of tubes, from an activity release point of view. You isolate --

MR. STELLO: Rate dependent. The maximum rate would, obviously, be the pipe and the rate would be lesser for some conbination of tubes.

DR. BUSH: So, you don't see this as a step such as in a steam generator, if you fail a pipe and the failure of tubes may have an effect on, say, the operability of the ECCS? That is really what I was trying to get at.

MR. STELLO: The only relationship that we see that has that coupling is in the steam generators.

DR. BUSH: That is what I wanted.

MR. STELLO: He may want to add something.

MR. TEDESCO: As a matter of our review procedures, Dr. Bush, we do reveal all the systems in the secondary clad and the auxiliary, involving the primary coolant, the process core and so on. All essential safety systems are redundant to any failure in one of those systems should be isolated with this emphasis and the condenser doesn't close the

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heat exchangers in this area.

DR. BUSH: The reason I ask the question is the way in which the allegation was phrased, one, and two, when I attempted to find a response to the allegation, I was unable to do so. I must not have looked to the right case.

MR. D. ROSS: The staff testimony on this starts about Section 2, Roman numeral 2.

DR. BUSH: That is what I have in front of me.

MR. D. ROSS: Page 110, item J and continues through 117.

DR. BUSH: Which sets of testimony? I have three sets.

MR. D. ROSS: This is the March 2 testimony, responding to Bridenbaugh, Hubbard and Minor, the NRC's testimony. Roman 2, starting at page 110 and continuing through page 17.

DR. BUSH: I am confused. I am looking at the Bridenbaugh, Minor and Hubbard testimony, February 18, 1976 --

MR. D. ROSS: Roman 2.

DR. BUSH: I see lots of words about containment, et cetera, and I get over to steam generator failure and leakage, and I go to page 116 and it is all steam generator. That is why I asked the question. I can't find anything relevant to heat exchangers, which was in the original Bridenbaugh, Hubbard statement. That is the reason I asked

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the question. It may be and I couldn't find it, but that is what I was looking for.

MR. D. ROSS: It is further back in the report. I will dig it out in a minute.

DR. BUSH: I only raised it, Mr. Chairman, in the sense that the question we were requested to respond to has to do with heat exchangers, which is more general than steam generators, I think. That is the end of my statement.

MR. D. ROSS: Dr. Isbin, we understood at this point in the agenda there would be opportunity for the PWR vendors to comment as might be appropriate.

DR. ISBIN: Yes.

MR. D. ROSS: So, I suggest you turn it over to

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them alphabetically or in some systematic fashion.

DR. ISBIN: Do we have a response from Westinghouse?

MR. DOCHERTY: I am Pat Docherty from Westinghouse.

What I would like to do is give you a general overview of the procedure that we use to assure steam generator tube integrity during LOCA.

The insurance comes essentially by way of three steps.

First, in the collapse test for tubes performed for unfloored tubes, for cracked tubes and for thin tubes. And using these test results in conjunction with stress analysis performed in the tubes and the tube sheet, for loads resulting from imposition of LOCA forces, plus the safe shutdown earthquake forces.

And, thirdly, a development of plugging criteria that assures tubes are rendered out of service before theycome within the possibility of failing under the load imposed by these materials.

(Slide.)

Now, the LOCA transient and the effect on the steam generator is that the hydraulics are characterized by a very rapid reduction in the primary side pressure and propagation of a rarefaction wave for the steam generator tube sheets.

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Now, the hydraulics in response to the LOCA, in the tube sheet, what occurs is that the pressure very rapidly drops to a value of around 1300 psi, reducing the delta P across the tube from the nominal steady state value of the order of 1200 to 1400 psi.

Higher primary side pressure to lower secondary side pressure and reversing this delta P.

With the rarefaction wave and the reduction of system pressure on the order of 1300 psi, the resultant reduction across the tubes becomes relatively small for a significant portion of the accident.

This reduction is on the order of 25 milliseconds for 1800 psi.

So with this reduction, the mechanism postulated for tube failure would most likely be in the collapsed mode, with the pressure on the secondary side being higher than the primary side pressure.

Thus what I have here and what I am presenting, a table of data as the results of tests performed on a steam generator tubes in the collapsed mode.

What we have here is maximum pressure that was reached is 10,000 psi, with no collapse for the unplugged tube.

Now, what is presented here are various flaws that were machined into the tubes or flats, or reductions of the tube wall that were machined into the tubes.

These tubes were also tested.

The most severe of these being a two-inch flat with 25 percent of the wall remaining and the corresponding collapse pressure of 2200 psi.

Now, this is to be compared with the maximum load imposed upon the tube in the collapsed mode for the LOCA transient on the order of 1000 psi, which is the difference between secondary side maximum pressure and the containment pressure of the system.

(Slide.)

Now, in addition to those collapsed mode tests, a series of leak tests were run for tubes with cracks machined in the tubes.

What we accomplished with the test was to establish a tech spec limit, the maximum leakage we allowed and identify that in terms of a crack size.

The scale on the bottom is relatively hard to read.

This is crack size in inches, .6, 5, 4, 3, 2, 1. And this is flow rate and gpm.

After you cross the 1 gpm axis you attain a crack size of about .6 inches, which is the critical crack size below which you never expect any growth of the crack.

That .6 critical crack size can be compared to the crack sizes that were machined into the tubes that were tested.

(Slide.)

All the way up to an inch and a half.

So by imposing the 1 gpm tech spec limit for leakage, we assure ourselves that the tests that were run in the collapsed mode were sufficient to encompass the range of cracks expected in the reactor during operation.

(Slide.)

Now, going one step further, what we did was perform a stress analysis on the steam generator tubes for the imposition of the LOCA and the safe shutdown earthquake loads.

We nodalized the tube sheet and what we found was the most severe stress placed upon the tubes occurred just about the region of the U bend in Node 16, identified here.

(Slide.)

Now, this stress level, which I have a transient of, is indicated here and indicates a maximum value of about 50,000 psi.

This is to be compared with the steady state membrane stress of approximately 16,000.

This point is slightly in error (indicating) on mn5

the normal steady state operation.

(Slide.)

Now, in addition, any tube degradation, tube degradation and tube, generator tube problems have occurred here, so it is appropriate to compare the stress problems within the tube sheet to the steady state full power levels.

(Slide.)

What I have is a stress transient for the nodes near the tube sheet.

Now, again, the initial starting point is in error. It should occur up at 16,000 psi and you can see that as a result of the LOCA hydraulics you have a very rapid drop in the stress, on the tubes at the tube sheet accompanying the rarefaction wave and a rapid drop in primary site pressure to 1300 psi and this stress level is maintained at a very low value for a significant portion of the transient.

So that as far as tube stress at the tube sheet is concerned, the worst stresses imposed upon the tubes were at full power steady state operation and the prime situation of a LOCA transient upon the tube, particularly the tube region in that area, reduces the stresses on the tubes considerably.

Now, an analysis also performed for thinning tubes and thin walls, indicate even with a tube thinned to 40 percent of its original thickness, that such a tube, even if the 40 percent thinning were considered at the most severe location --

(Slide.)

-- is well within the ASME conditions for a faulted condition and it will be able to maintain its integrity.

Are there any questions?

DR. BUSH: I hear you. I am not sure I am convinced, however.

I think you are running a lot of static tests and making some inferences for the dynamic phenomena. I am not at all convinced, one, that it would necessarily be the same, but let me postulate something and see what you say.

If I assume a seismic event, and I apply the forces function to the shell and to the support of the tube sheets, I think from inertial effects I would not necessarily expect the bundle to behave as an integral whole and, therefore, I could put bending moments on the tubes at the support level, not necessarily at the bend but some other locations.

Now, the tests you have run I can also postulate because I know it is possible to have cracks that occur circumferentially rather than axially.

It is not too uncommon a phenonena.

The static loads that you are discussing, which are very high, I will agree, I am not convinced will necessarily be a one-to-one model, for dynamic loads.

In a bigger pipe I am certain that is the case because the dynamic loads will impose a totally different force feed.

Perhaps the need, because of the flexibility at which they can respond, it may not be a significant factor, but, the tests that you have discussed don't necessarily convince me that that is true.

MR. DOCHERTY: The evidence that I can offer, perhaps to address that question, is there were additional tests run with a bending moment imposed upon the tube. These were burst tests rather than collapse tests, and the original series of test was run for the burst mode and then tests were replicated again with the bending moment imposed upon the tube and the bending moment is on the order of 44,000 psi which is comparable to the bending moments you are talking about.

DR. BUSH: Except it was probably a static bending moment.

MR. DOCHERTY: Yes, sir, that is true. But the imposition of that bending moment on a tube with a crack tended to reduce its bursting limit on the order of 10 percent.

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Tests are referenced in the supplemental information provided in WCAP-7832.

DR. BUSH: Well, I get my gut feeling on these small sections would be that it may not be a factor, but I am not sure how I can prove that it isn't, and I am not sure that your data proves it isn't.

MR. DOCHERTY: I think the most significant point I want to make is that the areas where there has been degradation observed, that the stresses are essentially all membrane stresses, at essentially zero bending stresses indicated, in the region near the tube sheet and for that region the stresses are higher at full power operation than in the LOCA event.

DR. CATTON: Have two-phase flow instabilities during rapid depressurization with parallel flow paths been a consideration?

MR. DOCHERTY: No. This was modeled as a single bundle with the blowdown tube code.

DR. CATTON: Do you think that parallel flow path instabilities could be a problem?

MR. DOCHERTY: I can't imagine that even with flow instabilities and parallel path instabilities that you get pressure reductions lower than on the order of 1300 psi.

DR. CATTON: I am talking about vibrations and n9et

shaking and this sort of thing.

MR. DOCHERTY: This hasn't specifically been addressed.

What is done is that the shaking from the external forces imposed from loop movements during the hydro -- as a result of hydraulic transient is imposed.

DR. CATTON: This would be a different frequency and would follow the rarefaction wave. It has not been addressed.

MR. DOCHERTY: That's right.

DR. ISBIN: Thank you, Pat.

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DR. BUSH: Has the Staff looked at this dynamic loading aspect? My concern may be totally unrealistic. That is why I would like to have you tell me, if it is true.

MR. KNIGHT: Jim Knight, Regulatory Staff. Yes. We have looked at the analyses that have been performed here. In the total analysis that is done, the seimic loads as a time function, the socalled shaking load that is the response of the entire reactor coolant system which would be the shaking of the actual steam generator and the passage of the rarifaction wave through the tubes are all combined in the designed stress analysis.

Am I getting at the question in your mind?

DR. BUSH: Well, subsequent analysis, the experience they have in looking at them is an extremely complicated one and as a matter of fact not one that the code normally addresses so I guess it's really a question of whether indeed you have done a dynamic analysis.

MR. KNIGHT: Yes. This is the case where certainly as you point out it's not an analysis required by the code. It was an analysis requested by the Staff. And as you point out, they are very sophisticated and have difficult analyses.

The greatest inherent conservatism in the summation just presented, as you probably noticed, the analysis simply based on a single tube, in essence, free in space, under these loads. In reality, of course, you have the entire tube bundle,

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you have the supports which are not considered in this analyses, that provided damping, a good deal of damping and restrict the motion of the tubes.

DR. BUSH: Well, that is true, but also the supports, they apply a different forcing function or a different inertial characteristic of the tubes and therefore I suspect if you were to look at the loads as a function of the tube in a free field, under this circumstance, as contrasted to one that had a series of supports through it differently, that function differently, that where the overall amplitude might be less, the spike amplitude would be greater. That is my question.

MR. KNIGHT: We have taken an independent look at this oursleves, using the personnel and computer programs available at the Naval Research Ship and Development Command and all of the evidence that we have gathered to day shows that what you see is a great increase in dampening and resistance, that makes the stress level shown in this particular analysis presented quite conservatively.

DR. BUSH: There certainly should be a backup, because I would think some of the programs related to the submarine heat exchangers would cover this.

MR. KNIGHT: That is the reason we went to this.

DR. BUSH: That answers my question.

DR. ISBIN: Would Combustion Engineering like to contribute?

VOICE: I don't know of anything I could add at this point to what the previous speaker and our previous presentations on this subject, but I would be happy to answer any questions that the Committee might have.

DR. ISBIN: Any other comments the Staff wants to make on this item? Otherwise we will go on to the next item.

MR. D. ROSS: No.

DR. ISBIN: We planned to move Item 10.

MR. ETHERINGTON: Does B&W have anything to add.

DR. ISBIN: I didn't call on them but if they want to respond, they can.

(No response.)

DR. ISBIN: Item 10 deals with the Staff listing of operating BWR plants, but perhaps before you want to pick up that item, if you want to pick up the additional items from previous working groups, this might be the appropriate time.

MR. STELLO: I wonder if I can ask Gail Ross and General Electric Company to present the results of an analysis we asked them to do during the last working group session in Chicago. The question raised was what would happen if the main steam line isolation valve closed at the maximum rate they can close at and what would the resultant pressures and heat fluxes be, were that event to show that it is not of any major safety significance? We are trying to say not that this will happen, but to try to find a way if you will to bound the problem.

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They have done such calculations and I would ask if they could at least describe the results of those calculations and perhaps leave copies or at least a copy of the results of the calculations so that it can be included in the record, if you would permit, Mr. Chairman.

DR. ISBIN: Yes. By all means. Are you ready to respond, Mr. Ross.

MR. G. ROSS: Yes, sir. I am going to leave with you two sets of curves. One is for three seconds which is the minimum allowed by tech specs and the other one is for a one-second closure of the main steam line. This is ramming the steam line valve home at the fastest rate we believe possible. We think it's somewhere between one and 1-1/2 seconds so the analysis was done at one second.

The maximum pressure we got out of that was the 1157 psi. The normal set point for the safety valves is 1210 for this. This was done for Monticello. The safety relief valve for that plant are 1075, 1969 and 1985. What really happens in this particular type of a transient when the main steam isolation valves ram close you get a signal for scram when three of the valves get moved 10 percent, so what is really turning it around so there isn't any consequence is a scram, initiated at 10 percent valve closure.

What you see is no increase in the heat flux. It's less than the 100 percent you started with. And the neutron

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flux goes up to 144 percent but it's for a very short period of time so it really hasn't any consequence. I will leave the curves with you, both for the one second and three seconds.

Are there any questions?

DR. BUSH: How sensitive is the amplitude of the pressure ramp distance from the vessel to the valve? Obviously I don't think you can give me a quantitative one but you did this for Monticello.

MR. G. ROSS: Yes.

DR. BUSH: I guess the question is, if I look at the position of the valves at plant X and I put a bounding value which is obviously a function of the architect-engineers, what is the range about X, in other plants? In other words, it could be 150 foot closer, 10 foot closer or what have you.

MR. ENGEL: The main steam line piping is finished by General Electric. It's almost identical for each plant size and all analyses are done with the specific plant configuration.

DR. BUSH: You are telling me the distance to the first valve is essentially fixed.

MR. ENGEL: Right. It's fixed. It's only when we go to other transient-like turbine trips that steam line pressure differs.

MR. STELLO: While Mr. Ross is up there, there was one more question raised as a carryover item. That was with relationship to the recommendations that were referred to in the report from the three engineers, that suggested that the General Electric Company or someone else had made some recommendations with respect to fog nozzles, and new fuel storage facilities, and as I indicated at the earlier subcommittee meeting, we were not fully aware of such recommendations and we would refer the question to the General Electric Company for an explanation of what the recommendation was and its significance.

MR. G. ROSS: I have a slide I would like to get to respond to that.

(Slide.)

First, I want to make it clear that it doesn't matter -- I mean there is no consequence if it's completely dry, partially flooded or fully flooded. We are talking about the new fuel storage. So if it is completely dry or partially filled or completely flooded, there is no problem. We originally looked at the partial moderation back in 1967. And then there is an ANS Committee that came out in August of '74 and what they said there, you must consider the optimum moderation. That is where we really started looking at this one more time.

Also there was a standard review plan which said this must be considered a new design. So we went back and looked at each one of the BWR plants at that time and based on our review of that, we found that normally storage of new fuel is covered with steel or concrete covers. Also the new fuel storage elements are covered with plastic bags. Now, to get into a problem with this mist, you must get complete fog down in around completely the steel elements. It just isn't around the outside.

It must get completely within the contents of the new fuel storage. In fact, you must cover a volume that is 25 by 6 by 12, with a uniform mist somewhere in approximately 0.1, plus or minus. We believe also that this is highly unlikely. In fact we looked at the probabilities of that considerably, and you can't get that from a sheet of water. A sprinkler system won't give it to you. Also, hydrogenous foam won't get there. We look at that and concluded it wasn't a credible event. We looked at the possibilities of that.

We looked at first the probability of whether you have a major fire on the refueling deck, and that is like a probability of 10^2, and whether then also it's up there in the new fuel storage area. That is a probability of like 10^2. Anyway we went through this and came up with a total probability like 10^6. If you look at that and take away some of the conservatism and we feel it falls below 10CFR 100 and even if you consider the event happening, it falls below the limits of 10 CFR 100, so first we don't believe it's a credible event. Second, even if it does happen it doesn't have any consequence.

The consequence is below that acceptable for 10 CFR 100.

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MR. STELLO: Mr. Chairman, I hope first that your probability statement is 10^{-6}.

MR. G. ROSS: 10^{-6}.

MR. STELLO: The question on the floor was whether or not General Electric Company has ever made a recommendation to anyone regarding new fuel storage. And what the form and nature or reason for the recommendation was?

MR. G. ROSS: Okay. We went out and looked at some of the plants and found some did have variable fog nozzles around them, and we suggested they remove them and also remove the burnable material in that area to further reduce this improbable event.

MR. STELLO: Did this relate only to facilities that were under construction and were using the spent fuel storage as a new fuel storage facility? Is that the concern?

MR. G. ROSS: That was primarily the concern, yes. We have a large number of fuel elements.

MR. STELLO: Was there any concern for fuel that would normally be stored in new fuel storage?

MR. G. ROSS: No.

MR. STELLO: That is all.

DR. BUSH: Is there still another carryover item?

MR. STELLO: Yes. There is one more. The question that was raised on Tuesday was what test program would be conducted to measure the events that follow on actuation of a

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relief valve in the BWR Mark I containment system. We did not have all of the specific details of what would be done in the test and how it would be conducted and how it would be instrumented and how that would be used to answer the question of fatigue life on the torus shell structure. I wonder if I can ask Mr. Paulson to go quickly and present what that test will consist of and follow it up quickly by a reiteration of the fact that we don't expect the problem of fatigue life. Very briefly a comment on that following Mr. Paulson's discussion.

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(Slide.)

MR. PAULSON: I want to summarize the Monticello safety relief valve actuation tests which are part of the Mark I owners group long term-program.

The purpose of these tests was, one, to obtain strain data for fatigue life evaluation of torus structure as a result of the actuation of the safety relief valves.

Secondly, to obtain pressure, temperature, and water level data to evaluate the effect of pressurization in the relief valve discharge piping and in the torus.

(Slide.)

Some of the measurements that will be obtained at Monticello are as follows: First, pressure on the torus skin.

They will obtain the transient response by transducers during the actuation of the safety relief valves.

Secondly, they will obtain pressure and temperature measurements in the relief valve discharge pipes in order to verify or develop a forcing function model.

They will also obtain water level and the discharge piping to evaluate the effect of consecutive valve actuation.

Pressure and strain gauge measurements will be obtained during multiple valve actuations, to help in defining an analytical model and also in determining the torus fatigue margin on the plant life.

We will obtain the temperature in the torus pool to evaluate local mixing effects during discharge.

The water level in the torus pool will be obtained to monitor the motion of the air bubble as it leaves the ram's head.

There are two more. Strain gauge and accelerometer measurements will be obtained on the relief valve discharge piping and also on structural supports.

(Slide.)

Finally, accelerometer measurements will be developed to develop the load being transported through the foundation mat and other structures.

(Slide.)

Briefly, here is a schematic, looking down at the Monticello torus.

What I have done here is just alphabetically numbered the relief valve discharge lines.

The tests will include individual actuations of these valves, A, E, F, D, G, and B.

In addition, valves A and F will be simultaneously actuated and also valves A and E.

Then valves A, E, and G will be simultaneously actuated.

Finally, they will actuate valve A sequentially through a five-second discharge, followed by a brief reclosure

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and then a second five-second discharge.

(Slide.)

Just to give a very brief summary of the instrumentation that will be available, on the torus wall sucking header and torus columns, there will be total of 70 sensors with 164 channels of data being recorded.

These are accelerometers and strain gauges.

26 sensors in the torus pool.

38 in the safty relief valve discharge pipe and supports.

And three on the pedestal and base mat area.

And to finally discuss the schedule.

(Slide.)

The tests were originally scheduled to be conducted for the end of February of this year, but were postponed because of the institution of the drywel to wetwell delta P operation.

So-called delta P fix.

The Mark I owners now have proposed rescheduling the tests for May of this year.

That concludes my brief overview. If there are any questions members of the Staff would be glad to answer them.

MR. STELLO: I wonder if we could very quickly summarize the questions related to the fatigue that will follow from these tests and hopefully clarify the record that this is a problem of no probable force at the moment.

MR. SHAO: ASME Section 3 fatigue analysis was performed and from the analysis it showed that the torus is good for 5800 cycles.

Assuming a 40-year life, that means every year it can take about 145 cycles.

For each actuation there are eight cycles.

So you divide 145 by eight, it's good for 18 actuations per year.

And we feel this is a reasonable number.

It also should be noted that ASME Section 3 fatigue curve has a safety margin of 20-odd cycles.

If we say 18 cycles, essentially 19 times 20 means 360 cycles.

We hope this analysis will be confirmed by the Monticello testing.

MR. STELLO: Thank you, Mr. Chairman. We are ready to go back to the original schedule.

DR. ISBIN: Thank you.

MR. D. ROSS: On item 10, we would like to take item C first and Dan Fieno of the Staff has a couple of charts.

(Slide.)

MR. FIENO: The material that I am presenting was discussed in detail at another Subcommittee meeting by Dr. Richings.

I would like to go over the allegation, then to give a summary of what our status is on the rod sequence control system.

The allegations basically were that the electronic patches have been added to mitigate mechnical deficiencies and that the patches add to complexity of operation and are frequently ignored and that mitigating systems should be improved and made mandatory.

(Slide.)

Now, basically our probabilistic study by Dr. Richings has led to the conclusion for the 10 plants listed here from Oyster Creek to Vermont Yankee that basically the probability of the rod drop accident is less than 10 the minus 12 per reactor year.

Based on this, we have determined that no fact in the RSTS is necessary.

However, these plants do have rod worth minimizers to aid the operator in performing his withdrawal sequence.

Now, plants such as Browns Ferry 1, Peach Bottom 2 and 3, and Cooper, Duane Arnold, Hatch, and the rest of various versions of the rod sequence control system.

In particular, for example, Brown Ferry 1 has what they call a group C rods. The first three will be updated to the group notch control, for the RSTS.

Plants such as Fitzpatrick, Brunswick 2, and Browns Ferry 2 have group notch control at outset.

But basically this is the status of the plants as we see them.

The important point here is that we do not believe that these systems are just patches to the systems.

Basically, they are aids to the operator in performing his withdrawal sequences.

This is all I have to say on this. It's just basically a summary of the status.

DR. ISBIN: You mentioned a probability of less than 10 to the minus 12.

MR. FIENO: For the 10 reactors which do not have RSCS.

DR. ISBIN: What would be the probabilities for the rest of them?

MR. FIENO: Basically the probability was based on the 10 older plants, so if you add 10 more plants it would be a little more.

Basically it's the same ball park. Very improbable type of accident.

DR. ISBIN: Let me see if I fully understand.

You are considering an accident of less than 10 to the minus 12th.

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MR. FIENO: That's correct.

DR. ISBIN: And for the first 10 reactors, no action is required.

MR. FIENO: No backfit action. That's correct.

DR. ISBIN: And for the rest of them, action has been taken?

MR. FIENO: That's correct.

DR. ISBIN: What is the criterion that you use for whether or not action should be taken?

MR. FIENO: Basically I think the question is at the newer plants --

DR. ISBIN: Historically this has been considered and therefore you are continuing on with it but if you had a probability of an event of less than 10 to the minus 12th, would you consider that a significant action should be taken?

MR. FIENO: I think the answer is, it's really a question of the historical precedent.

In other words, the current analysis, the probability method, if those had been present at the time one would come to the conclusion that one would not need an RSCS here per se.

I don't know if that answers your question.

In other words, once the decision had been made to install an RSCS, then the question comes up, do you need it for the older plants and I think the answer is not really if you belive a probabilistic analysis.

DR. ISBIN: All right.

MR. STELLO: We went over the probability analysis at a previous meeting.

I think in order to fully answer those questions we would have to repeat some of that.

DR. ISBIN: I will go back and read it.

MR. STELLO: What I was going to suggest, that particular question was addressed at another working group meeting and without kind of summarizing what we did there again, I think it would create an incomplete answer on this record.

I might ask, the same question really comes up at three working group meetings.

It came up at the Tuesday meeting, partially today and it will come up again for the purposes of evaluating reactivity effects at the meeting I think on April 7th, if I recall.

DR. ISBIN: No. Our purpose today is not to discuss the reactivity effects but was merely to present a status of all of these items on the various reactors, but as long as you mentioned probability, I asked the question.

MR. STELLO: The intent was not to cover probability today, because we had previously covered it.

I wonder if I might ask you to look at Tuesday's transcript. It wa by Dr. Richings and it would give a more

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DR. ISBIN: All right.

MR. D. ROSS: Item 10-D, Roy Woods of the Staff will speak to this subject.

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(Slide.)

MR. WOODS: I am going to give a very brief summary of what the contention was and what our response was, and then give a brief status of the plants. The contention was, after having mentioned that several plants had had failures of the feed water stagers and that vibration of the spargers induced by the flow of the feedwater through it, resulted in early failure of the sparger and required re-design and replacement of the sparger under extremely difficult field conditions. Such replacements required significant personnel radiation exposure and outage time.

The cracking and breaking of the sparger creates a very unsafe condition. No way has been developed to provide on-line detection of this failure. How many existing plants have this defect? How will we discover the defects before it is too late?

As we said, our testimony and as Ron Engle said this morning, we don't see the safety concern, with the feed water sparger vibration. The cracking progresses very slowly. The best evidence of that is experienced. There have been several failures. What we mean by failure is, you observe a crack and what the ex-GE's employees testimony implied by failure was complete failure, where the whole sparger falls off or something like that. So, the failures we have observed have been just cracks. Okay. The reason for that is, there aren't any large stresses to cause complete failure once

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you get small cracks, the delta P across the sparger is very small. As Ron Engle pointed out this morning, you would get control room indication if you had complete feed water sparger failure, which we have never observed or a very large opening because you would get cold water mal-distribution.

You would see local power spikes and corresponding compressions on the local power range monitors and you might see it on the on-line computer calculating the nuclear limits.

So, the first one, we don't expect the thing to fail. Even if it did fail you would observe it in the control room, but even if so, we would want to talk about what would happen if it did fail, so we point out, the feed water would still get in the core and you would not be causing a LOCA. I can make that point by showing you what the thing looks like.

(Slide.)

I apologize for going over this. Most of you probably know it. But this is the safe end, the nozzle and the reactor vessel. This is the thermal sleeve. This one is part of the feed water sparger. The sleeve and the sparger itself. The part that is vibrating is this thermal sleeve and the rest of the sparger and it is only connected to the pressure boundary of the system by these bolts, so to get complete failure, what they meant by complete failure would be a crack all the way across here or across here and the

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feed water still comes in and gets in to the core and there is no possibility of a loss-of-coolant accident. You haven't put a hole in the pressure boundary of the vessel. That is basically in a nutshell why we don't think it is a safety problem. You are not putting a hole in the vessel.

If the thing did fail completely, which is what I was just describing, it would be entirely internal effects and you would not have a loss-of-coolant accident, so you are reduced to these possible effects of failure. You can have flow blockage by pieces, but, as Ron Engle pointed out, you are not very likely to get pieces in a position that could cause severe consequences.

You are not likely to be able to go through the jet pump and if you did, you don't have the flow velocities necessary to sweep large parts up to the flow, to the orifices to the fuel assemblies.

Damage to the core spray piping is unlikely. If it did happen it would be detected immediately by a system that is desired to detect exactly that kind of damage. It is a delta P signal, which I can explain if you like, but it suffices to say you would know immediately if you broke one of the internal core spray pipes. Jet pump damage is not likely. If you did damage a jet pump, it would be an operational problem and not a safety problem. As I pointed out before, the feed water mal-distribution would be a major problem.

but it would be an operational problem. NOt a safety problem.

(Slide.)

I have this one slide that comes from a November meeting that I believe GE presented to us.

(Slide.)

This is a summary of what has been seen. Mill-stone, now, here again I better tell you, failures mean only they observed some cracks. Not necessarily even all the way through the sparger. It could be just surface cracks. These are the various plants tha. have been fixed up in one way or another. I don't propose to read all that, but what it means after 21.7 months of operation, they saw some cracks and fixed it. The way they fixed it, I guess I will have to go back to another slide.

(Slide.)

The basic problem, I don't really want to explain the whole cause of the problem, but it is vibration of this part, caused by a bypass leakage flow through this area. This is the nozzle and this is the thermal sleeve, so this is a blown-up view of this part right here. The bypass leakage flow through this tiny annulus, that is the primary cause of the vibration that causes the cracking, that causes the potential failure, so the fix that GE proposed was either to weld the thing, as shown here, or just to expand this to a larger diameter so there is less leakage flow, so there is no

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vibration. That has been done. This expanded sleeve fix on Duane Arnold, Cooper, Fitzpatrick, Hatch 1 and I believe Millstone has that fix. Yes.

Other plants, as I understand it, the plans are each time they take the head off they look with probably binoculars from the refueling crane and if they spot any cracks, they put in one or the other of these two fixes.

I think the operating plants,it will be the expanded sleeve fix. That is all I had planned.

DR. BUSH: Shall I say I have some reservations? I might agree that we have not faced, as is the case with a severe accident, but I can't help have a case such as Fermi 1, though it is a different reactor, was faced with the same problem. We had flow blackage and fuel melting, so whereas it may not be the ultimate design basis accident, it could be pretty severe, so pieces of material that got -- that went down and then began to block the channels, I think could represent a rather severe case.

Furthermore, the design is such that if we didn't raise this as an issue, we have a nozzle problem above and beyond this, which is in essence directly related to the design and fit of the sparger. Not the sparger, per se, but the tube that goes in there, the thermal sleeve.

I understand the necessity for the sparger and I see your arguments regarding the severity or lack of severity of the accident. 481 164

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My question fundamentally is, this is another case of a design that has been modified and modified and modified again, and so I would like to ask, when can we get a design with, shall I say, with a highdegree of confidence, that it will be reliable and in itself by failing, wouldn't cause other problems?

MR. WOODS: Are you referring to the fact that Millstone had proposed three fixes that didn't fix the problem and then the fourth fix you are saying --

DR. BUSH: I can take Millstone, yes. That is the classic case, of course. My concern is, I would hope we could converge on something in the reasonably near future so that we can eliminate this, because even though one can view that the failure of a sparger per se does not represent a severe accident, it begins to get us on a series of paths where we don't really know as much as we would like and we get in an initiating mode, so I would like to see us in the condition where we have taken appropriate action so we won't be faced with a problem of a failure which, in turn, will initiate some form of an accident.

MR. WOODS: As I tried to say --

MR. STELLO: It was a long question and it certainly went to an issue that I think the people that can only answer it, is when we will have a particular design that will remove that concern, is the General Electric Company and I would like

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to refer them to the specific answer. When will they have such a design that satisfies the concerns that have been raised by Dr. Bush.

But the one question that you did raise, I think speaks to the concern of the safety issue with respect to blockage. We were talking about a gross failing dis-sparger, where all of a sudden it could fall out and, yes, the potential exists for blockage, but we don't think it is likely because the pieces would not get in the jet pumps and the lower plenum that could be floated up to block anything of significance.

But even more important is the fact, I think that if such an event occurred, you would see it and recognize it and you would shut down because it would clearly be a condition that would be easily recognized as an abnormal condition for the plant, but I would like the General Electric Company to respond to the general issue that you raised.

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DR. BUSH: I might mention in the past, they have failed to recognize the incidents until they got into the fuel melting phase.

MR. ENGEL: First, let me say a couple of words about the flow blockage situation.

The unique location of the feedwater spargers is such that in order for a piece to go from the spargers into the area of the fuel assembly and the orifices which are of concern, one must go through the jet pump area.

To get through the jet pump, it gives you a relatively limited size.

We looked at blockages of jet pumps. In fact, we have had blockages of jet pumps in operation, and there is no way you can get any fuel failures.

We typically analyzed flow stoppages in ten jet pumps. This does not lead you to a critical power-type concern, so you then start looking at the possibility of pieces getting through the jet pump, and then somehow getting back up to the fuel assembly in that orifice.

The largest piece that can get through a jet pump is like two square inches.

DR. BUSH: Is the statement equally applicable for Oyster Creek and Nine Mile Point, too.

MR. ENGEL: No. Those are quite different, looking at the jet pump design. But to get through a piece of that

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size, it is coming through the jet pumps with a significant downward velocity and it is going to end up on the bottom of the vessel.

The velocities in the lower plenum are of the order of a couple of feet per second.

That is not sufficient velocity to raise that density. Then, as far as your other question as to when are we going to have a fix for the problem. I think today we believe we have a fix, called Sparger 4, for Millstone.

It has operated for eleven months, been inspected with no indications of any cracking. That same design has been tested in the test facility in San Jose which I identified the cause of the vibrations being the bypass flow.

So we have done both cold flow testing which, to identify the cause and to implement this particular fix, the fix has operated and there are no indications of any problems.

All I can say is only time will tell whether or not that is, in fact, the final fix.

DR. BUSH: Do you think it eliminates the feedwater nozzle problem, too?

MR. ENGEL: It minimizes the bypass flow going around the thermal sleeve.

By minimizing that, you are minimizing the

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cycling on the nozzle. So it is a step in the right direction.

Again, only inspection data on those plants will tell you what the status is on the cycling in that area.

DR. ISBIN: I have perhaps a few questions.

In the NRC reply to the Joint Committee on page 214, you talk about the possibility of flow blockage. You recognize that you need a large blockage to cause fuel melting.

You continue, in following the hypothetical accident, you talk about damage would be confined to the blocked bundle and would not propagate to adjacent bundles. Whose scenario is that? Is that a staff position? Is that a GE position.

MR. STELLO: The analysis presented is an analysis the Staff believes describes the scenario? I think the General Electric Company probably needs to address whether they agree or disagree wiith what we wrote. I think that is a fair question.

I think if you don't ask it, I would like to, whether they agree [illegible] ree with what we said.

MR. WOO[illegible] you can answer it. We were basically agreeing with the GE Topical Report that makes that argument. We have reviewed that report. We agree with

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it. I am not sure if we agree with their specific numbers. I think they conclude it takes, 90 percent flow blockage or something, and I am not quite sure we would go quite that high, but we would go a long ways in that direction.

MR. STELLO: I think I would like the question answered by the General Electric Company, if they would look at page 214 and say whether they agree with the facts stated there, and whether anything needs to be changed.

DR. ISBIN: I was going to suggest only, Vic, that perhaps you might be overstating your position, about propogation to adjacent bundles. I thought, for example this was one of the features of PVF to determine propagation, the nature of the blockage and the melting in a particular bundle might be such that it could propagate.

? PVF

The question hasn't been fully resolved?

MR. STELLO: There is no question that that is one of the reasons for running PBF.

? PBF

However, I think in a matter where a judgment is made, before you get to such results, is a statement of what our judgment is now, and what we have done is we have stated that that is our judgment.

Of course, no one could ever preclude theoretical possibilities. It is a theoretical possibility that damage would propagate.

However, I think the configuration we are dealing

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with BWR bundles, and any time you have reached the bundle would allow flow to come in from the bypass region, it allows us to make judgments that we don't believe the propagating matter is right for consideration here.

Perhaps your view that we have overstated, well, I don't think so, but our view remains, I think it is a reasonable judgment.

If anyone on the Staff would like to voice an opinion different to the one that I did, I would invite any member of the Staff to do so.

DR. ISBIN: I am only trying to clarify the statement. I think we have done that.

MR. STELLO: Are you going to allow the General Electric Company to answer my question? I hoped you would.

DR. ISBIN: We may come back to you, if we have time.

MR. ENGEL: I concur in Vic's statement. We do agree with the Staff conclusion.

DR. ISBIN: I assumed that you would.

MR. WOODS: I would be amazed if they didn't.

DR. ISBIN: Let me ask you the second point. If you have a known flaw in a system, and you are willing to look at the eventual potential consequences, and then there is an accident and you are talking about the single failure criterion, does this single failure criterion

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recognize that you might have been dealing with a system which included a flaw and, therefore, could fail additionally? In your analysis, you believe that with the single failure criterion, you have, indeed, bounded all of the possibilities. That is so stated, I think, in the response, but I am asking you a more general question, that if you start with a recognized flaw, which you are following, should the single failure criterion be expanded to include any additional failures?

Have you thought about that? Just put it that way.

MR. STELLO: I have thought a great deal about it. Single failure criterion embodied in the regulation already reflects consideration that if the accident itself can introduce other effects, the other effects have to be, by definition, included in your analysis as having occurred with the accident, then the single failure has to be applied thereafter, but I don't think the single failure itself represents any bound for all that can't happen.

The possibility that you suggested, that do I need to re-examine single failure criterion, in light of perhaps there could be some part of a system which is perhaps representative of less margin than I would like it to have, and I think the answer is, that has to be part of the judgment that one needs to make to assess whether the

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plant can continue to operate without any further restrictions and sometimes further restrictions are necessary.

I will give you a direct example. in the case of channel boxes where we did not have the view that if you had an accident you could count on the behavior of the box, with respect to the accident, and what we did, we caused the power level of the reactor to be decreased, so that we could, in fact, count on the analysis as being a correct analysis in the event of the accident.

So whenever we are faced with this situation, we do, in fact, take appropriate action, so that our single failure criterion remains valid. That is exactly what we did in the case of the channel box.

We expected channel boxes would, in fact, fail and could fail in the event of an accident, and therefore took the appropriate measures so that the margin we would require, including consideration of single failures, was adequate.

DR. ISBIN: I think that is a good answer. Thank you.

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MR. D. ROSS: Go back to the Item 10, now, loose parts monitoring and Tom Novak has a couple of slides.

MR. NOVAK: I an trying to be responsive to the Subcommittee's request for a discussion on loose parts monitoring, in reviewing the testimony from the three GE, I think the allegation with regard to flow-induced vibration problems both in PWRs and BWRs sets the groundwork for the discussion.

There were three ways, at least I identified, to respond to the allegation. I am here to concentrateon that one which deals with loose parts monitoring. What I have is a summary of the current operating reactor position with regard to specific systems designed to monitor either parts that are vibrating in excess of a design limit or in fact are loose and are vibrating.

As you can know, there are plants operating today which do not have what we would call specific loose parts monitoring systems. They would rely on the other features of their design, both going back to the preoperational tests and surveillance. I can answer specific questions with regard to loose parts monitoring, if there are any.

DR. BUSH: Tom, your figures with regard to General Electric on one of 20, what plant is that, pray tell?

MR. NOVAK: That is Monticello.

DR. BUSH: Is that the one where they tested a system and they said it would work but they didn't think they

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would use it?

MR. NOVAK: Actually, we quickly --

(Slide.)

I note here Quad Cities was also included as a Yes. Monticello has installed in it what is called the B & W design for loose parts monitoring. I have had discussions with those gentlemen as late as this morning. The system was installed after construction of the plant.

Basically, it relies on acoustic sensors, located around the vessel on the recirc line. They would s _ that they have had fairly good success with this unit. It's been in operation something on the order of two years. One incident where they noticed that they were able to pick up a vibrating drain line, for example. This has been really the only occurrence of substance, wherein an action taken by the operator of the plant more or less came about as a consequence of his monitoring his loose parts monitoring system.

DR. BUSH: Then the background noise has been enough to stamp out the signal with regard to such an event.

MR. NOVAK: I specifically discussed whether background noise, because of boiling phenomena and so forth, would have an effect. Obviously, where you locate the sensor is going to make a point. These locations are reasonably remote from boiling. There is one specifically up near the main isolation steam line va ve and some noise level from steam

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separators has been determined.

The noise level is evidently of such a nature that it's sufficiently low, it can be assumed to be the background level and then alarms are just set at some level above this background level. I think in discussing this with Northern States people, they are generally satisfied with it and they intend to keep it in operation.

DR. BUSH: Thank you.

DR. ISBIN: Thank you.

Next item.

MR. STELLO: May I take this opportunity to come back to the Reed report?

DR. ISBIN: Yes.

MR. STELLO: We have had an opportunity to talk some more since the question came up this morning. What we have done is talked about all of the things that are in the report. And I have designed perhaps the best way to try to address that question is perhaps to address it in a very personal way. That is kind of me to you as to how I think you would react if you personally read the report.

I have looked at it myself. I came to the conclusion that I think if you personally read the report that you would agree with the observation that I made earlier, namely that all of the concerns were concerns that you had at one time or another considered.

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The difficulty of, in trying to identify the Committee as a whole, to be able to again make that same observation with respect to each individual member, I felt that that was a task that was difficult for me to try to take on. I feel, since I have known you personally longer, I felt I could make that observation perhaps with respect to you, since I think many of the concerns that you have expressed I have had the opportunity to hear and I wouldn't want to be in a position to have to try to do that for each individual member of the Committee, which I think then speaks to the question of the Committee.

I don't know if that helps you, but that is the best we are able to do I would like to leave it there.

DR. ISBIN ...That will be accepted in that fashion with no additional comment. Thank you.

MR. STELLO: Mr. Coffman is next for Item 10B.

MR. COFFMAN: I would like to discuss the current status of the operating plants concerning the channel boxware problem. Just a couple of comments to refresh your memory concerning the channel boxware problem. It's caused by the vibration of the instrument tub .

It runs along the channel corner, channel box corner, and this vibrat ion is excited by flow through the one incy bypass wholes.

(Slide.)

In the lower core support plate, which are just

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barely visible on the Vugraph. The modification which was developed by General Electric to eliminate this significant vibration impacting on the channel box corner, was to plug both the plug, the one inch by wholes in the lower core plate and to drill two small wholes to replace that flow area in the lower tie plate of the fuel assemply, which is down here, on the Vugraph it just kind of show as a couple of lines.

(Slide.)

Now to the status. There are three conditions. All the reactors have plugged these one inch bypass wholes in the lower core support plate. And some of the reactors are in a configuration where none of the lower tie plates in the assembly of any holes drilled in them.

Three of the reactors will be in a configuration where they have three -- where they will have some holes drilled, and then, too, which are in the first stages of licensing, will be with all of the lower tie plates having had two holes drilled in them.

Some power restrictions have resulted from the plugging of the one incy bypass whole. These have been primarily due to either MAPLHGR limits, or MCPR limits. These are local limits rather than general limiting on the entire reactor. Some of these reactors would not be at 100 per-ent power due to other limitations, so to assess the

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impact on power generation capability, due to the plugging of these holes needs to be qualified. For example, on Brunswick, it's not Brunswick. It's currently under review for this reload. It's not yet determined what it's allowable power for the cycle . It was operating at approximately 50 percent power, but that was because of an attempt to reduce the flow through the core and mitigate the impacting of the tube on the channel box corner.

Cooper's station and Duane Arnold were really the only ones actually li-ited in power due to plugging. Cooper's Station was at 85 percent power due to a limit on its MAPLHGR curve -- on its MAPLHGR.

The Duane Arnold was limited to ab [illegible] 85 percent. It will be starting up.

In the beginning of this next cycle, it will be limited to between 90 and 92 percent power and at the end of cycle, it will be limited more like 80 or 85 percent. Fitzpatrick, it's expected to reach approximately 85 -- 88 percent. Hatch is 90 percent. Hatch is at 80 percent. Well, that is due to the plugging. Hatch has some other restrictions which are limiting it to about 80 per cent.

The last three are expected to reach full power, the last three of those which have no drilled assemblies, are at full power due to plugging.

However, Pilgrim is limited otherwise to 98 percent.

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The three which will have some drilled, will be at 100 percent and the ones we are about to be licensed and go to -- all drilled assemblies will be at 100 percent also.

MR. STELLO: Would you take the mike off. We can hardly hear you back here what that hum.

MR. COFFMAN: You recall last time I tried to do with it, but I was told to put it on so I didn't argue this time.

DR. ISBIN: Just go ahead.

MR. COFFMAN: The monitoring of this modification of the plugs can be -- the drilling of holes is accomplished on these reactors by primarily three means. That is, the accelerometers which are based at the bottom of the instrument tubes, tip trays, noise monitoring, and then the surveillance visual inspection of the channel box corners at the end of the cycle.

The visual inspection has actually been accomplished on two of the reactors of the Vermont Yankee, you will recall some time ago a had 27 of the channel boxes inspected after it had operatated for a period of ten months in a plugged only configuration.

We just recently, within this week, it was reported to us, and it was reported to you this morning also, hat 47 of the channels of Duane Arnold had been inspec[illegible] after 9 months of operation and the condition of their channel box

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corners was equivalent to the condition of the BWR 3s which did not have the bypass wholes drilled in the lower core plate, nor did they have this difficulty -- this significant vibration problem.

The plugs are being examined also. The plugs have been examined in -- that were removed from Vermont Yankee and we have a commitment from Duane Arnold to remove two plugs at the end of this next cycle which will be a cumulative of approximately 20 months of operation, for surveillance, for primarily for relaxation of the spring.

That concludes the current status. I will attempt to answer any questions.

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DR. BUSH: What is the Staff position with regard to this item?

Is it considered that the "fix" resolves this so that it can move over and be a resolved item?

MR COFFMAN: Yes, sir. The status is that the ultimate fix, the drilling of all the lower tieplates and the plugging of all the holes has been approved.

It was evaluated and the SER was issued March 2nd.

So it is considered a resolved item.

But in getting from here to there we have had to approve the monitoring.

DR. BUSH: Which SER?

MR. COFFMAN: The title is rather lengthy, but it is the title, A Generic SER on the Modification of the Entire Core to Eliminate Vibration from One-Inch Bypass Holes.

I can make sure that you have a copy of that.

MR. STELLO: I can assure you they have a copy.

DR. BUSH: Let me ask my second question.

Are we supposed to evaluate it?

I have a personal interest. I have been collecting information on this.

MR. STELLO: Do you think the committee wants to evaluate it?

DR. BUSH: That is a committee decision.

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MR. STELLO: We have been keeping the committee informed as we went through each step of the process. The SER that Frank described is a summation of all the previous aspects of the problem pulled together for I hope the last time.

I had given much thought as to whether or not I should ask the committee to review it, but if the committee feels that they would like us to come down and duscuss the item with them, why, we would be more than happy to do so.

I think we have had previous discussion on this subject and we would be pleased to entertain the concept of coming down in the near future at a subcommittee meeting.

Perhaps that would be a useful thing to do. And if you would allow us to replace this item on the resolved item list. In that context, it might be worth while.

But I leave that for consideration of the committee with the expectation that they will get back to us on it.

DR. BUSH: A lot of the questions were on a power hole basis which presumably could be relaxed.

It was in that context.

MR. STELLO: Perhaps we can bring it up again.

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May I suggest at the April 6th and 7th generic subcommittee review that we bring up this subject and discuss it further and leave possible committee action to follow from that meeting, if you would agree.

DR. ISBIN: Spence agrees.

DR. BUSH: I have to chair one or the other, so I guess it doesn't make much difference.

MR. ROSENTHAL: We are to Item 8.

MR. SOBON: Our part in introduction here is we have the chairman for the Mark I owners group present. He is to make a summary statement on the status of the Mark I effort. Pretty much a reiteration of what has been given to the subcommittee and full committee at the Vermont Yankee meeting again.

Then we have, again, summary statements by Mr. Stark on some of the scaling issues that were raised relative to that meeting again.

He will also address the questions that were raised relative to load margins or sensitivities, if you would. And then some comments from Bill Cooper of Teledyne and Norm Edwards of NuTec relative to structural interaction.

The remainder of the presentations I believe are planned from the Staff.

MR. KEENAN: Tom Keenan, Chairman of the Mark I

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owners group. I also represent Vermont Yankee.

I would like to start out with a very quick summary of the Mark I owners group.

We have been in existence for approximately a year. We are a group composed of sixteen companies which own and operate Mark I BRWs.

We are formed as an ad hoc committee to review loading phenomena that were identified last year for the torus of the Mark I.

We have utilized the common approach, pooled our resources and talent to expedite our solutions to the problem.

We have a number of consultants.

The General Electric Company is the program manager for us. And they provide testing consultation and engineering services.

Bechtel Corporation is retained as a subcontractor for the structural analysis.

In addition, we have Teledyne Materials Research a technical consultant.

Dr. Cooper is here. He will address the group shortly.

In addition, we have NuTec, a California-based consulting company, that is also a technical consultant to our organization.

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We also have Electric Power Research Institute who has done some testing and continues to be an additional consultant to the owners group.

Shortly after we were formed we broke down our solution or proposed solution to the problem into two programs, a short-term program and a long-term program.

The short-term program, the purpose of it was to conduct analysis of critical structural systems or elements and do limited testing as deemed necessary to provide increased assurance that the Mark I containments would maintain their function against the most probable course of the LOCA event, considering the latest information that became available to us on pool dynamic loads.

I think the important points to remember about this short-term program is to improve maintenance function and to establish areas which would require more detailed analysis in the long-term program.

The long-term program had a purpose of combination of testing and analysis, provide a generic basis for demonstrating the adequacy of the Mark I containments for the life of the plant.

By evaluating the designs under loads created by the LOCA and safety relief valve operations established by this criteria.

Our present status is we have completed the short-term program from the technical standpoint. We are presently preparing the documentation on the results of the short-term program for submittal to the NRC. And we are in the final stages of putting together the long-term program, and we will be making a submittal in the near future to the Staff for their review.

In regard to the allegations made by Messrs. Bridenbaugh, Hubbard and Minor in their testimony before the Joint Committee, we of the owners group have reviewed that testimony and have found nothing of which we were unaware in regard to the Mark I program.

As has already been stated, we are addressing the relief valve problem.

In their testimony, the former GE employees had indicated that there was no inherent program for relief valve testing and it is our position that this is erroneous.

The safety relief problem is being considered as part of the long-term program and the Monticello Testing Program described previously is the first step.

That will be conducted in the near future, so we feel we are adequately addressing relief valve problems.

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Tht concludes my remarks.

As Mr. Sobon indicated to you, we are going to summarize som additional areas that we have covered previously and re-review some of them and cover some areas where we feel that there has been continuing interest as a result of our previous meetings on the subject, so we will be covering scaling and load margins and then a discussion of structural interaction by Dr. Cooper of Teledyne and Dr. Edwards of NuTec.

At this time I would like to turn it over to Mr. Stark from GE.

DR. ISBIN: Would you mind repeating the schedules for several reports that you listed?

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CHAIRMAN MOELLER: Any questions for Mr. Dyekman?

There bei.g none, thank you, sir.

Does that complete your opening statement then, and at this time, then, we will move to the NRC staff, and ask Mr. Bournia for a review of outstanding unresolved issues, and the staff's conclusions on this application.

MR. BOURNIA: Mr. Chairman, I'm Anthony Bournia. I was recently assigned to be the Regulatory Project Manager for concluding the radiological safety review of the Washington Public Power Supply System's application for the construction permits for Projects No. 3 and No. 5.

With me today are Mr. Parr, the Branch Chief of Light Water Branch 3, and various members of the staff that have performed the review.

In the following remarks I will first briefly summarize the chronology of some of the major milestones attained to date in the review, and secondly, I will present the open items in our review.

I should point out at this time that these projects are the first nuclear projects that have referenced CESSAR-80 since the issuance of the preliminary design approval.

Another thing I should point out before I go into my main discussion is that in the safety evaluation report we inadvertently did not address to th other owners of the WPPSS nl #25

MR. KEENAN: We have recently reassessed the critical path elements of our whole program and we are in a position now of completing the short-term program and it includes the long-term program. A larger scale model which we have committed to build will be considered part of the long-term program.

So to go back to the submittals, I indicated we are preparing the documentation for the short-term program for submittal to the Staff and that will be sometime in May.

And it is our present intention that the long-term program, formal submittal to the Staff, is what we believe a long-term program should be, will be also early May.

DR. ISBIN: Fine.

Thank you.

MR. STELLO: Today we made a special effort to assure that all of the consultants that have dealt with the question of the adequacy of the scaling that has been used in the tests for the load measurements, that all of those experts are here today.

We would hope that we could in a very forthright and clear manner set forth all of the questions, answer all of the concerns related to anything on scaling, because we have all the people that have dealt with this

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question available today.

DR. ISBIN: All right. How much time do you have?

MR. STELLO: I would encourage, if there are any questions in that regard, that now is the time to bring them up.

We have the people here who can address them and we are willing to stay as long as the committee is willing to stay to make sure that they get all the information they need.

DR. CATTON: I think we pretty well addressed them at the last subcomm ttee meeting.

MR. STELLO: When I walked away I had the impression that there were questions that were not answered and I wanted to make sure I have the people here today.

MR. STARK: Mr. Kennan just spoke about the creation --

DR. ISBIN: Do you want to identify yourself again?

MR. STARK: I am Steve Stark for GE.

Mr. Keenan spoke about the establishment of the Mark I owners group to evaluate loads that were not previously considered in the design of the Mark I torus.

(Slide.)

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The evaluation of the loads has been broken down into a short-term program and long-term program.

The short-term program is aimed at the rapid assessment of the containment integrity to justify the continued operation of the plant until a detailed evaluation can be performed.

That detailed evaluation will be performed in the long-term program.

The first part of the short-term program was to categorize the loads into those that are significant on the structure and those that are relatively not as significant.

One of the loads that turned out to be more significant was the torus pressure loads developed during the pool swell.

It was decided to obtain the evaluation of these loads through testing.

A one-twelfth scale test facility was built so that we could both have rapid evaluation of loads and also do it accurately, one-twelfth scale being large enough to get good measurements and also small enough so that you could build it rapidly and do the testing.

To assure that the tests were performed representatively, scaling laws were determined and the purpose of developing these scaling laws was to make sure mm4

that the controlling phenomena were identified, that the boundary conditions for the tests were controlled as necessary, and that the scaling factors could be established so that once the pressures that were measured at the twelfth scale could be ratioed up to representative factors -- representative pressures for the full-scale.

The scaling analysis divided the torus into three regions; those being pool water, the torus air space and the bubble.

The pool water is a three-dimensional flow system; mass, momentum and energy have to be considered.

The torus air space and bubbles are noble systems and only mass and energy and the [illegible] have to be considered.

(Slide.)

For the conservation equations and also the state equations --

DR. CATTON: Excuse me. You didn't consider the water in the downcomer separately?

MR. STARK: That is also a three-dimensional system. Non-dimensional variables were defined and the conservation equations were non-dimensionalized.

In going through this process, scaling factors are defined such as the pressure that has to be scaled mn5

by the linear ratio o. ... test facility.

Also the non-dimensional parameters are defined.

These are multipliers for the non-dimensional areas and in evaluating them we get some additional scaling factors but we also find out which are the controlling phenomena in the tests, such as we can show that surface tension was not an important effect here.

(Slide.)

The result of the scaling laws showed that we needed geometric similitude and that six other conditions had to be met. That is equal densities, gravities, ratio of specific heat and the other constraints shown here.

As far as the evaluation of negligible phenomena we determined that acoustic heat conduction viscous and surface tension effects were not as important in controlling the phenomenon.

This is the scale for the model, for the facility, in a linear dimension, so it would be for our dimension, one-twelfth.

(Slide.)

Before we perform the test we had to know what boundary conditions we wanted to provide in controlling the tests.

Those are given here in the left-hand columns, the parameters, and then the desired conditions such as the

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initial pressure had to be 1.225 psia.

The degree to which we modeled the controlling phenomena are given in the right-hand column, and we got a very good control of the test.

The only thing I would want to add, for the pressure in the enthalpy flux that we had to run the large orifice, medium orifice and extrapolate.

DR. CATTON: What again is this on there, medium orifice and large orifice?

What is that all about?

MR. STARK: We could not duplicate exactly the transient pressure during the LOCA given in the FSAR.

DR. CATTON: Pressure in the dry well?

MR. STARK: Yes.

DR. CATTON: Okay. I understand. Thank you.

The pluses and minuses that you have on that figure have nothing to do with accuracy; is that correct. Just the degree to within which you want the model?

MR. STARK: No. Those were the scatter in the results, such as for the medium orifice --

DR. CATTON: Is this the scatter in your measured results?

MR. STARK: Yes.

DR. CATTON: Your instrumentation has increased by a factor or two? Wasn't it .05 psi last time?

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MR. STARK: This wasn't the scatter of the instrumentation. This is the scatter in the result.

For the medium orifice transient pressure, the average value for the pressure rate was 12.5 psi per second, but we had one run that was .2 higher, one that was .1 lower.

MR. CATTON: This is a scatter in your representation of the scale model.

MR. STARK: How well we duplicated our desired control conditions.

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DR. CATTON: Thank you.

MR. STARK: The group at the March 5th meeting showed interest in knowing what the design loads were for the upward and downward phenomena. The topic for load on the structure in the LOCA for the ultimate load was discussed. But not discussed at that time because it had not yet been evaluated was how much the pressure could be increased for the LOCA, the LOCA dynamic pressure beforethe ultimate capability of the structure was reached.

We have now gone through that evaluation. We first went through an evaluation of the sensitivity to increase in the LOCA pressure, and then with the sensitivities, using those as a tool, we have evaluated just how much we could increase the pressure.

Can we double the pressure before the ultimate limit of the structure is exceeded.

DR. CATTON: Double what pressure?

MR. STARK: LOCA pressure.

For example, for the reference plant, the dynamic pressure for the upper load is 5.6 psi. So we asked ourself, for example, could we possibly double that pressure and not exceed the ultimate capability for uplift?

DR. CATTON: Acting on the torus. From 5 to 10, acting on the torus, that is aferage across the torus.

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MR. STARK: Yes. That's correct. What we considered for downward loads were welding, pens, lugs and colums for the downward load and the upward load, the tie down system which is a system composed of any components. We have examined in detail in the sample case.

(Slide.)

Vermont Yankee with drywell pressurized 1.7 psi. For the downward load, the load over capability is given. And then the same number for the upward load. Then the new numbers, or the change in this ratio for ten per cent increase in the dynamic pressure load.

DR. CATTON: Is this load over capability with the mean values calculated, or have you included the .075 --

MR.STARK: Mean value.

DR. CATTON: What would that load/capability be of you took the error in your instrumentation to be minus .05 rather than minus .05?

MR. STARK: That ratio is up to a value of about .06. So this serves as a gamble in that case. Ten percent increase in the upward load increases the load over capability from .26 to .30.

DR. CATTON: So what we are looking at in the last column is the plus or minus factors in instrumentation.

MR. STARK: You can look at the increase of ten percent and quick figured telling me, it's about one sigma

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level for the instrumentation.

DR. CATTON: Thank you.

(Slide.)

MR. STARK: Using the previous information for the tools, we have extended it to determine, how much can we increase the pressure until the ultimate capability of the structure would be equalled.

For the downward load we can increase the pressure above its current value by 220 percent, or this could be interpreted as a safety factor of 3.2.

For the columns, we can increase it from its current value by 360 percent, and for the upward load, using the weakest link, which has a safety factor of 2.2, which are the pins on the base plate, we can increase the load from the present value by 161 percent, or the representative of a safety factor of 2.6.

This is with a delta P of the dry well of 1.7, which Vermont Yankee is now running at. These are typical results. They were done specifically for Vermont Yankee. There will be some variation from plant to plant but these would be the typical results with dry well pressurization.

DR. PLESSET: There was some question raised by Dr. Bush about the composition of the bolts, I think it was.

MR. KEENAN: Pins.

DR. PLESSET: Pins. That was not clarified for the

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public record as I recall, because we looked into it.

MR. KEENAN: Yes, sir. Well, the material is SA 93 Grade B 7 and the question related to the potential for embridling the material if improper heat treating had occurred. We had checked the records prior to the conclusion of the meeting and indicated that the record showed they were properly heat treated but the ultimate test is to do impact testing on the material, which we did and reported it to Mr. Debenedetto of the staff and the results are totally consistent with the requirements that an optional test was part of the specifications for the material.

Which is what Dr. Bush asked for, to conduct a test and show it did not become embridled and we did that. We consider the problem as closed.

DR. ISBIN: Thank you.

DR. CATTON: During the Vermont Yankee subcommittee meeting, there was a lot of concern with respect to scaling from the one twelfth to the one scale. That has nothing to do with now we scale, but it has to do with the factor of 1728 associated with the forces.

And now I see your chart here, and you indicate to me that you carry the error in your instrumentation up, if you do, you are only talking about plus or minus ten percent changes in the loads. I am wondering why were we so concerned last time? Something seems to be a little inconsistent

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now.

DR. PLESSET: I think he is all right.

Well, he can speak for himself.

MR. STARK: I agree with your assessment.

DR. CATTON: I am wondering why we want to run one sixth scale now.

MR. STARK: Because we want to evaluate the accuracy of the scaling loss. That is the primary laws. That is the primary purpose for running the larger scale test. I don't believe it's right to look at ratio going up in accuracy by a factor of 12 cubes. We want to look at the inaccuracy -- the inaccuracy we have are relative to the pressure. That factor is up by 12. You get the additional factor of 144 over the load which it is applied and we have a good knowledge of the area over which it is applied. You can go out with a tape measure and obtain that.

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DR. PLESSET: Maybe Mr. Etherington might want to bring up a point again about the effective mass that is being accelerated. Remember, you were concerned about that.

MR. ETHERINGTON: Yes. I don't remember exactly what I was concerned about.

MR. STARK: I think we responded to that question after lunch.

DR. CATTON: I do recall if you would like me to raise it.

MR. ETHERINGTON: Please do.

DR. CATTON: I think your calculation with respect to upward lift was that you would ignore the water above the bubble. This amounted to 20 percent of the water. The questions raised by both Mr. Etherington and myself were the fact that maybe you should consider something less, because water never acts like a solid body.

MR. STARK: Yes. We pursued your question.

DR. CATTON: You pursued the question and actually ran or did some computations of some kind over a two-day period, where you decreased the mass to 60 percent. Well, I am not sure that you completely satisfy the 60 percent. How did you come about obtaining this number of 60 percent? From my observations of your meetings I would have put it more like 40 percent, assuming I would ignore all water either on the side of the bubbles or over the bubbles.

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It looked like 40 percent was below the bubbles.

MR. STARK: What we did was do uplift calculations just as we had done with 80 percent water. We assumed for these several runs that 100 percent of the water mass was effective, then 80, 80, 70, and I think perhaps we got down to 50 percent or so and the conclusion was that we did not see a significant variation in the calculated uplift, all the way down to 60 percent of the mass of the water.

We have looked at the films and have made judgment that 80 percent of the water mass is a reasonable number to use.

DR. CATTON: Even though there is a significant amount of water that is standing in a vertical column, you assume that you can treat that as a dead weight?

MR. STARK: Yes. Not as dead weight. We are not considering it as dead weight here. We are considering it as a mass for inertia sake. That is the question at hand. Not the dead weight of the water. But the mass of it. If the water under consideration will move as the torus moves and for that water standing vertically to the side of the bubble, it's my conclusion that, yes, I would believe it would respond at the same acceleration as the torus.

DR. CATTON: So it's going to act as if it were solid.

MR. STARK: Yes.

MR. ETHERINGTON: But the water above the bubble, meri 3

its mass wouldn't be felt until the pressure catches up to it through the bubble.

MR. STARK: That is right. You would have to compress air bubbles until the mass would accelerate also. That was the 20 percent that we extracted out and the 80 percent inertia value.

DR. CATTON: My problem with that is that I don't agree with you.

DR. PLESSET: But he is running down to 60 percent. How did it vary when you went from 80 percent down to 60 percent?

MR. STARK: I am afraid I have only a very foggy recollection of the resulting numbers. I think it varied onl 10 percent down through about 60 percent, 60 percent water mass inertia.

DR. CATTON: That almost says that the water mass is relatively unimportant. At least 20 percent of it is not really important.

MR. STARK: That was the conclusion, yes.

DR. ISBIN: I think we will have to move along.

DR. CATTON: Okay.

DR. ISBIN: Vic, you mentioned you had a number of consultants, particularly with reference to scaling. Maybe we could save some time if each one of those consultants would like to give us a brief statement as to his interpretation of what the scaling has been and the adequacy.

MR. STELLO: I would be happy to do it that way. I hope however that all the questions that are raised have been explored to the satisfaction of all the people of concern, because I received a sense from the last meeting, when the consultants were not here, that there were still outstanding questions. I think that is my impression. So we would certainly be glad to do that, but I would also encourage anyone who has questions to ask them.

MR. KEENAN: Can we have our Dr. Cooper from Teledyne and the gentlemen from Nutec go through theirs?

DR. ISBIN: Do you want to do that first?

DR. COOPER: Dr. Catton is still here, so perhaps we can cover the scaling wall --

DR. CATTON: I don't think there was any question about that. It was the effect on the torus, not how the scaling was done in and of itself.

MR. KEENAN: I think they can help out this area.

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MR. COOPER: Bill Cooper from Teledyne.

I apologize for not having many copies here today. I got them at the last minute yesterday. In their testimony to the Joint Committee, the three former General Electric employees raised certain questions about the structural reliability of the Mark I containments, which I answered as an individual in testimony to the Joint Committee and which we have discussed here in the Vermont Yankee subcommittee meeting and in the full committee meeting. We have talked about a lot of aspects of this problem but the one area in which we were not as well prepared as we perhaps should have been prior to the last meeting, had to do with what we had and had not done about hydraulic structural interaction aspects of the problem. We did make a statement at that time that we had not specifically treated the hydraulic structural interaction aspect as fully as we might desire and as part of the short term program, but that we did plan to do this as part of the long term program.

As a consequence of this approach during the short term program, all structural test data, all load data, were obtained on relatively rigid structures. That is structures in a comparison to the annual plant could be considered to behave as rigid.

There were questions raised as to whether this was conservative, unconservative, or just what.

So what we thought we could do today would be to

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touch upon some of the bases for our judgment on these matters. There are two aspects of this which are important. First is, what is the hydraulic effect of impacting of the pool rising and impacting the ring header. The effect of the problem is important to the ring header itself and its behavior and the columns, the kind of information that was submitted in the first five volume report by the Mark I program, and then this same aspect, the ring leader effects, are important in looking at the overall torus behavior, because from this ring header analysis, we get certain loads which are applied on the torus.

The other loads that are applied on the torus are the pressure differentials between the bubble and the air space. And there are hydraulic structural interactions which occur with respect to that behavior within the torus itself. What we thought we would do to take, I would take up the subject of the hydraulic effects on the ring header and talk about why we believe those results that were presently being used to be very conservative and then Norm Edwards would talk about some of the results they have obtained from the torus, looking at the torus structure.

Now, there are a lot of words here mostly to help me get through this, but if we express the pool swell impulse on a pipe as a parabolic pressure versus time curve, the maximum pressure in this parabolic versus time curve, can be given in

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this report, as P max, subcontinent, depending upon the densities and so forth. Times a quantity, one minux VF over V0. VF is the final velocity of the ring header after impact occurs. V_0 is the pool velocity rising up to meet the pool header times the quantity V_0. V_0 was rigid.

How was that accomplished. The pipe was very thick. The pipe thickness was very large in comparison with the diameter of the pipe. Whereas in the actual structure, the pipe thickness is very small relative to the diameter of the pipel That is one effect.

The other effect is that in the PSTF tests, in which these basic data are obtained, the pipe was supported on a rather short span. Just to get a feel for this situation, if the pipe had not been supported, if these end supports had been taken away and the pool swell and come up and hit the pipe and allowed the pipe to rise as a rigid body, the maximum pressure would be reduced to about one third of that which was actually measured.

In other words, we can take this equation, take the maximum pressure we get with some final velocity, divided by the maximum pressure we got, express it as a capital P here and it's simply then dependent upon the velocity which the ring header takes upon impact, divided by the velocity of the pool.

As I say, if in the PSTF test, it simply did away

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with the support, you would reduce the maximum pressure to about one third of that value. Well, there is some of this kind of effect in the axial structure in that the beam frequencies of the ring header and the axial structure are much lower than they are in the test.

But the very important effect is this one of pipe thickness and a potential for ovalization of the pipe. That is when the pool swell hits a pipe, in the pipe which was tested, the pipe would remain round, whereas the actual pipe would deform in this manner.

(Slide.)

In deforming in this manner, the bottom of the pipe takes on some velocity and we can use that simple relationship to get a feel for the importance of the effect.

So although we did not have the capability in the short range program to do the fluid structure interaction -- by the way, we tried it with the only available program we thought might be able to treat the problem and that program could not treat the problem, and its improvements on that that we are planning to use during the long range program.

So we did try to do something else to get a feel for this.

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jon: #29

In our short-term effort the ring header, in our short-term structural effort the ring header was generally represented as a beam structure without taking any credit for the cross-section of the structure.

There was an analysis run by Bechtel where they took and represented a portion of the ring header between the supports and the position half-way between two supports and they represented this as a shell model, as a finite element analysis, representing the shell structure in detail, representing the supports in detail.

Now, it was necessary to somewhat redefine the pressure versus time to do this analysis from that which was used on the essentially solid pipe section because in doing this analysis we now had to account for the fact that when the water is, say, just there, the pressure is only acting over a small width.

And as the water comes up, the pressure is acting over a greater width.

So the curve, the pressure versus time curve, was redefined and expressed as a function of the wetted angle and this curve is included in Figure 4.9-1 of the earlier report submittal.

You can see it is essentially the same curve. It does give the same total force time history as does the curves which were used for the beam type analysis.

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Well, this particular picture I have got on the board here is a sketch of the actual results of that analysis.

The dotted shape is shown as dotted. Deformed shape is shown.

You can see the ovalization which occurs.

This particular cross-section is in a region where one would get the largest deformation of this nature.

It turns out that it is only very close to the supports and the ring stiffener that the deflection shape differs much from this.

We will look a. that in a little more detail.

In the next curve we have a plot of the deflection versus time at the top, and at the bottom of this pipe.

(Slide.)

It looks like this. You can see the top of the pipe is basically standing still. The bottom of the pipe is deflecting.

That is the majority of the deformation of this three-dimensional shell type ring header is a crushing ring mode type of deformation.

This is a plot of vertical displacement versus time.

Superimposed on that is a plot of the water displacement. And you see what happens is that as time goes on, and if you tried to put this load on a pipe, the pipe

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starts moving faster than the water.

That is even better shown by the following curve which gives a few more points down near the bottom of the pipe.

(Slide.)

These happen to be velocity versus time curves.

This is at the bottom of the cross-section.

The next line is there and the squares represent about the end of the time -- about the depth of submergence at which the impulse is over with.

The excitment occurs in that first small submergence of the body.

Here, rather than plotting displacement versus time as on the previous curve, we plot velocity versus time.

Here is the pool velocity.

Well, what this says is that if we try to place this pressure time history on this shell and if we consider it is a real pressure time history, that the velocity of the shell exceeds the velocity of the water, so the point of the whole discussion is that it is physically impossible to get the kinds of pressures and loads that we are using in our analysis because if one tried to place that load history on this particular structure the loads would go away. The loads would start to decrease and would even go away at times very short in the consideration.

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Now, what we need to do, obviously, is not put on pressure versus time, but put on the water actually hitting the structure as a function of time.

As I say, this we plan to do in long-range programs.

But we believe that these kinds of results prove without any doubt that the use of test data obtained from a test vehicle that is so rigid that no ovalization can take place, or very, very little ovalization can take place, is extremely conservative in doing the analysis on these structures.

We cannot state exactly how much we should reduce these loads. We have estimates that go from, say, a half to a tenth. That is, that our loads are high by a factor someplace between 2 and 10.

But these at the moment have to be regarded as the estimates of various individuals who have looked at the situation and the long-term program does include some analyses that could resolve this problem.

Now, the situation, as I have just described it, I would like to remind you again, is important for two reasons.

One is that the analyses that have been submitted and documented previously for the ring header and the ring header supports themselves, have assumed this cross-section on5et

to be rigid.

So the loads imposed during those analyses we believe are very conservative.

The relief I am talking about here, then, also follows through to the loads that exist on the header support column as they tend to lift the torus structure as being one of the two loads that we impose on the torus.

What I suggest we do is have Norm Edwards proceed to tell you about the results or the opinions he can gain from looking at the 3-D torus analysis.

DR. CATTON: You did the same type of analysis with respect to the deflection of the torus itself, due to downward loading?

MR. COOPER: Yes, sir.

DR. CATTON: Why? Because I think we agreed with you on this aspect of it last time.

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#30

DR. EDWARDS: My name is Norm Edwards.

I am from Nutec. Our role in Mark I program is primarily one of a technical monitor, acting on behalf of the Mark I owners' groups.

We are the eyes and ears, if you will, of the Mark I owners' group on a day to day basis, keeping track of the activities of GE and GE's consultants.

When particularly important points are under consideration, we may bring in an additional consultant or do additional work ourself to assure that proper consideration is being applied to these particularly important items.

I would like to take a few minutes to describe to you some work that we have done, and particularly the meaning of that work as it relates to an understanding of the importance of fluid structure interaction.

It was mentioned at the March 3rd ACRS Subcommittee meeting that a 3-dimensional finite limit model had been used to analyze a 32nd segment of the torus, so that an evaluation could be made on the accuracy of column loads that were being computed by less sophisticated methods.

Working with axisymmetric loads as postulated for short-term program (slide) the torus is constructed of eight minor cylinders.

A 1/32nd segment of the complete torus represents a totally accurate model for predicting the overall behavior of the torus.

The finite element model, if I can go briefly through it ---

(Slide.)

I show you this just to give you a feeling for the thoroughness of detail that was used.

It was modelled with shell plate elements. The reinforcing ring at the miter joint was modelled with beam elements as were the two torus support columns.

The torus support columns were assumed to be fixed at their base. The implication of this is, if there is a tendency for uplift, the results of this work are no longer applicable, because this model assumes that the torus -- the torus columns are fixed at their base against uplift.

So the importance of this work relates primarily to an evaluation of load in the column during the downward loading phase of it.

One of the concerns raised on March 3rd is that (slide) some pressure traces from the 12th scale model test indicated that -- this is during the downward load phase of the pressure, indicated not one but -- not two but four, I should say, pressure spikes, and the GE analysis of that data has concluded that it was probably adequate to (slide) use just the first two spikes in the structural analysis and use a smooth curve from that point on.

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And the concern raised was maybe if these other two pressure pulses had been included that that would have excited a natural mode of vibration of the structure that would have resulted in actually larger column loads than were computed.

To address this concern, we took a second look at some of the results from the finite element analysis to see if we could draw any conclusions from those results.

(Slide.)

The parameter that we focused on was to plot the total column load versus time.

Now, this is the some of the two column loads versus time.

And you see the shape of the curve is very, very similar to the shape of the loading curve.

In fact, if I put the loading curve superimposed on the response curve, and line up the scale, you will see that during the maximum downward loading phase we are getting essentially a quasi-static response. With a slight modification -- well, it is essentially a quasi-static response, folloing the downward load.

Now, that if -- let me speculate anyway. If the load had been completely smooth, I would say the loading curve had been completely smoth like this, I would speculate that the response would have probably come along like this.

I postulate that this slight decrease in load

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is a reflection of the change in pressure at this point. This pressure spike.

DR. CATTON: That was only one of the questions.

The other had to do with information like Dr. Cooper presented, which shows how the torus shell is moving with time.

DR. EDWARDS: I will be happy to answer your questions, but as I received instructions from the Chairman today, I will have to wait until I am finished to do that. That is one of the points.

But the conclusion is, it seems reasonable to me to assume that additional loading pulses in the downward load would have produced additional variations in theresponse, but the magnitude of those variations are apparently so small that it seems equally reasonable to conclude that the additional loading pulses would not have produced a load greater than what we computed for the maximum downward load.

One of the concerns abut not being able to model the actual fluid structure interaction, is that possibly a fluid structure model would have natural modes of vibration that the uncoupled model does not have, and further, that the loading would be such that if those additional natural frequencies and mode shapes existed, they would be excited in the coupled system, whereas, they do not show any response in the uncoupled system.

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This concern can be at least partially resolved by recognizing that the uncoupled model which was used, had some 77 natural modes of vibration in the frequency range of zero to 50 cycles per second.

Thus, if there were components in the loading function, components in the frequency spectra of the loading function, that wanted to excite a particular natural mode, it had its opportunity to do it, because there were 77 of them to pick from.

The concern that we talked about a little bit earlier today, is the business about how much mass of the water is effective with the structure.

The problem, of course, is that the water that is effective in the vibration of the structure, the percentage of water is different for the different mode shapes.

For example, if the structure had a natural mode of vibration that involved no deformation of the torus, but simply bouncing on the columns, then 100 percent of the water would be effective mass.

If on the other hand, you are considering a mode that involves only local vibrations of the shell, then a very small portion of the water acts as effective mass.

Since it was the purpose of this short-term program work to determine the overall response of the structure, it was felt that 80 percent was the reasonable value to use for the effective mass.

I believe GE reported to either the Subcommittee or the Full Committee Meeting on March 5th, that they had reviewed films of the 12th scale model test and had concluded that possibly 73 percent of the water was setting, waiting to be excited along with the structure.

Dr. Catton'sinstantaneous analysis of the film came up with 40 percent, I guess, but the point is, it is conservative to be on the high end, from the standpoint of evaluating the response of the structure, to the maximum downward load.

We considered one other point. That is the response to the downward load was essentially quasi-static.

This means that the load application was slow, if you will, relative to the important natural frequencies of the structure. That is, the structure had a larger frequency, relative to that of the applied load.

Yet the result of using a large amount of water mass as being effective was to decrease the natural frequencies and thus produce conservative values for the dynamic load.

So this reinforced my feeling that 80 percent of the water mass versus some lesser value is conservative for the prediction of maximum column loads.

I would be happy to try and answer any questions.

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DR. ISBIN: I think before we get to questions, let's look at the rest of the agenda. The Subcommittee chairman plans to conclude the meeting at 5 o'clock. We have some items which we have not yet covered. We had added some items from previous working groups which put us a little behind time. If those of you who have questions feel that the questions must be answered at today's meeting, I would say go ahead, but if you think you can postpone the questions, I would suggest that we go on to the other participants.

DR. PLESSET: I just had a very question. What do you think the physical mechanicanism gives these four pulses on the pressure loads?

DR. EDWARDS: Well, I don't feel like I am qualified to comment on the results of the twelfth scale model test.

DR. PLESSET: I was just asking, what do you think produces that?

DR. EDWARDS: I don't have an explanation.

DR. PLESSET: Does anybody have an explanation? I think it would be somewhat important.

DR. CATTON: When everything is so conservative, I think it's important.

MR. MOODY: What direct frequencies were those at? We have seen oscillations like this in other tests that we have assigned to bubble oscillation.

DR. PLESSET: That is what I thought, too, without

dh2

benefit of all your contact with it. Is that scale the way you are scaling things?

MR. MOODY: Yes.

DR. PLESSET: It's an oscillation of the bubble.

MR. MOODY: If it is an oscillation of the bubble, it should be scaled and it is.

DR. PLESSET: I think you ought to look at that again. That is all I want to say.

DR. CATTON: I already checked that and the frequency did scale appropriately.

DR. PLESSET: If the size of the bubble is behaving right, then perhaps, hes.

DR. CATTON: What I wonder is, you would think you would see --

VOICE: I am sure the buble scaling is correctly taken into account in the scaling laws they are applying.

DR. PLESSET: Even with the walls and all the rest of them.

MR. STONEN: Yes.

DR. PLESSET: I would like to have a guarantee. That is a good way to leave it. I am not so sure.

DR. CATTON: I didn't notice ih the movies any fluctuations.

DR. ISBIN: I think we need to go on.

MR. STELLO: Herb, you had asked that the other consultant make a statement. I would like to insure that the statements they make direct themselves to the concern. I understood Dr. Catton to say that we don't need to address the question of sc ling. That is satisfactory.

DR. CATTON: As far as I am concerned. That doesn't mean the rest of them.

MR. STELLO: You did have a problem with the way the results are applied in the applications. Is that what I heard you to say?

DR. CATTON: Yes.

MR. STELLO: Those to me are kind of synonymous. I wonder if we can make sure that perhaps we get a statement of what the concerns are and of the consultants address those concerns and maybe that would be the quickest way to be sure that we cover those areas. Not to debate them.

DR. CATTON: I think that was already done. That is why I addressed -- I don't know where he is, this fellow over here, when he had that other column with the 10 percent. What I was concerned about was the error in the pressure being carried up to full scale.

MR. STELLO: The last time we met, I know you had a number of concerns that were not satisfied on the record, when the consultants weren't here.

Now do I understand you have gotten all the information you think you can usefully get?

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DR. CATTON: I have concerns aside from the scaling.

DR. ISBIN: Let me interject here. I don't think that that question really can be answered, Vic. As you well know, we will always want more information no matter what you tell us. I think ifyou have a chance to look at the record and you think that the participants have answered questions pertaining to the record, that Ivan is reasonably well satisfied for the time being, I would like to leave it at that and not try to complete it.

MR. STELLO: I am most concerned about the facing, the questions being asked are being asked in a meeting where the people that can properly address those questions are here, and this time I made a sincere effort that by God, we are going to be ready. We are ready.

Low and behold, at som future meeting we will get some more questions that will be ou phase again and I don't like the way the record looks.

And I want to go on record saying I don't like it.

DR. CATTON: Last time we agreed with Professor Stonen with respect to the scaling. There were other questions with resp o the structural hydraulic interpretation. They are of different nature. But the scaling part of it I think we agree that the method of scaling was -- there were no questions with respect to that.

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MR. STELLO: I am satisfied if you are satisfied. The people are here.

DR. CATTON: There are other questions now.

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MR. STELLO: I don't know what those questions are.

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DR. ISBIN: The Chairman is going to suggest that we move on to the next item, which is still part of 8.

MR. D. ROSS: We only have a certain amount of time and we were going to try to find out which of the items might be addressed first. 5:00 o'clock will hit here before the end of the presentation does.

DR. ISBIN: Let me first, of all, check. Does EPRI have an additional presentation? Or was the presentation you made on th[illegible] all that you were going to say?

MR. FERNANDEZ: There is an additional item on the agenda concerning EPRI's activities in the Mark I issue, and I can very briefly address that.

DR. ISBIN: Please do it right now.

MR. FERNANDEZ: During the short-term program, EPRI did conduct scale-model tests on behalf of the Mark I owner's group. These were 1/10th scale model tests of a single pair of downcomers. The tests were performed in a somewhat different manner from that in which GE performed their tests. Nevertheless, when we looked at the results from our tests and their tests, they gave essentially very similar results. Similar results in bubble shapes and form, the pool surface profile as it swells up, and fairly close agreement in impact velocities also. That was probably the most significant activity that we were involved in during that early phase.

DR. ISBIN: All right. Thank you, Tom.

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MR. LAINAS: Gus Lainas with the staff.

I have the agenda for the remaining presentation, and I might make some recommendations since we are running short of time. This is what we had planned to talk about.

(Slide.)

MR. LAINAS: That was Mark I and II containments and the pressure supression testing, and the erosion of design margin. Further, it was going to go into the objectives of each of the short-term and long-term programs and its schedule, and the status of the Mark II.

Now, I believe that you have heard most of this and my recommendation would be -- and you also heard pool dynamics -- we will not be presenting anything new. So I would suggest that we give you the handouts on my presentation, which you have, on pool dynamic loads and go right into structural analysis, which you haven't heard from Dick Stuart. And then finish the pressure supression testing and erosion of design margin.

DR. ISBIN: That would be fine.

MR. STUART: My name is Dick Stuart from the NRC staff.

(Slide.)

MR. STUART: I am here to discuss the structural and mechanical aspects of the pool dynamics issue, specifically with regard to Mark I and Mark II.

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I have several slides which discuss a summary of the Joint Committee testimony with regard to structural and mechanican issues of the Mark I and Mark II containments.

(Slide.)

I then have several slides which discuss the overall Mark I program, the structural analysis for that, the Mark II program, its structural analysis, the acceptance criteria for Mark I and Mark II, and those modifications that have been made to date.

Most of the information from this point on, after a summary of the Joint Committee Testimony, has been presented before to the ACRS. I can cut my presentation short, if you like, from this point on, but let me present the summary of the Joint Committee testimony.

(Slide.)

MR. STUART: Basically I have two slides which outline the concerns as they were addressed by the GE representatives. And the summary of the NRC staff response as it appears in testimony.

Briefly, I will go through. I don't plan on dwelling on any one point unless there are questions.

First item, the torus thiickness on Oyster Creek and Nine Mile Point was inadequate. In fact, this situation arises from the fact that the torus design pressure for Oyster Creek and Nine Mile Point is 35 psig, relative to the torus p 32-4

design pressure of 56 psig for all other plants. In fact, this arises from the situation of convenience of making the structural proof test of the torus and dry well at the same point in time. Previously, for these two plants, they had to block off the vent pipes betw en the torus and the dry well, such that independent dry well and torus tests could be conducted. In fact, in all plants the requirements are 25 psig for Oyster Creek and Nine Mine Point. And I think the highest pressure on any plant we expect to have on the torus is something like 28 psig. So there is a considerable amount of design margin on those plans in which the dry well and torus are tested at the same point in time.

The second item, the load combinations, were not complete. In fact, for Mark II and Mark III, this concern is not correct. We used load combinations, or load combinations are being used which are equivalent to current Code. They are not identical to current Code because pool dynamic loads are not included in current Codes. "Equivalent" means that combinations, bounding combinations were considered and design base events were considered, such as a nonmechanistic actuation of a single safety relief valve in combination with the maximum pool swell loads.

For the Mark I short-term program, governing load combinations were used which included seismic loads. The complete set of load combinations one would normally use in a ip 32-5 Mark I, II, or III plant were not included; however, those loads which were not considered have relatively minor effect on the overall stresses that result from use of those equations, "relatively minor" means somewhere in the neighborhood of 5 to 10 percent.

The reason why they were used for the short-term program, reduced set of equations, was for the speed of performing the calculations in obtaining relatively quick results of the status of Mark I.

For the long-term program, load equations such as those used for Mark II and III will be employed. end 32

start 33 Third Item: Safety relief valve discharge was not considered with pool swell. In fact, for Mark II and II, all plausible combinations of safety relieve valve discharge phased in time are considered in combinations with pool swell. In addition, one actuation for one safety relief valve is combined superimposed on top of the maximum pool swell loads.

For the Mark I short-term program, no mechanistic combination of a single safety relief valve is combined with the maximum pool swell loads. However, for the long-term program, we anticipate that similar combinations will be used, combining a single safety relief discharge with the maximum pool swell similar to Mark II and Mark III.

The claim was made that nominal seismic accelerations were used. In fact, a screening of all Mark I plants for the lp 33-1

short-term program resulted in finding that 0.15 G is the maximum value of the SSE on current dockets of all of the Mark I plants. And that value was used for the horizontal ground acceleration in the short-term program, so, in fact, it encompasses all the Mark I plants.

The next item, Mark I's do not satisfy current Code and design margins are eroded. Basically this statement is true if one were to look back to the original design margins that existed. In fact, we recognized this throughout the short-term program; however, for the long-term program the intention and the purpose of the long-term program is to restore the design margin that existed when the plants were originally licensed.

Well, what solids do we gain, then, during the long-term program? In fact, what has been done during the short-time program is to evaluate the margins so that no loss of containment function would occur, and that we would have a factor of safety of at least 2. The ability of structures to sustain loads even with limiting yielding, which is short of failure is evaluated, and this affords reasonable assurance for public health and safety in reserve structural capacity, or by modifying, such as the delta P modifications that have been done on all plants to gain additional margin.

In fact, there have been other structural modifications which I will discuss if you desire later on in

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my presentation.

A claim was made that Mark I strain limits were specified rather than stress limits to disguise nonconformance with the Code. In fact, as we reported to the ACRS previously, we do have limits that go a limited distance beyond their yield point. In order to express -- truly express the safety margins of those particular limits, one must express this level of strain at which these limits are elongated and compare that to the ultimate strain in order to evaluate their safety margins. That has been done in all cases and the resulting safety margins have been evaluated through strain comparison.

(Slide.)

The claim was made that the analytical models have been refined if the results have been proven to be unfavorable. In fact, this is true. However, this is standard structural practice: that one performs a basic analysis and uses an elementary or rudimentary analysis technique. If, in fact, the results are -- if you are not satisfied with the results, the results yield higher stresses than what the component can take. The first thing one does is define the analysis technique.

The best example is use of the elastic analysis. One changes analysis technique and goes into an elastoplastic analysis. That has been done in the short-term program to assess the margins for failure of any given element.

A claim was made that loss of torus water may occur or ECCS piping may loss function.

Once again I refer back to the margin of safety of 2 that we have in the short-term program, measuring as our yardstick to insure that all components of at least the margin of safety of 2, and the torus shell certainly falls in that category. In addition, an analysis has been performed on all plants which have exhibited an uplift greater than 0.2 inches.

ECCS piping flexibility analysis was performed to determine the effect on ECCS piping of this uplift. In fact, all plants have reported to us that they can satisfy Code requirements for their calculated uplift, which have varied between 0.2 inches and one inch of total torus uplift.

The next item, 9. Seismic slosh or some other loads may uncover the vents. We haven't yet investigated this actually; we have investigated this for Mark I, II, and III and find that the pool frequencies are extremely low. There are no apparent low exciting frequencies such that we can get a seismic slosh either to OBE, SSE, or any of the pool dynamic phenomena. This is being currently investigated on Mark IIIs and it is going to be part of the Mark I long-term program.

However, our best estimate at this time is that there will not be vent uncovering because we don't have the low exciting frequency, and we don't anticipate in the long-term program that this should present any problem.

A claim was made that no competent structural consultant would testify that Mark I's are safe.

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In fact, Dr. William Cooper who is here today of Teledyne has made a statement in front of the Joint Committee that he believes that the Mark I containments are safe. And I assume would support our recommendations of at least the margin of safety of 2. He has sent a letter to the Joint Committee stating his position in this regard and in addition, Mr. Robert Keever of Nutec has also volunteered to come forth with additional testimony or statement indicating the capability of these Mark I containments.

MR. STELLO: Dick, I assume we have some structural engineers on the staff who also agree who are competent, I hope.

MR. STUART: I think we have several of them, myself included.

MR. STELLO: Thank you.

MR. STUART. Mr. Chairman, would you like me to further abbreviate my presentation or should I proceed as I have outlined?

MR. STELLO: May I suggest that there is one thing we do have outstanding and that is the question of hydrodynamic structural interaction, the calculation referred to last time that we were going to at least summarize today.

MR. STUART: I have, as I told the committee last time, I have performed some calculations which I am prepared to present copies to Dr. Catton and anyone else who would like a copy of those calculations. We have sent a copy of these calculations to Dr. Zudans who is a consultant at that particular subcommittee meeting. I have discussed these calculation with Dr. Zudans.

After that time we arranged a meeting with him to discuss the problem of hydrodynamic structural interaction. This meeting lasted 3 or 4 hours. We went over the results presented by Dr. Edwards today thoroughly. Dr. Zudans has told me that I could use his name. He sent Shalley (phonetic) to say that he could provide a report to the committee. He sent Shalley corrobating the fact that hydrodynamic structural interaction is not a significant problem for the short-term program, and I would like to offer that as a statement to the committee.

DR. CATTON: I have to leave, but I would like to get a copy of this from you and just ask one question, then I will go.

MR. STUART: I have one for you, too, Dr. Catton, before you go.

DR. CATTON: There are 2 kinds of hydraulic structural interactions I was concerned with, and one more than the others. One had to do with the relationship of the water that was in the torus and the movement of the torus boundary. And I noted that calculations were made of the various nodal points on the bottom of the manifold that runs all the way around and you have velocity on the order of 3-, 400 inches per second.

Have those kind of calculations been made for the bottom of the torus? It would be nice to see a plot of that type as a function of time and superimposed on that the pressure acting on the whole torus as a function every time someone would have an idea how the torus itself was moving relative to when the upward pressure occurred.

MR. STUART: I agree with you one hundred percent.

DR. CATTON: Depending on whether they are in phase, out of phase, this would answer all the questions. Is that what you have here?

MR. STUART: No. I don't. What you have done there are some scoping calculations to find another which significant modes would effect the total overall upward and downward loads.

I present those to you. You can review those. Your particular question, we have discussed with Dr. Edwards and Dr. Zudans. We all concur that that would be valuable information. I understand that Dr. Edwards of Nutec is going to go back to try to pull out that information.

However, the thrust of his presentation today was, we believe it has no overall effect on the torus supports.

DR. CATTON: On the downward loading I agree with you, which was his conclusion.

MR. STUART: And we certainly agree there may be some localized effects you are referring to.

DR. CATTON: In essence, it would decrease the effect of massive water. If it were exactly in phase it may decrease it to some small value.

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MR. STUART: I would like to ask Dr. Edwards if he has had a chance to gather data and, if so, what are the results?

DR. EDWARDS: The velocity data you are referring to is not available at this time. However, it can be made available.

DR. CATTON: I am saying sequence it with the pressure data.

DR. EDWARDS: The work initially done using the THREE D finite limit model was primarily for the purpose of evaluating the column load and hence the values of velocities at the several node points were not printed out with the run.

DR. CATTON: They were not important to that consideration?

DR. EDWARDS: Exactly. We all attempted to recover that information. Dick is referring to a discussion regarding the local shell membrane stresses. Shell membrane stresses versus column loads. I can report that the maximum shell membrane stresses remote from the miter joint are of the order of 3 psi, versus an allowable value of the order of 20 ksi. This means that if there were significant dynamic load amplification in those stresses, it could be very significant without causing a problem.

MR. STUART: The thrust of those statements -- and I was looking for those kind of numbers -- was even if structural hydrodynamic interaction was significant action, there appears to be a margin of 6 to 7, up to the capacity of the shell in that particular region.

DR. CATTON: I had both kinds of questions. It sounds like you answered the one and the other remains to be seen, to ferret it out from the data that is already available; is that correct?

MR. STUART: Fine.

DR. CATTON: Even then, if it looks bad doesn't mean it is.

MR. STUART: Do you have any other questions of of hydrodynamic interaction that you would like to address at this time?

DR. CATTON: Not at this time.

MR. STELLO; I would like to suggest you make an effort to get the information requested and perhaps we can ask some future subcommittee meeting, I think perhaps on April 7th, if they will allow us to insert that information in the record.

DR. ISBIN: Fine.

MR. STELLO: And we will be sure that Dr. Catton gets a copy personally.

DR. ISBIN: Thank you, for everyone, for this subject. We are going to move on.

MR. ANDERSON: Cliff Anderson, NRC staff.

(Slide.)

MR. ANDERSON: I am addressing the allegations ip 35-3

regarding in adequate pressure suppression testing.

Basically, the allegation was made with regard to a limited number of tests, specifically, with regard to Bodega tests and a little bit more specifically, with regard to the tests that reduce vent submergence.

The concern that was there was the potential for reduced submergence as a result of slosh, and that at this reduced submergence you would not have adequate condensation in the system.

(Slide.)

These allegations are made primarily with regard to the full-scale Bodega tests, and with regard to the full-scale Humboldt tests.

What I am going to be talking about here, quite specifically, are the tests related to reduced submergence for both of these. In one case, the special run in about early 1960, in one case we dealt with a 1/48th segment full-scale test for Humboldt, and in the other case, we dealt with a 1-112th segment for the Bodega facility.

DR. ISBIN: Can I ask you: Will your discussion this afternoon include any material other than what the staff has submitted in the joint testimony? Joint Committee testimony?

MR. ANDERSON: You can assume that we have read thoroughly the testimony. Some of us even perhaps participated

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in the suppression studies years ago, so we are all acquainted with the history of the vapor suppression. All I was doing was summarizing. I can go basically to the most important point which summarizes the whole thing.

(Slide.)

These are the Humboldt tests that related to tests at variable submergence. 37 tests were down at the nominal submergence of 6-foot. A much more increased submergence of 12 feet, and then this number of tests, probably 6 tests, reduced vent submergence, and a number of those 6 tests were done with the vent terminating above the pool surface.

The basic conclusion that was drawn was that there was rapid and efficient condensation for that complete range of vent submergence, including the vent terminating above the pool surface.

Now, it was basically these Humboldt tests which allowed us to make that conclusion.

(Slide.)

Also the Bodega tests, there were 45 Bodega tests, 43 at 4-foot, and one at 5 feet submergence. The allegation was they were not adequate, but when you consider the Bodega tests to go with the Humboldt tests, we believe there is adequate tests to show there is complete analysis --

MR. STELLO: Can I interrupt?

There is another item that the Subcommittee meeting p 35-5

on Tuesday said you would cover, and I don't know whether they told you or not, but they scratched it from their agenda. They said you would pick up the acceleration of the pressure vessel pedestal at this Subcommittee. We are prepared to talk about that. I know you said you wanted to leave at 5:00. If we talk real fast, we can try to cover this. If you have any more questions on this, maybe we can cover it.

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DR. ISBIN: I was going to thank Mr. Anderson's brief, concise summary. We do want to talk about erosion of design margins as well.

MR. STELLO: We are willing to stay as long as you are, Mr. Chairman.

(Slide.)

MR. STUART: Basically, the contention is, a summary of the concern, is the testimony alleges that the reactor vessel support pedestal could be caused to vibrate or accelerated by load originating in the pressure suppression pool if the design basis pipe rupture were to occur.

DR. ISBIN: Again presume we have read completely your testimony.

MR. STUART: Yes, sir.

(Slide.)

I would like to show first a picture of a Mark II. The concern is that the vibrations which occur in the pool will act upon the reactor vessel pedestal and that these have not somehow been included in the design.

In fact, the pressures which occur due to pool dynamics phenomena are design basis loads on the reactor pedestal in the Mark II containment.

(Slide.)

On the Mark III containment, note that the pedestal occurs here. The closest pool location to the pedestal is here.

However, there is a possible load path for vibrations in the pool through the vents hitting the weir wall and being transferred through this portion of concrete to the reactor vessel pedestal. In fact, in the dynamic analyses, these assemblies are modeled and there is some load that is transferred back through the pool, through the vents, against the weir wall.

There is a weir wall pressure load which is included as part of the Mark III design basis load. So, in fact, in essence, although one does not look specifically at those loads and how they effect the pedestal, they are included indirectly by applying loads to the weir wall through the pool.

(Slide.)

In the Mark I containment, the load path, the closest load path from the pool to the reactor vessel pedestal is that the loads that occur within the pool, would have to go through the torus, the ring stiffener, through the torus supports, across through the base mat, all this mass of concrete, on up into the reactor vessel pedestal and finally through the skirts into the reactor vessel.

No one has really, except these 3 GE engineers brought this up as a possible concern but to address the concern, the staff has essentially evaluated the maximum amount of energy which occurs during the pool upward and downward portion of the load phenomena and related that amount of energy to the amount of energy that one could develop in an SSE, and, in fact, the amount of energy throughout the -- for the total pool upward and downward load is about 1/40 of the amount of energy used as a design basis during the SSE. So, in fact, in essence, what we are saying is we don't believe that on Mark I that there is a sufficient amount of energy to cause any significant motion of the reactor vessel support. (Slide.)

I have another slide but I think that basically summarized our position.

DR. ISBIN: But nevertheless you are planning to check this out at Monticello in a similar pre-test.

MR. STUART: Monticello has the reactor vessel pedestal instrumented with accelerometer to determine what effect, if any, will occur on the reactor pedestal.

DR. ISBIN: Thank you.

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Do you have any questions?

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MR. ANDERSON: Should I make the assumption that you have read the testimony?

DR. ISBIN: Yes.

MR. ANDERSON: I think we did discuss this and maybe you have some questions now or should I go in depth into this?

DR. ISBIN: Well, we do have a question.

Harold, you were going to ask a question in general at this point, I believe.

This is on the erosion of design margins, the cumulative effects.

MR. ETHERINGTON: I'm sorry. I don't really recognize what question I had there.

DR. ISBIN: Okay.

Let's first ascertain what the presentation is that you are going to give.

MR. ANDERSON: I was going to discuss the erosion of design pressure margin in the Mark I containments resulting from the removal of baffles. The purpose of the baffles was alleged to be as an anti-slosh device.

DR. ISBIN: Okay.

This is one of the parts of it. Right.

Why don't you summarize what you think on that situation. That would be fine.

MR. ANDERSON: Let me skip right through to the basis for the placement of the baffles.

(Slide.)

MR. ANDERSON: In the first place.

DR. ISBIN: Right.

MR. ANDERSON: There were 2 considerations. One of them was that the Bodega tests indicated removal of the baffles could lead to an increase in the suppression chamber pressure over and above that resulting from the transfer of air from the dry well to the wet well.

That was the prime reason for putting the baffles there in the first place. It was only a secondary consideration that, since the baffles were in there for that other reason, they also would serve as an anti-slosh device.

(Slide.)

The baffles were removed because, in late 1960s they found there was damage on a number of the baffles. The damage resulted from actuation of relief valves and primarily it was baffles in the immediate vicinity of the relief valves.

(Slide.)

The reason for removing them, considering both of the reasons for putting them in there, were primarily here, they went back an took a look at the Bodega tests and, in doing this, there is indication that the pressurization rate of the chamber was too high. It was high by something like a factor of 4.

This resulted in a higher pressure than would have resulted -- let me put it another way. This indicated that the pressure was, in fact, determined by the air transferred from the dry well to the wet well. That was the primary concern.

Now, with regard to the allegation -- the allegation was it was due to primarily to the azimuthal sloshing, and as Dick Stuart said, there was no reason for the waves in the first place.

If you go back to the Humboldt and Bodega tests, there is indication you get good condensation even when you have events above the pool.

DR. ISBIN: And your final conclusions now are what?

MR. ANDERSON: The final conclusion, this was the prime reasons they were in there. I have not exceeded as a result of taking those baffles out, they have not changed anything with regard to the long-term pressure, the design pressure so, therefore, there is no erosion of design margin as a result of the removal of those baffles.

DR. ISBIN: Could we refer to the staff's section on this? I though that you had still another phase to your conclusions, which would call for perhaps a consideration of these baffles should they be desirable?

MR. ANDERSON: I think what we said, we have not received any indication from any of the licensees that they intended to put those back in. We haven't called for anyone to put them back in.

DR. ISBIN: Can you show me where it is in the docket so I can find it?

MR. TEDESCO: 2099 to 102. 2101, the last page. The last sentence in the first paragraph.

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DR. ISBIN: If the need for baffles or other internal structures is identified, the staff will take appropriate action.

MR. TEDESCO: Yes.

MR. ANDERSON: But it has not been identified at this point.

DR. ISBIN: I though that that was a reservation on your part and that there could be a need.

MR. ETHERINGTON: I took it as a rebuttal of Bridenbaugh, et al. charges that the design had been deteriorated.

DR. ISBIN: Had not?

MR. ETHERINGTON: I think it will speak for itself. I didn't read it as something that was to be required.

MR. TEDESCO: We have the Mark I program broken down into long-term and short-term program. The short-term program is being wound up now. The basis of the results that we have received for the short-term time intervals, we have found no reason to reconsider the need for baffles. That is what that statement identifies.

However, in the long-term program we will have further analyses and further tests as well as the development of appropriate modifications, and whatever action that will be taken will depend upon the results. If it comes out that perhaps some benefit might be derived from the use of baffles, we will evaluate it. At the present we see no need for it.

DR. ISBIN: Okay.

I think I interpreted it the same way, with more emphasis on the long-term, which as you have indicated would restore almost completely all of the design margins that you thought that you had, including the need for baffles if that should be so indicated.

MR. TEDESCO: Yes.

DR. ISBIN: Are there other items?

MR. STELLO: No.

DR. ISBIN: The committee would like to thank all of the participants for coming, the staff, GE, their consultants, the Mark I-Mark II group, EPRI, and others. I have no additional comments.

Do you have any last-minute words, Vick?

MR. STELLO: Just thank you for your patience in staying with us to get through all of the items that were on the agenda. I know it was rather long and a hard day. We would wish to join you in thanking again all of the people who had to come some great distances and who didn't get to say much, but I think their presence here was well worth time and effort and they were ready to respond to all questions that could come up.

Thank you.

DR. ISBIN: Ross, do you have any comment?

MR. G. ROSS: No. We want to thank you for giving GE the opportunity to come in and respond. We appreciate it.

DR. ISBIN: The meeting is concluded.

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(Whereupon, at 5:03 p.m., the meeting was adjourned.)

CORE SPRAY DISTRIBUTION
SUMMARY OF ALLEGATION

- PRESENT TEST PROGRAM IS INADEQUATE TO DEMONSTRATE GOOD COOLING - "COLD TESTS"
- CORE MELTDOWN COULD RESULT FROM INADEQAUTE SPRAY FLOW
- EUROPEAN TESTS SHOW STEAM UPFLOW COULD PREVENT COOLANT DELIVERY TO RODS

1974 ASEA/ATOM SPRAY NOZZLE TESTS

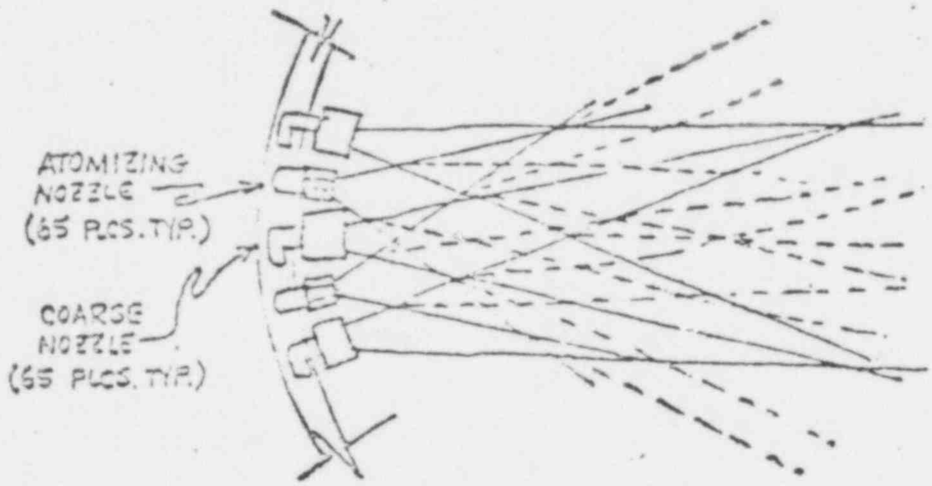
- TEST DESCRIPTION
  - SINGLE SPRAY NOZZLE, VERTICAL ORIENTATION
  - TRAJECTORY ≈ 2 FT
  - STEAM ENVIRONMENT
  - VARIABLE PRESSURE, WATER TEMPERATURE
- NOZZLE DESCRIPTION
  - CENTRIFUGAL ATOMIZING TYPE
  - FINE, HIGH-VELOCITY DROPLETS
- CONCLUSION RELEVANT TO GE-BWR
  - IN STEAM, SPRAY CONE ANGLE SENSITIVE TO:

    AMBIENT PRESSURE

    SPRAY WATER TEMPERATURE

EFFECT ON BWR CORE SPRAY DISTRIBUTION

- GE BWR CORE SPRAY SYSTEMS DESCRIPTION (TYPICAL)



- SUPERPOSITION, INTERACTION BETWEEN NOZZLES AND BETWEEN SYSTEMS CONTRIBUTES TO UNIFORMITY OF DISTRIBUTION
- TWO INDEPENDENT FULL-CAPACITY SYSTEMS

PWM 3/25/76

POOR ORIGINAL

PROGRAMS TO STUDY EFFECTS

- EXPERIMENTAL PROGRAMS HAVE BEEN CONDUCTED TO:
  - QUANTIFY SINGLE NOZZLE PERFORMANCE

    RESULTS:

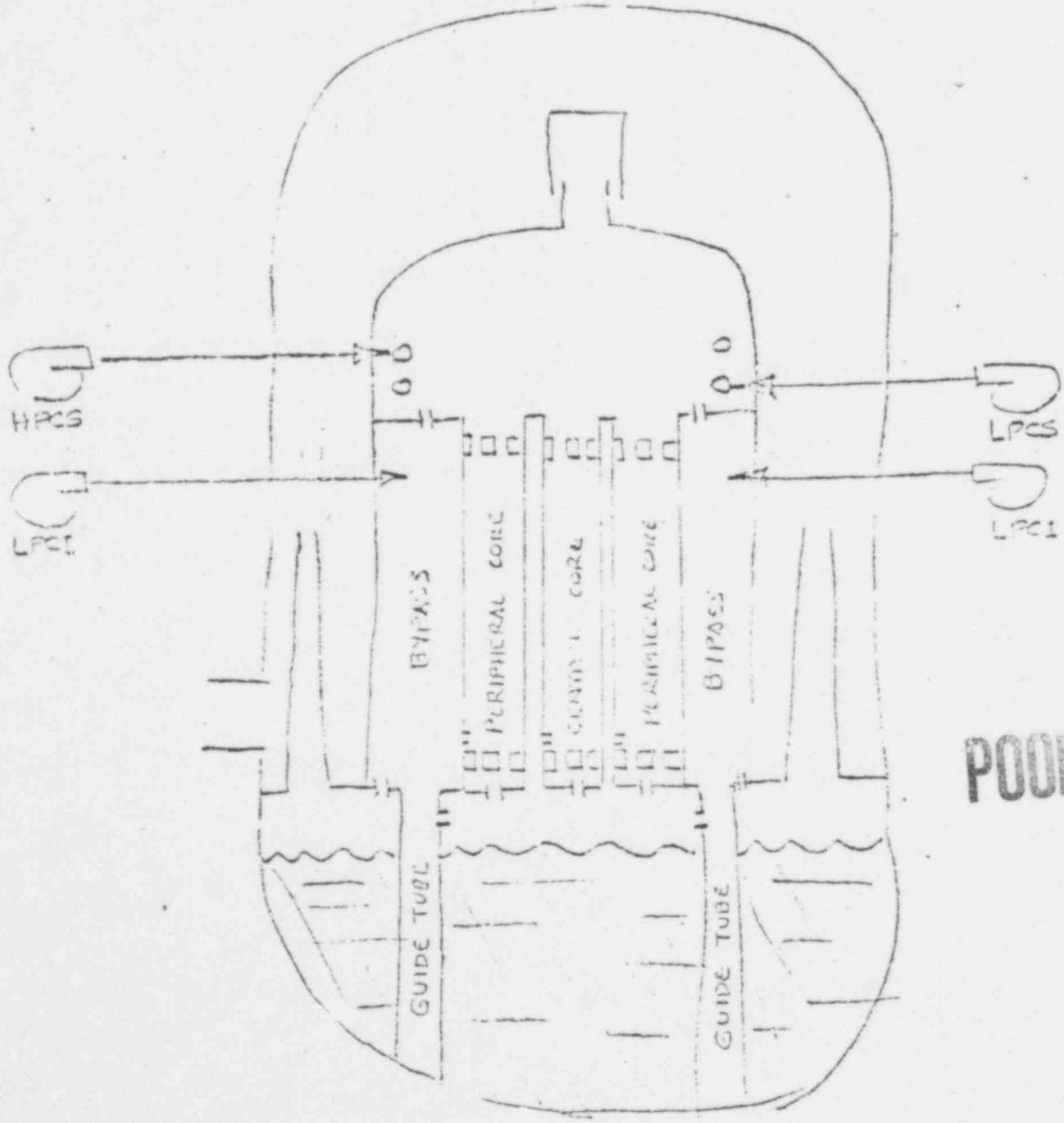
    - EFFECTS SIMILAR TO ASEA TESTS WITH "ATOMIZING" NOZZLES
    - LITTLE EFFECT ON COARSE DROPLET "OPEN ELBOW" NOZZLE
    - CONTRACTION AND PATTERN SHIFT OF "VNC" NOZZLES
  - MEASURE SENSITIVITY OF DISTRIBUTION IN FULL-SCALE TESTS

    RESULTS:

    - BWR CORE SPRAY DISTRIBUTION CAN TOLERATE VERY GREAT NOZZLE PATTERN CHANGES
    - SUPERPOSITION OF FLOWS FROM MANY NOZZLES CONTRIBUTES TO UNIFORMITY OF DISTRIBUTION
- EXPERIMENTAL PROGRAMS ARE UNDERWAY TO:
  - QUANTIFY MULTINOZZLE INTERACTION
  - MEASURE DISTRIBUTION WITH STEAM ENVIRONMENT EFFECTS SIMULATED
  - DETERMINE INTERACTION WITH COUNTER-CURRENT FLOW/INVENTORY
- ANALYTICAL PROGRAMS ARE UNDERWAY TO:
  - PREDICT SINGLE-NOZZLE PATTERNS AND DROPLET TRAJECTORIES
  - PREDICT OVERALL DISTRIBUTION

TREATMENT OF RELATED EFFECTS IN EVALUATION MODEL

- COUNTER CURRENT FLOW LIMITING MODEL
  - CORE BEHAVES AS SINGLE AVERAGE POWER CHANNEL
  - CCFL AT TOP OF FUEL ASSEMBLY RESTRICTS DOWNFLOW
  - LIQUID NOT PASSING DOWN IS "THROWN AWAY"
- EXPERIMENTS DEMONSTRATE SUBCOOLING "BREAKS DOWN" CCFL
  - CCFL "BREAKDOWN" IN ONLY THE PERIPHERAL ASSEMBLIES WILL PASS FULL CORE SPRAY FLOW
  - LIQUID ACCUMULATION ABOVE CORE CANNOT OCCUR
- CALCULATED REFLOODING DELAY MUCH MORE SIGNIFICANT THAN VARIATIONS IN SPRAY HEAT TRANSFER



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DISCUSSION OF SPECIFIC ALLEGATIONS

- COLD TESTS USED TO MEASURE SPRAY DISTRIBUTION
  - TRUE: STEAM UPDRAFT EFFECT ON TRAJECTORY IS SIMULATED IN FULL-SCALE TESTS IN AIR

- NO "ACTUAL THERMAL TESTS"
  - UNTRUE: HOT TESTS UNDER REACTOR CONDITIONS ARE USED TO MEASURE:

    SINGLE NOZZLE PATTERNS
    SPRAY WATER PENETRATION INTO FUEL ASSEMBLIES
    UPDRAFT DUE TO VAPORIZATION
    SPRAY HEAT TRANSFER
    REFLOODING HEAT TRANSFER

- CORE SPRAY MUST BE EFFECTIVE IN "SECONDS" TO PREVENT CORE MELTDOWN
  - UNTRUE: SEVERAL PHENOMENA ARE INVOLVED

    BWR IS SELF-COOLING FOR 30-40 SECONDS (UNTIL ECCS INITIATION)
    REFLOODING PROVIDES 1000F MARGIN TO MELT WITHOUT ANY SPRAY HEAT TRANSFER

- STEAM "BLASTING" WILL PREVENT SPRAY DELIVERY
  - UNTRUE: UPDRAFT DOES DELAY DELIVERY TO LOWER PLENUM, BUT DOES NOT PREVENT IT (EVEN WITH CURRENT CONSERVATIVE MODEL)

7

EFFECT ON BWR SAFETY

- EXPERIMENTAL PROGRAMS HAVE INVESTIGATED GE-BWR CONE ANGLE CHANGES AND EFFECT ON SPRAY DISTRIBUTION: CONTINUING PROGRAM UNDERWAY

- BWR CORE SPRAY DISTRIBUTION IS NOT STRONGLY SENSITIVE TO INDIVIDUAL NOZZLE CONE ANGLES
  - SUPERPOSITION, INTERACTION PREDOMINATE
  - TWO FULL-CAPACITY SYSTEMS - HEAT TRANSFER CALCULATIONS ASSUME MINIMUM FLOW FROM ONE SYSTEM

- PEAK CLADDING TEMPERATURE IS RELATIVELY INSENSITIVE TO SPRAY HEAT TRANSFER
  - REFLOODING TERMIANTES HEATUP TRANSIENT WITH NO CREDIT FOR SPRAY

- RELATED EFFECTS ARE TREATED CONSERVATIVELY IN EVALUATION MODEL
  - COUNTER-CURRENT FLOW LIMITING
  - NEGLECT OF UPPER PLENUM INVENTORY
  - DELAY OF REFLOOD MUCH MORE SIGNIFICANT THAN VARIATIONS IN SPRAY HEAT TRANSFER

- CONCLUSIONS
  - STEAM ENVIRONMENT EFFECTS ARE SIGNIFICANT ON SOME NOZZLE TYPES
  - SYSTEM DESIGN MINIMIZES SENSITIVITY OF FINAL RESULT (PEAK CLAD TEMPERATURE) TO NOZZLE CHANGES
  - VERY CONSERVATIVE TREATMENT IN EVALUATION MODEL

POOR ORIGINAL

FLOW INDUCED VIBRATIONS

- FEEDWATER SPARGERS

  20 PLANTS INSPECTED - 6 EXHIBITED CRACKS

  SIGNIFICANT CRACKING CAN BE DETECTED - POWER ASYMMETRY

  FIX DESIGN TESTED & INSTALLED

  FAILURE CONSEQUENCES EVALUATED - NO SIGNIFICANT SAFETY CONCERN

  - CHANGE IN FEEDWATER SUBCOOLING
  - FLOW BLOCKAGE (JET PUMP/FUEL INLET)
  - CORE SPRAY SYSTEM

- IN-CORE VIBRATIONS

  CAUSE IDENTIFIED - INTERIM CORRECTIVE ACTION TAKEN

  FIX AVAILABLE AND TESTED

  - NOT NECESSARY TO DRILL IRRADIATED FUEL
  - ECONOMIC DECISION

  LPRM FAILURES NOT RELATED

  CHANNEL DEFLECTION (ROUNDING) PREVIOUSLY REPORTED

  - OPERATIONAL HISTORY DEPENDENT
  - BYPASS FLOW ACCOUNTED FOR IN SAFETY ANALYSES

GE AGREES WITH STAFF ASSESSMENT

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CRD DESIGN

- EOC SCRAM REACTIVITY

  OPERATIONAL DATA ACCOUNTED FOR IN PLANT OPERATION

  MOST LIMITING CYCLE CONDITION

  FIX HAS ECONOMIC NOT SAFETY BASIS

- CONTROL ROD LIFE

  SHUTDOWN MARGIN TESTS DEMONSTRATE ROD CAPABILITY

  NO BORON LOSS FROM PRODUCTION RODS

  SIGNIFICANT ONLY FOR MANY TUBE FAILURES

- COLLET TUBE CRACKING

  NO DRIVE FAILURE TO OPERATE

  DRIVES TESTED TO 6 TIMES EXPECTED LIFETIME

  FAILURE IS DETECTABLE BY SURVEILLANCE TESTING

- ROD DROP ACCIDENT

  OVERALL PROBABILITY OF EVENT SMALL WITHOUT OPERATOR AIDS

  ROD DROP ONLY SIGNIFICANT AT LOW POWER
  - RBM NOT DESIGNED FOR THIS EVENT
  - EXPOSURE TIME MINIMAL

  RWM AND RSCS DESIGNED AND GOOD OPERATIONAL HISTORY

  DRIVELINE INTEGRITY NOT AFFECTED BY COLLET TUBE OR CHANNELS

GE AGREES WITH STAFF ASSESSMENT

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NOZZLE BREAK BETW[illegible] VESSEL WALL AND BIOLOGICAL SHIELD

"A NOZZLE BREAK OCCURRING BETWEEN THE VESSEL WALL AND THE BIOLOGICAL SHIELD SURROUNDING THE VESSEL COULD CAUSE MOVEMENT AND FORCES ON THE VESSEL THAT MAY HAVE INCALCULABLE RESULTS."

"FROM PAST EXPERIENCE WITH PRIMARY PIPING SYSTEMS, CRACKS ARE MOST LIKELY TO OCCUR AT THE VESSEL SAFE END (POINT A ON FIGURE 1). THIS IS THE MOST SUSCEPTIBLE POINT FOR AN INSTANTANEOUS PIPELINE BREAK. FAILURE COULD CAUSE AN INSTANTANEOUS PRESSURE WAVE TO BUILD UP BETWEEN THE INSIDE OF THE BIOLOGICAL SHIELD AND THE OUTSIDE OF THE REACTOR PRESSURE VESSEL. THIS INSTANTANEOUS PRESSURE WAVE COULD BE HIGH ENOUGH TO CAUSE THE VESSEL TO MOVE SIDEWAYS, OR TO TIP OVER AGAINST THE FOUNDATION CAUSING SEVERE AND UNKNOWN DAMAGE. SUCH AN ACCIDENT WOULD CERTAINLY CAUSE GROSS DISTORTION OF THE REACTOR INTERNALS, DISRUPTION OF THE CORE, AND POSSIBLY PREVENT INSERTION OF CONTROL RODS TO SHUT DOWN THE REACTOR. THE EMERGENCY CORE COOLING LINES COULD BE SEVERED AND RESULT IN THE "INCREDIBLE" ACCIDENT. THIS ACCIDENT POTENTIAL EXISTS IN MOST LIGHT-WATER REACTORS, AND SHOULD BE THOROUGHLY EVALUATED BY THE NRC."

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POOR ORIGINAL

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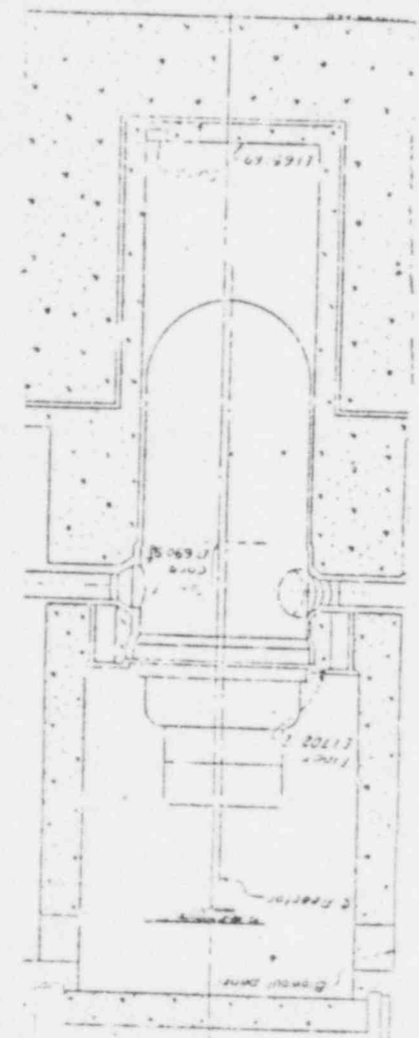
SUMMARY OF STAFF RESPONSE

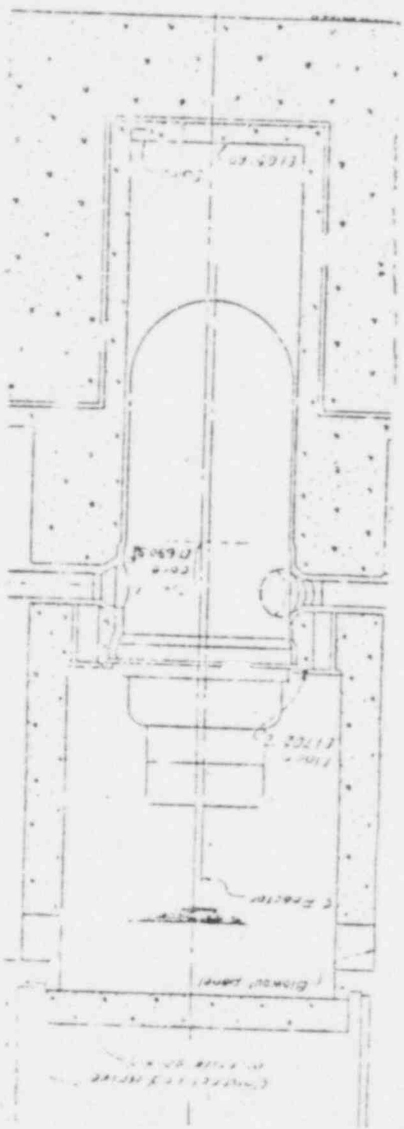
DESPITE THE LOW PROBABILITY OF OCCURRENCE PROTECTION AGAINST EFFECTS OF PIPE RUPTURE POSTULATED TO OCCUR AT THE NOZZLE SAFE ENDS HAS BEEN A DESIGN REQUIREMENT FOR ALL LWR'S FOR MANY YEARS.

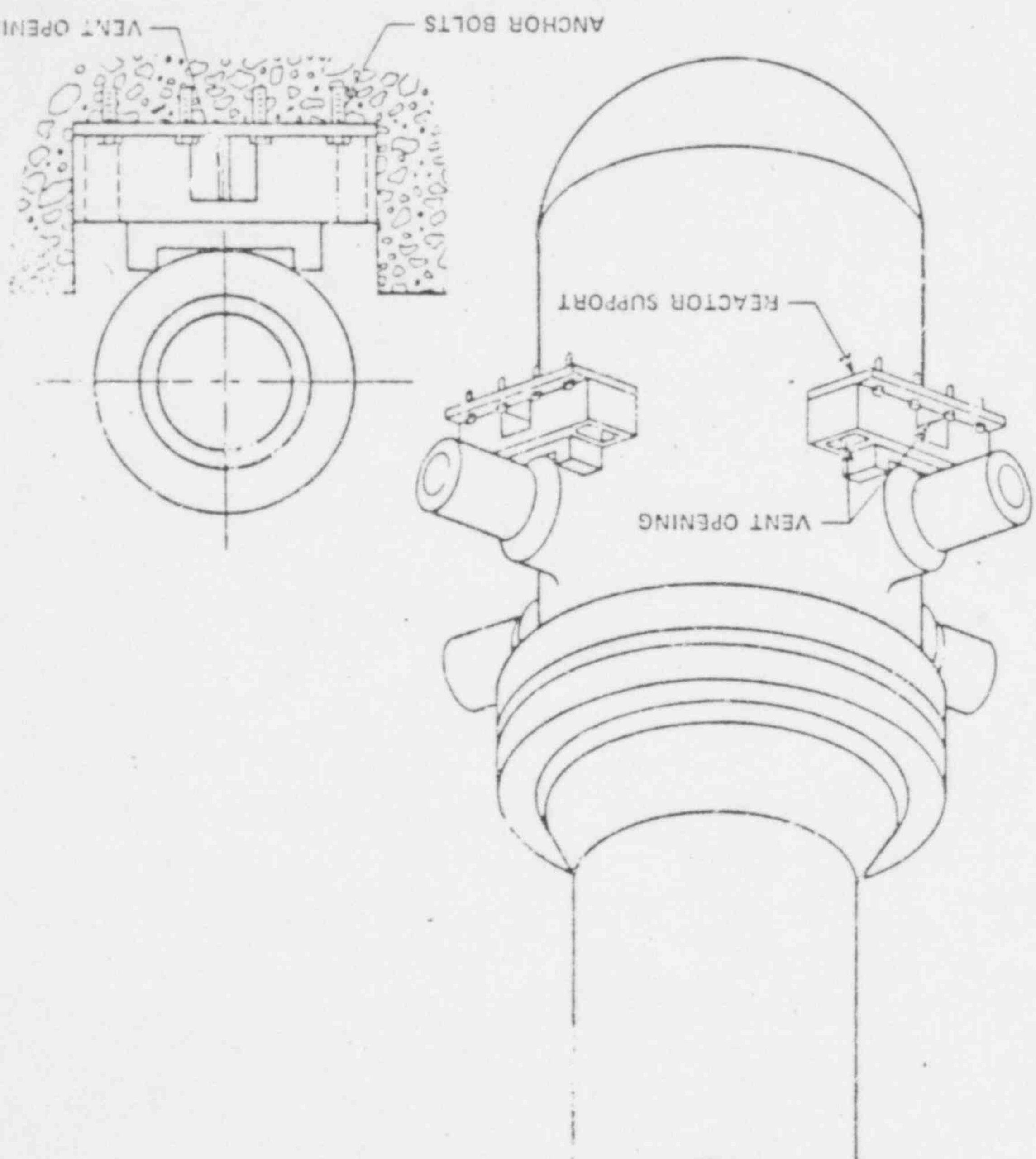
IN ADDITION TO "INSTANTANEOUS PRESSURE WAVES" EFFECTS OF SYSTEM REACTION FORCES AND TRANSIENT INTERNAL FORCES MUST ALSO BE EVALUATED.

RESULTS ARE CALCULABLE USING COMMON TECHNIQUES IN FIELDS OF FLUID AND STRUCTURAL DYNAMICS.

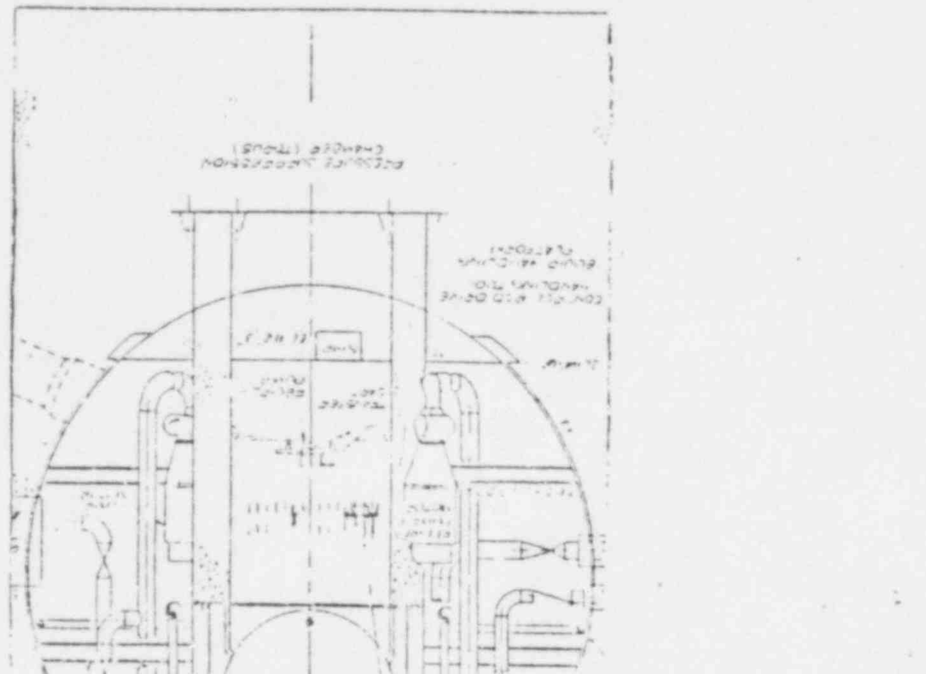
487 262



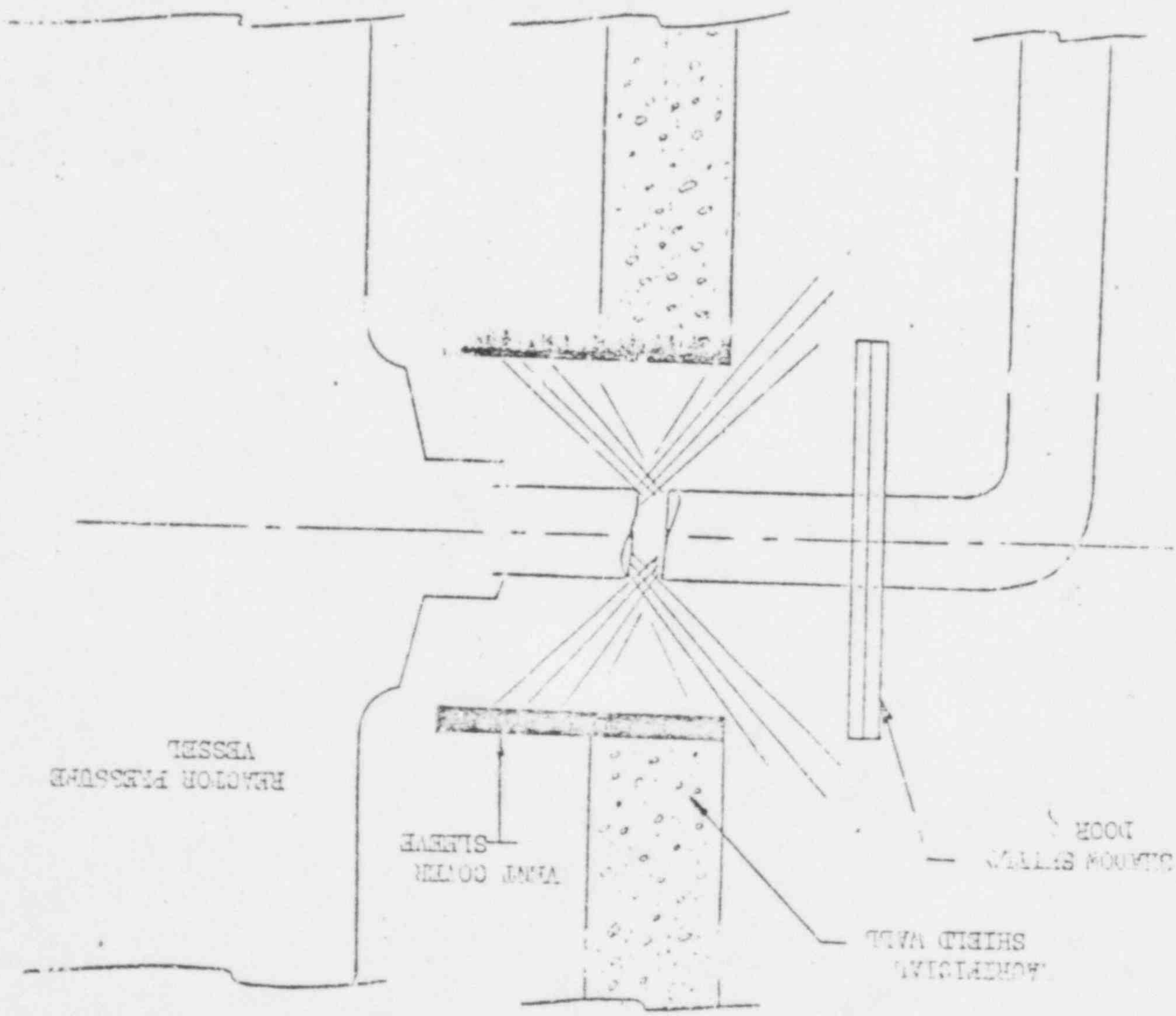


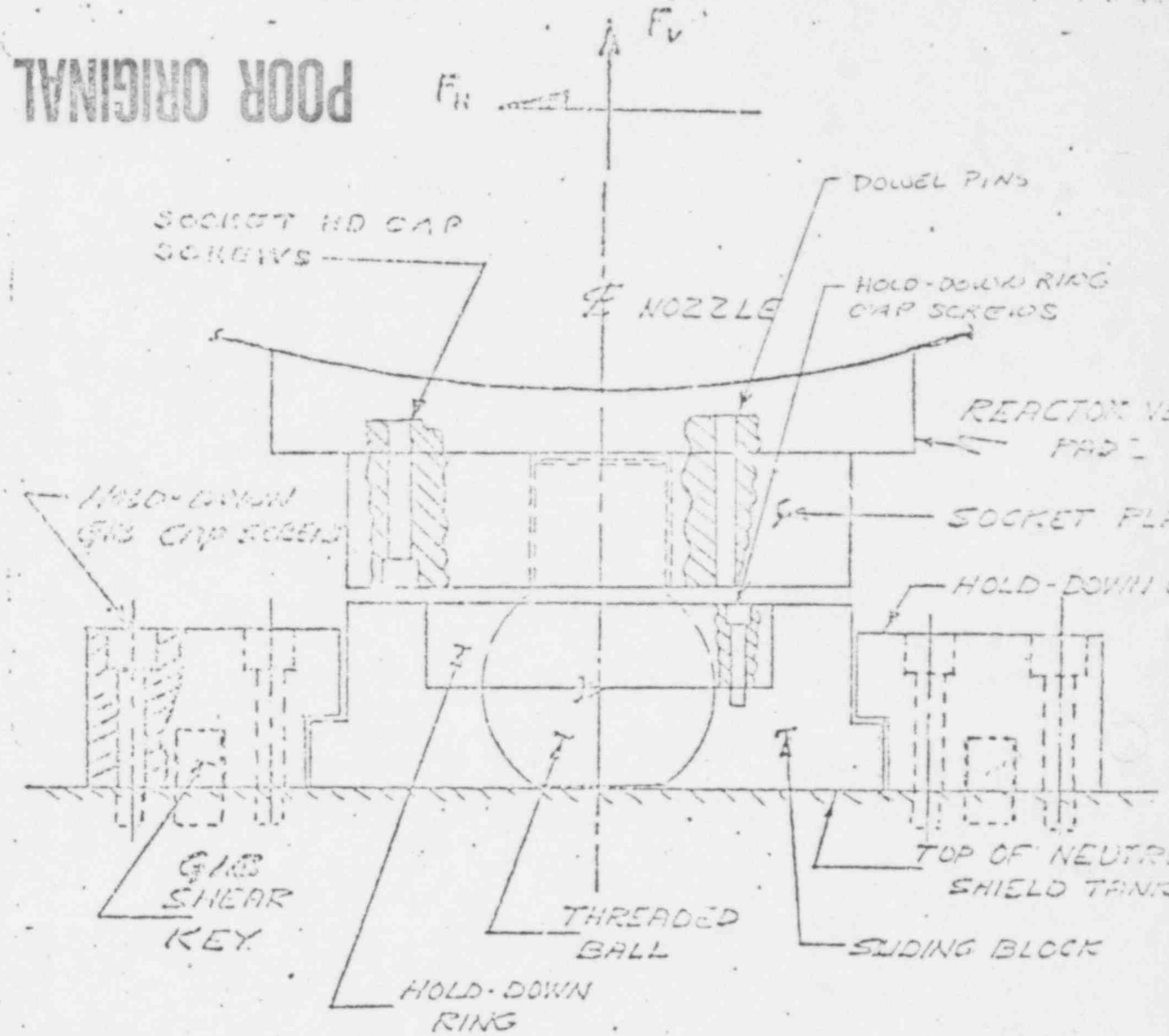

VENT OPENIN
ANCHOR BOLTS
REACTOR SUPPORT
VENT OPENING

15



83 266


REACTOR PRESSURE VESSEL
SLEEVE
SHIELD WALL
DOOR

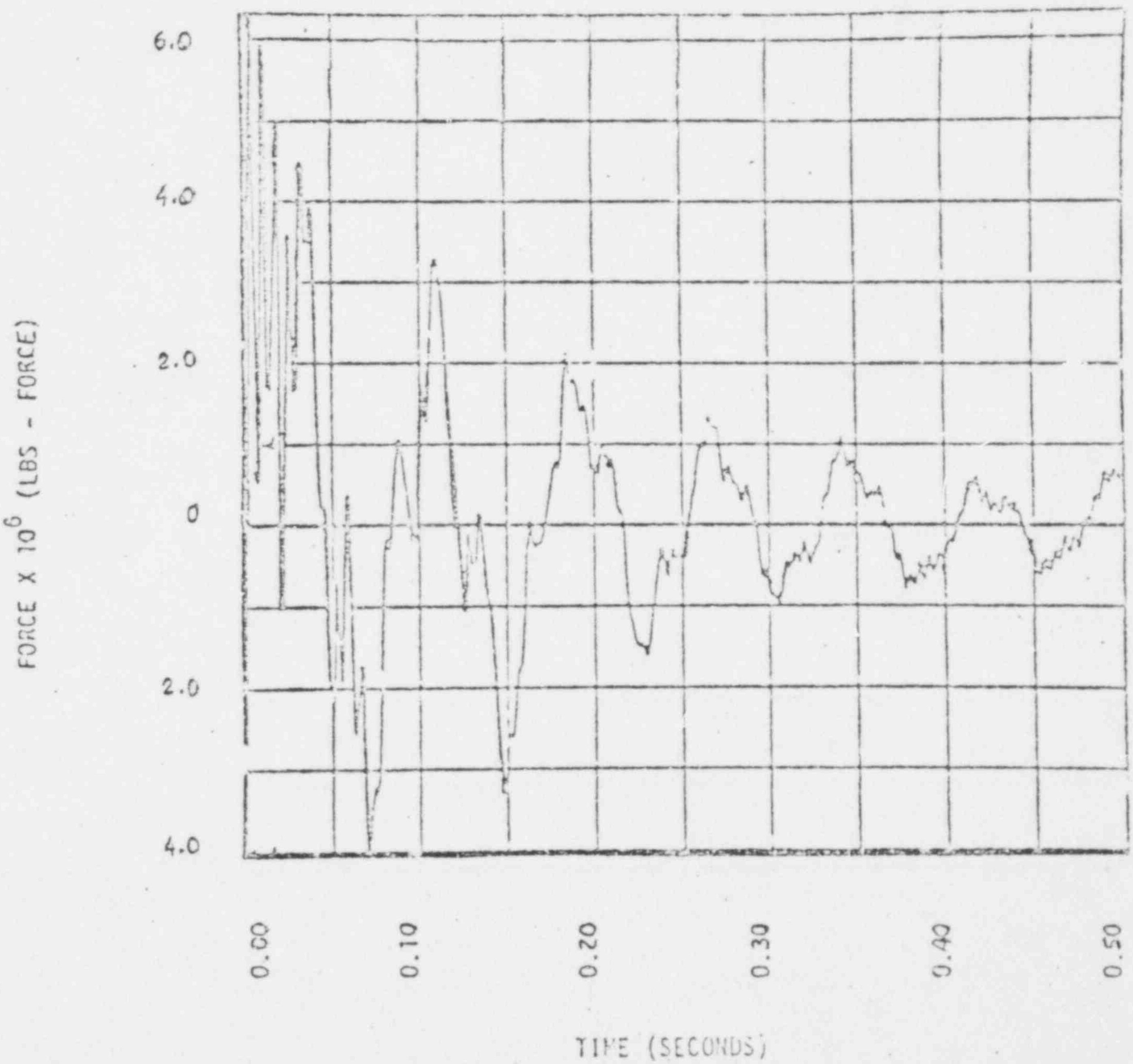


REACTOR PRESSURE VESSEL SUPPORTS
SLIDING FOOT ASSEMBLY

17

HORIZONTAL FORCE ON VESSEL

ASYMMETRIC INTERNAL PRESSURE LOADING ON RPV DUE TO COLD LEG BREAK



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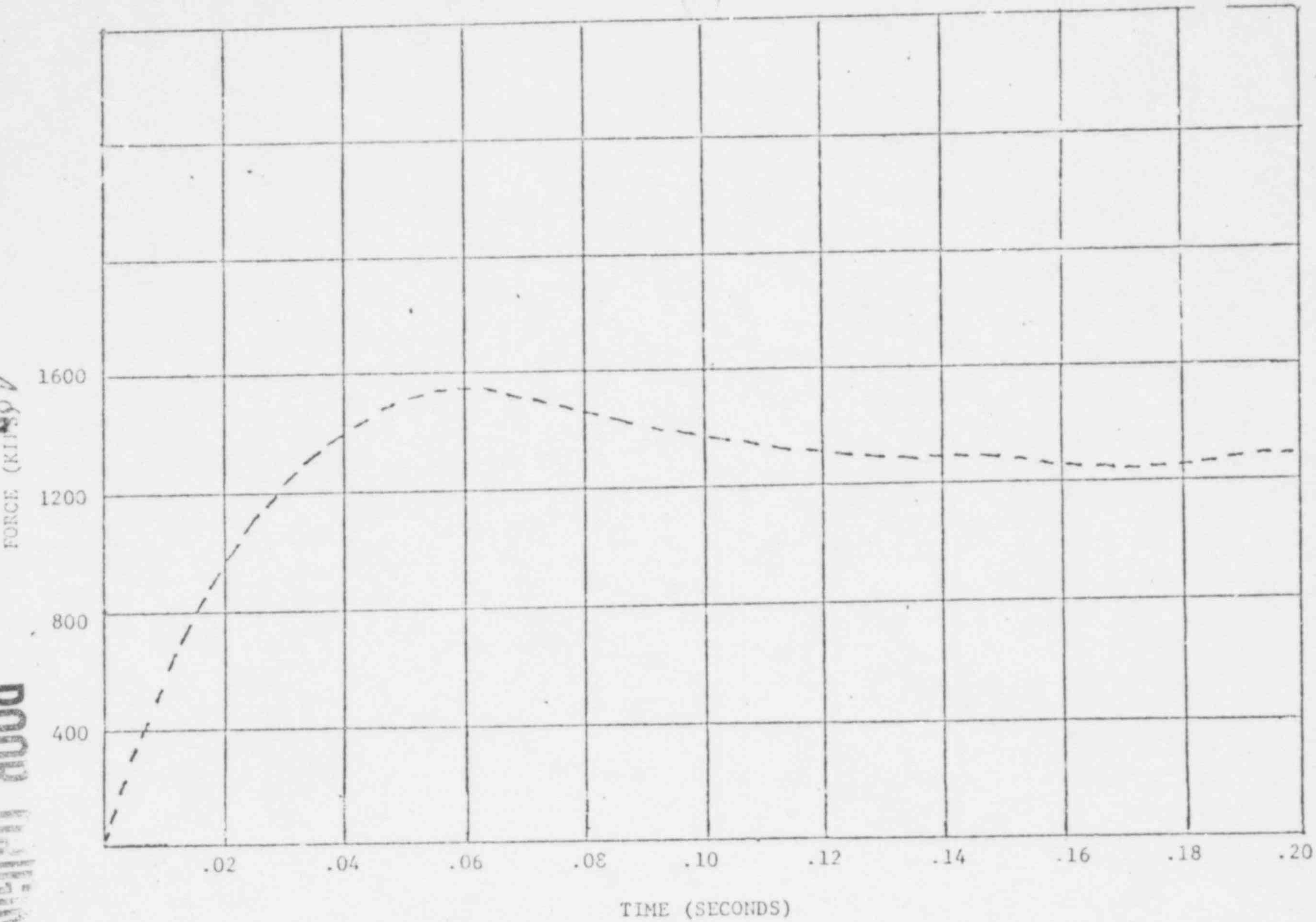
1600
1200
800
400
FORCE (KIPS)
.02
.04
.06
.08
.10
.12
.14
.16
.18
.20
TIME (SECONDS)

489 270

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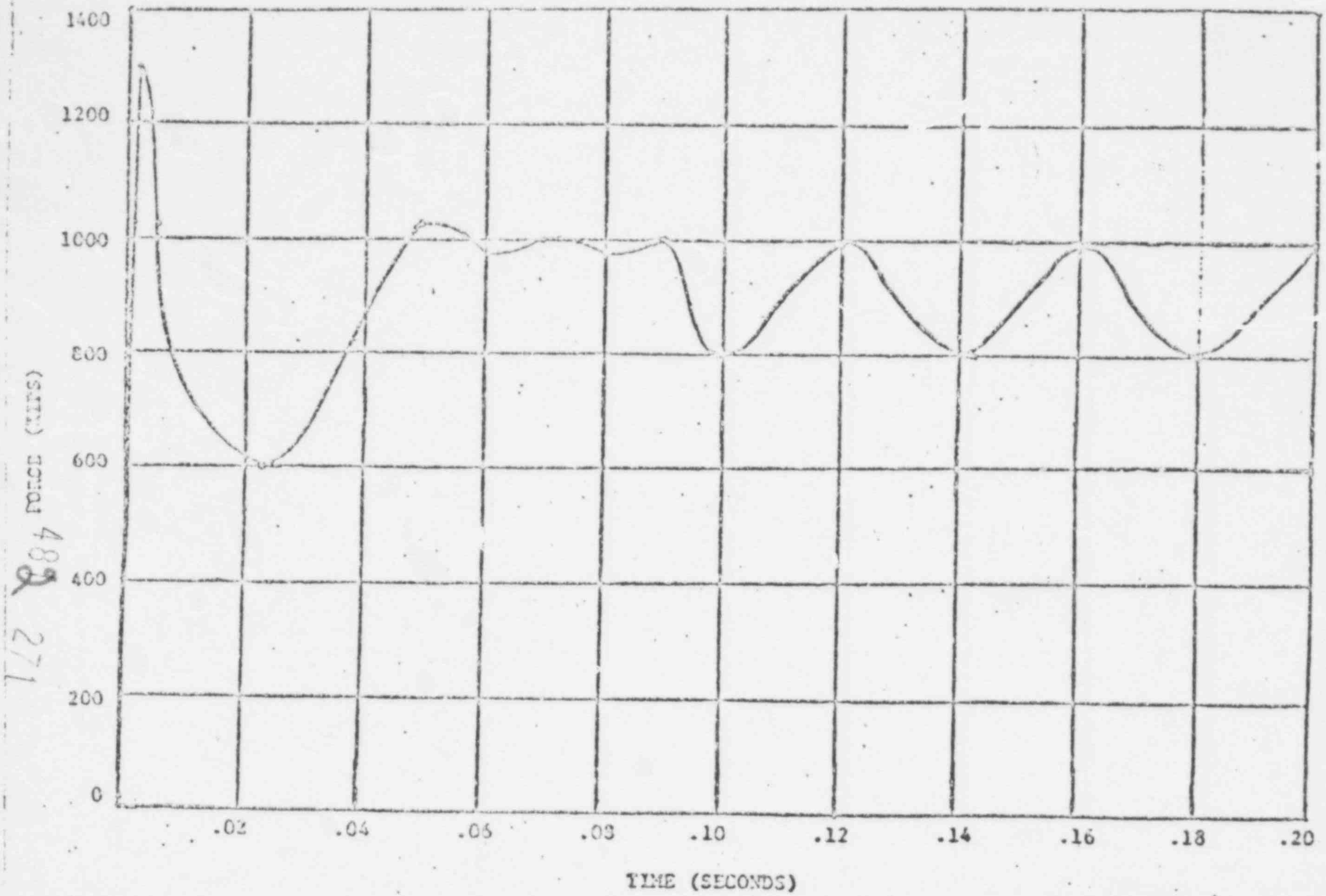
RPV JET REACTION F[?]CE



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RPV Nozzle Support Shear Load Path

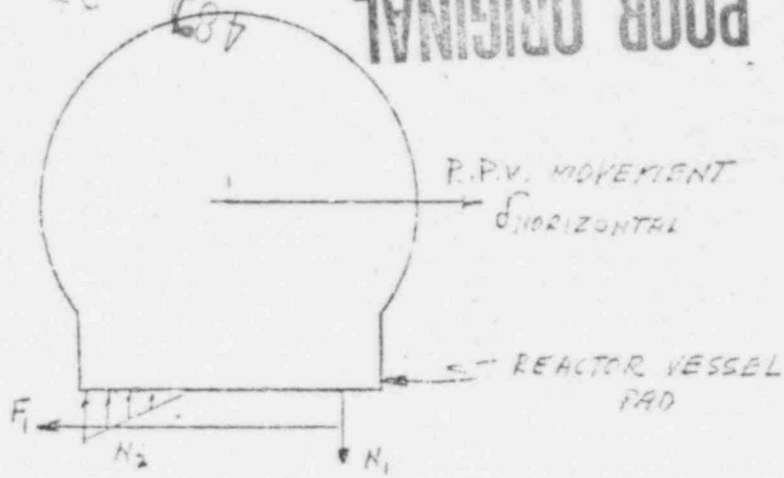

FIG. A

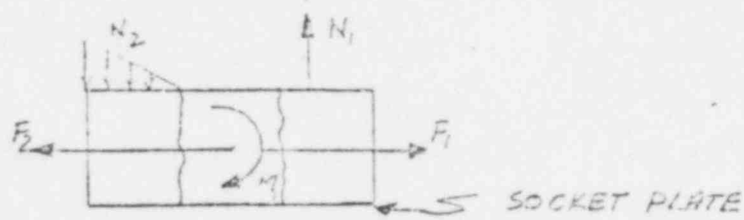

FIG. B

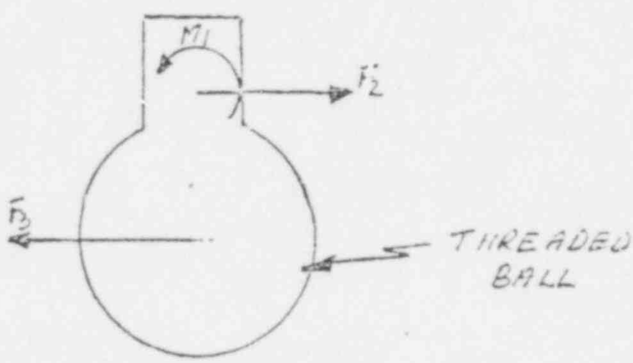

FIG C

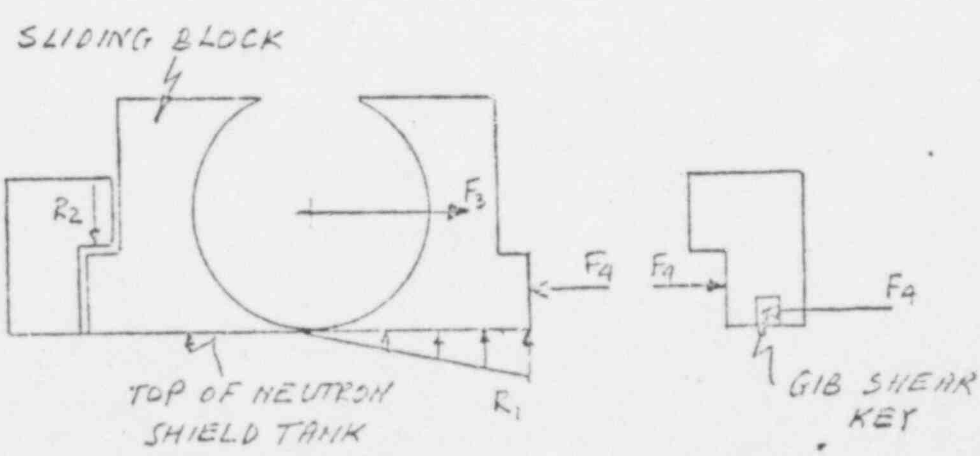

FIG. D

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POOR ORIGINAL

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PUMP FLYWHEEL MISSILES GENERATED BY REACTOR COOLANT PUMP OVERSPEED

ALLEGATION

AS A RESULT OF A REACTOR COOLANT SYSTEM PIPE RUPTURE AND THE BLOWDOWN OF REACTOR COOLANT THROUGH THE REACTOR COOLANT PUMP, THE PUMP IMPELLER MAY ACT AS A HYDRAULIC TURBINE CAUSING THE PUMP, MOTOR, AND THE FLYWHEEL TO OVERSPEED AND BECOME POTENTIAL SOURCES OF MISSILES.

THE POTENTIAL FOR MISSILES FROM PUMP OVERSPEED REMAINS AN UNRESOLVED SAFETY PROBLEM FOR INDIAN POINT 2 AND 3, AS WELL AS OTHER PLANTS.

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REPORT ON
REACTOR COOLANT PUMP OVERSPEED
DURING A LOCA

AUGUST 3, 1973

CONCLUSION

WE BELIEVE THAT, BECAUSE OF THE SMALL LIKELIHOOD FOR THE OCCURRENCE OF A PUMP OVERSPEED EVENT THAT COULD SERIOUSLY INCREASE THE CONSEQUENCES RESULTING FROM A LOSS-OF-COOLANT ACCIDENT, THE ACTION BEING TAKEN BY THE STAFF TO ASSESS THIS PROBLEM IN A GENERIC FASHION OUTSIDE THE CONTEXT OF INDIVIDUAL APPLICATION REVIEWS IS AN ACCEPTABLE COURSE TO FOLLOW.

BASES FOR CONCLUSION

1. FLYWHEELS ARE SIMPLE DEVICES
2. MATERIALS PROPERITES ARE KNOWN
3. REG. GUIDE 1.14 ADDRESSES DESIGN AND INSPECTION
4. ONLY POTENTIAL MECHANISM FOR SIGNIFICANT OVERSPEED IS A LOCA
5. SPECIFIC LOCA PROBABILITY IS LOW

PIPE RUPTURE	$10^{-6} - 10^{-5}$
RESTRAINT SYSTEM FAILURE	$10^{-2} - 10^{-1}$
MISSILES CAUSE ADDITION CONSEQUENCES	$10^{-3} - 10^{-2}$
OVERALL PROBABILITY	$10^{-11} - 10^{-8}$ PER FACILITY YEAR

6. PRESENT ANALYTICAL CALCULATIONS ARE CONSERVATIVE
7. ELECTRICAL BRAKING CAN LIMIT OVERSPEED

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ALLEGATION

"LIGHT WATER REACTORS HAVE BEEN PLAGUED BY NUMEROUS FLOW INDUCED VIBRATION PROBLEMS IN BOTH BWR'S AND PWR'S."

RESOLUTION

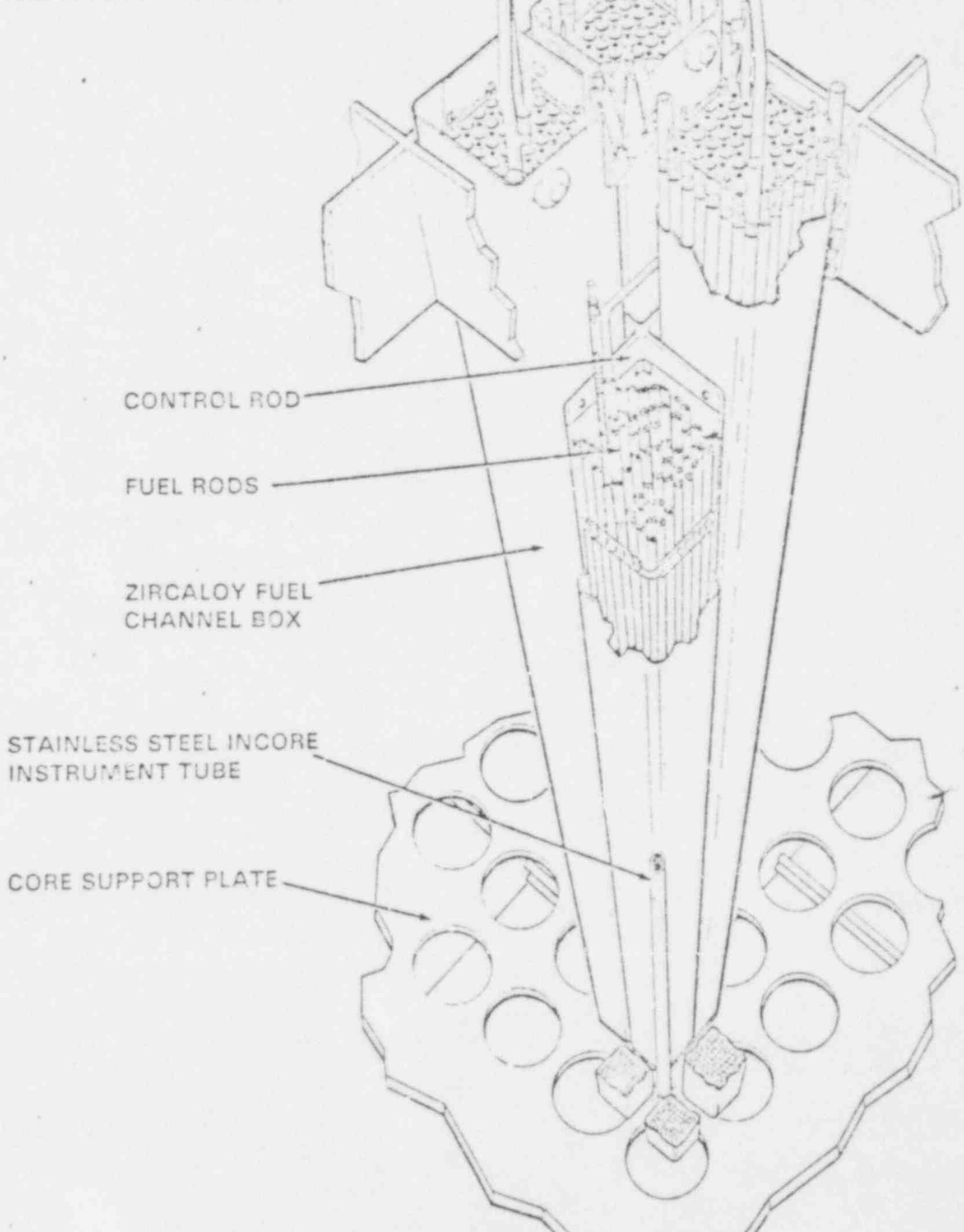
- REGULATORY GUIDE 1.20
- LOOSE PARTS MONITORING PROGRAMS
- DESIGN MODIFICATIONS AND/OR SPECIALIZED SURVEILLANCE

LOOSE PARTS MONITORING PROGRAMS

OPERATING REACTORS

BABCOCK & WILCOX	(4 OF 4)
COMBUSTION ENGINEERING	(3 OF 5)
WESTINGHOUSE	(5 OF 14)
GENERAL ELECTRIC	(1 OF 20)
TOTAL	(13 OF 43)

BWR FUEL ASSEMBLIES, CHANNEL BOX AND INSTRUMENT TUBE



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STATUS OF MODIFICATION
TO ELIMINATE LPRM - CHANNEL WEAR

REACTORS WITH NO DRILLED ASSEMBLIES

- BRUNSWICK 2
- COOPER STATION
- DUANE ARNOLD
- FITZPATRICK
- HATCH 1
- POINT BEACH 3
- PILGRIM
- VERMONT YANKEE

REACTORS WITH SOME DRILLED ASSEMBLIES

- BROWN'S FERRY 1
- BROWN'S FERRY 2
- PEACH BOTTOM 2

REACTORS WITH ALL DRILLED ASSEMBLIES

- BROWN'S FERRY 3
- BRUNSWICK 1

LOOSE PARTS MONITORING PROGRAMS--continued

GENERAL ELECTRIC

HUMBOLDT BAY	No
DRESDEN 2, 3	No
FITZPATRICK	No
MONTICELLO	Yes
BRUNSWICK 2	No
BROWNS FERRY 1, 2, 3	No
PEACH BOTTOM 2, 3	No
MILLSTONE 1	
DUANE ARNOLD	No
HATCH	No
NINE MILE POINT	No
FITZPATRICK	No
VERMONT YANKEE	No
OYSTER CREEK	No
BIG ROCK POINT	No
QUAD CITIES	Yes
COOPER	No
DRESDEN 1	No
LACROSS	No
PILGRIM 1	No

FEEDWATER SPARGER VIBRATION

- NOT A SAFETY CONCERN
- CRACKING PROGRESSES SLOWLY
  - EXPERIENCE
  - NO LARGE STRESSES, ΔP SMALL
  - CONTROL ROOM INDICATION (COLD WATER MALDISTRIBUTION)
    - ON LINE COMPUTER (MCPR)
    - LPRM
- EVEN WITH COMPLETE FAILURE
  - ONLY INTERNAL PARTS FAILED
  - ALL FEEDWATER WOULD STILL ENTER CORE
  - NO LOCA POSSIBLE

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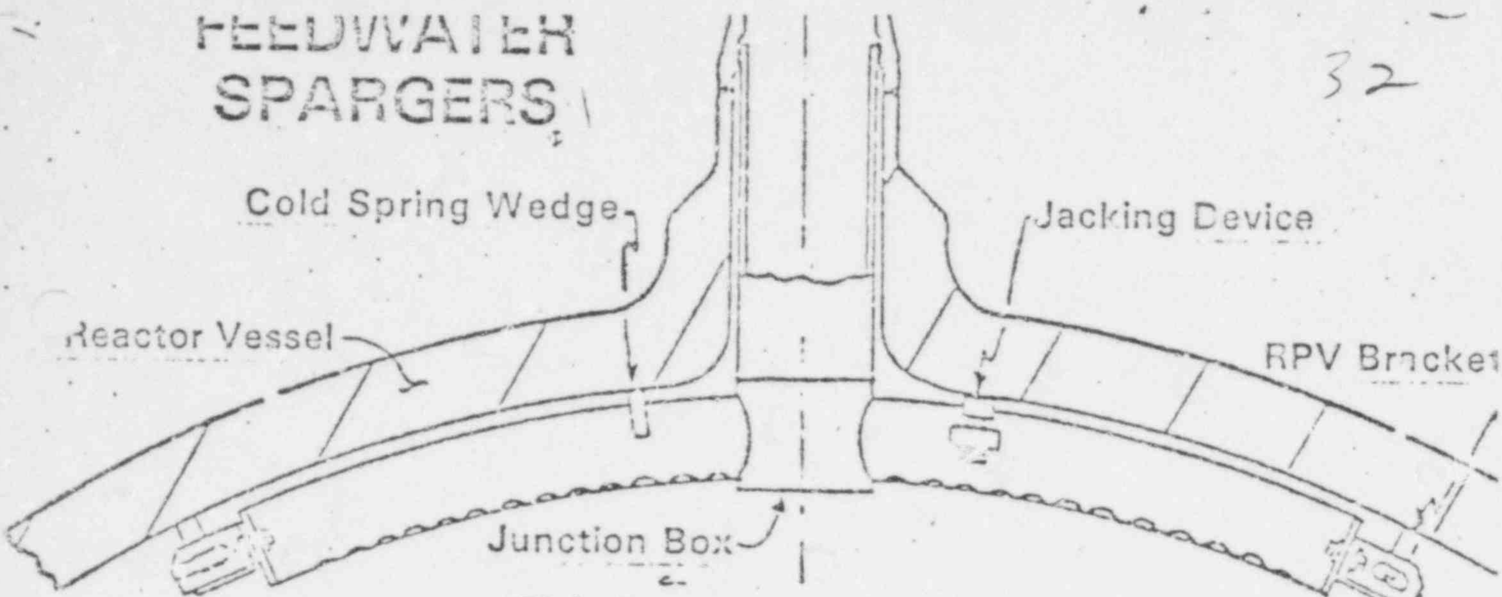
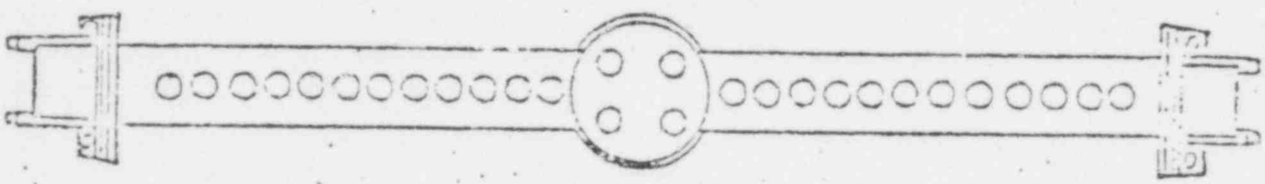
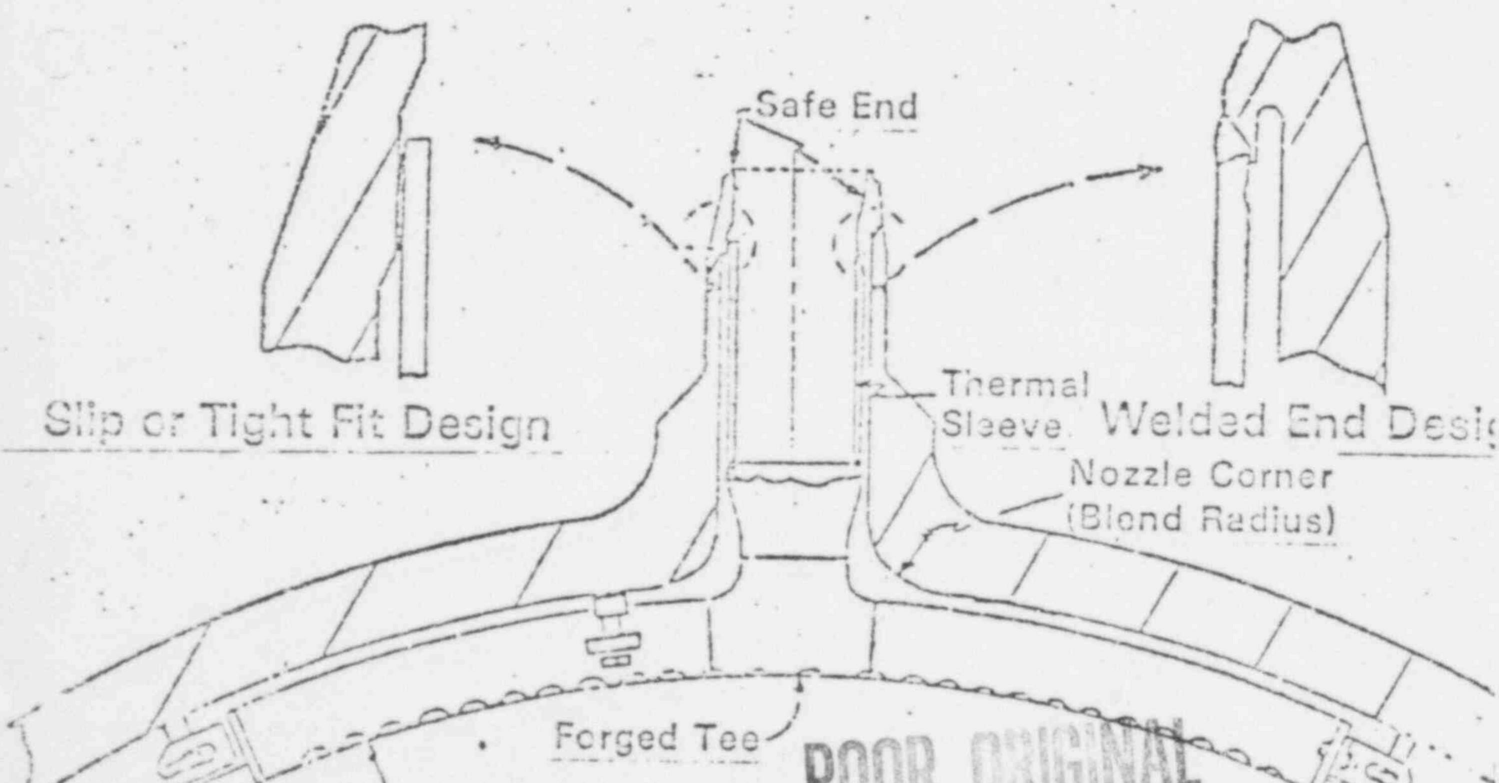
FEEDWATER SPARGERS



Old Juunction Box Design





New Forged Tee Design

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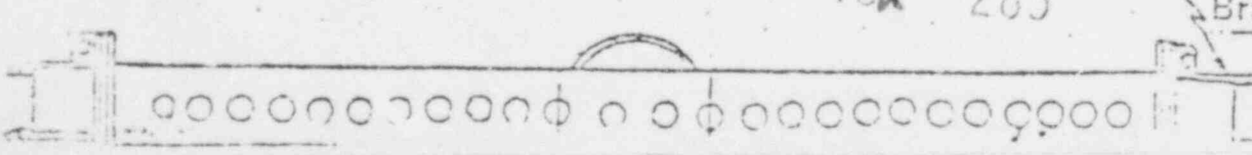

POSSIBLE EFFECTS OF FAILURE

- FLOW BLOCKAGE BY PIECES
  - HIGH % BLOCKAGE NEEDED TO CAUSE PROBLEM
  - PATH AND FLOW VELOCITIES LIKELY TO PREVENT BLOCKAGE
- DAMAGE TO CORE SPRAY PIPING
  - WOULD BE DETECTED
  - DAMAGE TO BOTH SPRAYS NOT CREDIBLE
- JET PUMP DAMAGE
  - UNLIKELY
  - NOT SAFETY PROBLEM
- FEEDWATER MALDISTRIBUTION
  - WOULD BE DETECTED
  - OPERATIONAL PROBLEM

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LOOSE FEEDWATER SPARGER - VIBRATION EXPERIENCE

29 PLANTS OPERATING

20 PLANTS - SPARGERS INSPECTED (13 UNDERWATER VIS; 7, PT)

13 UNDERWATER VISUAL

7 PT EXAMINATION

7 PLANTS SPARGER PROBLEM

1 U BOLT FAILURE

6 SPARGER FAILURE

PLANTS	FIRST OPN.
MILLSTONE - 3 FAILURES	11/70
SPARGER 1 - 16.0 MO. OPERATION	
SPARGER 2 - 1.1 MO. OPERATION	
SPARGER 3 - 12.3 MO. OPERATION	
SPARGER 4 - 11.0 MO. OPERATION - NO FAILURE*	
FOREIGN PLANT - 1 FAILURE - 21.7 MO. OPERATION	6/71
DRESDEN 2 - 1 FAILURE - 37.8 MO. OPERATION	4/70
DRESDEN 3 - 1 FAILURE - 32.6 MO. OPERATION	7/71
QUAD CITIES 2 - 1 FAILURE - 23.0 MO. OPERATION	4/72
HUMBOLDT - FWS OK; "U" BOLT FAILURE ONLY - 127.3 MO. OPERATION	4/63
MONTICELLO - 1 FAILURE - 39.5 MO. OPERATION	3/71

POOR ORIGINAL

* ONLY REPLACEMENT TIGHT FIT SPARGER INSPECTED AFTER OPERATION - NO CRACKS

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ROD DROP ACCIDENT AND PATCHES

FEBRUARY 18, 1976 ALLEGATIONS

- ELECTRONIC "PATCHES" HAVE BEEN ADDED TO MITIGATE MECHANICAL DEFICIENCIES
- "PATCHES" ADD TO COMPLEXITY OF OPERATION AND ARE FREQUENTLY IGNORED
- MITIGATING SYSTEMS SHOULD BE IMPROVED AND MADE MANDATORY

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ROD DROP ACCIDENT AND PATCHES

SAFETY ISSUES

- FUEL FAILURE
  - (1) ENTHALPY OF 170 CAL/GM; CLADDING FAILURE THRESHOLD
  - (2) ENTHALPY OF 280 CAL/GM; SPECIFIC ENERGY DESIGN LIMIT
  - (3) ENTHALPY OF 425 CAL/GM; PROMPT FUEL DISPERSAL THRESHOLD

- MECHANICAL DAMAGE

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ROD DROP ACCIDENT AND PATCHES

SUMMARY RESPONSE

- ROD DROP ACCIDENT (RDA) WITH SIGNIFICANT CONSEQUENCES REQUIRES MULTIPLE INDEPENDENT FAILURES AND ERRORS
- PROBABILITY OF THE SIMULTANEOUS OCCURRENCE OF THESE EVENTS IS EXTREMELY LOW
- IN SPITE OF THIS LOW PROBABILITY, SYSTEMS TO PREVENT INCORRECT CONTROL ROD PATTERNS (A NECESSARY INGREDIENT FOR A SIGNIFICANT RDA) HAVE BEEN DEVELOPED, EVALUATED, AND APPROVED BY THE STAFF, AND INSTALLED

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ROD DROP ACCIDENT AND PATCHES

SYSTEMS DESIGNED TO PREVENT INCORRECT ROD WITHDRAWAL

- Rod Worth Minimizer (RWM)
  - Computer system
  - Earlier plants
  - Technical Specification - more stringent than previously
- Rod Sequence Control System (RSCS)
  - Hardwired system
  - Newer plants (BWR/4s)
  - Designed, approved, and in actual use
  - Technical Specifications on operability
- Rod Pattern Control System (RPCS)
  - Hardwired system
  - Future plants (BWR/6)

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ROD DROP ACCIDENT AND PATCHES

CONCLUSIONS

- SYSTEMS (RWM, RSCS, RPCS) ARE NOT "PATCHES" FOR MECHANICAL PROBLEMS WITH RODS, BUT ARE MONITORS OF OPERATOR WITHDRAWAL ACTIONS
- ROD BLOCK MONITOR (RBM) HAS NO RELATION TO ROD DROP ACCIDENT. THE RBM IS OPERATIVE IN THE POWER RANGE, WHERE RDA IS OF NO CONSEQUENCE, TO PREVENT LOCAL FUEL DAMAGE CAUSED BY A ROD WITHDRAWAL ERROR TRANSIENT.

  PROBABALISTIC ANALYSIS DID NOT TAKE CREDIT FOR THE RWM OR RSCS
- NO ADDITIONAL REQUIREMENTS OR EQUIPMENT APPEAR TO BE NEEDED

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ROD SEQUENCE CONTROL SYSTEM (RSCS) SUMMARY

FOR OPERATING BWR'S

BWR CLASS	PLANT	RSCS	COMMENTS
2	OYSTER CREEK	NO	
	NINE MILE POINT-1	NO	
3	DRESDEN 2&3	NO	NO BACKFIT OF RSCS REQUIRED BASED ON CONCLUSIONS FROM A PROBABALISTIC ANALYSIS
	QUAD CITIES 1&2	NO	
	MILLSTONE-1	NO	
	MONTICELLO	NO	
	PILGRIM-1	NO	
4	VERMONT YANKEE	NO	
	BROWNS FERRY-1	YES	GROUP C RODS FOR FIRST CYCLE; GROUP NOTCH CONTROL AT FIRST RELOAD.
	PEACH BOTTOM 2&3	YES	
	COOPER	YES	
	DUANE ARNOLD	YES	SIMPLE NOTCH CONTROL FOR FIRST CYCLE; GROUP NOTCH CONTROL AT FIRST RELOAD.
	HATCH-1	YES	
	BROWNS FERRY-2	YES	GROUP NOTCH CONTROL AT FIRST CYCLE.
	BRUNSWICK-2	YES	
	FITZPATRICK	YES	

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MONTICELLO SAFETY/RELIEF VALVE TESTS

PURPOSE:

- STRAIN DATA FOR FATIGUE LIFE EVALUATION OF TORUS STRUCTURE
- P, T, AND WATER LEVEL DATA TO EVALUATE EFFECT OF PRESSURIZATION IN R/V DISCHARGE PIPING AND TORUS

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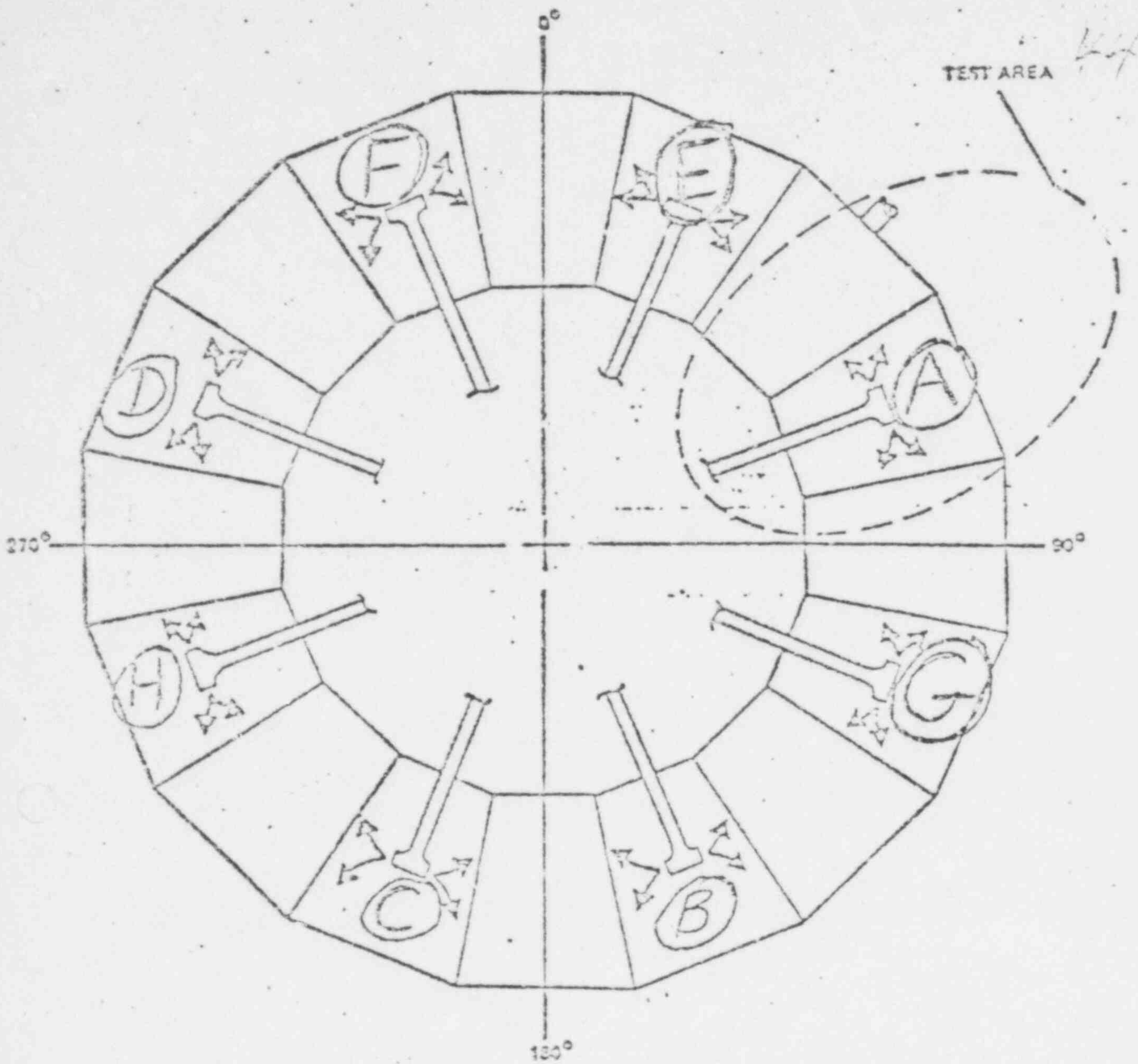
MEASUREMENTS:

- P ON TORUS SKIN
- P & T IN R/V DISCHARGE PIPE FOR FORCING FUNCTION MODEL
- WATER LEVEL IN DISCHARGE PIPING TO EVALUATE EFFECT OF CONSECUTIVE VALVE ACTUATION
- P & STRAIN GAGE MEASUREMENTS DURING MULTIPLE VALVE ACTUATIONS TO REFINE ANALYTICAL MODEL AND DETERMINING TORUS FATIGUE MARGIN OVER PLANT LIFE.
- T OF TORUS POOL TO EVALUATE LOCAL MIXING EFFECTS
- WATER LEVEL IN TORUS POOL TO MONITOR MOTION OF AIR BUBBLE

- STRAIN GAGE & ACCELEROMETER MEASUREMENTS ON R/V DISCHARGE PIPING & STRUCTURAL SUPPORTS
- ACCELEROMETER MEASUREMENTS TO DETERMINE LOADS TRANSMITTED THROUGH SUPPORT COLUMNS TO FOUNDATION MAT & OTHER STRUCTURES.



- ACTUATE A, E, F, D, G, AND B INDIVIDUALLY
- ACTUATE A AND F SIMULTANEOUSLY
- ACTUATE A AND E SIMULTANEOUSLY
- ACTUATE A, E, AND G SIMULTANEOUSLY
- ACTUATE A SEQUENTIALLY THROUGH 5-SECOND DISCHARGE, BRIEF RECLOSURE, AND 5-SECOND DISCHARGE

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POOR ORIGINAL

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INSTRUMENTATION

(SENSORS/CHANNELS)

	TORUS WALL, SUCTION HEADER, & TORUS COLUMNS	TORUS POOL	S/R VALVE DISCHARGE PIPE & SUPPORTS	PEDESTAL AND BASEMENT
TOTAL:	70/164	26/26	38/47	3/9

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SCHEDULE

- TESTS WERE SCHEDULED FOR END OF FEBRUARY 1976 BUT WERE POSTPONED BECAUSE OF OPERATION WITH "ΔP FIX"
- MARK I OWNERS HAVE PROPOSED RESCHEDULING TESTS FOR MAY 1976

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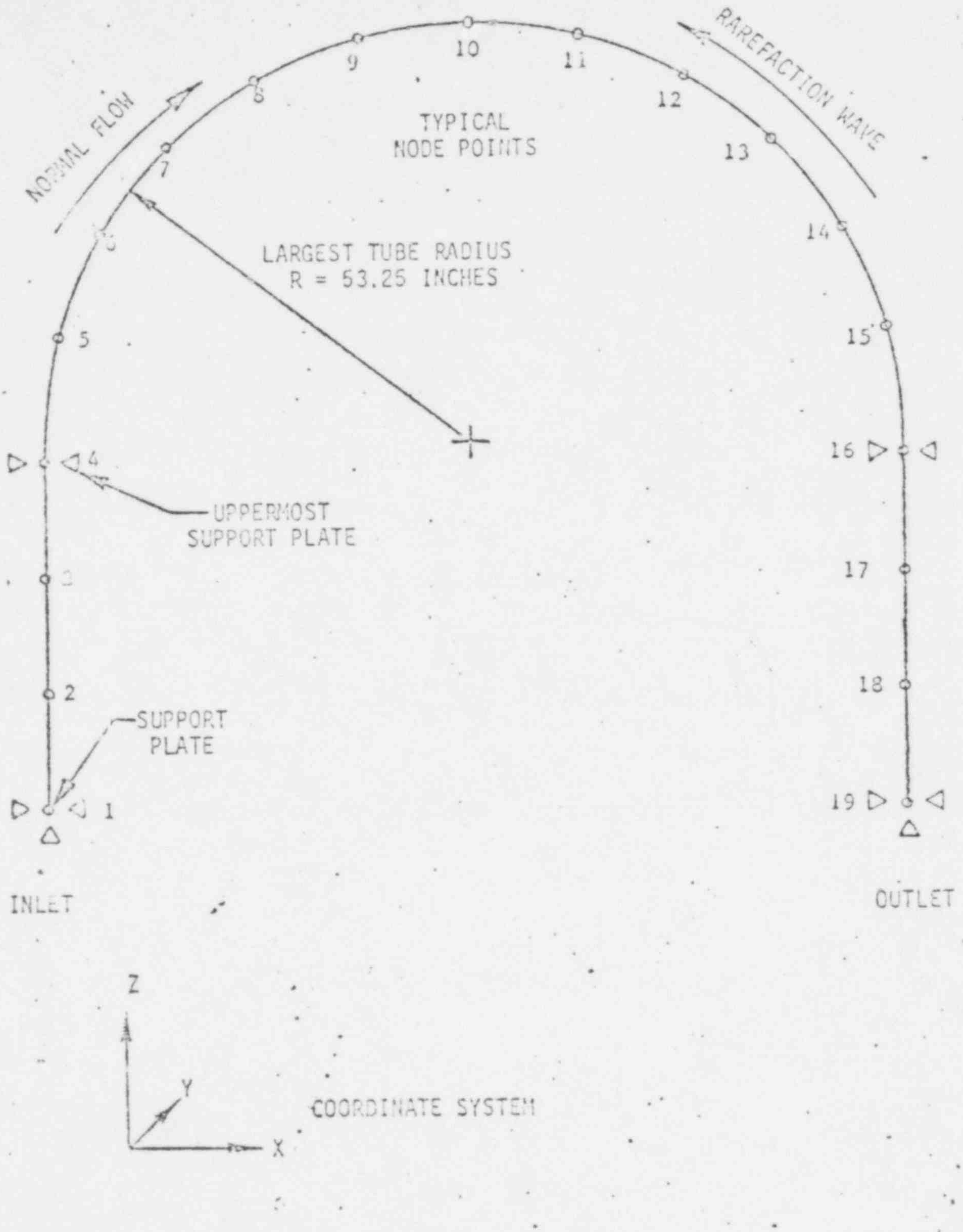

Figure 3.1-2 STASYS Tube Model - Elastic Pipe Elements Model D Steam Generator Largest Tube

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POOR ORIGINAL

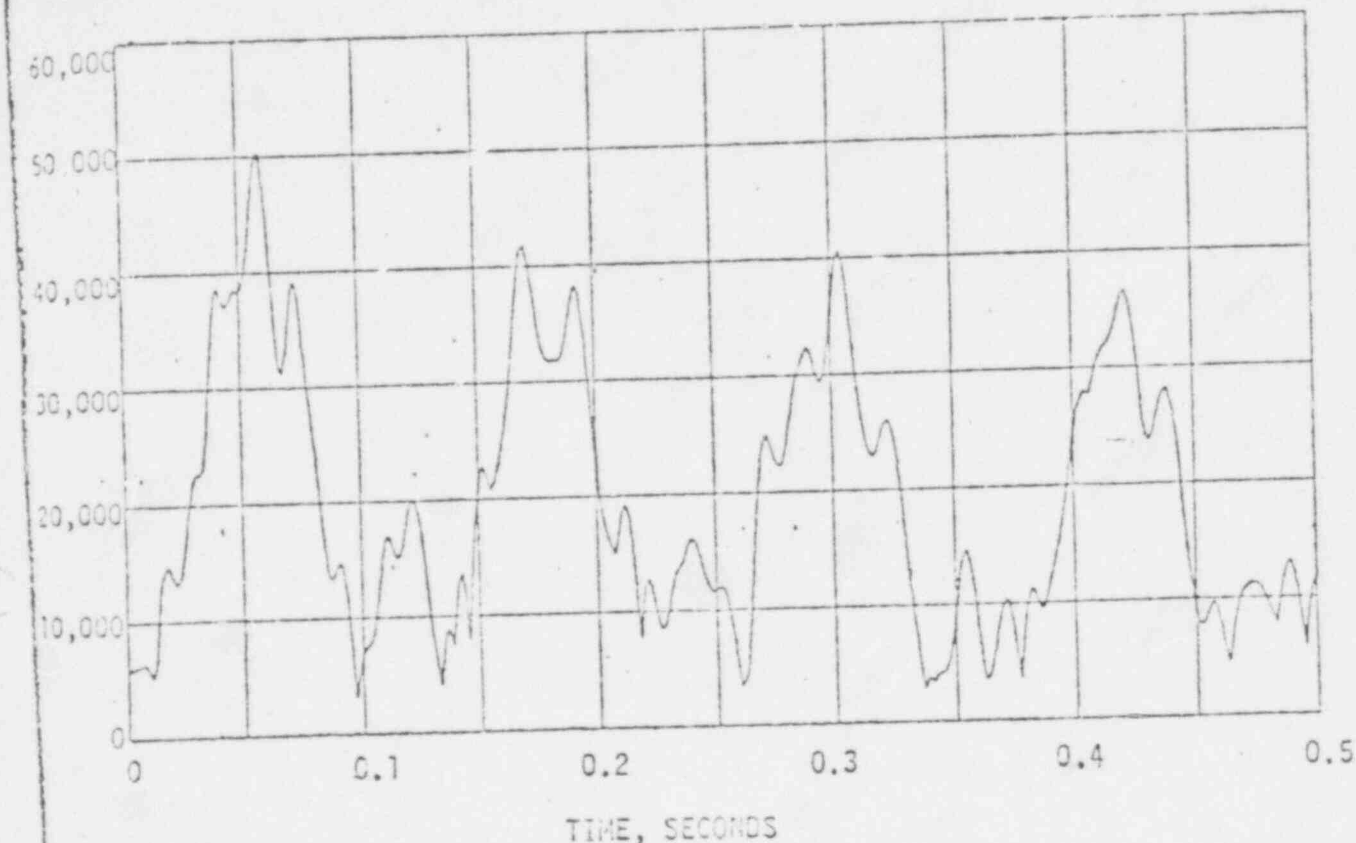

Figure 3.1-11 Maximum Stress Intensity on Tube Outer Wall
Node Location 16 - Total LOCA Effect

POOR ORIGINAL

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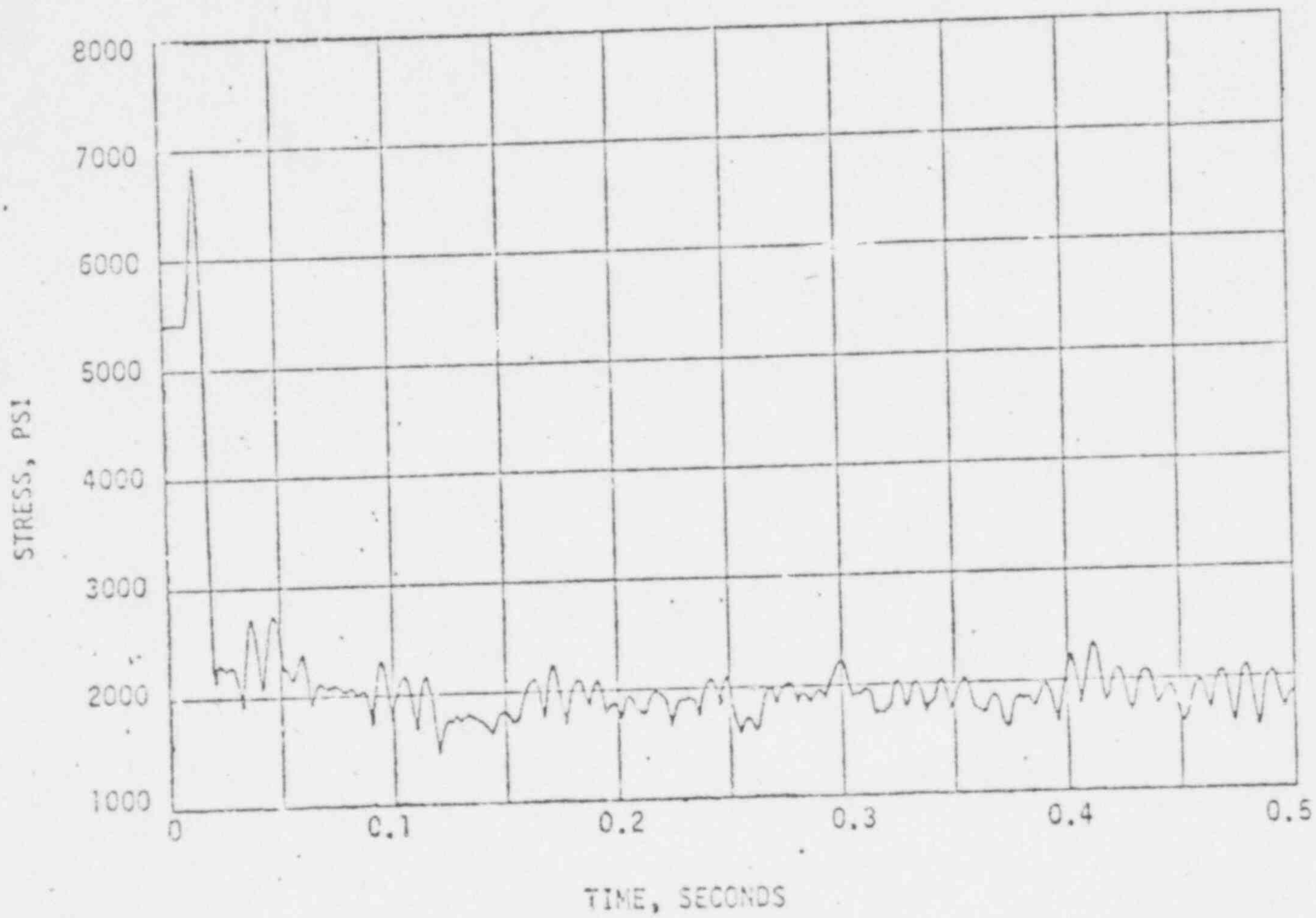

POOR ORIGINAL

Figure 3.1-3 Maximum Stress Intensity on Tube Outer Wall
Node Location 1 - Total LOCA Effect

482 300

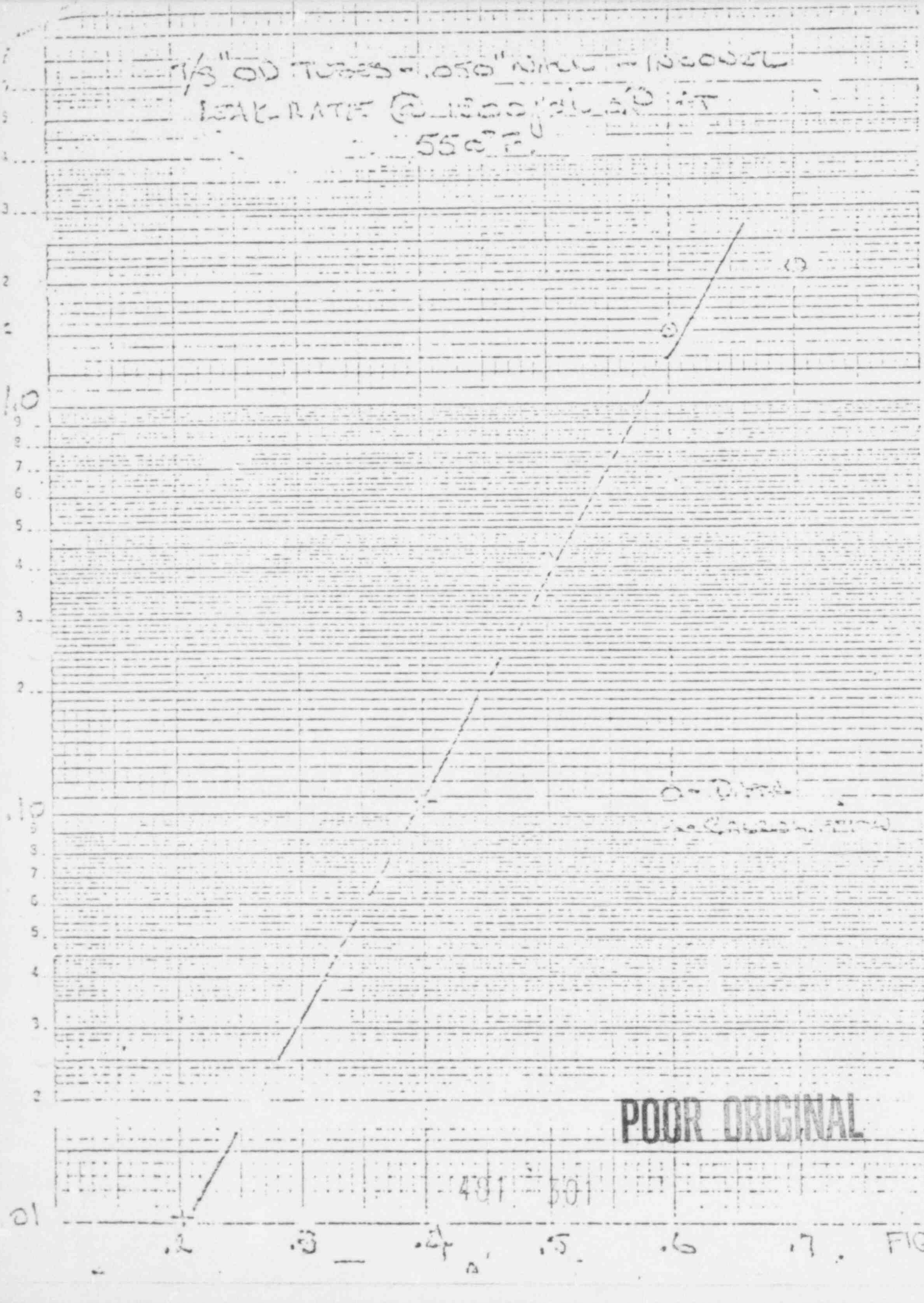

FIG 2

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TABLE 5-1
INCONEL 600
(Tubing with through and partwall slots)

0.87 inch OD by 0.048 inch wall

Specimen No.	Defect Type	Collapse Pressure (psi)	Maximum Pressure (psi)
3742-9-2-B	Unflawed	No Collapse	10,000
3734-5-1	1.50 inch partial through wall flaw	6500	-
3742-12-1	1.50 inch partial through wall flaw	6900	-
3742-8-1	1.25 inch partial through wall flaw	8500	-
3742-5-3	1.50 inch through wall flaws	6900	-
3742-5-5	1.25 inch through wall flaw	7000	-
3742-12-5	0.80 inch through wall flaw	8650	-
3742-12-4	0.8 inch	7200	-
3742-9-2-4	Preflattened tube 0.560 inch across flats	1750	2200
3742-5-4	1.5 inch through wall flaw	No collapse	5000
3742-M	Ten 1.5 inch through wall flaws	No collapse	5000
4554-IAN-1	2.0 inch long flat 25% remaining wall	2400	2875
4554-IAN-2	2.0 inch long flat 25% remaining wall	2250	-
4554-IAN-3	two adjacent 2.0 flats 25% remaining wall	2275	
4554-IAN-4	two adjacent 2.0 flat 25% remaining wall	2200	2400

POOR ORIGINAL

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MARK I POOL SWELL MODEL LAWS

1/12TH SCALE TESTS PERFORMED TO DETERMINE POOL SWELL TORUS LOADS

MODEL LAWS ESTABLISHED SO THAT CONTROLLING PHENOMENA ARE SCALED

TORUS DIVIDED INTO THREE REGIONS

- POOL WATER - MASS, MOMENTUM + ENERGY
- TORUS AIR SPACE - MASS, ENERGY + STATE
- BUBBLES - MASS, ENERGY + STATE

CONSERVATION EQUATIONS NON-DIMENSIONALIZED

- NON-DIMENSIONAL VARIABLES DEFINED
- SCALING FACTORS FOR VARIABLES DEFINED
- NON-DIMENSIONAL PARAMETERS DEFINED
- CONTROLLING PHENOMENA DEFINED

REQUIREMENTS TO SATISFY SCALING LAWS

GEOMETRIC SIMILITUDE $(X_{i_M} = X_{i_F} \frac{S_M}{S_F})$

$$\rho_M = \rho_F$$

$$g_M = g_F$$

$$\gamma_M = \gamma_F$$

$$(P - P_i)_M = (P - P_i)_F \frac{S_M}{S_F}$$

$$P_{i_M} = P_{i_F} \frac{S_M}{S_F}$$

$$(\dot{m}_R h_{o_R})_M = (\dot{m}_R h_{o_R})_F \left(\frac{S_M}{S_F}\right)^{7/2}$$

NEGLIGIBLE PHENOMENA

ACOUSTIC

HEAT CONDUCTION

VISCOUS

SURFACE TENSION

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<u>SATISFACTION OF SCALING LAW REQUIREMENTS</u>

PARAMETER	1/12TH SCALE TEST DESIRED	1/12TH SCALE TEST RESULTS
INITIAL PRESSURE	1.225 PSIA	1.225 ± 0.025 PSIA
TRANSIENT PRESSURE	16.45 PSI/SEC	MEDIUM ORIFICE 12.5 $^{+0.2}_{-0.1}$ PSI/SEC LARGE ORIFICE 18.1 $^{+0.3}_{-0.6}$ PSI/SEC
ENTHALPY FLAX	8.6 BTU/SEC	MEDIUM ORIFICE 7.7 $^{+0.1}_{-0.2}$ BTU/SEC LARGE ORIFICE 12.85 $^{+0.15}_{-0.35}$ BTU/SEC
SUBMERGENCE	4 INCHES	4 ± 0.1 INCHES
DRYWELL/WETWELL ΔP	VARIED	VARIED TO WITHIN ± 0.1 INCHES H_2O
TEST SECTION	DIAMETER 31 INCHES WIDTH 7.39 INCHES	31.00 ± 0.6 INCHES 7.5 ± 0.1 INCHES
DENSITY OF WATER	61.7 TO 62.4 LBM/FT^3	62.3 TO 62.4 LBM/FT^3
SPECIFIC HEAT	1.399 TO 1.401	1.400 TO 1.401

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POOR ORIGINAL

SENSITIVITY OF STRUCTURAL LOADS TO CHANGES IN LOCA DYNAMIC PRESSURE

DOWNWARD LOAD

- WELDS
- PINS
- LUGS
- COLUMNS

UPWARD LOAD

- TIEDOWN SYSTEM

SAMPLE RESULTS

VERMONT YANKEE WITH ΔP_{DW} = 1.7 PSI

DOWNWARD LOAD	LOAD/CAPABILITY	CHANGE IN RATIO FOR 10% CHANGE IN PRESSURE LOAD
- WELDS	.43	0.03
- COLUMNS	.32	0.02
UPWARD LOAD		
- TIEDOWN SYSTEM	.26	0.04

50

SAMPLE RESULTS

VERMONT YANKEE WITH $\Delta P_{DW} = 1.7$ PSI

DOWNWARD LOAD	REQUIRED INCREASE IN LOCA DYNAMIC PRESSURE TO REACH ULTIMATE STRUCTURE LOAD
- WELDS	220%
- COLUMNS	360%
UPWARD LOAD	
- TIEDOWN SYSTEM	161%

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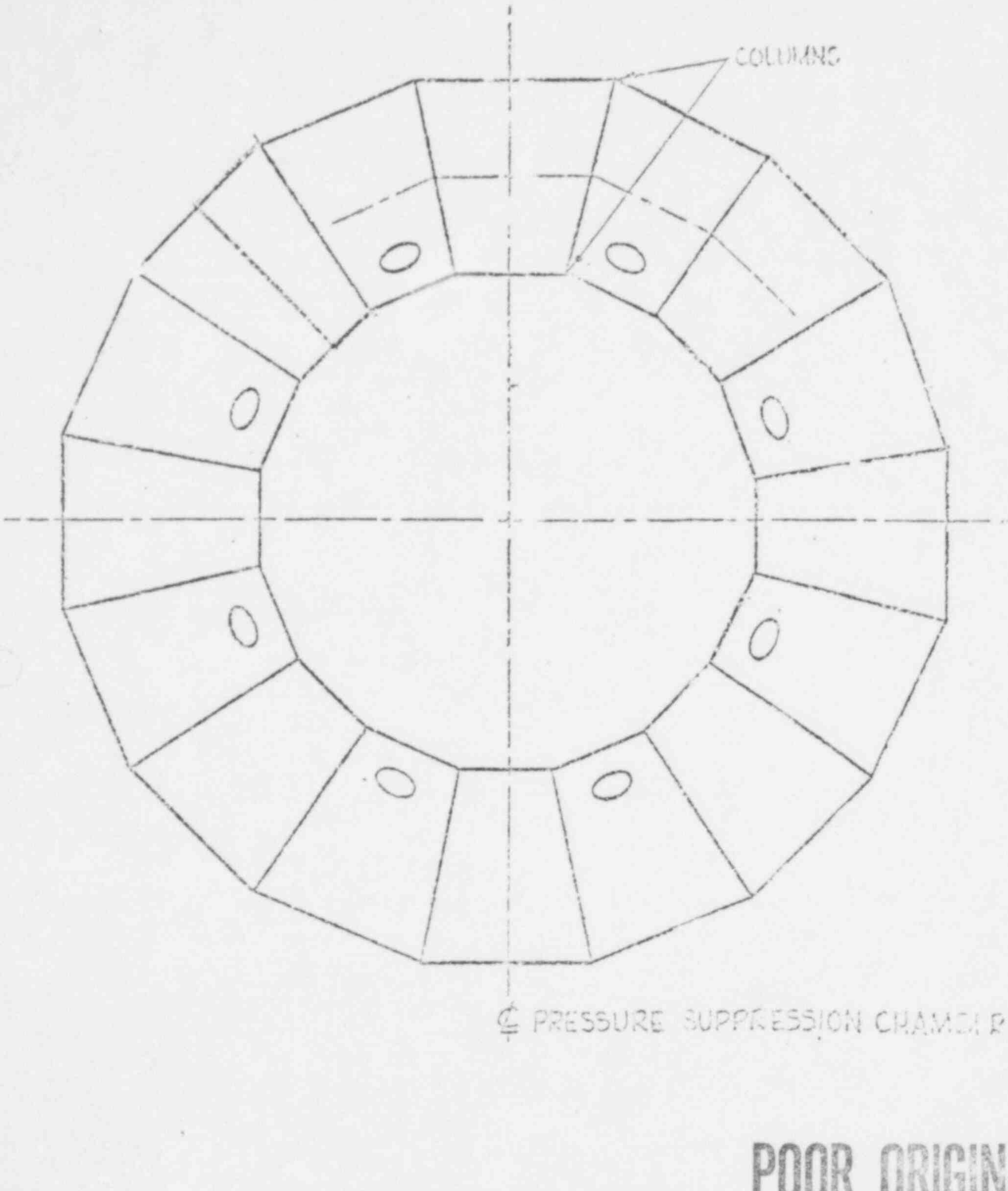


POOR ORIGINAL

FIGURE 2.0-2 PLAN VIEW OF SUPPRESSION CHAMBER - SCHEMATIC

482 310

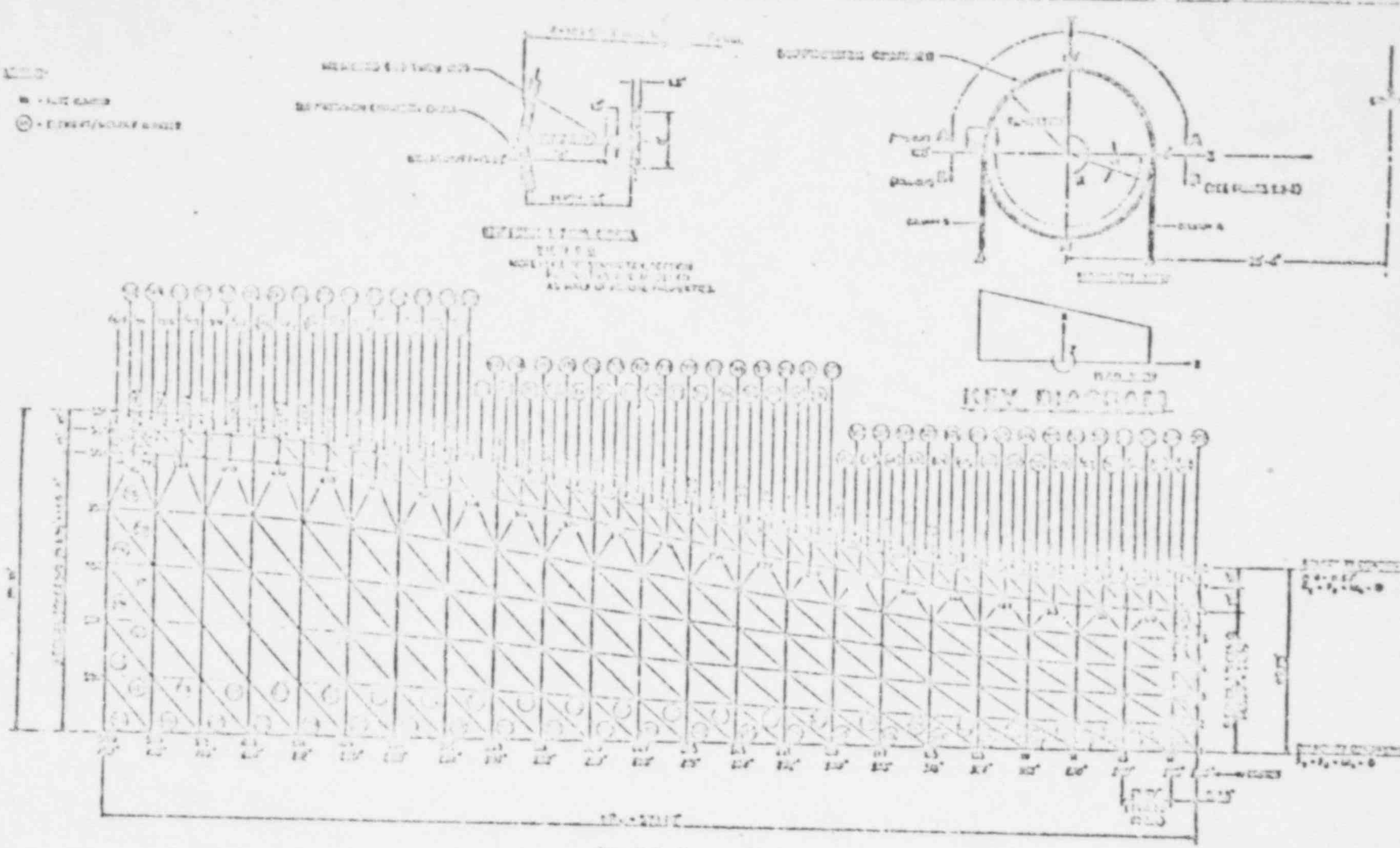
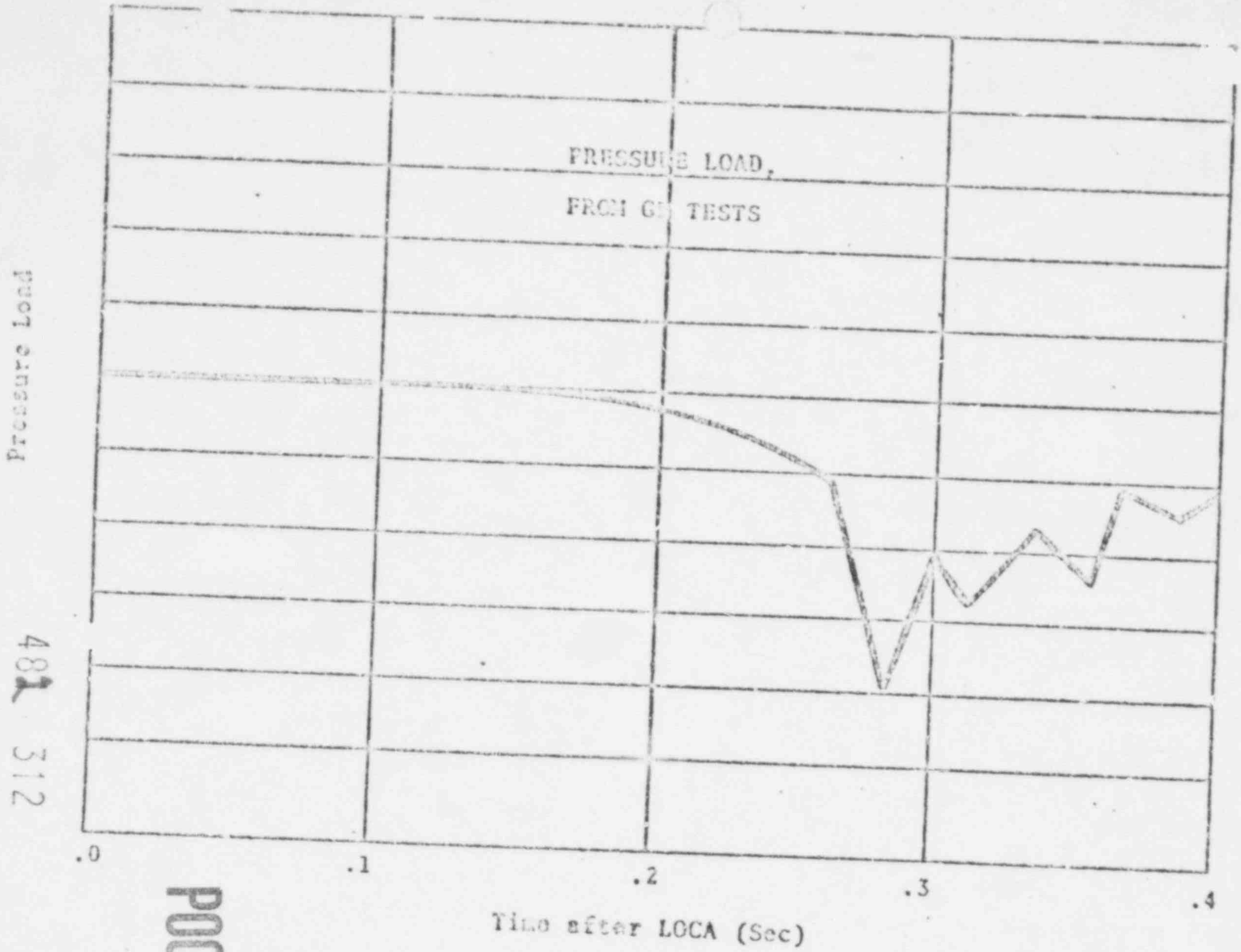


FIGURE 2.8-1 [illegible] SUPPRESSION CHAMBER (DEVELOPED)

nutech

60



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POOR ORIGINAL

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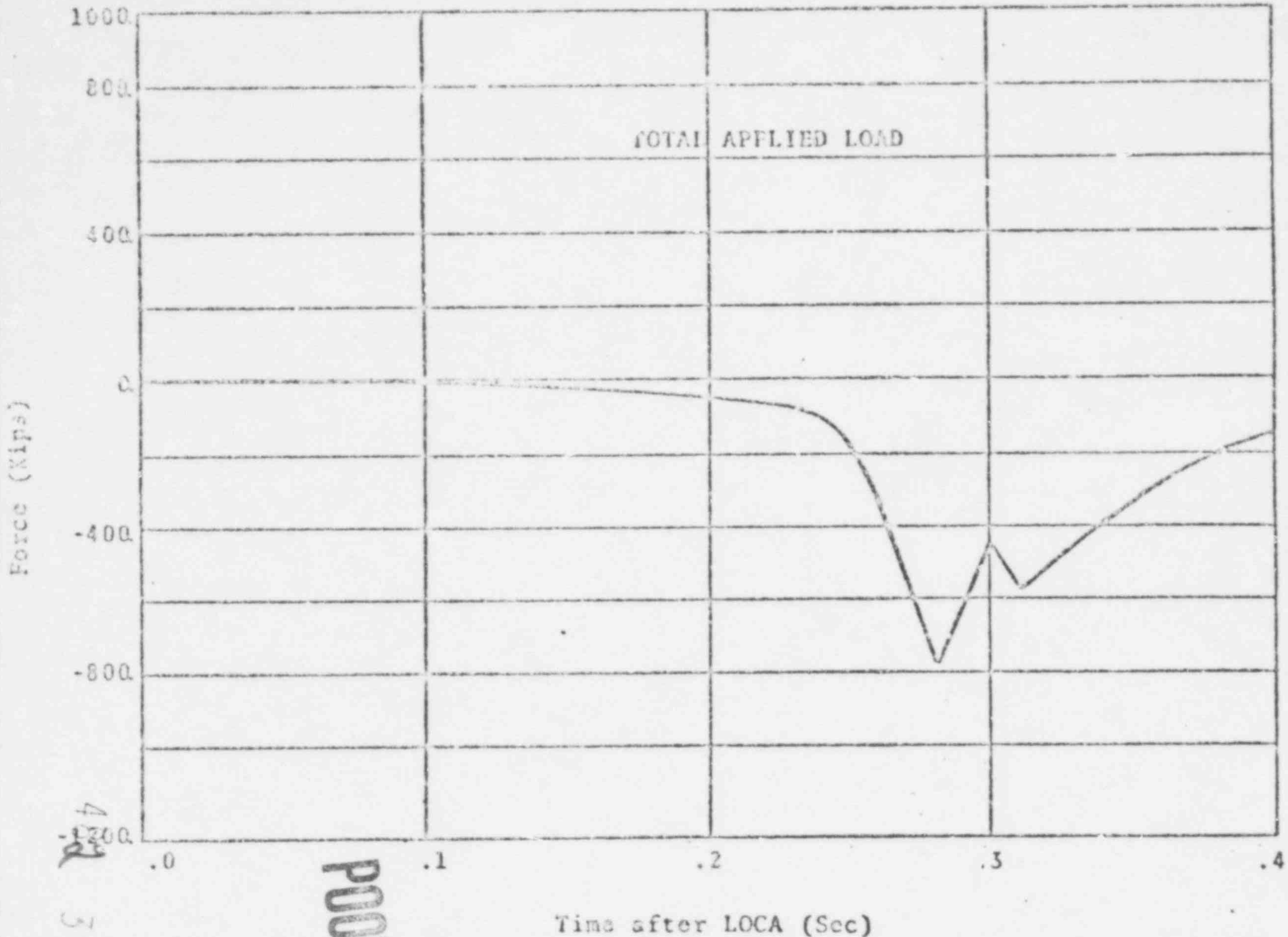
TOTAL APPLIED LOAD
1000.
800.
400.
0.
-400.
-800.
-1200.
.0
.1
.2
.3
.4
Force (Kips)
Time after LOCA (Sec)

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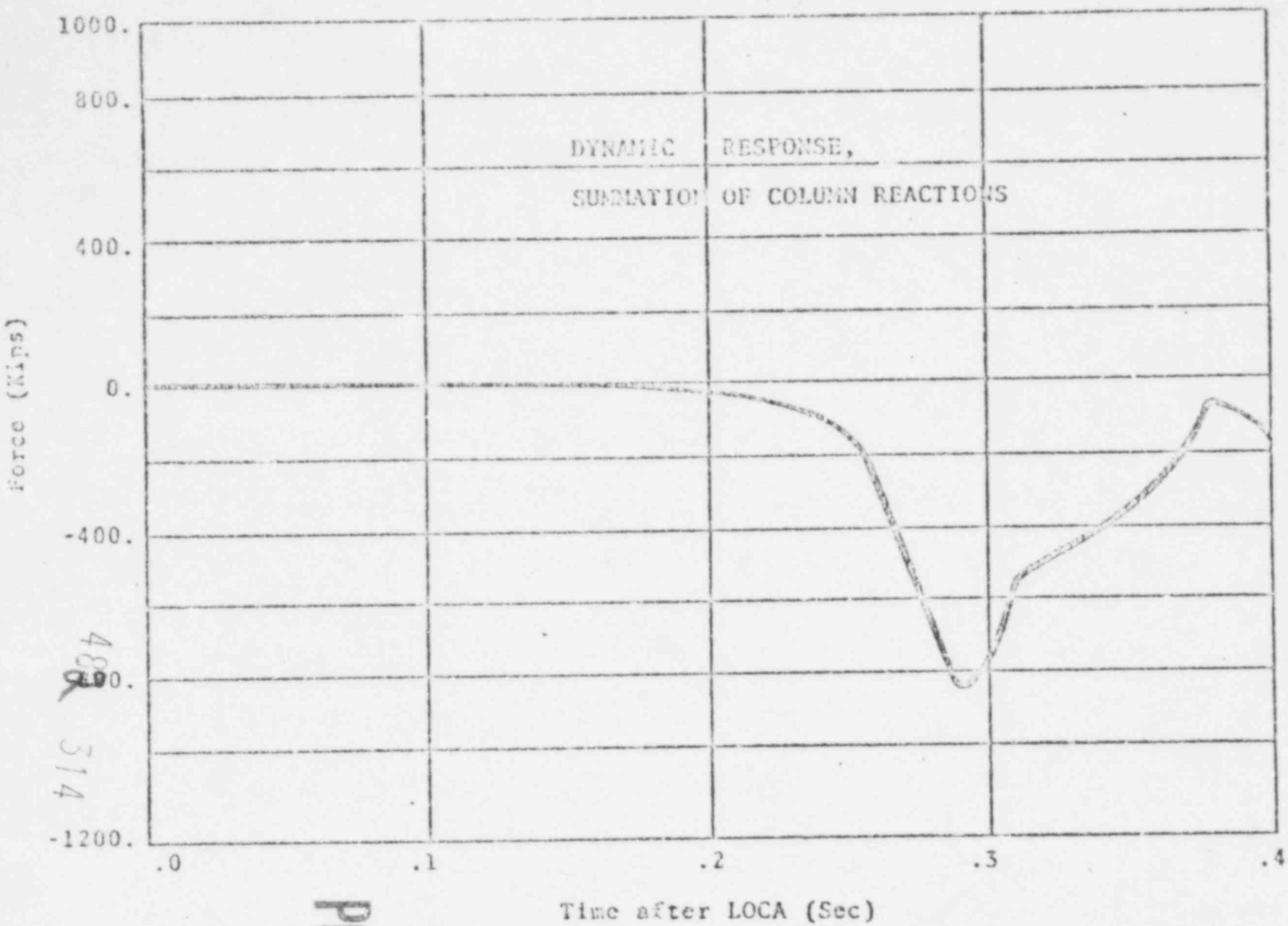


488 314

POOR ORIGINAL

If the pool swell impulse on a pipe is expressed as a parabolic pressure-vs.-time curve, the maximum pressure, P_{max}, is

$$P_{max} = C\left[1 - \frac{V_f}{V_o}\right]^2 V_o^2 \qquad \text{(Vol. V, p. 13)}$$

where:

V_o = pool velocity at impact.

V_f = pipe velocity after impact

C = a numerical quantity depending upon dimensions, densities, etc.

For the GE tests, $V_f \doteq 0$ and

$$\left[P_{max}\right]_{GE} = C V_o^2$$

If $V_f \neq 0$,

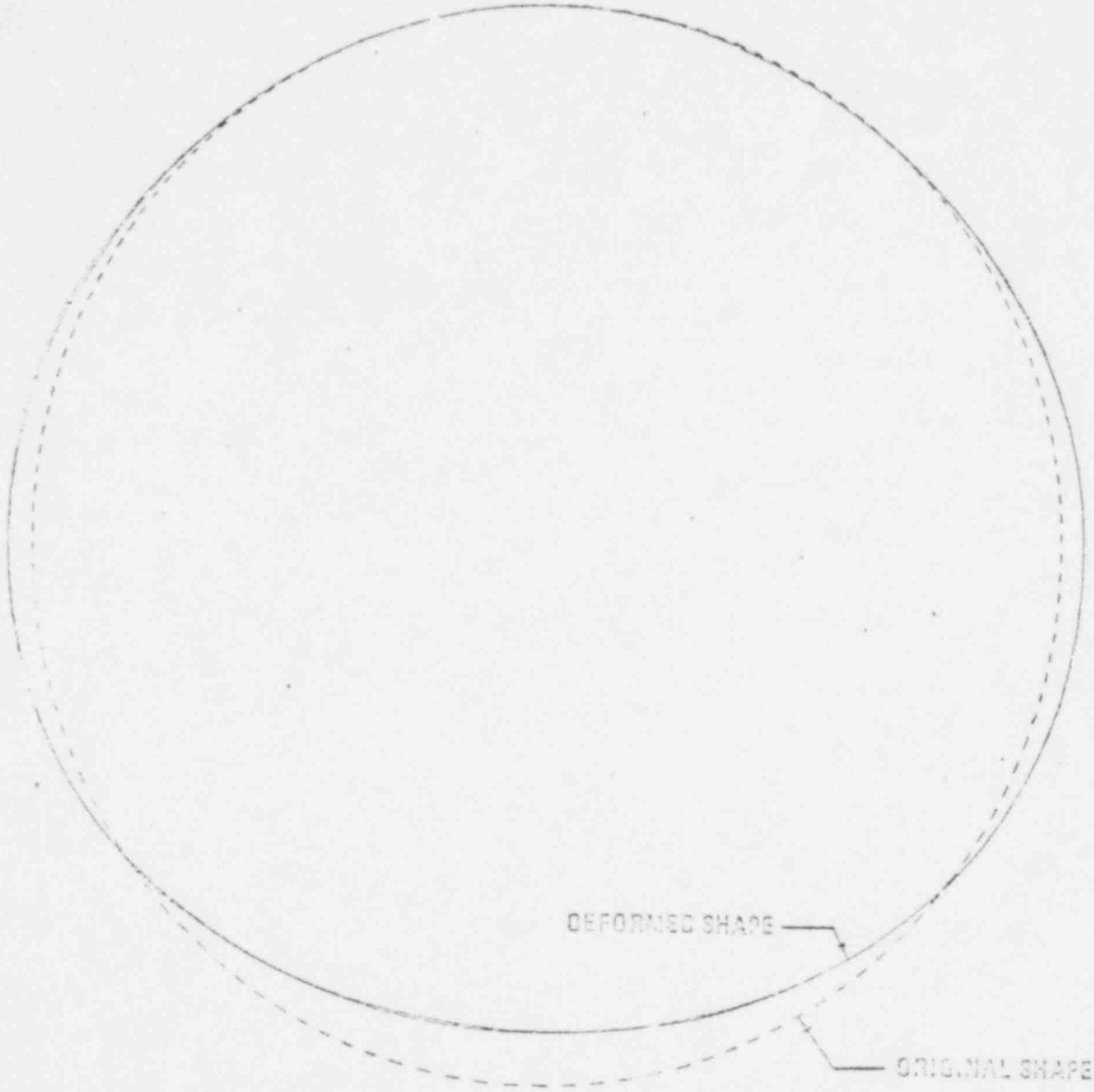
$$\frac{\left[P_{max}\right]_{V_f}}{\left[P_{max}\right]_{GE}} = \left[1 - \frac{V_f}{V_o}\right]^2 = \rho$$

For the tests, if the ends had not been supported $V_f \doteq 0.4 V_o$ and $\rho = 1/3$, even with the thick pipe used.

With pipe thickness scaled, as was pipe diameter, ovalization of the pipe produces local velocities which reduce the local peak pressure.

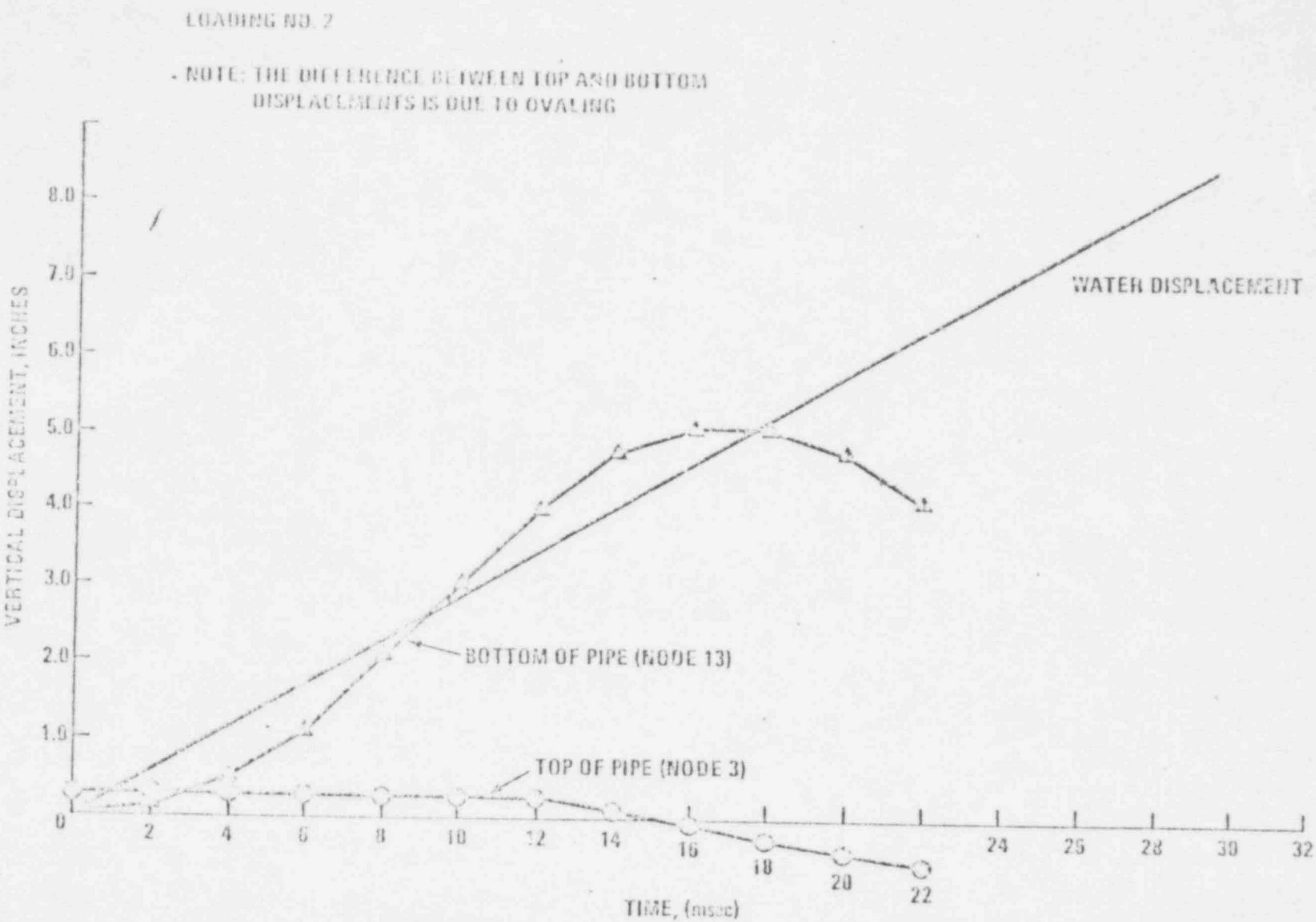
The finite element analysis reported in Vol. IV, Section 5.3.1 illustrates this behavior.

POOR ORIGINAL



RING HEADER DISPLACEMENT
DUE TO OVALING

FIGURE 5.6 - 6



DISPLACEMENT VS TIME AT RING HEADER CENTERLINE BETWEEN SUPPORTS

FIGURE 5.8 - 4

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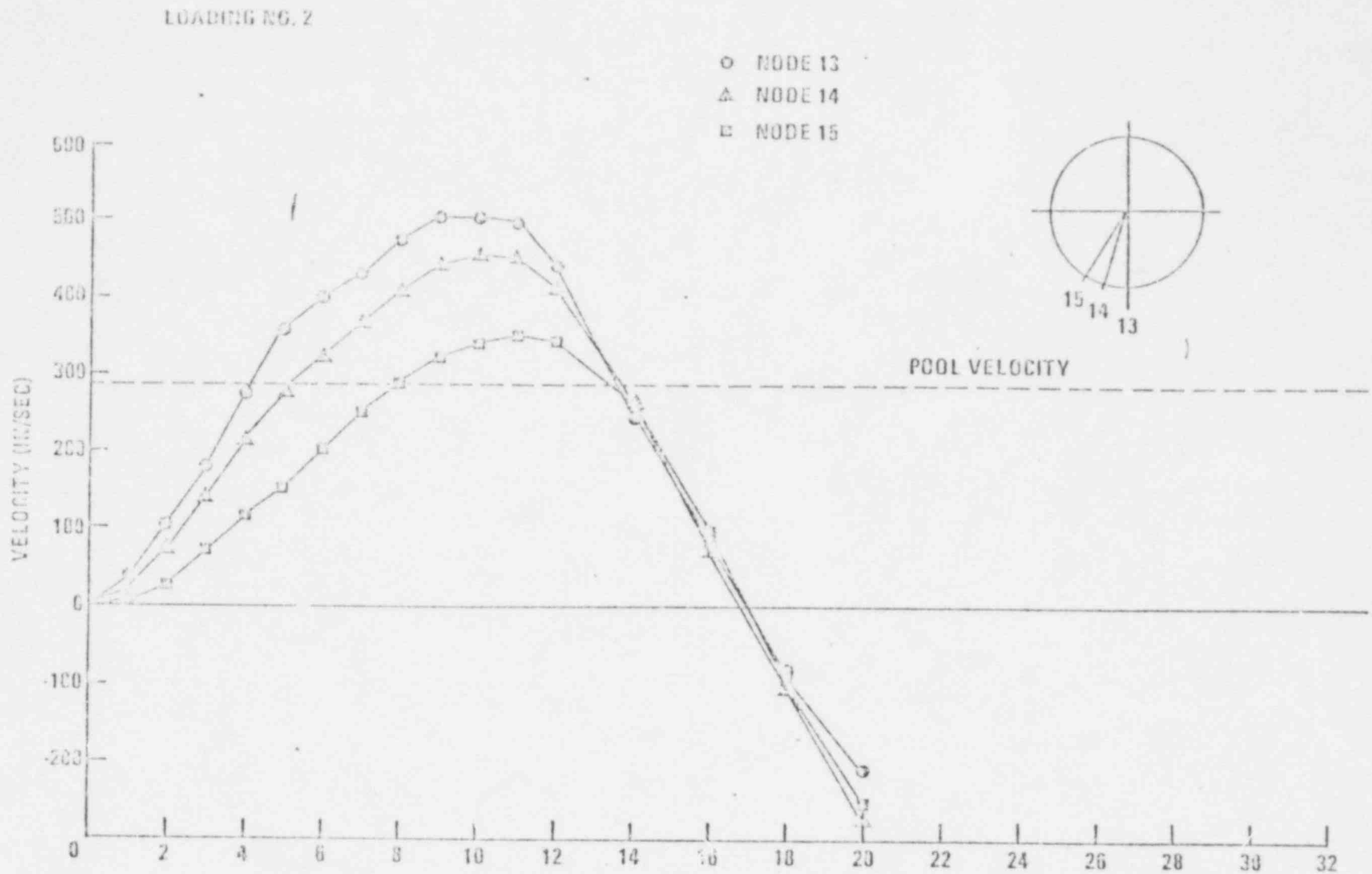

NODAL VELOCITY VS TIME AT CENTERLINE BETWEEN VENTS

FIGURE 5.3 - 7

POOR ORIGINAL

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6 8

MARK I-MARK II POOL DYNAMICS

ACRS WORKING GROUP 4

MARCH 25, 1976

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CONTAINMENTS

MARK I AND II CONTAINMENTS

A. MARK I EVALUATION PROGRAM

1. OBJECTIVE OF EVALUATION PROGRAM

2. SCHEDULE

B. STATUS OF MARK II EVALUATION PROGRAM

C. POOL DYNAMIC LOADS

D. STRUCTURAL ANALYSIS

PRESSURE SUPPRESSION TESTING

EROSION OF DESIGN MARGIN

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SUMMARY OF CONCERN

THE TESTIMONY STATES THAT THE CRITERIA AND ASSUMPTIONS WHICH HAVE BEEN ADOPTED TO EVALUATE THE ACCEPTABILITY OF CONTINUED MARK I PLANT OPERATION ARE NOT CONSISTENT WITH CURRENT STANDARDS AND ARE NONCONSERVATIVE.

SUMMARY OF RESPONSE

THE CRITERIA BEING USED TO EVALUATE CONTINUED OPERATIONS ARE DIFFERENT FROM CURRENT ACCEPTABLE STANDARDS BUT STILL PROVIDE REASONABLE ASSURANCE FOR PUBLIC HEALTH AND SAFETY BY VIRTUE OF:

A. INHERENT STRUCTURAL CAPABILITY TO MAINTAIN CONTAINMENT FUNCTION;

B. INCREASE IN STRUCTURAL MARGINS THROUGH MODIFICATIONS-WHEN IDENTIFIED.

C. LOW PROBABILITY OF THE ACCIDENT.

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OBJECTIVES
OF
MARK I CONTAINMENT EVALUATION PROGRAM

1. SHORT-TERM PROGRAM

DETERMINE AS QUICKLY AS POSSIBLE THE INHERENT STRUCTURAL CAPABILITY OF THE CONTAINMENT TO WITHSTAND POOL DYNAMIC LOADS.

A. IDENTIFY SIGNIFICANT LOADS.

B. PERFORM STRUCTURAL ANALYSIS TO EVALUATE CONTAINMENT CAPABILITY.

2. LONG-TERM PROGRAM

RESTORE THE PLANT THROUGH MODIFICATIONS, IF NECESSARY, TO RESTORE ORIGINAL DESIGN MARGINS.

A. CONFIRM LOADS THROUGH ADDITIONAL LARGE SCALE TESTS.

B. REANALYZE THE STRUCTURAL CAPABILITY USING CURRENT CRITERIA.

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MARK I EVALUATION PROGRAM

1. GE SHORT-TERM PROGRAM DOCUMENTATION COMPLETE	MAY 1976
2. PLANT UNIQUE ANALYSIS (STRUCTURAL)	JUNE 1976
3. NRC EVALUATION OF SHORT-TERM PROGRAM	JULY 1976
4. 1/4 SCALE TESTS	DEC. 1976
5. OTHER TEST	(NOT DEFINED AS YET.)

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MARK II CONTAINMENTS

1. ELEVEN PLANTS:

   ZIMMER } CURRENTLY UNDER REVIEW
   SHOREHAM }

   WPPS UNIT 2
   NINE MILE UNIT 2
   LASALLE 1 AND 2
   SUSQUEHANNA 1 AND 2
   LIMERICK 1 AND 2
   BAILLY

2. NEDO-21061, MARK II CONTAINMENT DYNAMIC FORCING FUNCTIONS INFORMATION REPUT UNDER REVIEW.

3. NEDE-21078, TEST RESULTS EMPLOYED BY G.E. FOR BWR CONTAINMENT AND VERTICAL VENT LOADS UNDER REVIEW.

4. POOL SWELL TESTS TO BE REPORTED (APRIL 1976).

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MARK I - MARK II POOL DYNAMICS

ACRS WORKING GROUP 4

MARCH 25, 1976

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MARK I POOL DYNAMICS

UPWARD-DOWNWARD LOADS ON TORUS SUPPORTS IDENTIFIED AS MOST SIGNIFICANT

. 1/12 SCALE TESTS
. VERMONT YANKEE

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GE 1/12 SCALE TESTS

NRC CONCLUSIONS

- SCALING IS REASONABLE
- SEGMENT TEST IS ACCEPTABLE FOR SHORT TERM
- NEED LARGER SCALE TEST TO CONFIRM SCALING
- NEED 3-D TEST

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UPWARD-DOWNWARD LOADS
GE 1/12 SCALE TESTS

CONSERVATISMS:

(1) FSAR DRYWELL PRESSURE RATE

- SUDDEN AND LARGE RUPTURE
- CONDENSATION IN DRYWELL

(2) AIR VENTING FROM DRYWELL TO POOL

UNCERTAINTIES

(1) NO ERROR ANALYSIS INCLUDED IN DATA APPLICATION

(2) METHOD OF EXTRAPOLATION OF TEST DATA TO INDIVIDUAL PLANTS

(3) LOAD MEASUREMENT METHODS

- INTEGRATION OF PRESSURE TRANSDUCER DATA
- VENT HEADER REACTION FROM PSTF

NRC CONCLUSION: LOADS ARE BEST ESTIMATE

POOR ORIGINAL

VERMONT YANKEE

UPWARD LOADS

ΔP (PSI)	POOL VOL. (FT^3)	UPWARD P (PSI)	DEFLECT. (IN
1.7	68,000	0.52	0.19
1.7	70,000	0.92	0.35

ALLOWABLE UPWARD PRESSURE = 1.5 PSI (1.0" DEFLECT.)

NRC CONCLUSION: LOADS ARE ACCEPTABLE BASED ON STRUCTURAL MARGIN FOR SHORT TERM

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PRIMARY POOL DYNAMIC LOADS
MARK II

1. POOL SWELL IMPACT AND DRAG
2. DOWNCOMER LATERAL LOADS

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TEST PROGRAMS

1. UPWARD - DOWNWARD TORUS LOADS
   - 1/12 SCALE SEGMENT (GE)
   - LARGER SCALE SEGMENT (GE)
   - NRC - LIVERMORE
2. OTHER PRESSURE SUPPRESSION PHENOMENA
   - 4T (MARK II)
   - PSTF (MARK III)
   - NRC - LIVERMORE
   - MARVIKEN
   - KWU
   - LOFT

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ANALYTICAL PROGRAMS

1. GENERAL ELECTRIC
   - . SIMPLE SLUG MODELS EXIST FOR POOL SWELL
   - . MORE SOPHISTICATED MODEL TO INCLUDE BREAKTHROUH AND FROTH CONDITIONS AS PART OF MARK II PROGRAM

2. NRC

   A. LIVERMORE-POOL DYNAMIC MODEL
   - . PRESENTLY 2-D, AIR, ONE VENT
   - . 2-D, AIR-STEAM, MULTIPLE VENTS - 10/77
   - . EVENTUALLY 3-D
   - . APPLICABLE TO BOTH LOCA AND SRV

   B. LASL
   - . THEORETICAL AND CORRELATIVE RELATIONS FOR MASS AND ENERGY TRANSFER (CONDENSATION)
   - . INPUT TO LIVERMORE CODE

   C. UCLA
   - . LABORATORY SCALE TESTS FOR BASELINE DATA FOR CODE CHECKOUT

8?

3/25/76

STRUCTURAL / MECHANICAL

SUMMARY PRESENTATION OF

MARCH 2, 1976 TESTIMONY TO

JOINT COMMITTEE ON ATOMIC ENERGY

ON

MARK I AND MARK II

CONTAINMENT

POOL DYNAMICS

TO

ADVISORY COMMITTEE ON REACTOR SAFETY

WORKING GROUP #4

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A. SUMMARY OF JCAE TESTIMONY

B. STRUCTURAL ANALYSES

1. MARK I

a. INTERNALS

b. TORUS SHELL AND RING GIRDERS

c. TORUS SUPPORTS

d. PIPING

2. MARK II

C. ACCEPTANCE CRITERIA

1. MARK I

2. MARK II

D. MODIFICATIONS TO DATE

1. MARK II AND MARK III LINERS

2. BROWN'S FERRY INTERNALS

3. VERMONT YANKEE TIEDOWNS

4. "Δp" DIFFERENTIAL PRESSURE MODIFICATIONS

POOR ORIGINAL

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SUMM[illegible] OF MARCH 2, 1976 TESTIMONY TO JCAE

NO.	POOL DYNAMICS STRUCTURAL CONCERNS	SUMMARY OF NRC STAFF RESPONSE
1.	TORUS THICKNESS - OYSTER CREEK & NINE MILE POINT I	TORUS DESIGN P=35 PSIG - OTHERS, P=56 PSIG FOR TESTING CONVENIENCE - REQUIRED P≈ 25 PSIG
2.	LOAD COMBINATIONS NOT COMPLETE	MARK II & III EQUVALENT TO CURRENT CODE - MARK I SHORT TERM = GOVERNING COMBINATIONS (W/SEISMIC) - MARK I LONG TERM = CODE
3.	SRV DISCHARGE NOT CONSIDERED WITH POOL SWELL	IN MARK II & III MANY SRV DISCHARGES COMBINED - MARK I SHORT TERM NO MECHANISTIC COMBINATION - MARK I LONG TERM, SRV/POOL SWELL COMBINATION SIMILAR TO MARK II & III
4.	NOMINAL SEISMIC ACCELERATION USED	0.15g ENCOMPASSES CURRENT DOCKET ACC. ON ALL MARK I's
5.	MARK I's DO NOT SATISFY CODE & DESIGN MARGINS ARE ERODED	TRUE, BUT FOR LONG TERM, CODE WILL BE SATISFIED - FOR SHORT TERM, MARGINS ARE EVALUATED SO THAT NO LOSS OF CONTAINMENT FUNCTION WITH AT LEAST FACTORS OF SAFETY OF TWO(2) - THE ADDITIONAL CAPABILITY OF STRUCTURES TO SUSTAIN LOADS EVEN WITH LIMITED YEILDING WHICH IS SHORT OF FAILURE IS EVALUATED - REASONABLE ASSURANCE FOR PUBLIC HEALTH & SAFETY BY RESERVE STRUCTURAL CAPACITY OR MODIFICATIONS - "Δp" MODIFICATION TO GAIN MARGIN
6.	MARK I STRAIN LIMITS SPECIFIED TO DISGUISE NONCONFORMANCE WITH CODE	BEYOND YIELD, STRAIN MUST BE SPECIFIED TO ASSESS SAFETY MARGINS

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SUMMARY (CONT.)

NO.	POOL DYNAMICS STRUCTURAL CONCERNS	SUMMARY OF NRC STAFF RESPONSE
7.	ANALYTICAL MODELS REFINED IF RESULTS UNFAVORABLE	TRUE, STANDARD STRUCTURAL PRACTICE REFINES SUBSEQUENT ANALYSES (I.R. ELASTIC TO ELASTO-PLASTIC) TO ASSESS TRUE MARGINS
8.	LOSS OF TORUS WATER MAY OCCUR OR ECCS PIPING MAY LOSE FUNCTION	TORUS SHELL FACTOR OF SAFETY TO LEAKAGE > 2 - ECCS PIPING ANALYZED FOR TORUS UPLIFT $\geq$ 0.2 "→ 1" TO CODE
9.	SEISMIC SLOSH OR OTHER LOADS MAY UNCOVER VENTS	NATURAL FREQUENCIES OF POOL ARE LOW - NO APPARENT LOW FREQUENCY EXCITING FORCE - MARK I LONG TERM WILL STUDY
10.	NO COMPETENT STRUCTURAL CONSULTANT WOULD TESTIFY THAT MARK I's ARE SAFE	DR. WILLIAM COOPER OF "TELEDYNE" (TESTIMONY AND LETTER) - MR. ROBERT KEEVER OF "NUTECH"

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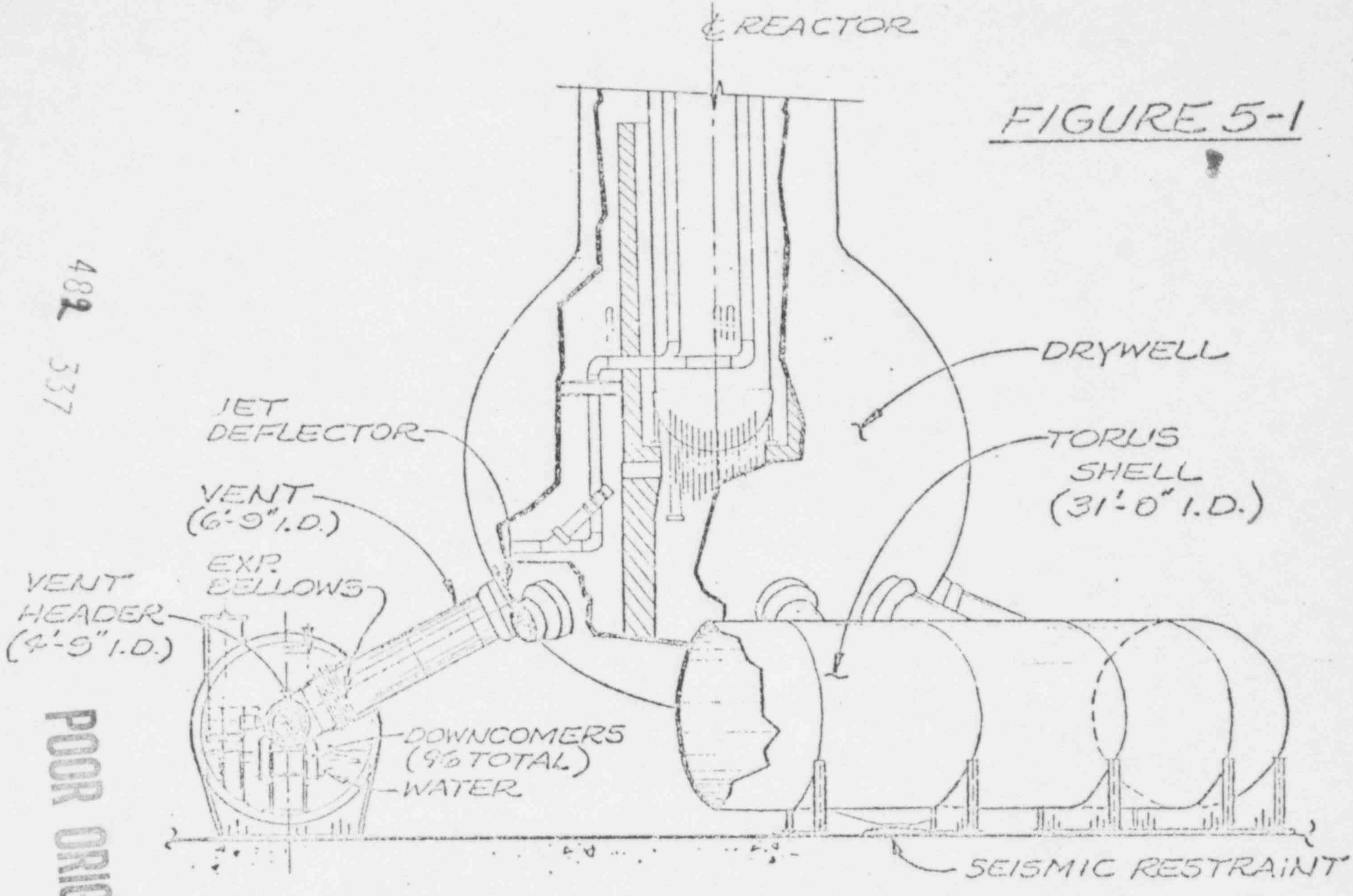
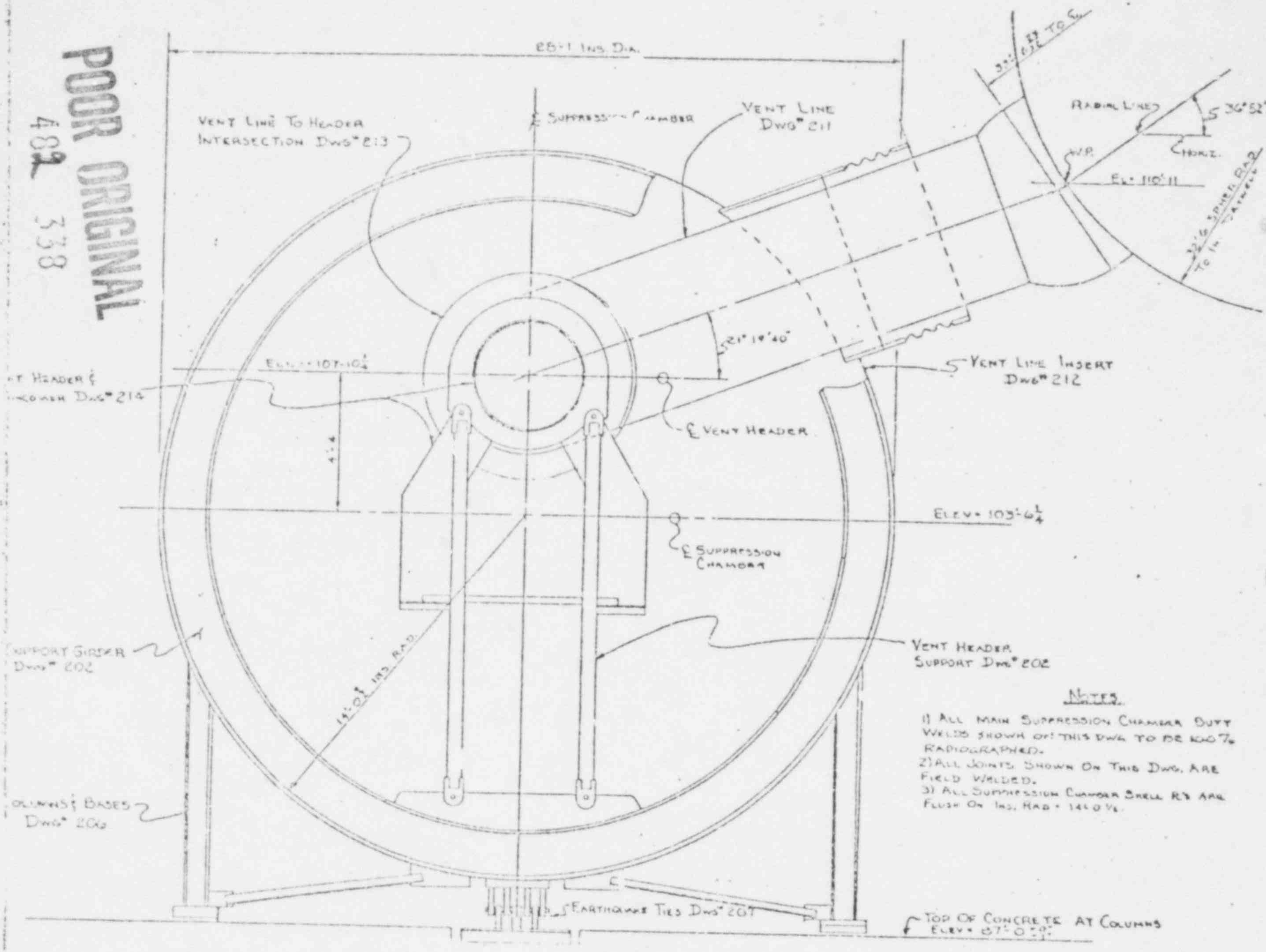
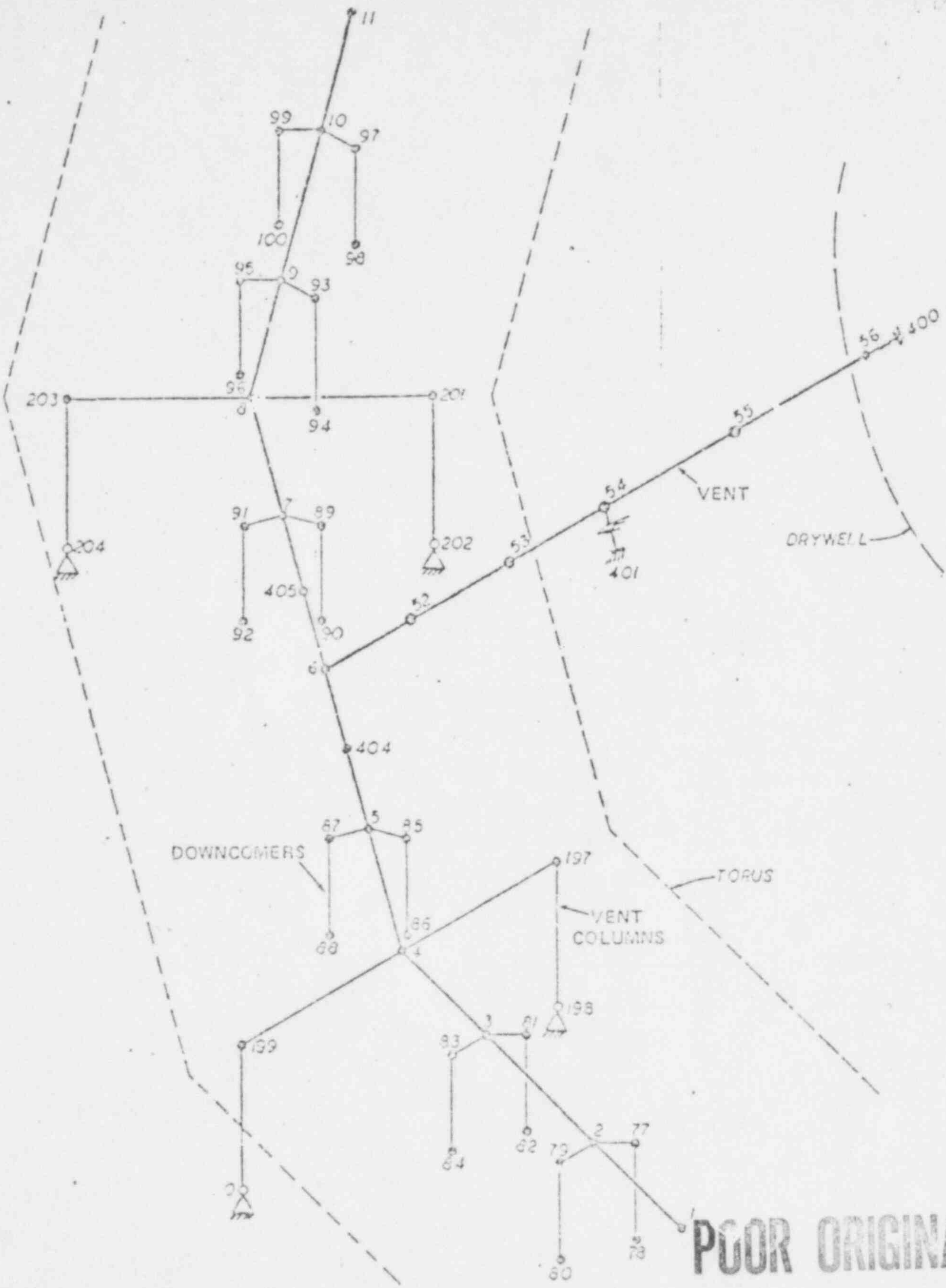
FIGURE 5-1



MARK I PRESSURE SUPPRESSION CONTAINMENT SYSTEM

482 337

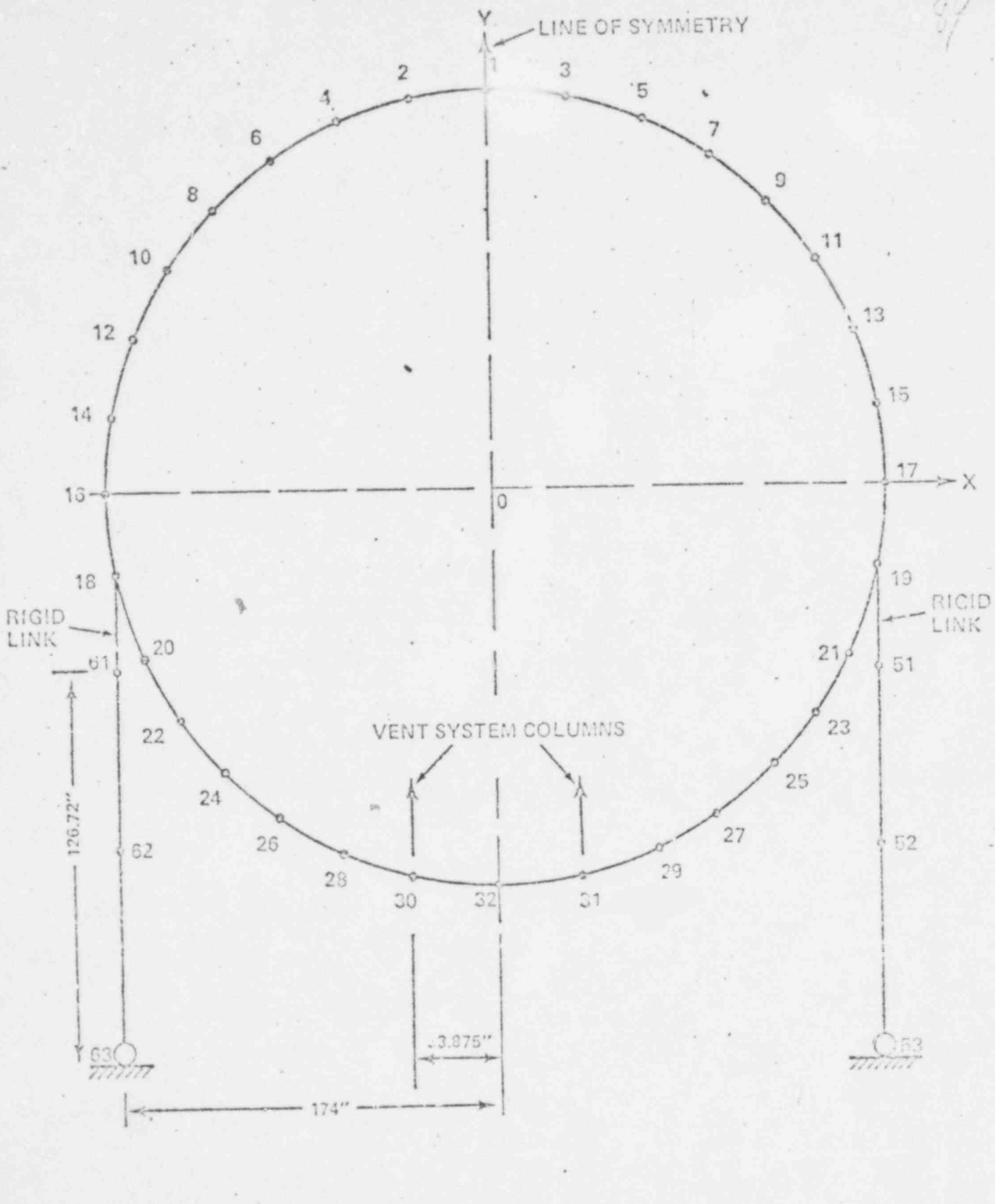

28'-1 INS. DIA.
Vent Line To Header Intersection Dwg# 213
℄ Suppression Chamber
Vent Line Dwg# 211
Radial Lines
36°52'53"
Horiz.
W.P.
El. 110'-11
21°19'40"
Elev. 107'-10¼
...t Header & ...cower Dwg# 214
Vent Line Insert Dwg# 212
℄ Vent Header
4'-4
Elev. 103'-6¼
℄ Suppression Chamber
...upport Girder Dwg# 202
14'-0⅝ Ins. Rad.
Vent Header Support Dwg# 202
...olumns & Bases Dwg# 206
Earthquake Ties Dwg# 207
Top Of Concrete At Columns Elev. 87'-0
Notes.
1) All Main Suppression Chamber Butt Welds Shown On This Dwg. To Be 100% Radiographed.
2) All Joints Shown On This Dwg. Are Field Welded.
3) All Suppression Chamber Shell R's Are Flush On Ins. Rad. = 14'-0½.



POOR ORIGINAL

PEACH BOTTOM
MATHEMATICAL MODEL

FIGURE [illegible]



TORUS RING GIRDER MODEL

POOR ORIGINAL 482 340

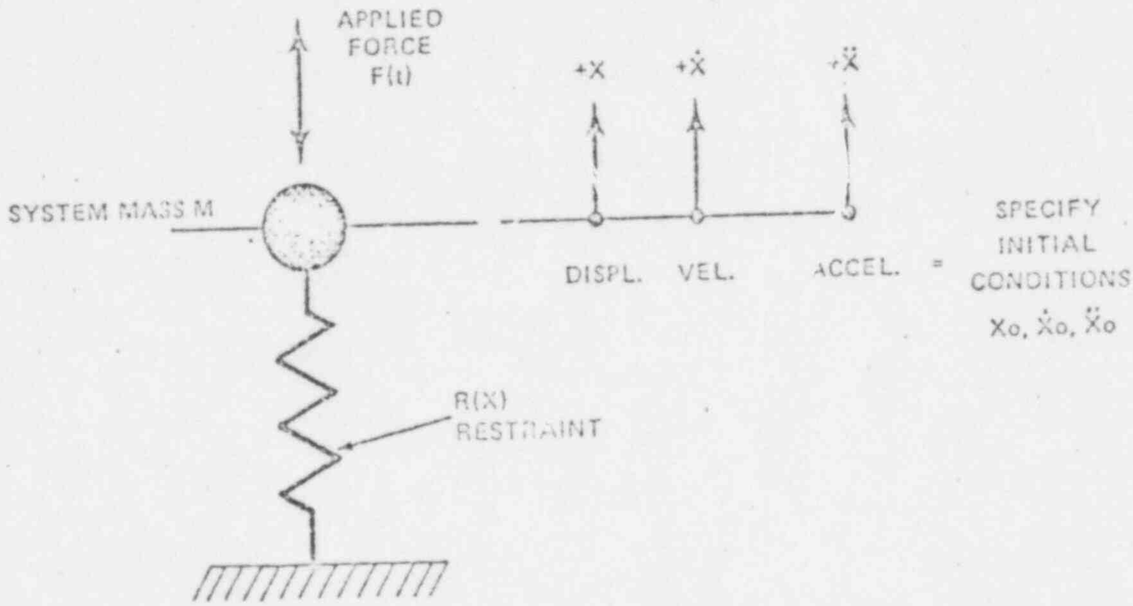


FIGURE 1

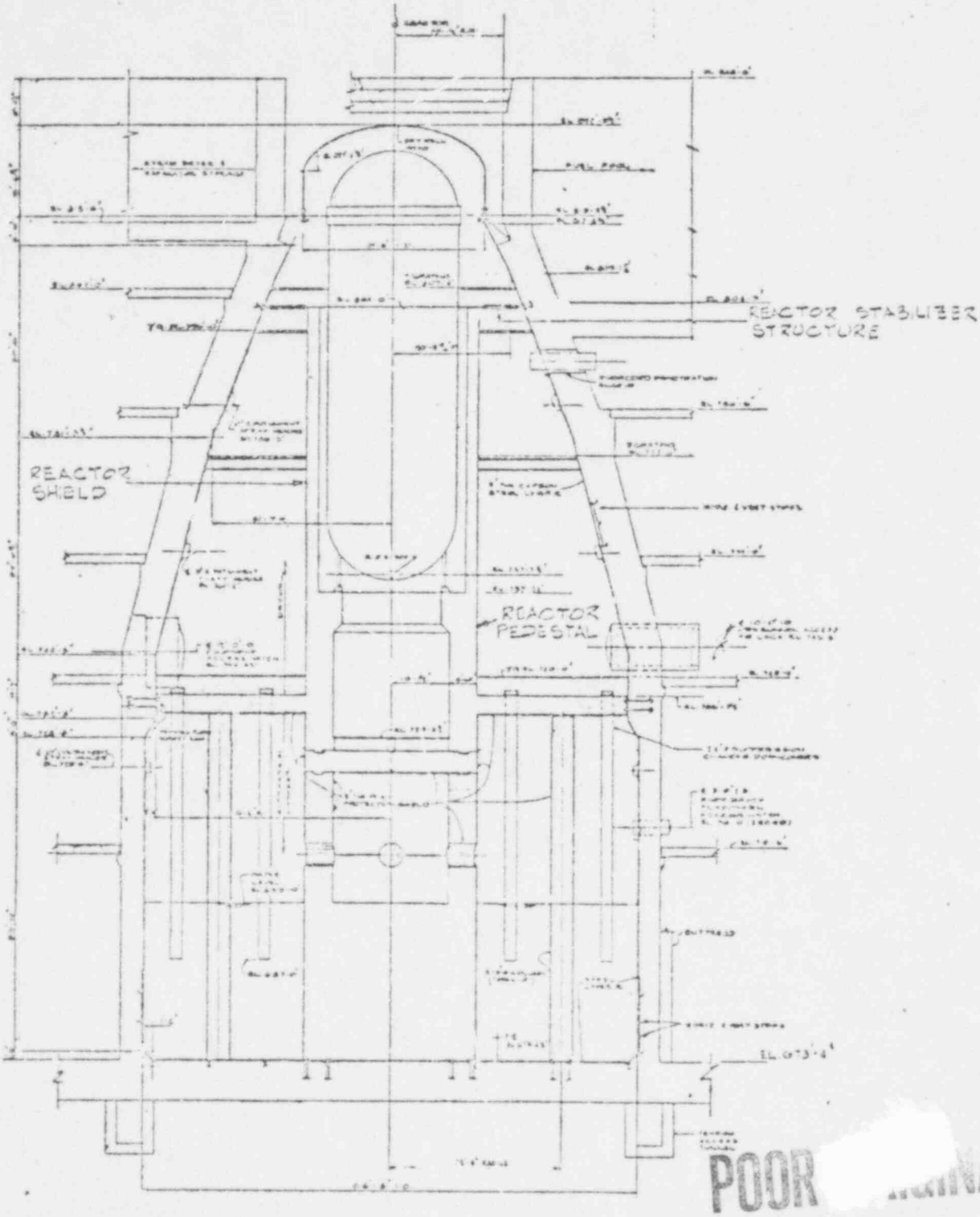
SINGLE MASS SYSTEM PARAMETERS

PENETRATION					ATTACHED PIPE				MAXIMUM STRESS DUE TO 1.0 IN DEFLECTION
DESIGNATION	QUANTITY	SIZE	SLEEVE THICKNESS	FUNCTION	SIZE	SCHEDULE OR THICKNESS	MATERIAL	REMARKS	
X 205	1	20 in	1.031 in.	Vacuum Relief From Building	20 in	1.031"	CS-1	*at .6" =26000 psi	43300*
X 208 A X 208 B X 208 C X 208 D	4	10 in	.594 in.	Electromatic Relief Valve Discharge	10 in.	STD	CS-2		9236 13529 14940 9522
X 210 A X 210 B	2	16 in	.844	RHR and Core Spray Test Lines	12 in.	STD	CS-1	12 x 16 reducer	13566 3903
X 211 A X 211 B	2	6 in	.432	RHR to Torus Spray Header	4 in.	STD	CS-1	4 x 6 reducer	13566 390[illegible]
X 212	1	9 in	.500	RCIC Turbine Exhaust	8 in.	STD	CS-1		20673
X 218	1	18 in	.938	Torus Purge Vent Outlet	18 in.	STD	CS-1		11883
X 221	1	24 in	1.219	HPCI Turbine Exhaust	24 in.	STD	CS-1		5478
X 222	1	2 in	.218	Condensate From HPCI Turbine Drain Pot	2 in.	80			<15000
X 223	1	2 in	.218	Condensate from RCIC Turbine Drain Pot	2 in	80	CS-1		1034
X 224A X 224B	2	24 in	.375	RHR Pump Suction	26 in	.375	CS-1	24x26 reducer	4312 6904
X 225	1	24 in	.375	HPCI Pump Suction	16 in	STD	CS-1	16x24 reducer	2939
X 226A X 227B	2	16 in	.375	Core Spray Pump Suction	12 in	STD	CS-1	12x16 reducer	6826
X 227	1	6 in	.375	RCIC Pump Suction	6 in	STD	CS-1		9200

489 342

POOR ORIGINAL

92



LA SALLE COUNTY STATION

MARK II DESIGN ASSESSMENT REPORT

FIGURE 1.1-1

PRIMARY CONTAINMENT

48_ 343

9 5

MARK I

ACCEPTANCE CRITERIA

A. SHORT TERM

   a. NO LOSS OF FUNCTION

   b. MINIMUM FACTOR OF SAFETY OF TWO (2)

B. LONG TERM - EQUIVALENT TO CODE MARGINS OR CODE IF APPLICABLE

TABLE 4.1-1

DESIGN LOAD COMBINATIONS

EQN	LOAD COND	D	L	F	P_O	T_O	R_O	E_O	E_{SS}	P_B	P_A	T_A	R_A	R_R	SRV	ADS	ALL	ASYMMETRICAL
1	Normal w/o Temp	1.4	1.7	1.0	1.0	-	-	-	-	-	-	-	-	-	1.5	0	X	X
2	Normal w/Temp	1.0	1.3	1.0	1.0	1.0	1.0	-	-	-	-	-	-	-	[illegible]	0	X	X
3	Normal Sev. Env.	1.0	1.0	1.0	1.0	1.0	1.0	1.25	-	-	-	-	-	-	1.25	0	X	X
4	Abnormal	1.0	1.0	1.0	-	-	-	-	-	1.25	-	1.0	1.0	-	1.25	X	0	X
4a		1.0	1.0	1.0	-	-	-	-	-	-	1.25	1.0	1.0	-	1.0*	0	0	0
5	Abnormal Sev. Env.	1.0	1.0	1.0	-	-	-	1.1	-	1.1	-	1.0	1.0	-	1.1	X	0	X
5a		1.0	1.0	1.0	-	-	-	1.1	-	-	1.1	1.0	1.0	-	1.0*	0	0	0
6	Normal Ext. Env.	1.0	1.0	1.0	1.0	1.0	1.0	-	1.0	-	-	-	-	-	1.0	0	X	X
7	Abnormal Ext. Env.	1.0	1.0	1.0	-	-	-	-	1.0	1.0	-	1.0	1.0	1.0	1.0	X	0	X
7a		1.0	1.0	1.0	-	-	-	-	1.0	-	1.0	1.0	1.0	1.	1.0*	0	0	0

LOAD DESCRIPTION

D = Dead Loads

L = Live Loads

F = Prestressing Loads

T_O = Operating Temperature Loads

R_O = Operating Pipe Reactions

P_V = Operating Pressure Loads

SRV = Safety/Relief Valve Loads

E_O = Operating Basis Earthquake

E_{SS} = Safe Shutdown Earthquake

P_B = SBA and IBA Pressure Load

T_A = Pipe Break Temperature Load

R_A = Pipe Break Temperature Reactions Loads

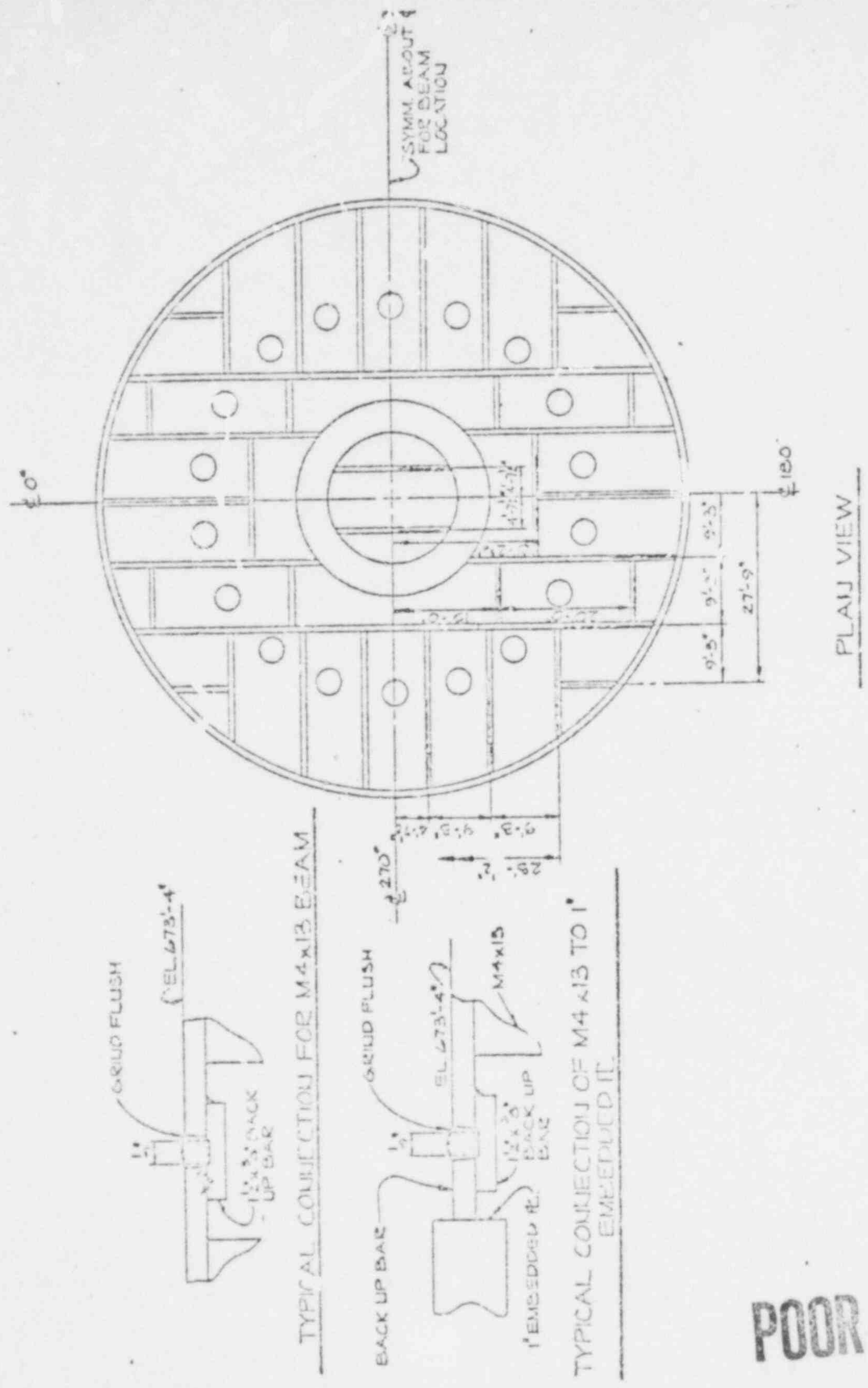
P_A = DBA Pressure Loads (including all pool hydrodynamic loadings)

R_R = Reactions and Jet Forces Due to Pipe Break

* ONE SRV WITH LOCA

489 345

POOR ORIGINAL



POOR ORIGINAL

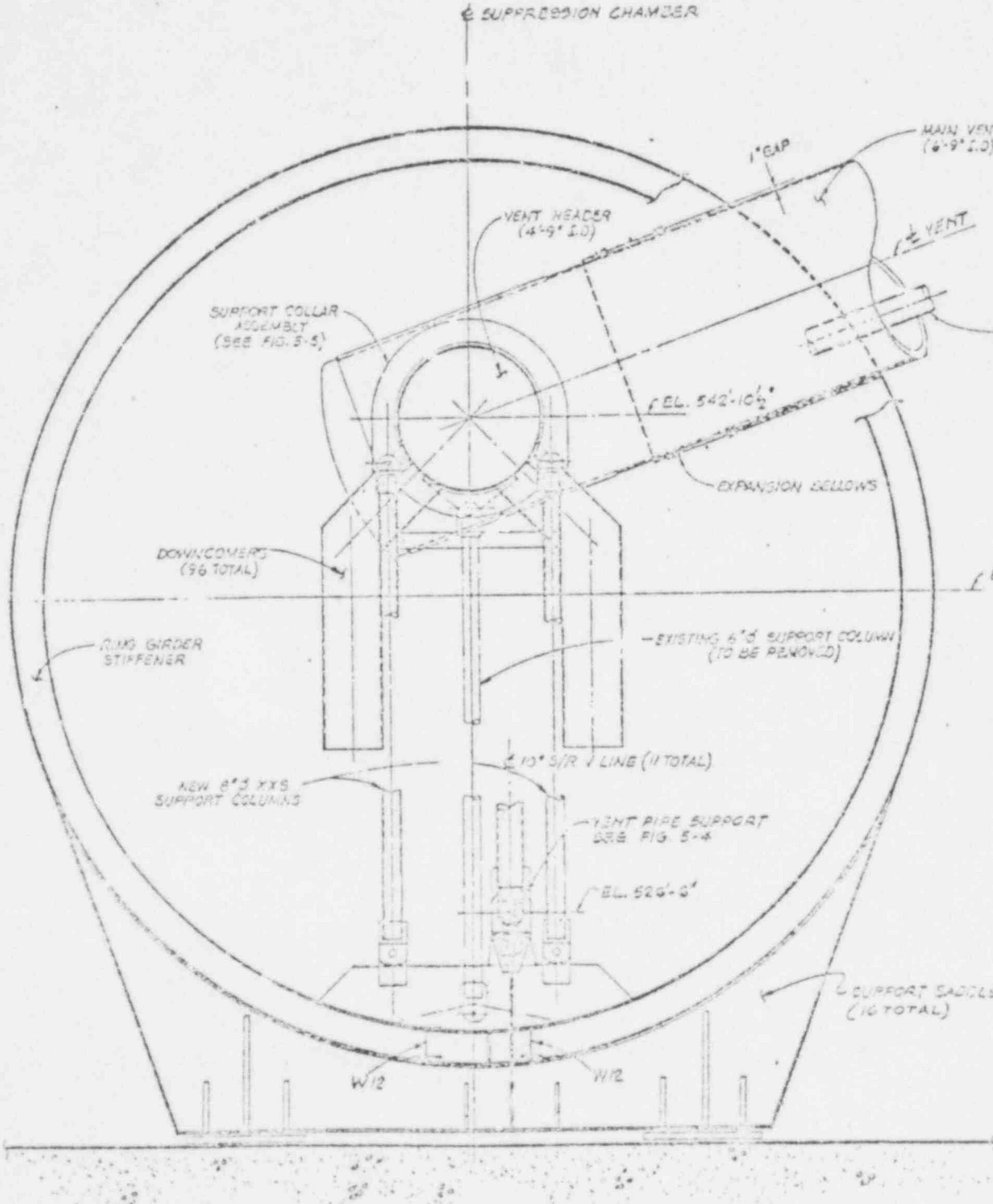
LA SALLE COUNTY STATION

MARK II DESIGN ASSESSMENT REPORT

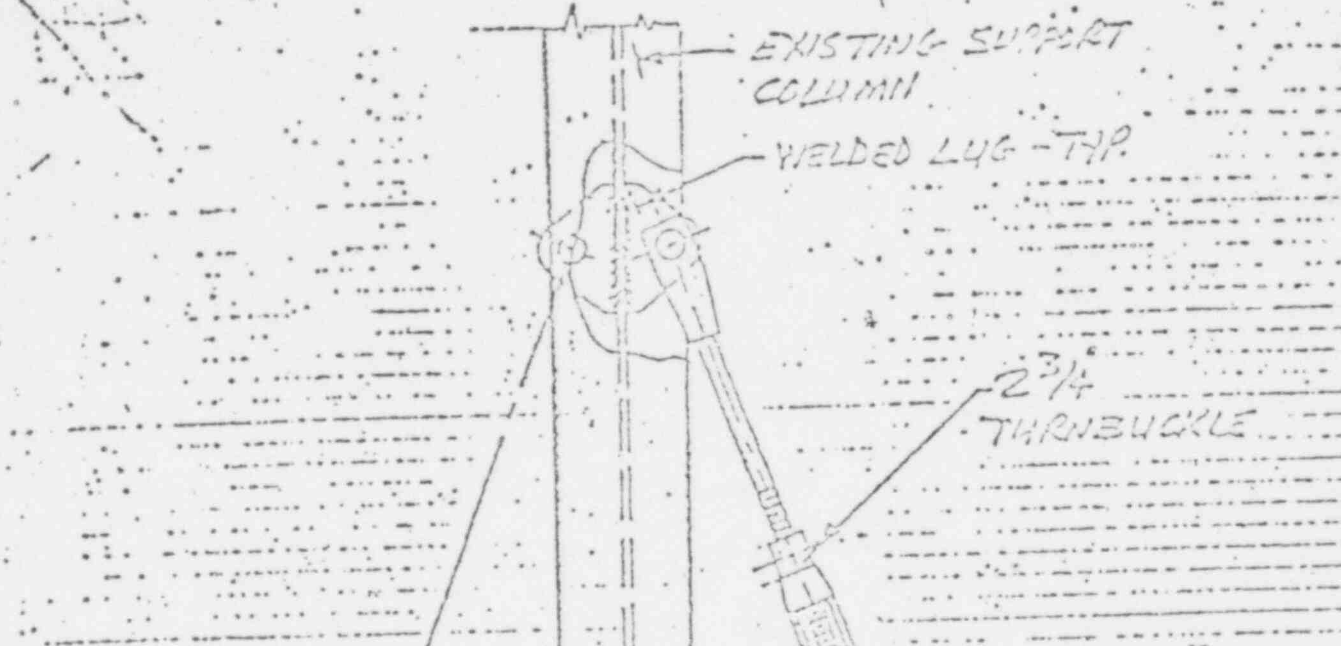
FIGURE 4.2-1

BASE MAT LINER DETAIL

482 346



TORUS CROSS-SECTION
FIGURE 5-2



PROPOSED COLUMN TIE-DOWN FIX

32 Columns

256 1 1/4" core drill

482 348

9 8

STAFF REPORT ON
EROSION OF DESIGN MARGINS
AT THE MARCH 25, 1976
ACRS SUBCOMMITTEE MEETING
WORKING GROUP NO. 4

(C. ANDERSON)

DESIGN MARGINS

"DESIGN MARGINS ARE CONTINUALLY ERODED BY MODIFICATIONS RESULTING FROM PROBLEMS THAT HAVE OCCURRED DURING PLANT OPERATION. A GOOD EXAMPLE IS THE REMOVAL OF BAFFLES FROM THE TORUS SEVERAL YEARS AGO. THE FIRST MARK I PLANTS WERE BUILT WITH ANTI-SLOSH BAFFLES INSTALLED IN THE TORUS. THE PURPOSE OF THESE BAFFLES WAS TO ENSURE THAT WAVES WOULD NOT BE BUILT UP IN THE POOL RESULTING IN GROSS DISRUPTION TO THE POOL SURFACE AND MAKING INEFFECTIVE THE DOWNCOMER SUBMERGENCE."

(FEB. 18, 1976, TESTIMONY)

100

EROSION OF DESIGN MARGIN

A. DESIGN MARGIN

MARGIN FOR MARK I CONTAINMENT

B. MARK I POOL BAFFLES

1. TYPICAL BAFFLE ARRANGEMENT
2. STATUS OF POOL BAFFLES IN MARK I PLANTS
3. ORIGINAL DESIGN BASIS
4. REMOVAL OF BAFFLES
5. REPLACEMENT OF BAFFLES

101

MARGIN FOR MARK I CONTAINMENT PRESSURE

PEAK CALCULATED PRESSURES

DRYWELL	WETWELL
41-58 PSI	20-29 PSI

DESIGN PRESSURE

DRYWELL	WETWELL
62 PSI	62 PSI (14 PLANTS)
	35 PSI (2 PLANTS)

482 352

102

STATUS OF POOL BAFFLES
MARK I PLANTS

BAFFLES PARTIALLY REMOVED

HUMBOLDT BAY

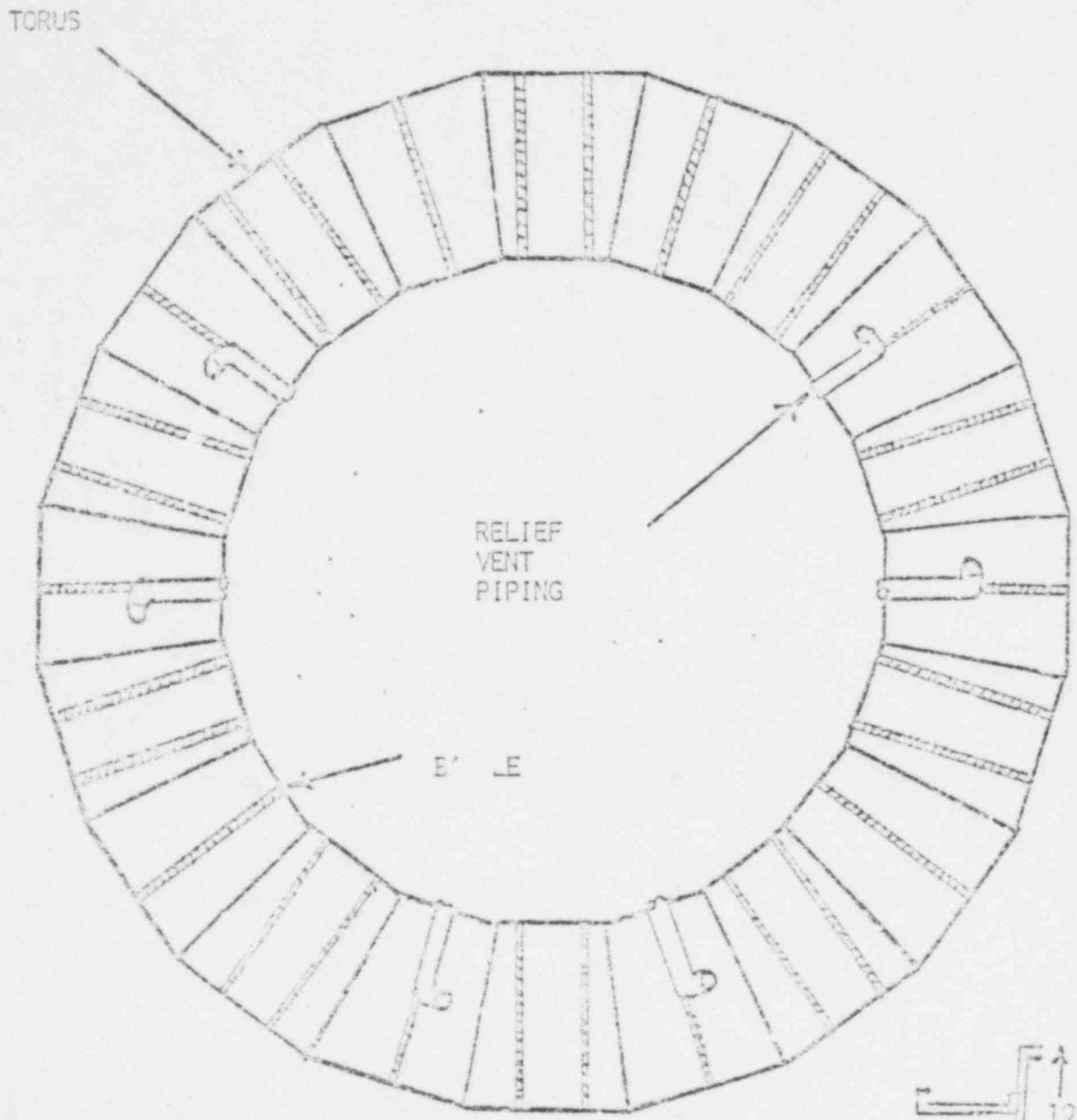
OYSTER CREEK

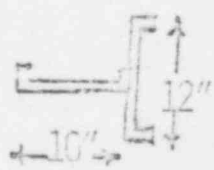
DRESDEN UNIT 2

BAFFLES COMPLETELY REMOVED

ALL REMAINING MARK I PLANTS

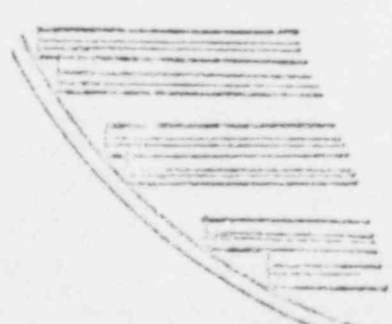
103





COMPONENT DETAIL

BAFFLE ARRANGEMENT IN TORUS



6 COMPONENT BAFFLE

104

DESIGN BASIS FOR MARK I BAFFLES

PRIMARY BASIS

BODEGA TESTS INDICATED THAT REMOVAL OF BAFFLES COULD LEAD TO POTENTIAL INCREASE IN THE WETWELL PRESSURE BEYOND THE PEAK PRESSURE DETERMINED BY AIR TRANSFER FROM DRYWELL TO WETWELL

SECONDARY CONSIDERATION

AZIMUTHAL SLOSHING - SLOSHING COULD RESULT IN WAVES WHICH MIGHT UNCOVER THE DOWNCOMER

REASON FOR
BAFFLE REMOVAL

RELIEF VALVE FORCES RESULTED IN DISPLACEMENT OF BAFFLES IN IMMEDIATE VICINITY OF RELIEF VALVE OUTLET PIPES

482 356

1?o

JUSTIFICATION FOR BAFFLE REMOVAL

PEAK WETWELL PRESSURE

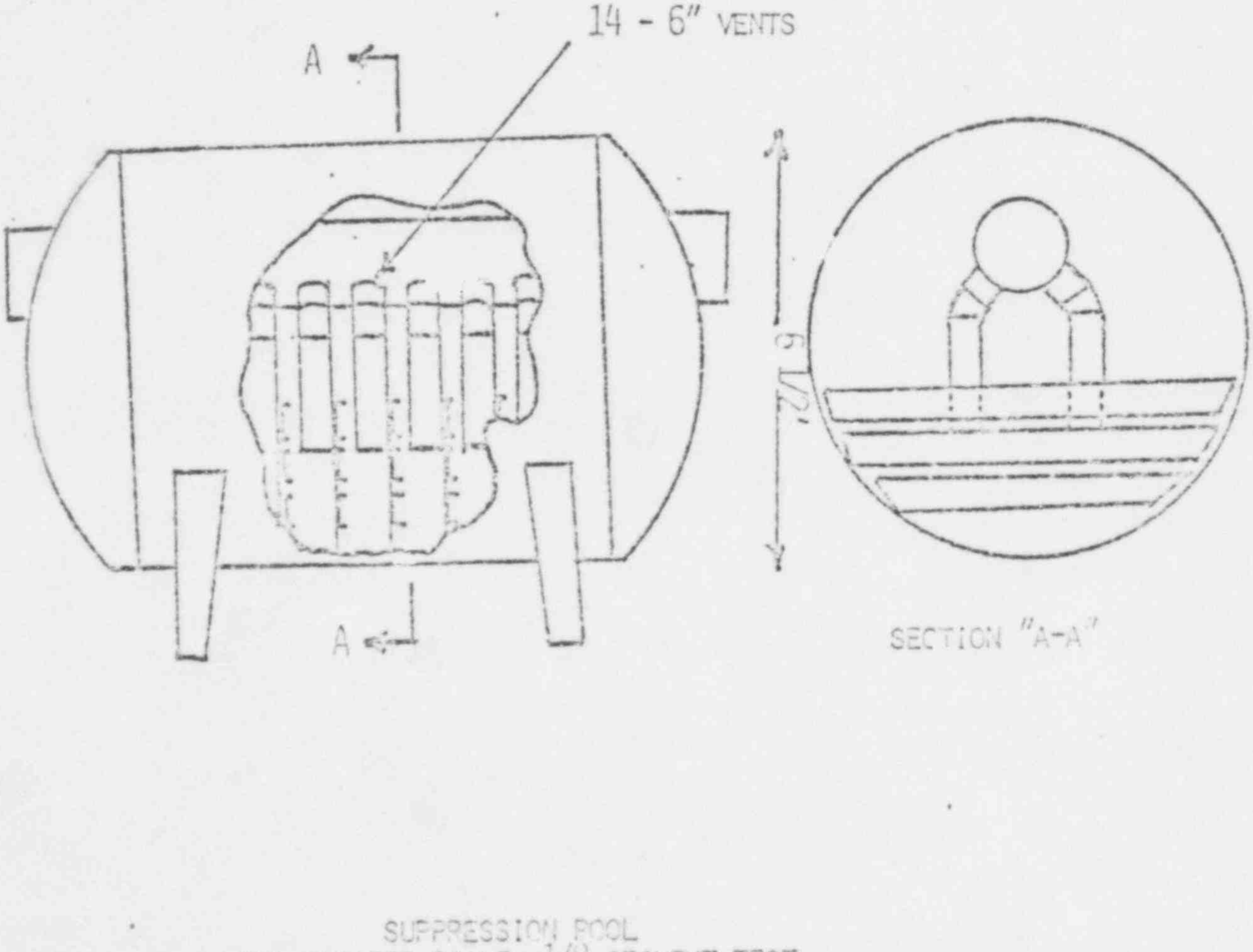
TESTS AND ANALYSES SHOW THAT REMOVAL OF BAFFLES WILL NOT RESULT IN AN INCREASE IN THE WETWELL PRESSURE ABOVE THE PEAK PRESSURE DETERMINED BY AIR TRANSFER FROM DRYWELL TO WETWELL

AZIMUTHAL SLOSHING

NO APPARENT MECHANISM FOR LARGE WAVES (LOW NATURAL FREQUENCY OF POOL)

HUMBOLDT AND BODEGA TESTS (GOOD CONDENSATION WITH VENTS UNCOVERED)

107



SUPPRESSION POOL
FOR QUARTER SCALE, 1/3 SEGMENT TEST

POOR ORIGINAL

482 358

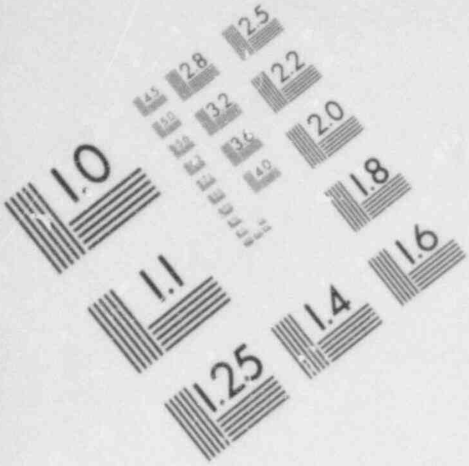
STAFF REPORT ON PRESSURE
SUPPRESSION TESTING AT THE
MARCH 25, 1976 ACRS
SUBCOMMITTEE MEETING
WORKING GROUP NO. 4

(C. ANDERSON)

PRESSURE-SUPPRESSION TESTING

"OTHER PROBLEMS THAT AFFECT THE PERFORMANCE OF THE PRIMARY CONTAINMENT ARE UNCERTAINTIES WITH REGARD TO THE TESTING OF THE PRESSURE-SUPPRESSION PHENOMENON. A LIMITED NUMBER OF TESTS WERE PERFORMED ON A SEGMENT OF THE CONTAINMENT THAT WAS PLANNED FOR THE BODEGA BAY PLANT (NEVER BUI' DEPTH OF DOWNCOMER SUBMERGENCE TESTING WAS VARIED, BUT MOST OF THE TESTS WERE PERFORMED AT ONLY ONE LEVEL OF SUBMERGENCE, FOUR FEET. WAVES GENERATED WITHIN THE POOL MAY UNCOVER THE VENTS; SEISMIC SLOSH MAY OCCUR WHICH MAKES PRESSURE SUPPRESSION INEFFECTIVE; AND SINCE THE LOSS-OF-CONTAINMENT ACCIDENT IS ALMOST IMPOSSIBLE TO MOCK-UP, COMPLETE VERIFICATION OF THE ADEQUACY OF THE SYSTEM TO WITHSTAND A LOCA HAS YET TO BE DEMONSTRATED."

(FEB. 18, 1976, TESTIMONY)



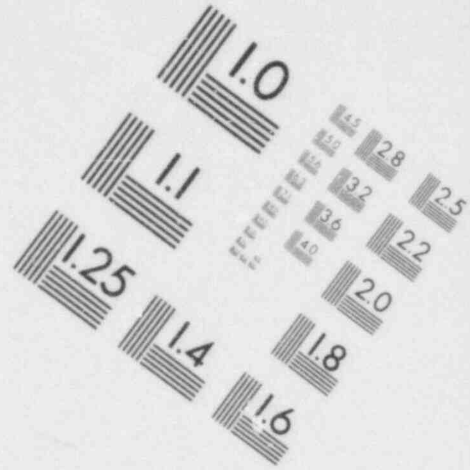
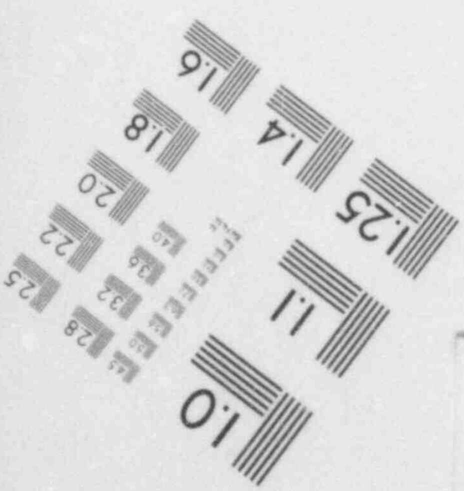


IMAGE EVALUATION
TEST TARGET (MT-3)



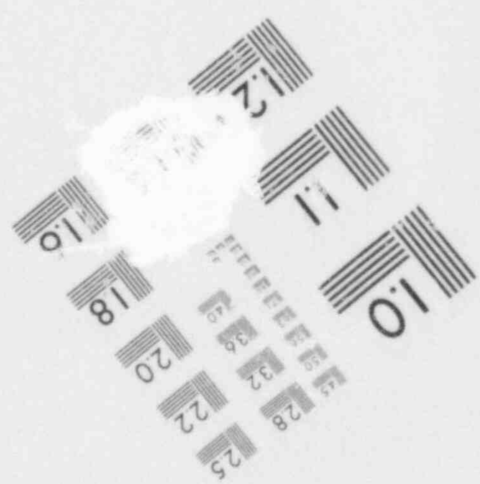
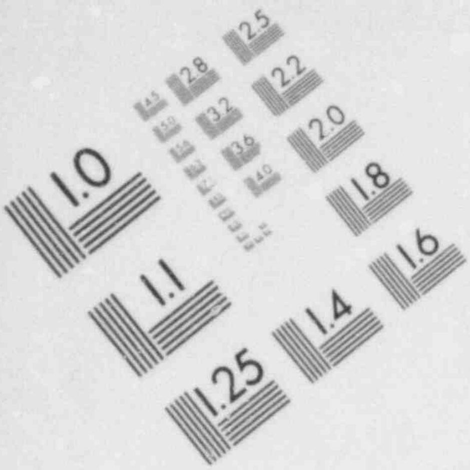


IMAGE EVALUATION
TEST TARGET (MT-3)

6″



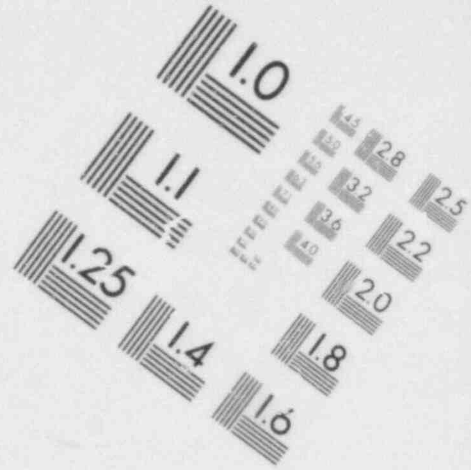
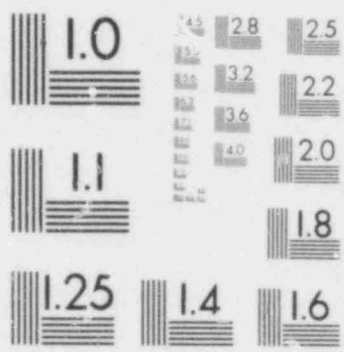


IMAGE EVALUATION TEST TARGET (MT-3)



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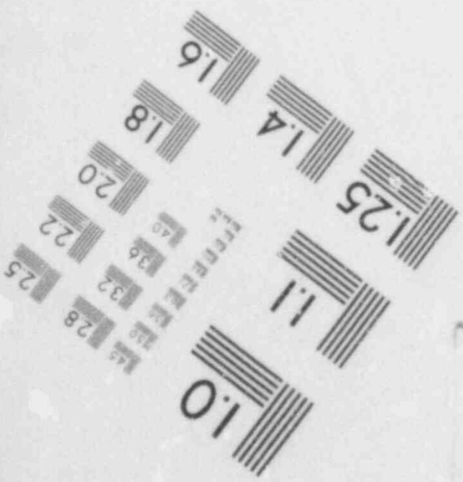


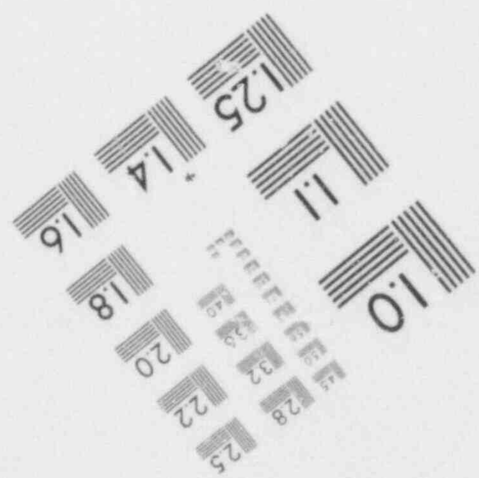


IMAGE EVALUATION TEST TARGET (MT-3)

110

PRESSURE SUPPRESSION TESTING

A. Humboldt 1/48 Segment Tests

1. Objectives
2. Test Configuration
3. Tests at Reduced Vent Submergence
4. Conclusions

B. Bodega 1/112 Segment Tests

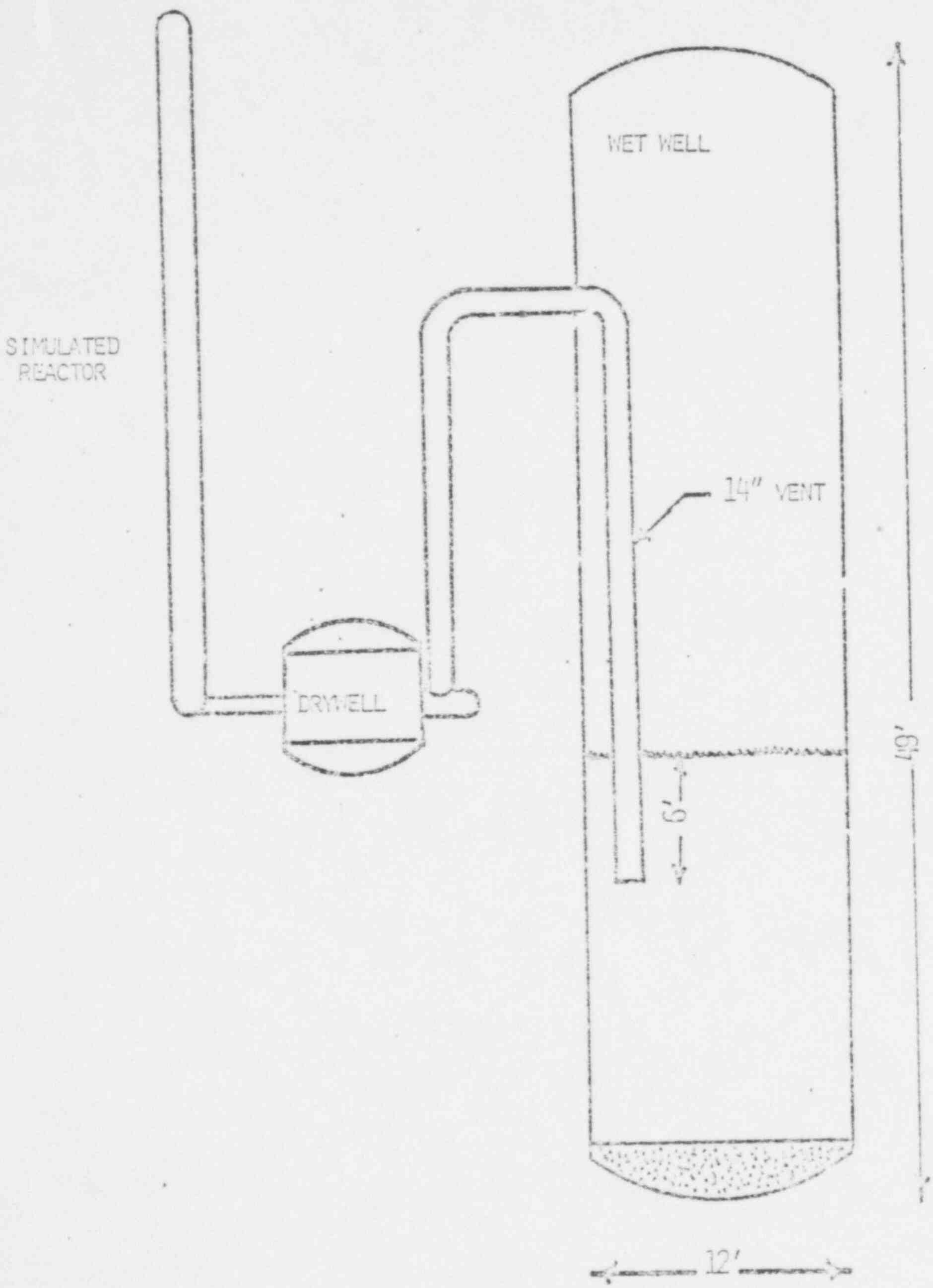
1. Objectives
2. Test Configuration
3. Tests at Reduced Vent Submergence
4. Conclusions

///

HUMBOLDT 1/48 SEGMENT TESTS

OBJECTIVES

1. COMPARE TEST RESULTS WITH HUMBOLDT DESIGN PRESSURES
2. INVESTIGATE EFFECT OF VARIOUS PARAMETERS ON STEAM CONDENSATION
   - FLOW RATE
   - VENT SUBMERGENCE
   - POOL TEMPERATURE
   - AIR
3. OBTAIN DATA ON
   - BLOWDOWN RATES
   - MODEL VERIFICATION
   - VENT LOSS COEFFICIENT
   - POOL DYNAMICS
   - ESTABLISH THERMAL LIMITS
   - MIXING



483 003

HUMBOLDT BAY TEST FACILITY FULL SCALE, 1/48 SEGMENT

POOR ORIGINAL

113

HUMBOLDT 1/48 SEGMENT TESTS

VENT DIAMETER 14"

NOMINAL SUBMERGENCE 6'

TESTS AT VARIABLE SUBMERGENCE

SUBMERGENCE	NO. OF TESTS
12'5"	1
6'	37
3'	1
-0.5'	2
-1.5'	1
-2.0'	1
-3.0'	1
	44

HUMBOLDT 1/43 SEGMENT TESTS

CONCLUSIONS

1. THE HUMBOLDT BAY PRESSURE SUPPRESSION CONTAINMENT SYSTEM COULD HANDLE THE MAXIMUM CREDIBLE OPERATING ACCIDENT WITH A LARGE MARGIN OF SAFETY.

2. STEAM CONDENSES RAPIDLY AND EFFICIENTLY IN THE POOL
   - VERY HIGH FLOW RATES CONDENSE
   - CONDENSATION IS RAPID AND COMPLETE OVER A WIDE RANGE OF VENT SUBMERGENCE EVEN WITH THE VENT ABOVE THE INITI POOL WATER LEVEL
   - POOL TEMPERATURE CAN BE HIGH (140°F - 160°F)
   - AIR FROM THE DRYWELL DOES NOT PREVENT EFFICIENT CONDENSATION

3. NO SEVERE VIBRATION OCCURED.

4. MIXING OF THE POOL WATER WAS EXCELLENT.

115

BODEGA BAY 1/112 SEGMENT TEST

OBJECTIVES

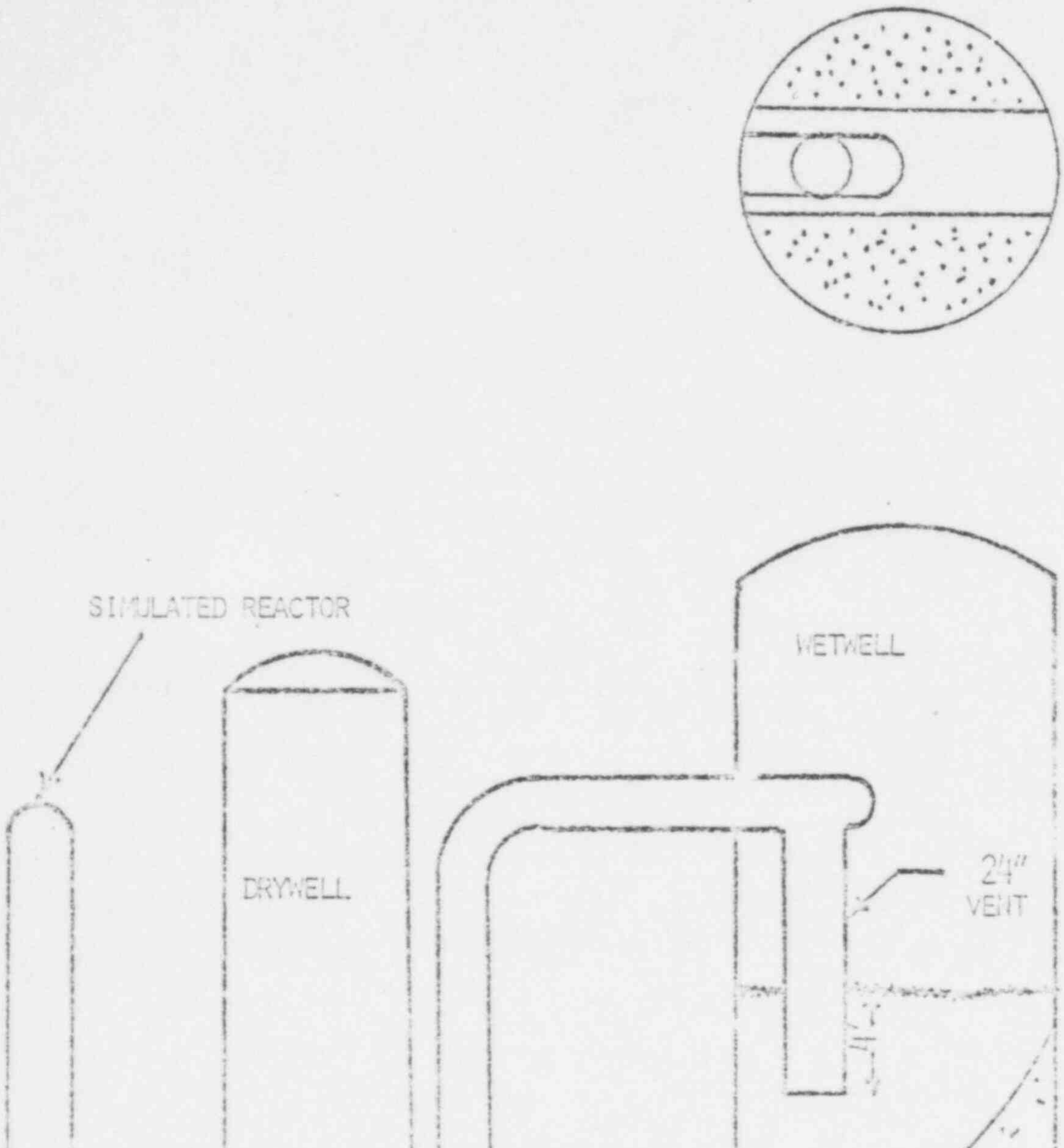
1. DEMONSTRATE ADEQUACY OF BODEGA BAY DESIGN (MARK I PROTOTYPE)

2. INVESTIGATE EFFECT OF VARIOUS PARAMETERS ON OPERATION OF PRESSURE SUPPRESSION

   - POOL WATER LEVEL
   - FLOW RATE
   - POOL AND DRYWELL TEMPERATURE

483 006

116



BODEGA BAY TEST FACILITY FULL SCALE, 1/112 SEGMENT

112

BODEGA BAY 1/112 SEGMENT TESTS

VENT DIAMETER	2'
NOMINAL SUBMERGENCE	4'

TESTS AT VARIABLE SUBMERGENCE

SUBMERGENCE	NO. OF TESTS
5'	1
4'	43
3'	1

1/3

BODEGA BAY 1/112 SEGMENT TEST

CONCLUSIONS

1. CONFIRMED ADEQUACY OF THE BODEGA PRESSURE SUPPRESSION CONTAINMENT DESIGN

2. RAPID AND EFFICIENT CONDENSATION WAS ACHIEVED IN THE POOL

   - VARIATIONS IN POOL SURFACE HEIGHT DID NOT AFFECT PERFORMANCE SIGNIFICANTLY

   - COMPLETE CONDENSATION ACHEIVED OVER A RANGE OF FLOW RATES

   - VARIATIONS IN DRYWELL TEMPERATURE AND POOL TEMPERATURE HAD ONLY MINOR EFFECT ON SYSTEM OPERATION

Pressure Vessel Pedestal Acceleration

"Another serious problem in "pressure suppression" plants is the possibility of vessel pedestal acceleration resulting from the loads on the pedestal created by the pressure wave developed in the suppression pool (see Figures 4 and 5). These loads on the structure may be transmitted through the soil and through the foundation causing the vessel support pedestal to vibrate to a degree greatly above the seismic design basis for the equipment. This condition could result in failure of the vessel and internals, failure of the core support structure, possible loss of emergency core cooling capabilities, possible loss of insertion capability of the control rods, and, again, untold effects by the accident that could occur. This condition should be thoroughly reviewed by the NRC."

Summary of Concern

The February 18, 1976 testimony alleges that the reactor vessel support pedestal could be caused to vibrate (be accelerated) by loads originating in the pressure suppression pool if a design basis pipe rupture were to occur.

ACCELERATION OF PRESSURE VESSEL PEDESTAL
SUMMARY OF DETAILED DISCUSSION

1. ISSUE UNDER REVIEW FOR MARK II AND III CONTAINMENTS WHICH ARE NOT EMPLOYED IN ANY CURRENTLY OPERATING PLANT. PRESSURE WAVE LOAD APPLIED TO EXTENSION OF PEDESTAL WALL. NATURE AND MAGNITUDE OF LOADS DISCUSSED IN GE REPORTS

   NEDO-21061 & NEDE 21061-P - "MARK II CONTAINMENT DYNAMIC FORCING FUNCTIONS"

   NEDO 11314-08 & NEDE 11314-08 - "INFORMATION REPORT MARK III DYNAMIC LOADING"

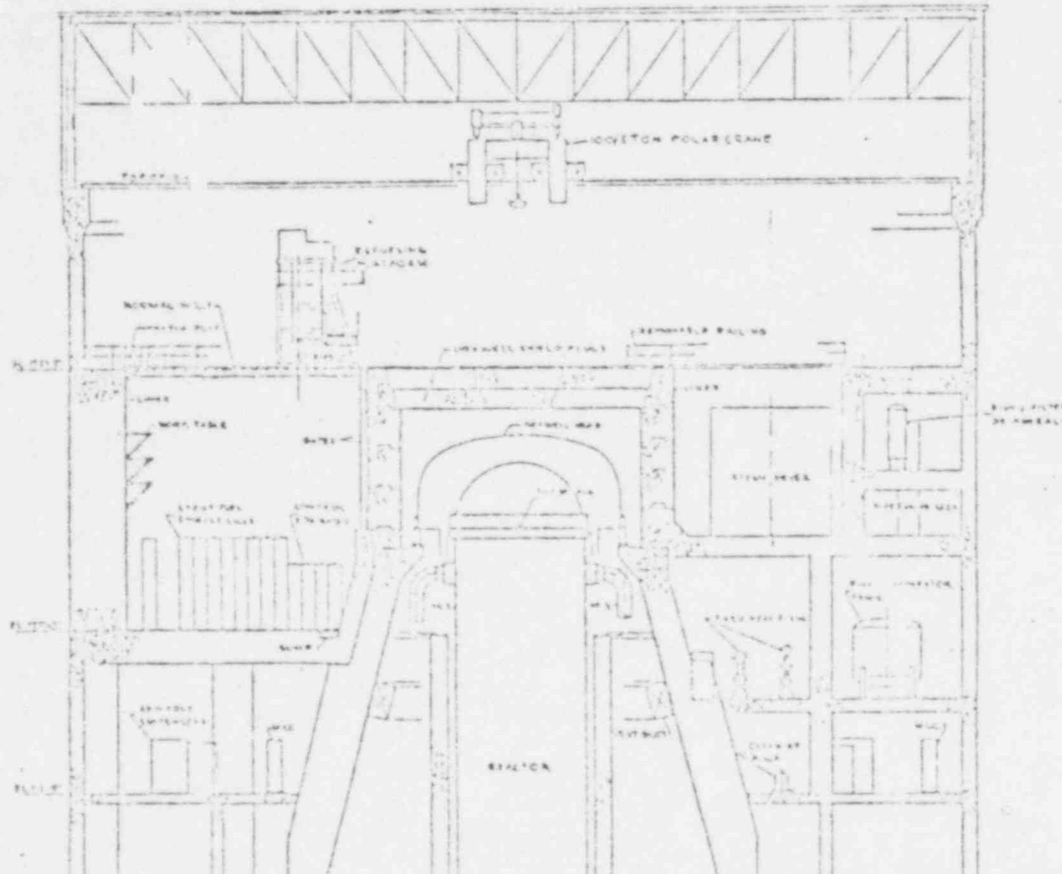
2. FOR MARK II AND III CONTAINMENTS ESTIMATES INDICATE THAT LOCA + SSE WILL CONTROL DESIGN OF REACTOR INTERNALS.

3. LOAD PATHS AND RELATIVE MASS OF SYSTEMS AND COMPONENTS INDICATES THAT THIS IS NOT A PROBLEM FOR MARK I CONTAINMENT. NO DIRECT LOAD CREATED BY PRESSURE WAVE APPLIED TO PEDESTAL WALL. STAFF HAS PERFORMED SCOPING CALCULATION WHICH INDICATES ENERGY GENERATED BY PRESSURE WAVE TO MOBILIZE MASS OF STRUCTURE MUCH LESS THAN THAT GENERATED BY TYPICAL EARTHQUAKE.

4. FOR MARK I CONTAINMENTS LOCA + SSE LOADS FOR REACTOR INTERNALS MUCH GREATER THAN LOADS TRANSMITTED THROUGH SOIL AND FND AS A RESULT OF SUPPRESSION POOL HYDRODYNAMIC EFFECT.

5. IN SUMMARY, THE STAFF HAS CONCLUDED THAT THE PRESSURE WAVES WHICH MAY DEVELOP IN THE SUPPRESSION POOL OF MARK I CONTAINMENTS WILL NOT AFFECT THE DESIGN OF THE PRESSURE VESSEL PEDESTAL OR THE REACTOR INTERNALS. THIS ISSUE IS NOT EXPECTED TO BE CRITICAL FOR MARK II AND III CONTAINMENTS; HOWEVER, THE MATTER IS UNDER REVIEW.

121

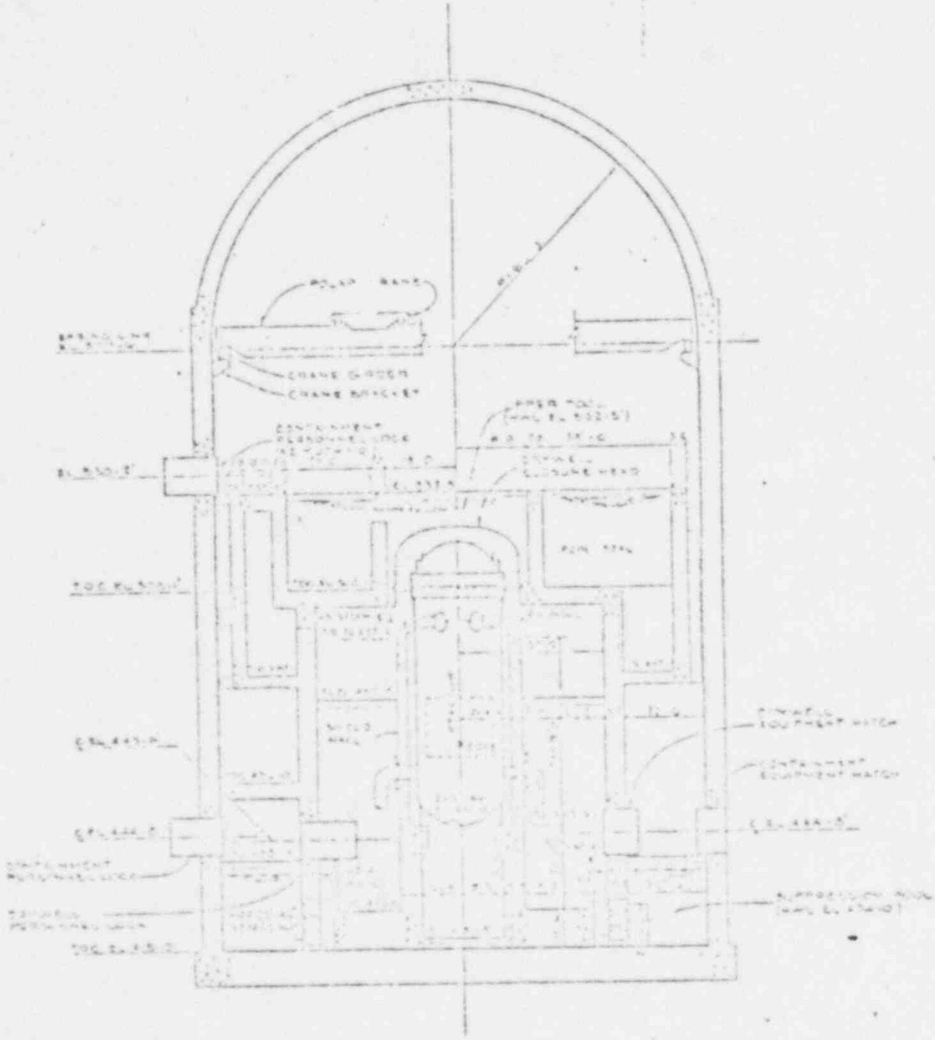


SECTION B-B

POOR ORIGINAL

483 012

122

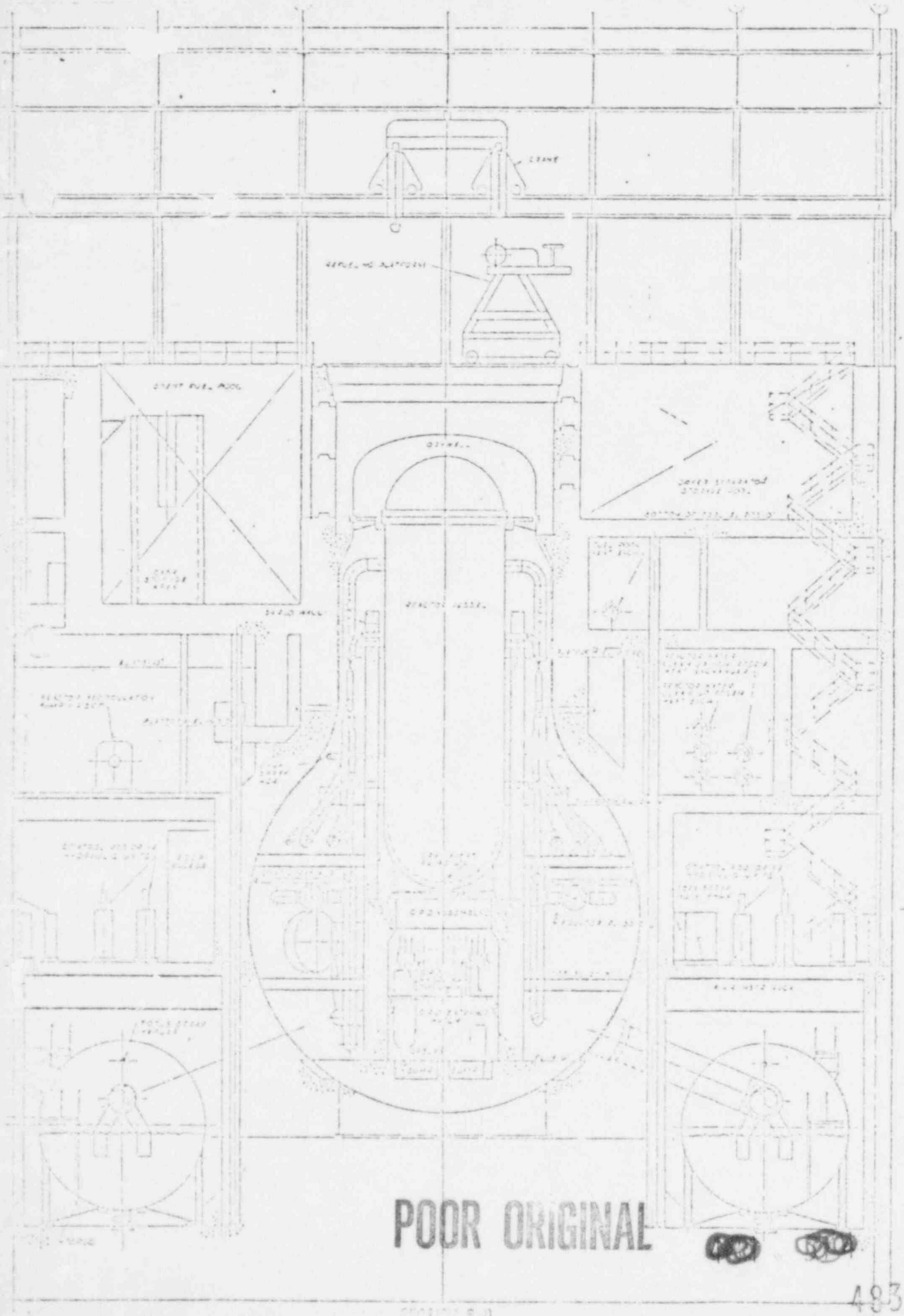


SECTION (A)

POOR ORIGINAL

483 013

/23



POOR ORIGINAL

483 011