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UNITED STATES NUCLEAR REGULATORY COMMISSION'S
ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

DATE: Thursday, January 25, 1990

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UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFETY

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ACRS SUBCOMMITTEE ON STRUCTURAL ENGINEERING

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AMFAC HOTEL
2910 Yale Boulevard, Southeast
Albuquerque, New Mexico

Thursday, January 25, 1990

The Committee met, pursuant to notice, at 8:34 a.m., CHESTER P. SIESS, presiding.

ACRS MEMBERS PRESENT:

CHESTER P. SIESS
DAVID A. WARD
CHARLES J. WYLIE

ALSO PRESENT:

MIKE BENDER
JOHN D. STEVENSON
CARSON MARK

PROCEEDINGS

[8:34 a.m.]

MR. SIESS: The meeting will come to order. This is the second day of our meeting on structural research.

And what we are going to take up today, looking at the one-page agenda -- I guess that is the only one most people have, right?

First, Brad Parks is going to give us a report on penetration research. This is the work on. This is the work on bellows and the inflatable seals on personnel hatches.

Then, again, Brad Parks on future plans for the containment program.

Then Walt von Riesemann and probably Jim Costello will present the, lead the discussion anyway, on the assessment of analytical methods, which had Dave Clauss's name on it, and we didn't get to yesterday.

And that will conclude the containment research at Sandia.

And the next item will be on the Category 1 structures work, or the sheerwall work, I'll call it. Roger Kenneally from NRC Research will do that.

Then, LANL will report on the model tests, the most recent round of model tests, and I think some summary of all of them.

Then Bohn from Sandia has been doing some analytical work on the effects of softening in the walls.

And that will conclude our presentations.

Brad Parks on penetrations.

MR. PARKS: Good morning. My name is Brad Parks from the Containment Technology Division at Sandia National Laboratories.

The first presentation that I will give is just a summary of all the containment penetration programs that have been conducted at Sandia.

Some time ago, a test of three different electrical penetration assembly designs, or EPA designs, were tested at Sandia. The test results have been reported in a NUREG report.

A single personnel airlock was tested by a contractor to Sandia. Again, those results have been documented in a NUREG report.

Also a test of typical compression seals and gasket materials and different configurations have been tested.

Because this work has been completed for some time, I don't plan to go into any further discussion regarding these test results, unless you have specific questions.

We recently completed a series of tests on typical inflatable seal designs. Those test results were recently reported and published in a NUREG report. Because they are relatively recent, I will go into a little bit of detail about those tests.

We currently have a series of tests that are ongoing of the pressure-unseating equipment hatch on the 1/6 scale model. The model was still intact and we were able to do additional testing on one of those equipment hatches. I will be talking in some detail about these tests. I will also discuss a series of bellows tests that we are planning to do.

Okay. Moving on to the inflatable seals. Just some background information.

Inflatable seals are used in personnel airlock doors and escape doors in about 10 percent of all the U.S. containments.

All the installations are either in PWR or Mark-III type containments.

MR. SIESS: So 10 percent have inflatable seals?

MR. PARKS: Yes, sir.

MR. SIESS: The other 90 percent have what?

MR. PARKS: The typical compression seal and gasket type.

MR. SIESS: That has been investigated previously?

MR. PARKS: That is correct.

MR. MARK: Is there just one supplier of inflatable seals?

MR. PARKS: Yes.

MR. MARK: Do other countries use them?

MR. PARKS: In France, they did use inflatable seals at one time. I understand that they have gotten away from the use of inflatable seals.

MR. MARK: The Japanese don't?

MR. PARKS: Not to my knowledge, no.

MR. SIESS: Now, one need for this information, of course, is in developing probability distribution curves for containment integrity in severe accidents, right?

MR. PARKS: That would be one need, yes.

MR. SIESS: In doing that for hatches with inflatable seals, somewhere somebody has to consider the probability that the supply of air to the seals isn't there.

Now, is that somebody else's job? Your methodology that will come out of these tests will not deal with that?

MR. PARKS: That's correct, what you are saying.

MR. SIESS: And the analysts, or the expert panel, or whatever it is that is going to come up with containment integrity data, will have to factor that other stuff in?

MR. PARKS: I would think so, yes. We assume that the air supply is there.

MR. SIESS: Okay. You also assume certain things about the air supply, as to where the valves are, don't you?

MR. PARKS: Yes. We basically assume that the seals are pressurized to whatever this normal operating seal pressure level happens to be.

MR. SIESS: Whether the pressure is valved on or off made a difference, didn't it?

MR. PARKS: Yes. In the tests that we did, it made a tremendous difference, yes.

MR. SIESS: That is something else that the analyst has to take into account.

MR. PARKS: Right.

MR. SIESS: What the configuration is.

MR. PARKS: Right. That is discussed in the NUREG report.

MR. SIESS: That will be part of the methodology.

MR. PARKS: Right.

MR. SIESS: Okay.

MR. PARKS: And I can go into detail, if you would like, about that. I don't plan on doing that.

MR. SIESS: No. Go ahead.

MR. PARKS: Yes. Now, the last point I would like to make here is that the normal operating seal pressure varies considerably from plant to plant, from as little as 50 psi in some plants to as much as 110 psi in other plants.

MR. SIESS: Now, is that because there are different seals or just simply different opinions as to what the pressure should be?

MR. PARKS: Basically different opinions about what the seal pressure needs to be.

The design pressure for the containments that these seals are used in varies quite a bit.

MR. SIESS: Does this vary with the design pressure?

MR. PARKS: To some extent, yes. The ice condensers, which have a very low design pressure, normally use around 50 or 60 psi in the seals. The large, dry PWRs, which have a larger design pressure, normally use a 90 to as much as 110.

MR. BENDER: Seal pressures are recommended pressure by the seal supply, or are they established by cut and dried methods?

MR. PARKS: The seal supplier, according to what they tell me, tells everybody to use at least 90 psi in the seals.

MR. SIESS: How much?

MR. PARKS: At least 90 psi is what they recommend for everyone. But they say these recommendations haven't been necessarily followed.

The minimum requirement that the seal supplier recommends is that the seal pressure is at least 30 psi greater than the design pressure of the containment. And that recommendation has been followed by all the plants that use the containment.

We show here just a typical application of inflatable seal in a personnel airlock. This would be the personnel airlock door, and of course the seals go around the perimeter of the door.

You can better see the application in this section here which is taken through one side of the door. A pair of inflatable seals are used on each door. One point of interest is that if for some reason we do lose the pressure in the inflatable seals, there would be a gap of about 3/8ths of an inch between the sealing surface of the door and the seals. So that is a potentially large leak area, if you did lose pressure to the seals.

We have conducted four series of tests, in which four different pairs of inflatable seals were tested. The first two series were of what we have arbitrarily called the old design and the last two are the new design. There is not all that much difference between the two designs. The primary difference in the new seal design, they have added a little additional strip of rubber to the sealing surface of the seal.

MR. SIESS: Was that what was to be an improvement?

MR. PARKS: Yes, it was.

MR. SIESS: Was it?

MR. PARKS: For severe accident conditions, in which you are worried about a lot of leakage, it really didn't make that much difference.

For design conditions where you are trying to prevent almost all leakage, it seemed to be a considerable improvement.

We tested two different aging conditions of the seals, for the old design and the new design. One pair was tested in the unaged condition; another pair was tested in the aged condition.

The aging consisted of the combination of radiation and thermal aging.

For each test series, or for all the test series, the tests were conducted in an air environment. We initially performed several tests at room temperature, because the room temperature tests were nondestructive. We could test the same pair of seals for several different seal pressures and develop quite an extensive data base just from one pair of seals that way.

MR. SIESS: That was your room temperature, not the containment?

MR. PARKS: Right. It was about 70 degrees, something like that.

MR. SIESS: Thank you.

MR. PARKS: After the room temperature tests were completed, we did some elevated temperature tests. These tests were conducted at constant temperature. The temperatures varied from 300 to as much as 400 degrees Fahrenheit during these tests.

MR. BENDER: That is air temperature, and not seal temperature; is that correct?

MR. PARKS: It is basically the same. The seal test picture is sitting inside a test chamber that is at, say, 300 degrees. We let the fixture soak until we get a uniform distribution through the test fixture.

Okay. What we are trying to accomplish in the test is pretty obvious. We wanted to determine what the containment pressure and temperature conditions would be for a given initial internal seal pressure that would cause significant leakage past the seals.

MR. SIESS: Did you have to define significant for leakage?

MR. PARKS: We arbitrarily defined significant leakage as 10,000 standard cubic feet per day, so that was about 1 percent of the containment volume.

MR. SIESS: One percent.

MR. PARKS: Of a 1 million cubic foot containment, yes.

Then once leakage began, we wanted to know how does leakage grow after the onset of leakage.

Okay. Just to save time, I would like to skip the next couple of viewgraphs, unless you have specific questions, and look at a plot of some typical results.

This particular plot is for the first test series at room temperature. What we are looking at here is leakage on the Y-axis versus chamber pressure that is equivalent to containment pressure on the X-axis. And there are several curves here.

Each curve corresponds to a different initial seal pressure that we applied to the seals.

For example, the first curve is for 50, we used 50 psi in the seals, and so forth, up to 100 psi. Again, these were at room temperature, so that we could test the same pair of seals several times without damaging the seals.

MR. MARK: You have the leakage rate in standard cubic feet per day. Is that from the seal as in the experimental setup or from the seal as you picture it in an installation? Because the lengths are different, I think.

MR. PARKS: That is a good point. It is from the seals in the experimental setup.

The total length of the seal in the experimental setups is about 100 inches, the perimeter. And a typical airlock door, you have around 240 to 300 inches. So you could take these numbers and scale them up accordingly.

MR. MARK: You would have to multiply these by 2.4.

MR. PARKS: Or 3 or something like that.

MR. MARK: Or so.

MR. PARKS: Yes.

MR. MARK: To get a real leakage rate.

MR. PARKS: Of what you would have in an actual containment door, yes.

MR. SIESS: And your 10,000 that you defined as significant doesn't seem to be very critical, does it?

MR. PARKS: I mean, it's going up very rapidly, right.

MR. SIESS: Now, do you understand why you have these little peculiar jogs in some of that? Are they important?

MR. PARKS: Here?

MR. SIESS: Yes.

MR. PARKS: No, I don't understand exactly what was going on. We've postulated that maybe it is the seal tube slipping over a little bit, and then maybe resealing a little bit, and then some additional pressure causes the leak to go up again.

We hadn't even, on this scale you can't see it all that much, but occasionally you have a little spike of leakage and then it would reseal and go on, until we finally got this big leakage.

MR. SIESS: Except for that, those curves, you could have almost scaled them, right?

MR. PARKS: Say from one to, going from one seal pressure?

MR. SIESS: Take the 100 and cut it in half?

MR. PARKS: That's fairly close, yes.

MR. SIESS: Except at 100 it doesn't start to leak until you get to 100, right?

MR. PARKS: Right.

MR. SIESS: But at 50?

MR. PARKS: At 50 psi --

MR. SIESS: Which is 50?

MR. PARKS: This is this solid curve here. We had basically no leakage until we exceeded 50 psi containment pressure, and then we got a big spike.

Now, as we continued to increase the seal pressure, we noticed that the containment pressure or chamber pressure necessary to cause leakage continued to be larger with respect to the initial seal pressure.

MR. SIESS: Oh, okay. So I look at 100.

MR. PARKS: Right. Here is 100.

MR. SIESS: It doesn't move until -- Now, does it stay down on the axis?

MR. PARKS: Yes. Here.

MR. SIESS: Nothing happens until it get to 100. Then it takes off.

MR. PARKS: Right.

MR. SIESS: Now, that little jiggle in there I'm going to ignore and assume it might have gone straight on up. I don't know.

I look at 80, and it is not doing anything until it gets to 80.

MR. PARKS: Right.

MR. SIESS: Right?

MR. PARKS: That is correct.

MR. SIESS: I look at 70, and it starts to leak at

60; 60 starts to leak at around 40; and 50 starts to leak at around 40.

MR. PARKS: There's a common point coming up here.

MR. SIESS: Well, I'm just looking at when it deviates from zero.

MR. PARKS: Yes.

MR. SIESS: And there is a different kind of behavior at the low pressure than it is at the high pressure.

Do you understand why?

MR. PARKS: At this level here?

MR. SIESS: Yes.

MR. PARKS: Not necessarily. I think as you increase the pressure, you are obviously increasing the amount of friction between the seal tube and the sealing surface, which helps to prohibit slippage of the seal tube and gives you a better seal.

MR. SIESS: But in your analysis of the results, you mainly looked at the --

MR. PARKS: Mainly looking at this level here, right, when we got leakage of this level.

MR. SIESS: Okay.

MR. PARKS: There are complete tables of the test results that were at room temperature and at elevated temperature and the handout material I gave you and to save time I don't plan to go into a lot of detail unless you have specific questions. I would like to just summarize the test results and maybe at least if there are any questions we can go back and look.

The one sort of general rule of thumb that developed as a result of these tests is regardless of the test conditions, we didn't get any significant leakage to occur until the chamber pressure again that's equivalent to the containment pressure exceeded the initial seal pressure level.

For example, if you had 90 psi on the seals you wouldn't expect leakage until the containment pressure exceeded 90 psi.

MR. WARD: That really depends pretty tightly on your definition of significant.

MR. PARKS: That's right.

MR. WARD: I mean because these things are sure lifting off the bottom before.

MR. PARKS: A little bit before, yes.

MR. SIESS: Well, we only looked at one curve but if I looked at 50 psi curve back there, it's starting to leak at 30. At 40 it's still got a little bit. At 45 it's got almost as much as -- you know, it gets around 2000 at 45 and 10,000 at 50. Now if you'd have taken 5,000 standard cubic feet per day instead of 10,000 it wouldn't have changed that first conclusion, would it?

MR. PARKS: It would have made it real close possibly to the initial seal pressure but basically what you're saying, right, I agree with you.

MR. SIESS: If you'd taken 2,000 --

MR. PARKS: It could have affected it.

MR. SIESS: -- then you'd have had to say chamber pressure exceeded 90 percent -- so the one to one is somewhat an artifact of significant.

MR. PARKS: I agree but relating this significant figure to what the risk people think is significant, they're normally not interested in any leakage until it exceeds 10 percent of the volume per day. Again, this is around 1 percent.

MR. SIESS: Some people are having a real problem with that, you see. There are a lot of people who think one-tenth of one percent is significant and when you spend a million dollars to make a leak rate test to find it out, you've got to believe it is significant.

MR. PARKS: Yes, I understand.

MR. SIESS: That 10 percent a day is not going to happen -- that's a ten to the minus six probability but the other one is probability of one that you're going to do it every ten years, so we have got to be careful with what's significant.

MR. BENDER: If 10 percent of the system volume per day is a number that people say is when they start to be concerned --

Do you know what the basis is for that?

MR. PARKS: No, I don't know.

MR. SIESS: The basis is dose's bite, roughly speaking. When you are doing severe accident and looking at real life and curie releases and what's really in the containment at the time it starts to leak, one percent a day isn't much of a hole.

MR. VON RIESEMANN: Obviously the number isn't precise but if you look, it depends on the reactor type and the time of the accident and all this, but if you look at off-site risk and consequences then an approximate number, 10 percent of the volume per day, is the threshold where you start causing consequences for off-site.

MR. BENDER: Well, of course it depends on what's in the containment.

MR. SIESS: You have got to do the whole accident sequence analysis. If it is a week old and it's all plated out or it's all settled out in aerosols and the containment just leaks through a small hole, that's one thing. If you blow the lid off of it and there's a big puff that takes it off, the aerosol's out, you know, it gets complicated as the devil.

MR. VON RIESEMANN: But if you're looking at the tech specs, a tenth of a percent of the volume per day, that's not going to cause any off-site -- that's design.

MR. BENDER: Yes, test requirement.

MR. SIESS: Of course a tenth of a percent of the volume per day at TID 14-844 source term can cause 250 rem dose.

MR. BENDER: Definite possibility.

MR. SIESS: There is this tremendous gap between the design basis, which says a tenth of a percent a day is going to cause this huge dose out there and the accident analysis which says it has to be 10 percent a day to do anything.

Now your No. 2 follows No. 1 -- it's not a -- it says once it starts leaking --

MR. PARKS: Right, okay. Once we have the initial onset of leakage, leakage grows rapidly.

MR. SIESS: But if I look at No. 2 by itself --

MR. PARKS: Yes, the little phrase in there to start that off.

For temperatures up to 350 degrees Fahrenheit we really didn't see any indications of any degradation in the seal material, on post heads the seal material looked basically the same as it did before the tests.

I have between 350 degrees and 400 degrees Fahrenheit, which happened to be the maximum test temperature.

MR. SIESS: Now what kind of temperatures did the accident analyst predict for the --

MR. PARKS: PWR's, 361 degrees is supposed to be the magic number, of the maximum temperature that's reached, 400 degrees Fahrenheit in the Mark III's.

MR. SIESS: So I'm looking at the temperatures that Mean used for task 5, which he got from Sandia he said, 550, you're talking Fahrenheit?

MR. PARKS: Yes.

MR. VON RIESEMANN: But that's Fermi which is what?

MR. SIESS: A Mark I.

MR. VON RIESEMANN: Mark I and this is not used on Mark I's.

MR. SIESS: Okay, 400 for Clinton; 360 for the PWR, okay. But now that really doesn't mean a hole in the containment. That only means leakage past the interior door.

MR. PARKS: Exactly. It's a good point.

The outside door would never get up -- if you assume these interior containment conditions of 400 degrees Fahrenheit, the outside door would never see that temperature.

MR. SIESS: I suppose if the seal completely deteriorated you could get enough hot stuff in there to get the outside door hot.

MR. PARKS: At 400 degrees Fahrenheit? I don't think so. There's no way, using the first personnel air lock test to draw from.

MR. SIESS: You get some kind of circulation, didn't you?

MR. PARKS: Right. The first air lock test that we did the inner door we were finally able to make leakage go past the inner door and with this very high temperatures, I think around 800 degrees Fahrenheit inside the inner door, the outer door never got above 300 degrees Fahrenheit.

MR. SIESS: You didn't have a large volume on the other side of that door to -- you still can't get in there?

MR. PARKS: It was still coming -- it has to go through the inner door to get to the outer door.

MR. SIESS: Right. Okay, so all that would happen at the high temperatures is you would lose one level of redundancy.

MR. PARKS: I think so. I think that's a very good point, yes.

MR. SIESS: That should give you some comfort because otherwise you're getting awful close.

MR. PARKS: Right.

Finally --

MR. SIESS: Excuse me, can those seals be made to resist higher temperatures or is that just inherent with the material? The seal manufacturer -- can he make a seal that could withstand 500 F.?

MR. PARKS: No, that question to my knowledge hasn't been asked.

The material that these seals are constructed from is an EPDM material system, black rubber substance. The curing for these seals is around 350 degrees, 300 - 350 degrees, so once you start exceeding that curing temperature it's really not all that surprising that we start to see the material begin to decompose.

MR. SIESS: The reason I asked you is yesterday Dave Clauss was saying we have got 100 containments out there. What are we going to do with them if they -- if this is a problem, seals are replaceable.

MR. PARKS: Right.

MR. SIESS: In fact, they are replaced. Nobody is going to leave the seal in for 40 years.

MR. PARKS: These seals are replaced every other year.

MR. WARD: Well, there are elastomers used in O-rings which are good Vicon compounds which are good at the higher temperatures but I don't know whether they make suitable, deflatable seals.

MR. WYLIE: This is what material?

MR. PARKS: It's EPDM, ethylene propylene.

MR. WYLIE: Ethylene propylene rubber material.

MR. PARKS: Yes.

MR. WYLIE: I suspect what you're talking about is silicon rubber.

MR. WARD: Yes.

MR. SIESS: You know, you'd have to ask the manufacturer.

MR. WARD: There's a whale of a difference in the properties of those.

MR. PARKS: Yes, it might not make suitable seals.

MR. SIESS: You know, he's never had any incentive to make the seal dc 500 because these things will --

MR. PARKS: Right. He's just looking at the design.

MR. SIESS: LOCA accidents are well below that. Again, I could look it up in that mean stuff.

MR. WYLIE: Have you run any tests where you soaked it at a max temperature and then backed off on the temperature?

MR. PARKS: No.

MR. WYLIE: You didn't?

MR. PARKS: The highest temperature that we looked at is 400 degrees Fahrenheit. We soak it at that temperature and then we start increasing the chamber pressure. We chill it that way.

MR. WYLIE: You didn't look to see what would happen if it cooled off?

MR. PARKS: No -- tried to get into a lot of detail about these tests but at 400 degrees Fahrenheit when we did have a big burst of leakage past the seals it was the result of the seals rupturing and once the seal ruptures it can't hold the pressure so you have a really large leak at that point.

The last thing I would like to mention is that based on the test results we did develop some very simple empirical equations to predict the containment pressure for a given seal pressure and temperature at which leakage past the inflatable seals could be expected.

MR. SIESS: And one manufacturer and a limited number of plants -- do you feel that that --

MR. PARKS: I feel comfortable that that covers it.

If there are no other questions about the inflatable seals I would like to move on to the equipment hatch tests that are --

MR. SIESS: That turned out to be a fairly simple little test program once you got that rig built.

MR. PARKS: Yes. The test actually went fairly smooth. We were happy with it.

MR. SIESS: Relatively simple. I mean it wasn't simple. It was rather complicated stuff.

MR. MARK: If you don't use inflatable seals, what do people do? Just metal to metal?

MR. PARKS: There's still a compression type seal there which you -- of various different designs.

MR. SIESS: Double dog-ears and stuff, O-ring type things that just compress.

MR. MARK: Well, that's a composite plastic material that also has a temperature limit.

MR. PARKS: Most of those seals are also constructed either of this ethylene propylene material, similar to what these seals are constructed of, or silicon.

MR. MARK: So they have the same temperature problems, just about?

MR. PARKS: There's a little bit of discrepancy there. The compression seals and gaskets test that we did, we didn't notice leakage there until we got up to around 600-650 degrees. I believe the difference is that we don't expect -- the compression seals are not subjected to nearly the stress levels that these inflatable seals are subjected to in its totally compression state and only in a groove. There's no place for them to go. These inflatable seals are under tension, you know.

MR. MARK: Thank you.

MR. SIESS: The others may go bad but there's no place for them to get out.

MR. PARKS: Exactly, and they don't actually begin to break down to a powdery material until a much higher temperature.

Moving on to the Equipment Hatch Test, I begin by mentioning that we've established a fairly simple analytical method to predict --

MR. SIESS: Before you get too far, how many of these do we have out there?

MR. PARKS: How many --

MR. SIESS: Pressure unseating hatches do we have? Not drywell heads, but --

MR. PARKS: The number -- I don't know. There is a significant number of pressure unseating equipment hatches, but the actual number --

MR. SIESS: Do we know where they are? Which plants have them, which designers used them, are they all BWRs, or are they all combustion engineering? What do we know about pressure --

MR. PARKS: As far as the survey of the containments that use equipment hatches, there was the Argon study that was done. They located pressure unseating equipment hatches in many different types of containments. This is a very common design, to my knowledge.

MR. SIESS: Well, that's what I am getting at. As an engineer --

MR. WARD: Do you have a reference to that?

MR. SIESS: The Argon -- yes, it's a NUREG.

MR. PARKS: It's a NUREG report. Now, I don't have the number.

MR. WARD: But it's an old one.

MR. SIESS: Oh, yes, 5 years ago.

What about the Mark 1 hatch? Is that always a pressure unseating hatch?

MR. PARKS: The equipment hatch?

MR. SIESS: Yes, what we're talking about right here.

MR. PARKS: I honestly don't know.

MR. SIESS: I would think it might be, because they haven't got a whole of room inside a drywell to put a hatch.

It may not be important, I don't know, but you know, as an engineer, I'm designing something to withstand several tens of psi pressure, and the normal way to do it would be to take advantage of the pressure to seal, and then to see these things hung on a bunch of bolts, there must be a reason for it. I can't imagine any engineer saying that this one is just as good as that one.

There must have been some reason for choosing the unseating versus the seating hatch, and I'm wondering whether it's geometry or physical access or whether there's good reasons for it, and there must be some relation to the types of reactors or containments out there.

MR. PARKS: I am not familiar with the background, unfortunately. Perhaps I should be more aware of it.

MR. WARD: Are you familiar with the Generic Issue 99? Or maybe Jim is. I don't know.

This concerns shutdown decay heat removal, and one of the major issues there was so-called mid-loop operation with the containment open, and a Generic Letter was issued, calling on licensees to, when they're in that sort of operation, to be prepared to slap the hatches on quickly and put the bolts on loosely.

We had some problem with that for this type of hatch, and we haven't been able to get very good feedback from the staff on how many of them are out there and what this really means. Maybe I should wait until you finish, you know, present what you're going to present, but do you have any information or any opinion on how inadequately-tightened hatches will -- of this type pressure unseating will tend to leak or not leak?

Maybe I should wait until I see what your talk is going to say.

MR. SIESS: You know, it just occurred to me that if you had an accident and wanted to get the hatch on, the lid on real quick, you only need to put three or four bolts in if it's a pressure seating.

MR. WARD: Yes.

MR. SIESS: If it's a pressure unseating, it takes a lot more, but you do it from the outside.

MR. PARKS: Right.

MR. WARD: Yes.

MR. SIESS: You could get everybody out.

MR. WARD: Yes, and keep working on it. So, maybe that's the argument. I don't know.

MR. SIESS: There is a tradeoff there.

MR. WARD: I just wondered if the people in NRR concerned with that have been picking your brains on this, since you've got -- but apparently not.

MR. COSTELLO: No, I haven't. Jim Costello from the NRC staff.

No, I haven't. We haven't been contacted in regard to that. The Argon report, which was not a survey of all plants, but it was an effort to do a comprehensive inventory among the -- in population, to look for differences in designs of penetrations, which were thought might have some significance for early leakage.

MR. SIESS: Jim, I don't know why the NRC finds it so difficult to get information.

Put a fax message out and send it to every resident inspector in every plant in the U.S., and I am sure that you could do that in 15 minutes -- they must have a system now -- and ask him to tell you how many pressure seating hatches there are in his containments and how many of the other kind, and you get the answers back the same day. You don't have to contract with Argon to find out how many hatches --

MR. COSTELLO: Well, I think, to respond to Professor Siess' question, I think it's pretty clear that nowadays, with everybody -- most people having PCs and accessible faxes, it's possible, at least in principle, to turn stuff around, information like that, a lot faster than we use to be able to, but I think we also gave the institutional questions about who has priority to impose upon the resident inspectors' time, and we're probably doing less well on that than we are doing on the machinery.

MR. SIESS: Okay.

MR. PARKS: Move on?

MR. SIESS: Yes.

MR. PARKS: Okay.

MR. SIESS: Let's get started.

MR. PARKS: We have developed a fairly simple, fundamental method to predict when leakage would occur of these pressure unseating hatches.

MR. SIESS: If all the bolts were in.

MR. PARKS: Excuse me?

MR. SIESS: If all the bolts were in.

MR. PARKS: Right.

MR. SIESS: Okay.

MR. PARKS: The structural response of the hatch and the sealing mechanism is determined from a strength-of-materials approach. One important criteria that we found when evaluating when leakage would be in is the amount of available gasket spring-back. This is just a measure of how much the gasket can be formed back to its original step, once this compressive load is removed.

MR. SIESS: It's the relative stiffness of the gasket and the bolts.

MR. PARKS: This spring-back, we're just referring to the gasket, but the leakage phenomenon definitely depends on the stiffness of the bolts.

MR. SIESS: The relative stiffness of the gasket and the bolts.

MR. PARKS: Yes.

The method that we're using to estimate the amount of leakage is based on a fluid mechanics approach, assuming that we have choke flow.

As I mentioned, there is a series of tests underway on the pressure unseating equipment hatch in the 1/6th-scale model.

Some obvious parameters that we're looking at is the type of gasket material, how it affects leakage, the effect of aging in the leakage behavior, the effect of total -- by "aggregate", we mean total bolt pre-load and the total bolt stiffness, and we're also looking at different loads inside the equipment hatch. By "loads", we mean the pressure and temperature conditions.

MR. SIESS: These are being done on the actual double hatch you had in the 1/6th-scale.

MR. PARKS: That's right.

MR. SIESS: Just by pressurizing in between them.

MR. PARKS: Right.

At this point, we have completed four ambient temperature tests, and as I'll describe, we have plans to do additional testing at elevated temperature.

One other point I should bring out --

MR. SIESS: That gives you two unseating heads, doesn't it?

MR. PARKS: No. We've actually welded the -- we welded the inner cover shutoff, because --

MR. SIESS: Because it wasn't typical or something?

MR. VON RIESEMANN: Wasn't strong enough.

MR. SIESS: Oh, wasn't strong enough. Okay.

MR. VON RIESEMANN: Not strong enough to take the loads.

MR. SIESS: Fine. Sure. Okay.

MR. PARKS: This method is being validated only on the unseating equipment hatch, but due to the similarity in design, we also think that the method should also be good for the unseating drywell heads.

MR. SIESS: Now, when they looked at drywell heads some time ago, the first time they did it, they thought there was going to be a lot of leakage, and then they looked at the different temperatures, and it turned out it wasn't going to leak it. So, is that the same thing?

MR. VON RIESEMANN: To paraphrase what Professor Siess is saying --

MR. SIESS: Well, you can explain it, because I don't

--

MR. VON RIESEMANN: The original investigations, done by other organizations, looked at the leakage through a drywell head in a BWR, and if you don't account for the thermal differential across the joint, you would show leakage at a very early pressure -- in fact, the load design, and obviously, in the integrated leak-rate tests, they don't leak, but now, if you look at the temperature differential --

MR. SIESS: Integrated leak-rate tests did not rate at temperature.

MR. VON RIESEMANN: But if you account for temperature on the joint, it would show that it takes considerably more pressure.

MR. SIESS: Okay.

So, when you say drywell heads --

MR. VON RIESEMANN: For the Mark 1's and 2's.

MR. SIESS: And 3.

MR. VON RIESEMANN: No. The drywell head in there is -- the pressure boundary on Mark 3 is the containment shell.

MR. SIESS: Okay. The drywell is not the pressure boundary.

MR. VON RIESEMANN: It's not the pressure boundary as far as offsite.

MR. SIESS: It's just the diversion boundary.

MR. VON RIESEMANN: Right.

MR. SIESS: It will leak like a sieve if it's not lined.

MR. PARKS: What we're looking at here is --

MR. SIESS: As far as the drywell head, this addresses only the load deformation effect on leakage. You'd have to figure out the temperature separately.

MR. PARKS: Right.

MR. SIESS: If the analysts can calculate how much the head moves, you will tell him how much it leaks.

MR. PARKS: Right.

MR. SIESS: Okay.

MR. PARKS: What we're looking at here is just the local area around the equipment hatch. We call it Equipment Hatch B on the 1/6th-scale model.

On this particular drawing, this is the inside of containment. Here's the liner.

This inner hatch cover has been welded shut here to prevent any possibility of leakage through this boundary.

What we're actually testing is this unseating equipment hatch out here. The hatch cover is attached to the sleeve by 20 symmetrically-spaced I-bolts around the perimeter.

The particular seal design that we have here is a tongue-in-groove design, we see up here in this corner.

MR. SIESS: Are those bolts pre-stressed?

MR. PARKS: Yes. They have an initial pre-load.

MR. SIESS: Is it like a torque-wrench type thing?

MR. PARKS: Right.

MR. SIESS: Are they pulled? No.

MR. PARKS: Well, they are supposedly immuni-axial tension.

MR. SIESS: Oh, these are the I-bolt-type things --

MR. PARKS: Right.

MR. SIESS: -- all along the outside. They swing down into place.

MR. PARKS: Right.

MR. SIESS: But you actually get the pre-load by torqueing, not by pulling.

MR. PARKS: Right.

With the tongue-in-groove configuration here, the seals actually sit in these grooves shown here. The seals are rectangular in cross-section.

As we were mentioning, there's initial pre-load applied to the bolts, so you have initial pre-compression of this sealing surface.

During the test, we pressurize this inner cavity with a nitrogen gas. As the pressure increases, we'll obviously reach a point in which the net axial force on the hatch cover relieves this initial pre-load. At that point, we'll have separation between the two adjacent sealing surfaces.

However, at separation, we wouldn't expect leakage, because the tongue is still in contact with the seal, and leakage shouldn't occur until this separation increases to the point in which we have a gap established between the tongue of the hatch cover actually separates from the seal itself.

MR. SIESS: Now, the pre-load is specified by the designer?

MR. PARKS: In actual containments, yes.

MR. SIESS: At what level does he choose the pre-load.

MR. PARKS: Again, the varies some from plant to plant. What we have found is that normally the pre-load is such that you wouldn't expect separation of these two surfaces to occur until the pressure is in the range of 10 to 50 percent beyond the design pressure.

MR. SIESS: Do you think they really -- the designer looked at the separation, or did he just look at the pre-load in relation to the pressure inside?

MR. PARKS: He looked at the pre-load in relation to this pressure here -- that would be my assumption, yes -- and made sure that the pre-load was sufficient that you wouldn't have separation until you got beyond the design pressure.

MR. SIESS: So, you think the pre-load was set at the design pressure.

MR. PARKS: Well, it's set at a factor of 1.1 to 1.5 above the design pressure.

MR. SIESS: Have you got any idea of what the tolerance would be on bolt pre-load that the designer would consider?

MR. PARKS: No, I don't have any idea of this tolerance.

MR. SIESS: If he wants the pre-load to not lift off during a structural integrity test, he is going to allow something for uncertainty in the torqueing.

MR. PARKS: What his thinking was as far as how much variability there might be, I don't know.

MR. SIESS: Walt, you're on a containment capacity expert panel. Did the panel have to address the uncertainties in this?

MR. VON RIESEMANN: Fortunately, we didn't have to face this issue.

MR. SIESS: Who did? Another panel?

MR. VON RIESEMANN: I don't think we had any containments with this.

MR. SIESS: Okay.

MR. VON RIESEMANN: But as we know, there is a large variation, and if you put a torque on, the actual load in a bolt can vary due to friction and many factors.

MR. SIESS: Yes.

MR. VON RIESEMANN: In our tests -- I may be jumping ahead -- we have strain gauges on there so we can measure the actual force, but in reality, there is a factor sometimes of 2, if you will, difference.

MR. SIESS: So, any methodology you come out with, again going back to the thought that you're trying to develop a methodology to hand over to the risk analysts, they want a CDF, or CFD, or whatever it is.

MR. VON RIESEMANN: CDF, yes.

MR. SIESS: CDF. And that would probably be one of the biggest variables in it, wouldn't it?

MR. VON RIESEMANN: Right.

MR. SIESS: Okay.

Is that part of your methodology, to include the -- quantify the uncertainties?

MR. PARKS: There's no probabilities in our methodology, no.

MR. SIESS: I got that impression from something yesterday, and yet, Dave said that, well, you know, our customer is the risk analyst, and they don't want point estimates. I mean you can't give them a point estimate. You can't shove it down their throats.

So, unless you've got, you know, frequency distribution curves, they are not going to buy it.

MR. VON RIESEMANN: But we can take the methods being developed here, include the variability, if you will, in the bold pre-load, and that will then give you the distribution function.

MR. SIESS: Yes. But somebody could arbitrarily --

MR. VON RIESEMANN: But we have to do some --

MR. SIESS: I mean you get an expert panel to decide on that, I guess.

MR. VON RIESEMANN: The other way to handle this, if you wanted to, if you wanted to be more certain of your pre-load, is to put a washer, if you will, underneath that measure.

MR. SIESS: But actually, there is nothing you could do in your tests and in the information you have to get data on variation in pre-load, not in what you're doing. If somebody wanted that, they'd have to go out to all these plants and do something about it. You can take it into account by estimating it, but as far as sensitivity, you know it's sensitive. It's probably the most important variable you've got.

You raise the same question about what the actual pressure was in the inflatable seal. They say they got 110 but is it really?

MR. PARKS: Okay, moving on to the test matrix that we're following, the LP3 test as shown here was the test that was actually done back at the time of the overpressurization test of the model. It's been done a couple of years now.

Recently, we've conducted HT-1 through HT-4. All of these tests are conducted at ambient temperature, whatever the local environment temperature happens to be out there. One of the test is a silicone material. The others are this, EP or ethylene phoporine material. We rated the amount of aging. Also, the total bolt pre-load applied and also, to determine what the effect of the bulk stiffness on the leakage behavior.

In a couple of tests, we've only used 10 bolts instead of the 20. In the other tests, we've used 20 bolts.

MR. SIESS: That's a partial answer to Mr. Ward's question.

MR. VON RIESEMANN: Yes, that's right.

MR. SIESS: Just -- because I'm going to ask you later -- can you give me the relationship between the bolt pre-load; is that one bolt pre-load?

MR. PARKS: This is a total bolt pre-load that is applied to the hatch. It's a sum of all the bolt pre-loads.

MR. SIESS: Could you tell me real easily what that is at p.s.i. pressure on the hatch?

MR. PARKS: Pressure where?

MR. SIESS: Inside the thing that you're measuring. You won't be having curves of pressure versus leak at somewhere.

MR. PARKS: Yes. What separation pressure does that correspond to?

MR. SIESS: Yes, what pressure does that correspond to? If you don't know offhand, don't bother.

MR. PARKS: Well, I just did this calculation a couple of weeks ago.

MR. SIESS: How was the area or the diameter?

MR. PARKS: The diameter is 40 inches and it's easy to back that out. I think this corresponds real close to the design pressure which is about 46 p.s.i. This 91.5 corresponds to a little bit more than 50 percent of the design pressure and the 114 corresponds to almost twice the design pressure.

MR. SIESS: The area is 400 times pi.

MR. PARKS: Okay, tests that we have planned to conduct begin from HT-5 to HT-11. This load condition B here -- those tests will actually follow the saturated pressure and temperature curves. So we'll be varying pressure and temperature as we step-wise load this thing and then for the final loading condition, we'll increase the temperature at the seals to a level that's approximately the same as the seal degradation temperature. The seal degradation temperature we observe from the compression seal and gasket test program.

So we will increase the temperature until we degraded the seals and then begin increasing pressure to see what the leakage behavior would be of a fully degraded seal.

This table shows the results of the ambient temperature tests that have been conducted already. I'll go through the results with you. The first test, this particular test happened to be of the aged seals. The maximum test pressure was 95 p.s.i. We observed leakage, a sudden initial spike of leakage between 90 and 95 p.s.i. Here we got real lucky. We calculated an initial leakage pressure of 93 p.s.i. So we were right on the nose with this particular test.

The leak rate that we measured at the maximum test pressure was 25 s.c.f.m. We didn't do so good on the predicted leak rate. We predicted 80 p.s.i. for these same conditions. The measured separation between the sealing surfaces at this maximum test pressure was 25 mils or 25 thousands of an inch. We calculated 23 thousands. Based on our measured properties of the seal, the mean available springback was 22 mils and based on the deviation in our measurements, we had a standard deviation of about 4 mils.

Going across the table here comparing our measured leakage initiation pressure to the calculated value and here we did a pretty good job, pretty close. Here our calculated or predicted value was a little bit higher than when we actually predicted initial leakage. Again, we're a little bit on the high side here. For this final test, we predicted a leakage to begin at 166 p.s.i. When we were up to 180 p.s.i., we still hadn't observed any leakage. The strain in the bolts was getting well into the inelastic range or into the inelastic range. We wanted to be able to reuse the bolts so we terminated the test at this point without going any higher in the pressure for that particular test.

Another thing to point out, for three of the four tests, these first three tests, leakage began when the available springback was within 1 standard deviation of the actual separation between the sealing surfaces of the hatch.

MR. BENDER: What do you envision you'll use these calculated rates for?

MR. PARKS: In our particular case, we're just trying to develop methods to be able to predict the amount of leakage. What they would be used for again would be the same goal as all of our other work.

MR. BENDER: Just to judge whether the calculation method is adequate or not, you have to have some feeling.

MR. PARKS: This method doesn't seem to be adequate. There seems to be a tremendous amount of variation in the actual leak area going through this -- between the hatch cover and the sleeve. We assume it's a uniform leak area all around the circumference. Obviously that's not the case at least to the -- leaks.

MR. WARD: How do you know that's obviously not the case? I guess I missed that. How do you know that leakage varies around the circumference?

MR. PARKS: Well, we assume that it's uniform. The reason we say it's obviously not the case -- well, one reason that comes to mind is we actually placed streamers around the circumference of this thing and we see leakage out one side and not the other.

MR. WARD: Okay. All right.

MR. SIESS: Well, it just isn't reasonable it would be uniform but they don't know --

MR. WARD: Okay, but they made some observations. I'm wondering. Let's see, the difference between 3 and 4 is this difference between 10 bolts and 20 bolts; is that right?

MR. PARKS: No actually, let me refresh my memory.

MR. PARKS: Three and four, the main difference is in the aging of the seal. Other than that, there's no difference. The pre-load is the same.

MR. WARD: Okay, two and three, the difference is in the number of bolts.

MR. PARKS: Right.

MR. WARD: But that's all taken into account appropriately in the --

MR. PARKS: In the analytical method, yes. It affects the bolt's thickness.

MR. SIESS: Let me look at that a minute.

MR. WARD: Well, it affects the bolt's thickness, but what about the deflection? Are you counting the deflection of the flange?

MR. PARKS: Of the hatch as it moves out?

MR. WARD: Yes.

MR. PARKS: Yes. That's a direct function of the bolt's thickness.

MR. WARD: Okay.

MR. PARKS: The only thing that's holding it is the bolts.

MR. SIESS: HT-1 was at 57.2 bolt pre-load.

MR. PARKS: Right.

MR. SIESS: Then what's the corresponding one at the higher bolt pre-load -- HT-4?

MR. PARKS: HT-4 is 91.5, something like that.

MR. SIESS: Now, the 57 corresponds to 46 p.s.i.

MR. PARKS: This corresponds if I remember to about 1.6 times the design pressure.

MR. SIESS: Well, I don't really care. I'm interested in numbers. Design pressures don't really mean anything to me.

MR. PARKS: That's 1.6 times 46.

MR. SIESS: Well, it's .8 of a p.s.i. per kip, a bolt load. So that's a nice round figure. The higher pressure is 72 p.s.i. and the other one is 46. HT-1 was loaded to about twice the pre-load pressure and started leaking -- let's see. The first three rows are pressures, right?

MR. PARKS: Right. This is a measured leakage value. This is the predicted.

MR. SIESS: Leak rate. Now the only pressures up there are the first three rows, right?

MR. PARKS: That's correct, yes.

MR. SIESS: So that didn't even start leaking until about twice the pre-load pressure.

MR. PARKS: Yes. That's right.

MR. SIESS: Which is a function of the springback and the freeload.

MR. PARKS: Right.

MR. SIESS: HT-4 didn't leak at all. Is that what it's saying?

MR. PARKS: It didn't leak at all during this test, no.

MR. SIESS: The free load pressure there was 72, so it got up to quite a bit more.

MR. PARKS: Right.

MR. SIESS: The only way you varied the free load pressure was simply varying the number of bolts.

MR. PARKS: Well, we varied the number of bolts. We also varied the amount of pre-load that was applied to each individual bolt.

MR. SIESS: Now which test would that be? HT-7 was the only one that was different. No. Of the 20 bolts, they were all at 91.5 except one.

MR. PARKS: That's correct. The HT-5 through HT-11 haven't been conducted at this time.

MR. SIESS: But of HLP3, HT1, HT2 which is 10 bolts, you have a varying free load there which was actually varying the torque on the wrench; right?

MR. PARKS: Right.

MR. SIESS: You had strain gauges on it.

MR. PARKS: We had strain gauges on the bolts.

MR. SIESS: Now, so how well -- that's taken care of in the analysis. We can't tell from what we have up here, can we?

MR. PARKS: How well the pre-load?

MR. SIESS: Yes.

MR. PARKS: This calculated value depends on the measured spring back and also the bolt pre-load.

MR. SIESS: HT-1 to HT-2, I have a pre-load that is varied, both of them have 10 bolts. Free load is different. That means the torque was different.

MR. PARKS: Right.

MR. SIESS: But also one was aged and the other was unaged. So they had different seals in them. So I can't compare just that one effect.

MR. PARKS: That's correct.

MR. SIESS: Then as we go down into the test you haven't made a pre-load -- the only variable in pre-load is the HT-7. Now is there something that HT-7 can be compared directly with? HT-7 is an EP168. I can't tell from the table. Can you tell me?

MR. PARKS: There's not another test in which the only thing is varied is the pre-load, no.

MR. SIESS: Why was the pre-load varied along with two or three other things? There's an awful lot of tests here. You call it a text matrix except it isn't. It's just a table.

MR. PARKS: A complete test matrix that varied all the parameters would be -- would involve many, many more tests than what we have here.

MR. SIESS: Yes, but for about one page of stuff, I can make a list like this of various combinations that would show me that. I can't even see the matrix you got, you see? It's more than two dimensions obviously, so you can't put it on paper but you could take it out -- I mean give me 15 minutes and I'll do it for you but why then is HT-7 in there at a higher a bolt pre-load? I mean you varied two things at the same time.

Apparently you don't think bolt pre-load is an important variable because you've got no test that shows you just the effect of bolt pre-load.

MR. PARKS: The bolt pre-load should be a fairly straightforward effect on the separation pressure.

MR. SIESS: But if I believe that, I don't need to make a test. That's an obvious assumption but we make tests to find out whether our assumptions are right.

MR. PARKS: That's correct.

MR. SIESS: If I looked at the tests, I'd say you don't think bolt pre-load is very important or uncertain. Of course, you know, it isn't uncertain here. You've got a strain gauge on there and if you believe the strain gauge, you know the pre-load but if you didn't have the strain gauge on it and had nothing but your torque wrench, you wouldn't know it.

I guess it just bothers me that you varied this thing but you also -- if you're not going to vary it systematically, I wouldn't have varied it at all. I'd just have varied the other things and kept that one constant and then I could have gotten a few more other variables there.

MR. PARKS: Okay, well your point is well taken.

MR. SIESS: Obviously, it doesn't make any difference when you get down to this and drawing the CDF but --

MR. WARD: Fred, I guess I still don't understand the difference between 10 bolts and 20 bolts. Is the head so stiff that there isn't any significant deflection between bolts?

MR. PARKS: Between bolts?

MR. WARD: Yes.

MR. PARKS: I would think not. I would think that --

MR. WARD: Okay. Well, it's not obvious to me but it is obvious to you that there's no significant deflection?

MR. PARKS: I wouldn't think so, no.

MR. WARD: So the only difference between 10 and 20 bolts is the pre-load or the bolt total strength?

MR. SIESS: There is a pretty good ring out there.

MR. PARKS: Right.

MR. SIESS: You see again, you can't get it -- you can't find two tests where that's the only variable.

MR. WARD: Yes, you can. Test 2 and 3. It's H2 and H3.

MR. SIESS: I'm sorry. You're right.

MR. WARD: That's also the difference in the pre-load if you assume that's all that amounts to but it's kind of a small difference.

MR. SIESS: No. That's what I was looking at. Two and three then.

MR. PARKS: Okay. We plotted displacement. What this actually is is the amount of separation between the sealing surfaces versus pressure for the four tests. What we're really trying, one important point, to show here is that the initial onset of leakage in each case occurred when the separation was within 1 standard deviation of the amount of springback that the seals have.

MR. SIESS: I'm having a little problem figuring out what's on there. There's solid lines and dashed lines but I don't see a legend.

MR. PARKS: At that scale, it's very difficult to find a legend or define the legend. Whatever. The solid line is a calculated response.

MR. SIESS: Okay.

MR. PARKS: The dashed line is the actual measured response.

MR. SIESS: Oh but you didn't try to calculate that unloading; did you?

MR. PARKS: No.

MR. SIESS: If you had, what would it have been, just a straight line back to the origin?

MR. PARKS: I would think so.

MR. SIESS: I guess I don't understand what's plotted there when they don't -- oh, I'm sorry. You've got something added on this one that I haven't got here.

MR. PARKS: Well, this -- what actually happened in this particular test was when we were going from 90 to 95 p.s.i., there was -- a thunderstorm moved through the area and we were actually unable to get a complete data scan at this point and I'm just postulating what it would have been at 95 p.s.i. We were able to measure the leakage from the flow meters because we get pretty much of a continuous output from the flow meters but we weren't able to scan all the data channels and get all the displacement readings.

MR. SIESS: The calculated went all the way up to some load that you didn't get the tests up to.

MR. PARKS: Right. The test was stopped once we developed this significant leakage in the seals. We didn't want to continue to increase the pressure, in some cases because we were developing inelastic strains in the bolts and we like to be able to reuse the bolts.

MR. SIESS: That first one is lousy but the two bottom ones look pretty good. The first break is what?

MR. PARKS: Which break? This one here?

MR. SIESS: Yes.

MR. PARKS: This is separation of the -- relief of the pre-load.

MR. SIESS: That's relief of the pre-load.

MR. PARKS: Right.

MR. SIESS: Now you only have -- you have a second break on the northeast one up there.

MR. PARKS: This one up here? This is when we actually started getting into the inelastic range on the bolts and were in the plateau region on the bolts, in the plastic region. They're starting to allow the hatch to move out at a much faster rate than when they were elastic.

MR. SIESS: Since you haven't exceeded the springback, these all stay linear and independent of the seal.

MR. PARKS: Linear and independent of the seal did you say?

MR. SIESS: I mean everything's nice and linear.

MR. PARKS: The calculated values, yes.

MR. SIESS: You haven't cleared the seal.

MR. PARKS: We haven't developed a gap between the flange.

MR. SIESS: In any of these, have you?

MR. PARKS: That's when we predict leakage to begin is when that gap would occur. That's just the dotted line going vertically here.

MR. SIESS: I'm having an awful difficult time reading. The vertical scale is displacement.

MR. PARKS: Yes. It's actually more correctly defined as a separation between the flanges.

MR. SIESS: Now, have we got some plots that show leakage?

MR. PARKS: No.

MR. SIESS: That's what you're trying to predict, isn't it?

MR. PARKS: We would like to be able to predict the growth of leakage also as we showed on the previous table. So far we haven't been able to do a very good job with that.

MR. SIESS: What would constitute a good job? How close?

MR. PARKS: With leakage a lot of times you're trying to stay within -- if you're within an order of a magnitude, you're not doing too --

MR. SIESS: That close, you think?

MR. PARKS: Well, for one test we were, the first test, we didn't -- when we measured 25, we calculated 80. For the last test, we were predicting 570 s.c.f.m. and we hadn't even developed a leak at that point.

MR. SIESS: Is that test plotted over here?

MR. PARKS: That's the one in the lower right hand corner.

MR. SIESS: You predicted it would leak 570 at what pressure?

MR. PARKS: At 166 p.s.i.

MR. SIESS: Can you find 166 on there?

MR. PARKS: A 166 is out herein somewhere.

MR. SIESS: Each tick is 10 -- 166, okay.

MR. PARKS: Each tick I think is actually five.

MR. SIESS: Yes, five. You got just a little above 166?

MR. PARKS: We got up to 180. Again, we stopped because of the inelastic strains in the bolts.

MR. SIESS: And didn't get any leakage.

MR. PARKS: Up to 180, we got no leakage.

MR. SIESS: Now, to what do you attribute that? Which if your calculations -- which of your assumed parameters is wrong or is it a combination of them?

MR. PARKS: I can only speculate at this point. My guess is these seals were aged in that particular test. The aged seals get very soft and pliable and it very well could be that they're actually pushed to the side of that tongue and actually stopped up the leak path on the outer side of the tongue.

MR. SIESS: So you think the error is in the seals.

MR. PARKS: That would be my guess, yes.

MR. SIESS: It's your calculation which is sort of an elastic calculation with springback. It doesn't represent what really happened.

MR. PARKS: That's my speculation at this time, yes. A lot of the others, particularly in the inflatable seals test, the aged seals a lot of times actually did better than the unaged because they were softened up a little bit and they were more pliable and a little bit sticky.

MR. SIESS: I get some comfort out of the fact that you're always in the conservative direction. I'm afraid that wouldn't help the risk analysts.

MR. WYLIE: Is that true of silicon rubber as well as EP?

MR. PARKS: The improvement in the aged?

MR. WYLIE: No, I mean that they soften and get sticky.

MR. PARKS: I wasn't around when we actually did the compression seal and gasket test to actually see what they look like. I don't know. I don't remember seeing anything in the report that said -- I think they tended to get more brittle with aging where the EPM materials tend to soften, very pliable.

MR. WYLIE: I notice you -- of course, you couldn't age these with radiation and you compensated by additional thermal aging. Is that a good simulation?

MR. PARKS: The radiation aging?

MR. WYLIE: Yes. On these materials. I thought they didn't seem to dry out and get hard with radiation.

MR. PARKS: The parameter that we're shooting for here when we're trying to simulate radiation aging with thermal aging is a compression set retention. It's a similar phenomenon as a spring-back. Once you deform the seals, it can spring back. With 200 megarads of radiation, which is normally applied to radiation aged something, you lose almost all the spring-back. It's about 95 percent compression set retention.

What we're trying to do in thermal aging is to thermal age enough so that we get about 95 percent compression set retention. That's what we were shooting for here.

MR. SIESS: Now, I'm looking back at those tests. Getting up to 180-200 psi and getting no leakage negligible, that's getting up pressures pretty high compared to anything we're talking about. There are some tests at 95 and 115. Both of those were at the bolts, right? HT-1 and HT-2?

MR. PARKS: Right.

MR. SIESS: So if all the bolts are in, you just don't get much leakage here. Even though you predict it in some cases, you don't get it. Even so, you would predict an initiation at a fairly high pressure which you wouldn't expect to get to.

The leakage would be directly proportional to the opening?

MR. PARKS: Right.

MR. SIESS: So if I look at the curves on displacement, that's a picture of what the leakage should look like.

MR. PARKS: And the way it should grow.

MR. SIESS: Yes. Now, this doesn't take into account any distortion in the shell.

MR. PARKS: No, it does not. The feeding equipment hatch is a pretty good distance away from the shell itself.

MR. SIESS: Is it that way in all the -- see, I think your equipment hatch is an oddball, isn't it, in the model?

MR. PARKS: The equipment hatches that I've seen have been some distance away from the shell.

MR. VON RIESEMANN: They can vary in actual practice. The distance from the containment wall --

MR. SIESS: See, this one looks like a personnel hatch where you've got the cylinder inserted in there. In the PWRs, the equipment hatch frequently is just --

MR. VON RIESEMANN: Particularly, the pressure seeding is almost flush.

MR. SIESS: The pressure seeding is flush and I've never seen an unseeding, I guess.

MR. VON RIESEMANN: They vary in design.

MR. SIESS: So they could be conceivably effected by the --

MR. VON RIESEMANN: On top of the effect of the pressure, right.

MR. SIESS: Again, I think if there is a deficiency here, it's not knowing the range of what the parameters are out there in real life. I think that would bother me more than anything else. Again, these are pretty high pressures.

MR. PARKS: I think everything that's up here has probably been mentioned.

MR. SIESS: Have you got any idea whether these things occur more in seal containments or concrete containments or vice versa?

MR. PARKS: I'm sorry. I didn't hear you.

MR. SIESS: Whether the pressure unseeding hatch is used more in steel or concrete containments.

MR. VON RIESEMANN: My guess would be it's in concrete, but I'd have to do a back check on that.

MR. SIESS: I think that somebody ought to do a survey just to find out what's where. If Argon didn't do it, it certainly could be done now.

MR. COSTELLO: Jim Costello from the NRC staff. I think we can quickly go back and at least retab what was done in the Argon survey, which was not all plants, but it was attempting at a significant cross section. Then, perhaps, we can look a little closer at some of the others.

MR. SIESS: Somebody at the NRC -- maybe the EDO could do it.

MR. WARD: You might want to contact Wayne Hodges, who was involved with GI-99. Supposedly, they were going to make this sort of survey and report back to us, but we haven't heard anything.

MR. COSTELLO: Thank you very much.

MR. PARKS: I think all of these have been mentioned before. I'll quickly go through them again. In three of the four tests, the significant leakage began when the separation was within one standard deviation of the main available spring-back.

From this, we've assumed that the spring-back is at least a reasonably accurate method of the gasket performance, because we had fairly good agreement between the analytical method and the measured results. At least the average response can be used with the available spring-back to predict initiation of leakage.

It seems the leakage is fairly sensitive to the available spring-back and, in most cases, we've actually over-estimated the actual leakage that occurred using our method.

MR. SIESS: You made four tests and you've got six more to make. Is that it?

MR. PARKS: Six or seven. I think there's actually seven.

MR. SIESS: It goes up to eleven. You've made one, two, three, four?

MR. PARKS: Right. HT-1 through 4.

MR. SIESS: Did I miss something earlier about the mean available spring-back? You made tests --

MR. PARKS: We actually measured the available spring-back of the gaskets with them in place in the equipment hatch. We know what the deformation should be with the hatch in place. We removed the hatch and measured where the seals have rebounded to. From that, we can estimate what the actual spring-back is.

MR. SIESS: And that's a stiffness measure, expressed as a stiffness?

MR. PARKS: Well, what we're actually measuring is just the actual shape of the seals once they have returned to their undeformed condition.

MR. SIESS: But one standard deviation of the mean available spring-back, somewhere there's a number in there.

MR. PARKS: What we've actually done --

MR. SIESS: The mean spring-back is a number?

MR. PARKS: It's an average of ten or twelve measurements around the circumference.

MR. SIESS: What are the units?

MR. PARKS: Inches; mils of inches, thousandths of inches.

MR. SIESS: It's actual dimension.

MR. PARKS: Right.

MR. SIESS: What was the coefficient of variation in your tests?

MR. PARKS: The coefficient variation on this particular measurement?

MR. SIESS: Yes.

MR. PARKS: I have --

MR. SIESS: I'm trying to get a feel for what one standard deviation is.

MR. PARKS: There's one table here where we've reported what the standard deviation is, this last row here.

MR. SIESS: Thank you. I missed that.

MR. PARKS: If there are no more questions about the equipment hatch test --

MR. SIESS: Still, I guess I'm a little bothered that I didn't see in that -- let me find it again -- leakage is very sensitive to the available spring-back. I guess the bolt stiffness or the bolt prestress is nowhere mentioned in here as an important factor. I would think that that is just as important as the available spring-back, isn't it?

MR. PARKS: On the leakage phenomena, yes. It is important.

MR. SIESS: I just don't see it mentioned under this. That's because I think you didn't think it was variable.

MR. PARKS: We felt we had a good control over what we were actually applying in the test.

MR. SIESS: And you do in the test, but you don't in practice.

MR. PARKS: That's correct. There is more variability in the real world than what we have here, obviously. Moving on to bellows. Some quick background information. Bellows are primarily used just in steel containments to minimize the popping loads that are imposed on the containment shell due to deferential movement between the shell and the pipe.

The two main types of bellows are the vent line bellows that are used only on Mark I containments and the other being processed piping bellows. The processed piping bellows are used on all types of steel containments, either BWR and PWR. All the bellows, at least that we surveyed, have been constructed of Type 304 stainless steel.

The next figure in the presentation shows the typical --

MR. SIESS: Somewhere, do you have a stress strain curve for 304 stainless?

MR. PARKS: Not in here I don't.

MR. SIESS: What does it look like? It is a highly ductile material?

MR. WARD: Yes. Very ductile.

MR. PARKS: Yes. Very ductile. It's gradual yielding type.

MR. SIESS: It's rounded curve.

MR. VON RIESEMANN: It is sensitive to, as you know, the forming. The yield depends very much on the process on which you form the bellows.

MR. SIESS: And the ductility also depends on it. I guess you use up a lot of ductility.

MR. VON RIESEMANN: Yes.

MR. PARKS: This is a cross section through a typical Mark I containment, just to show you the relative locations of the bellows. The vent line bellows are at the penetration of the vent line into the suppression chamber and the processed piping type bellows -- a typical application of the feedwater lines and the main steam lines.

MR. SIESS: Up there, you've got the main steam there. Put your pointer up there. Move it a little bit to the left. What is that thing?

MR. PARKS: This particular drawing, I don't --

MR. VON RIESEMANN: It's through the concrete and through the steel. I imagine that's what it's showing. The concrete backing on a BWR.

MR. SIESS: No. On the inside. That looks like a pressure seeding hatch.

MR. VON RIESEMANN: Okay.

MR. PARKS: For bellows, it's --

MR. SIESS: I don't think it's got anything to do with the bellows.

MR. VON RIESEMANN: It doesn't.

MR. SIESS: I think it's a picture of a pressure seeding hatch.

MR. PARKS: Could be.

MR. SIESS: I was speculating that the hatch would be on the outside in a Mark I because there is more room. I don't see any good reason for putting those hatches where they do.

MR. PARKS: A quick look at the vent line bellows. In most cases, the bellows are actually outside the suppression chamber, as was shown here. This is actually inside the suppression chamber.

Normally, the vent line bellows are what we call universal bellows or two bellows connected in series by common center spool. There are a few cases in which these bellows are actually -- out here like I've shown them -- inside the suppression chamber.

For the case that we show here, both of the bellows would be subjected to whatever the pressure environment of the suppression chamber would be. They are actually a part of the containment pressure boundary.

As the suppression chamber is pressurized and moves out radially, it tends to compress the bellows and because these vent line bellows are not perfectly radial to the suppression chamber, there will also be a lateral component imposed on the bellow.

MR. SIESS: That plate that's outside the bellows, that's just physical protection against normal --

MR. PARKS: This is just a protective cover.

MR. SIESS: Keep somebody from stepping on them.

MR. PARKS: Or keep something from being dropped on the bellows and damaging them.

Here we have a very similar, somewhat similar geometry for the process piping bellows.

One difference is that there is normally a guard pipe between the process pipe and the bellows, just to protect the bellows in cases of severe rupture of the pipe in this area.

Again, normally these bellows are outside the containment shield, so pressurization of the containment in its radial growth tends to compress the bellows. Any vertical growth of the containment poses a lateral load on this type of bellows.

MR. SIESS: Now, our concern is the possibility that the bellows will fail under severe accident conditions of temperature and pressure. Is that right?

MR. PARKS: Right. Probably the most important manifestation of a severe accident on the bellows is the actual deformation of the containment shell as it moves out due to its internal pressure, and this would tend to deform the bellows. And that would be more likely to cause the bellows to fail in the pressure.

MR. SIESS: So temperature can't be ignored, so the properties of the material change with temperature, but the temperature itself probably doesn't produce enough deformation to --

MR. PARKS: To hurt the bellows.

MR. SIESS: -- to hurt the bellows, as far as the bellows is concerned.

MR. PARKS: Right.

MR. SIESS: So you are really concerned about the, concerned that the movements may be larger than the bellows can accommodate?

MR. PARKS: That's right. And I will look at some speculation as to what would happen during a severe accident.

MR. SIESS: It is designed to accommodate what kind of movements?

MR. PARKS: Okay. That is the next slide here.

MR. SIESS: Okay.

MR. PARKS: Okay. The design conditions for these penetration bellows are normally supplied by the A&E for the containment. These conditions consist of pressure, axial deflection, lateral deflection, some small amount of rotation due to bending, and in just a very, very few cases there is actually a design rotation allowed in the torsional direction on the bellows.

The actual magnitudes of these conditions are based on a worst-case combination of the normal operating conditions, the design earthquake and LOCA conditions.

MR. SIESS: Are you going to show us how severe accident conditions compare to those design conditions in one of the slides?

MR. PARKS: I will mention it, yes.

The objectives of the containment program.

The first objective, that is perhaps obvious, is determine if the penetration bellows are a possible mode of failure during a severe accident.

And then, given that they are a contender, or a possible mode of failure, then we would like to be able to develop some methods to estimate what severe accident condition would likely cause the bellows failure. And in our efforts to accomplish this second objective, we have conducted a literature search of all past efforts regarding bellows; we have conducted some finite element analyses; and we are down, to get ahead of myself a little bit, we are down to this point here, getting ready to conduct some additional testing.

MR. SIESS: Going back to the design conditions, that were was axial and lateral deflection and rotation. Are those for design dominated by movements of the piping outside of the vessel?

MR. PARKS: From talking to the bellows manufacturers and some A&Es, those design conditions are primarily motivated by the movement of the containment shell itself. Normally, the process pipe is anchored in the shield building, which is just a short distance away from the containment.

MR. SIESS: Anchored?

MR. PARKS: Right. Rigidly anchored. So that the process pipe won't move.

MR. SIESS: Normal movements, then --

MR. PARKS: -- movements of the pipe?

MR. SIESS: Yes.

MR. PARKS: As far as affecting the bellows?

MR. SIESS: This is one of the things we worry about in the seismic stumpers, and all of that stuff, is anchoring pipe so that it can't take the thermal movement. Now you are telling me a steam line is coming out of the containment and being anchored just outside of the --

MR. PARKS: In the shield building.

MR. SIESS: Drywell?

MR. PARKS: Yes.

MR. SIESS: Literally anchored?

MR. PARKS: Yes.

MR. SIESS: I don't know what you mean by fuel building,

MR. PARKS: Shield building. Shield building.

MR. SIESS: Shield building.

MR. PARKS: Yes.

MR. SIESS: Shield building. You mean the containment reactor building for the BWR?

MR. VON RIESEMANN: Shield containments. And on the outside you have concrete.

MR. SIESS: This is a Mark-1 we are looking at in the example.

MR. VON RIESEMANN: Yes.

MR. PARKS: All steel containment.

MR. SIESS: Yes. But all steel containments aren't the same. I was looking at a Mark-1, where you have pipes that come out of the drywell and go considerable distances through various things. You have feedwater pipes; you have recirc steam lines, and all of that stuff. And I can't believe that there are thermal movements on rod force on that.

MR. VON RIESEMANN: What Brad is mentioning is on the steel containments.

MR. SIESS: That's something else.

MR. VON RIESEMANN: That's different. That is where they are anchored.

MR. SIESS: There are steel containments that have a concrete shield building five feet outside of them; there are steel containments that don't have a concrete shield building anywhere near them.

MR. VON RIESEMANN: Right.

MR. SIESS: Some of them are still out there. Everybody didn't have a shield building.

So I was looking back at the Mark-1, because I'm looking at things like water hammer, that might test bellows. There are lots of bellows, and they've been in there and they've been subjected to a lot of movement. And I'm trying to figure, you know, what's happened in the past.

MR. BENDER: What he is describing is a common practice that was developed just to accommodate these bellows. Because they couldn't stand very much movement with relationship to the pipe, the pipe had to be fixed at a very short distance from the bellows. That is just the way they were designed. They have movement on the other end.

MR. SIESS: If I go to a large dry with a steel containment, and I have pipes that go through it, they extend inside, they extend outside.

MR. VON RIESEMANN: There is flexibility in the inside and the outside to take care of long-term growth.

MR. SIESS: Yes.

MR. VON RIESEMANN: They have loops.

MR. SIESS: But if they are anchored five feet outside -- that is what I was getting at. How much can those pipes move. The designs are designs for axial deflection, lateral deflection, rotation due to bending, rotation due to torsion. Now, all of those movements are movements of the pipe relative to the containment.

MR. PARKS: Right.

MR. SIESS: If I am looking at a large dry, it can move, but not very large, compared to what the pipe moves. So most of that movement is in the pipe.

And so I was trying to figure out how much of the axial movement is from the pipe thermal movement, how much might be from water hammer, or whatever or whatever.

MR. PARKS: I don't know the breakdown. Maybe if I give you some idea of the magnitudes of these design conditions.

MR. BENDER: Nothing but thermal movement is dealt with in this.

MR. SIESS: Seismic. It says SSE.

MR. PARKS: The bellows are not designed to deal with that.

MR. BENDER: They just don't analyze for it, that is all I am saying.

MR. SIESS: Yes.

MR. BENDER: Whether it is there or not.

MR. SIESS: But it says here, the worst-case combination of normal operating, which would involve thermal; SSE, and how they get that, I'm not sure. Movement, particularly. They would have to analyze the piping, find some fixed point and see how much the earthquake moved it from there. And I've never seen a seismic analysis that did that sort of thing. And LOCA. Of course, you don't get much containment growth in LOCA pressure to what we are talking about in severe accident.

I guess what you said -- Go ahead, and let's see what comes out of this.

MR. PARKS: Design conditions compared to what would happen in a severe accident are very, very small, the actual magnitudes of the deformation.

MR. SIESS: Yes. That, I know.

MR. PARKS: Okay.

MR. SIESS: But what I am trying to consider is whether there are service conditions that they didn't design for that may not be very, very small.

Water hammers, for example, have been known to produce forces and movements that weren't designed for, but that are probably worse than anything we are going to see in a severe accident. Not necessarily for bellows, but for other things.

MR. PARKS: All right. This figure here is meant to give you an idea of what types of loadings the bellows would be subjected to during an overpressurization of the containment.

What we plotted here on the Y-axis is actual bellows in displacement.

MR. SIESS: These are just now the radial displacement of the containment due to these pressures?

MR. PARKS: Well, the axial compression actually imposed on the bellows is due to the radial growth.

MR. SIESS: Okay.

MR. PARKS: Okay. I have also plotted the lateral deformation that would be imposed on the bellows due to vertical growth of the containment.

MR. SIESS: And that is pretty high up?

MR. PARKS: Right. What I have shown here, this E-sub-L is the original undeformed length of the bellows. That is 12 inches, 18 inches, whatever. And on the X-axis we have plotted containment pressure.

Now, to the design pressure level, relatively speaking, the imposed axial compression and lateral deformation are relatively small. The containment is still in its elastic range.

Once we begin to yield the containment in the hoop direction, obviously the radial growth starts increasing more and more rapidly. At some pressure level above this yield pressure, the radial growth of the containment will be large enough so that the imposed axial compression is equal to whatever the original length of the bellows were. At that point, the bellows are fully squashed, or fully compressed. And we note that we have also got some simultaneously-applied lateral deformation, albeit relatively small, but it could be significant.

MR. SIESS: Now, the bellows is always oriented such that it would be compressed by a containment?

MR. PARKS: Not always. I don't know what the percentage is. In the vast majority of the cases, it is. But in a few cases, the bellows is actually inside the containment shield so that you can take all these loadings and reverse them.

In other words, rather than being compressed, the bellows are elongated, and they are subjected to external pressure rather than internal pressure.

MR. SIESS: If I have a bellows 12 inches long, I can't compress it 12 inches.

MR. PARKS: No. It is 12 inches minus whatever all those layer thicknesses happen to be.

MR. SIESS: What am I talking about?

MR. PARKS: Instead of 12 inches, it might be 11.2 or something like that. This is very thin material.

MR. SIESS: Okay. And how much can I expand it?

MR. PARKS: Well, normally, it depends on the convolution depth, obviously. Normally, the bellows people tell you about three times the length before it is fully extended.

MR. SIESS: So if this were the other direction, I would have a lot more room than the three times the length.

MR. PARKS: Before you fully extended it. Now, whether the bellows wouldn't develop a crack before it could be fully extended is what we are trying to find out.

MR. SIESS: That applies either way, doesn't it?

MR. PARKS: Sure. Correct. That is the next question that I am going to pose here.

Perhaps I will go ahead to the next page.

The obvious next question might be that, how does this pressure level which the bellows are fully compressed compare to the other possible failure modes of the containment shell?

Looking at the Sequoyah containment, as mentioned yesterday, there has been a finite element analysis of the Sequoyah containment shell. Rupture of the shell was predicted at 75 psi for the Sequoyah containment. At 74 psi, based on the same results from that same analysis, the radial growth of the containment will be such that the bellows are fully compressed.

So based on that conclusion, at least at Sequoyah, the bellows failure is definitely a possibility.

One thing I haven't mentioned, we think once the bellows are fully compressed, there will surely be a tear develop in the bellows material, the cutting of the end spools of the material itself. So that is at least an ultimate capability at that point.

MR. SIESS: Is containment deformation the only source of movement that you consider important?

MR. PARKS: For the severe accident conditions, yes.

MR. SIESS: A severe accident couldn't cause any pipe movement that would be significant?

Is there any way a pipe break could cause a bellows failure that would provide an opening to the outside?

MR. VON RIESEMANN: If you have an internal structure failure, say, I think that is what you are getting at, and you have a pipe failure.

MR. SIESS: A lot of our severe accident start with a pipe failure.

MR. VON RIESEMANN: Then you have to look to flexibility of that whole line and see what load it would put on the bellows. But we haven't done that yet. The information we have could be used for that.

MR. SIESS: So really, you are looking at how much deformation the bellows can take, but you are using to get some idea of how much you might have, looking at containment movements.

MR. VON RIESEMANN: Right.

MR. SIESS: There are other sources that might have to be looked at.

MR. VON RIESEMANN: Sure.

MR. SIESS: Okay.

MR. PARKS: That is correct.

Okay. One point that you mentioned is, will the bellows remain leak-tight up until the point they are fully compressed. And I'm beginning to get ahead of myself. We really, we don't have any past information to tell us about the ultimate compression.

MR. SIESS: The manufacturers have never tested bellows in compression, in tensions and failure?

MR. PARKS: They tell me that they have tested bellows in which they fully compressed them with no lateral offset. And in most cases they won't leak. They won't give you that information, though. They won't document it. They will tell you that they have done the test.

As far as the simultaneous application of compression and lateral offset and some rotation, there is no documented cases of that, either.

MR. BENDER: Well, a lot of that, unless you can deal with it in the circumstances, interpreting the results is not very useful.

At most you will just get a crack in the bellows, unless you have a major offset of the pipe.

MR. PARKS: To quickly review our efforts to date, we have completed a preliminary study. We've nearly conducted a worldwide search for bellows test data and past analytical investigation of bellows behavior. The conclusion of this investigation was that we really have not been able to find any test data that is applicable to this situation which really defines what the ultimate capabilities are.

I mentioned also that we conducted a finite element analysis. We've just tried to follow the bellows as far as we could go. We got to the point where they were about half way compressed, and we're stuck and can't go any further than that.

MR. MARK: Are the designs of those in the Japanese, German and French programs, similar to the ones you showed us?

MR. PARKS: The Japanese bellows; the basic concept is the same, yes. The Japanese tend to use bellows both inside and outside the containment. They have bellows outside, like I have shown, and they also have another bellows inside Other than that, the material that's most commonly used is the type 316 stainless steel.

MR. MARK: So the figure you might get would be applicable to them, or their data would be applicable for you?

MR. PARKS: Right, and we have been in contact with Japanese representatives and they say they don't have that type of data.

MR. WARD: Brad, when you say that they're both inside and outside, is that providing redundancy, or is that just half the motion is taken up by one and half by the other?

MR. PARKS: No, they're both fully subjected to the full motion of the containment. The indication that they give me is that those two bellows are used on either side of the penetration, just so that they can provide a continuous leak test of the penetration, as I was saying, pressurize that cavity and as long as the pressure remains constant, that penetration is not leaking.

MR. MARK: Okay, but would both have to fail before you'd get leakage?

MR. PARKS: In that case, yes. The first layer of defense is that inside bellows, and if that failed, then that leakage would pass to the outside one.

I think that I've already given this away, but the conclusions for the preliminary study are that we can rule out containment penetration bellows as a possible mode of failure, and because of the fact that these other efforts have been unsuccessful, we're only left with the alternative of conduction some additional testing of typical bellows.

MR. SIESS: Your finite element analysis just breaks down.

MR. PARKS: We wer [illegible]

MR. SIESS: What I am getting at is; the test program can be very extensive and test everything that's available, every possible thing, or the test program can be just enough to get data to --

MR. PARKS: Validate it.

MR. SIESS: -- validate a finite element analysis or any other kind of analysis. I'm not sure it has to be finite element. It might be a good empirical correlation of some kind.

MR. PARKS: It may be that in the worst case combinations of lateral deformation and axial compression, these bellows will still remain tight until they're fully compressed. If that's the case, then the problem is solved and there's no need for any sophisticated analytical methods.

But if the thing starts to develop a tear somewhere in between there, then we have to take a closer look at it.

MR. SIESS: I'm not really worried about cyclic loading that much.

MR. PARKS: For a severe accident, not. The design movements are so small that we don't think there's any fatigue damage to the bellows.

MR. SIESS: Your test program, which is your next slide, --

MR. PARKS: There's actually one more between there, but it's --

MR. SIESS: That's a fairly simple setup until you get to B, at the external pressure.

MR. PARKS: Basically, they've got to build a box around the bellows, I think.

MR. SIESS: Otherwise, all you've got to do is put a test rig on that puts the thing in like this and pull it or push it or whatever.

MR. PARKS: And move the bottom of the test.

MR. SIESS: If it's internal pressure, that's not too much of a problem; you could run it through the test rig. I think you could stop before you got to external pressure if you got decent results.

MR. PARKS: Possibly so, or maybe we can do the test where we elongate the bellows with simultaneous lateral offset and show that the induced stress is due external pressure is insignificant. That's not too bad ---

MR. SIESS: Do you really think that the internal pressure had a big effect on a failure?

MR. PARKS: No, I don't think so.

MR. SIESS: We're just moving this stuff through such god-awful deformations that -- that pressure is just putting --

MR. PARKS: A little additional stress on it. The primary mode of failure, I think, would be as a result of this extreme deformation. The pressures induced by the stress, induced by the pressure is not that great.

MR. SIESS: No. Now, what you're be planning is simultaneous axially and laterally?

MR. PARKS: Right.

MR. SIESS: That's what you'd expect to get, right?

MR. PARKS: That's the main objective of the test.

MR. SIESS: The object of the containment movement -

MR. PARKS: Right.

MR. SIESS: Whatever produces the lateral, whether it's the vertical movement of the containment or some internal structure movement, they would be correlated?

MR. PARKS: Right.

MR. SIESS: So you couldn't do this and then this.

MR. PARKS: No.

MR. SIESS: That's the easiest way to test it -- simultaneously.

MR. PARKS: I think that concludes the penetration.

MR. WARD: Could you go back to this picture for a minute.

MR. PARKS: The vent line bellows?

MR. WARD: Yes. Now, if the bellows fails, the leakage you get out, if the bellows tears, depends on the area -- the gap area between that protective sleeve and the little lip that's on there. That's a pretty difficult design.

MR. PARKS: This is not a real substantial structure and they really don't worry about that -- you know, how much gap you've got there. That might be something that's highly variable.

MR. WARD: So that really isn't something that can be counted on?

MR. PARKS: I wouldn't think so, no.

MR. SIESS: The gap between the pipe and the sleeve is not going to change.

MR. PARKS: Right, right here.

MR. BENDER: I think it all depends upon what the leak rate is. If it were just a crack in the bellows, you wouldn't need much strength in that to deal with the pressure, because that's not leak-tight closure.

But I just wondered what the typical design is. It could be designed so that it's robust and the leakage area there is small.

MR. PARKS: That's not the case. I have actually inspected some bellows and --

MR. SIESS: You can't rule out the fact that you cracked that bellows at one end and just blow the bellows out. You just can't assume that the only leak is --

MR. BENDER: I understand what you are saying, but back in the early days, there were long debates about the capability of that bellows to be blown out, and I think that subject has been looked at pretty carefully. I can't remember because it's 20 or 25 years ago or something.

MR. SIESS: I don't think they looked at severe accident deformation conditions where you're moving the containment several inches.

MR. BENDER: The pressures were light steam pressures.

MR. SIESS: No, this isn't a pressure problem. It's a deformation problem. I don't think anybody looked at bellows that would be completely collapsed.

MR. BENDER: This is obviously not the place to settle this.

MR. SIESS: I mean, 25 years ago, we certainly weren't putting severe accident loadings on containments.

MR. BENDER: There were conditions on the bellows that were even more severe, there were things like pipe --, for example, that had to be dealt with and deformation, but not this large.

MR. SIESS: You're talking about deformations of several inches that close the bellows.

MR. BENDER: What about pipe movement?

MR. SIESS: As a minimum, this would be eight tests -- a maximum of 8 tests, I guess. You've got four typical.

MR. PARKS: As a minimum, four tests. Right now, we have a contract out with various people to possibly conduct a test, and after -- depending on the cost of doing the test, ten we will fine-tune the actual test matrix.

MR. SIESS: What you're thinking of is that for processed pipe, that could be so big -- it could be this big, but I don't think you're thinking that big.

MR. PARKS: No, we're thinking about a 12-inch bellows.

MR. SIESS: So, it's scaled down for vent line.

MR. PARKS: The vent line bellows are huge. They're up to 10 feet in diameter.

MR. SIESS: Is there any reason to think that if you could get good test data on a 12-inch bellows that you --

MR. PARKS: That vent line bellows wouldn't --

MR. SIESS: That you'd have a pretty good feel for what was going to happen in the vent line? This would depend on the results.

MR. PARKS: It depends on the results. You have to look at things like the ratio of the convolution depth to the overall diameter and that affects the amount of code working actually in those convolutions, because with the large diameter bellows, if we're only going an inch with the convolution depth, there's not that much code working there.

MR. SIESS: See, if I'm worried about things like that, I don't have to test a whole bellows. I could take a piece of stainless steel that has that shape and do a fair amount of testing on that under fairly simple conditions, if

I'm just interested in the local effects. That's interesting.

MR. PARKS: That's all I have for the penetration.

MR. BENDER: Have you talked with your architect engineer consultants about this bellows behavior?

MR. PARKS: I have talked to them, yes.

MR. BENDER: How good is their memory?

MR. PARKS: They're the ones that supplied me with this some indication where the design conditions come from. When I asked them what would happen in a severe accident, they throw up their hands. They don't have any ideas. The bellows manufacturers don't have a good feel for what would happen under these combinations of --

MR. BENDER: You'd obviously have to give them some physical conditions to work with. Okay.

MR. SIESS: If there are no further questions, I'm going to declare a break.

[Brief recess.]

MR. SIESS: I'd like to get the opinion of the Subcommittee members.

The handout we just got is printed on one side of the paper, and some of them we have had have been printed on both sides of the paper. Now, from the standpoint of what you to haul around, take home, there is some advantage on printing it on both sides, but in going through it and trying to follow the discussion, I get lost when it's printed on both sides.

Now, which would you prefer?

MR. WARD: I seem to be able to handle both sides. I'm used to that.

MR. SIESS: Even though the staple is in the wrong corner, huh?

What about you, Charlie?

MR. WYLIE: I can handle it very well.

MR. SIESS: Well, then, maybe we ought to encourage people to use both sides of the paper so we can cut down on the briefcase weight.

MR. WARD: Especially at out-of-town meetings, yes.

MR. SIESS: Only out-of-town meetings. I don't care what you do in Washington.

Well, we settled that.

Okay. Future plans.

MR. PARKS: In this presentation, I'd like to first give you just an overview of all the future activities of the containment integrity programs and then talk specifically about the separate effects tests to investigate liner tearing that were mentioned yesterday afternoon.

As I mentioned, the primary emphasis here will be the separate effects test plans.

MR. SIESS: All of this relates to the model and to the other types of things we've talked about -- penetrations and seals and hatches.

MR. PARKS: Right.

MR. SIESS: This is future plans on relating to the integral tests, shall we say?

MR. PARKS: The separate effects tests is the primary emphasis, yes, to develop more information about liner tearing. There is no penetration or any other work in here.

MR. SIESS: Yes. Okay. I just wanted to get the scope clear.

MR. PARKS: Okay.

I mentioned the separate effects tests.

Re-test of the 1/6th scale model: As, perhaps, you're all away, as a result of the first overpressurization test, the containment failed as the result of a liner tear, which had sufficient area to prohibit any further increase in pressure of the liner. The basic structure is still intact.

There had been some speculation about what do we do with the model from this point forward. There's three different options that are being considered.

One is to repressurize the model. In this option, the -- some type of rubber membrane, rubber liner, would be used to seal the existing cracks, and then the pressure level would be retested, repressurized, hopefully to a higher pressure level than the first failure which occurred, and in that type of test, we would hope to develop more of a true structural failure of the containment.

Another option that is being considered is to do an aerosol test inside the containment.

MR. SIESS: Before you leave the first one, you would do other things to assure a structural failure? That is, you would do something about the hatches and the pipe penetrations, to be sure that they didn't go, or would you just fix the liner up and see what would go next? Because as I recall, the large equipment hatch was awful close to --

MR. PARKS: There was some leakage, we think, of the large equipment hatch --

MR. SIESS: Yes.

MR. PARKS: During the high pressurization test.

MR. SIESS: If you went much higher, you'd probably start leaking there.

MR. PARKS: That's a possibility, yes.

MR. SIESS: So, would you seal that off to get the structural failure, or would you just be honest and say now what's going to go next?

MR. PARKS: The proposals that I have been a part of, or at least, involved in to some extent, would just involve assuring that we didn't have the liner-tearing failure, and we'd repressurize the model. Whether the equipment hatches and other penetrations would be somehow fixed to prevent their leakage, I don't know if that topic has come up or not. We haven't looked at repressurizing the model in great detail at this point.

MR. SIESS: I think if you do, you ought to think it through a few scenarios. That is, if you fix up the leaks and you go up 2 psi higher and it starts leaking somewhere else, are you going to now go in and fix that leak? How far are you going to go to make the darn thing fail in shear down at the base, where we want it to fail?

MR. PARKS: Okay.

Another option that is being considered, I mentioned, is the aerosol testing, which aerosols would be generated inside containment, the effect of these aerosols on plugging up of the leakage through the cracks in the model, and it has been speculated that the aerosols might even stop -- plug up the existing cracks in the model such that the pressure could be increased.

MR. SIESS: Plugged the steel cracks, or plugged up pathways through the concrete, or both?

MR. PARKS: Both.

MR. SIESS: And this would be in lieu of, say, going in and injecting epoxy into the existing cracks, because that would seal up the next crack that occurs. Is that what you're thinking?

MR. PARKS: Yes. That was the thinking.

MR. SIESS: As I understand it, there are some incipient cracks in an awful lot of places.

MR. PARKS: Right.

MR. SIESS: So, it's not good enough just to patch the cracks that are there; you've got to take care of the one that's going to occur next.

MR. PARKS: Right.

MR. SIESS: Okay.

MR. PARKS: Again, this has been looked at, not in a great amount of detail, but it's just the considerations.

One other possibility that has been put forth is possibly doing some type of a hydrogen test inside the model.

MR. SIESS: Explosion.

MR. PARKS: The details I am not at all familiar with. I suppose it would be some sort of a detonation inside the containment.

All of these are very tentative options at this point. At this point, we don't have any firm direction from NRC as to which option they would choose of the three.

MR. SIESS: I assume you have ruled out putting a bladder in and filling it up with water.

MR. PARKS: That's not a consideration.

MR. SIESS: It's always possible.

Of course, those are options for re-test. There is, I think, to be weighted at the same time the non-re-test option and let's get in there and find out what happened --

MR. PARKS: Yes.

MR. SIESS: -- which is not minor. We might learn more from that than we do from the re-tests.

MR. PARKS: That has been considered, also.

MR. VON RIESEMANN: The peer review committee that we have is sort of mixed on what to do next. We have to meet with them and then with the NRC to find out what to do.

Obviously, there's pluses and minuses, no matter what option you take.

MR. SIESS: Well, I am not sure who is on your committee, but there's always -- if I wanted -- there is a strong desire to fail this thing structurally. You're a structural engineer. I am not particularly interested in the fact that liners crack. I got this thing up there, I'd like to make it go in some other way, but I'm not sure, if I were paying for it, I'd do it.

MR. PARKS: Another possible activity of the containment programs would be an establishment of a cooperative agreement between the Nuclear Power and Engineering Test Center, or NUPEC, as it's commonly referred to, which is from Japan, and the cooperative agreement would be between NUPEC and NRC/Sandia.

In November of last year, we had a meeting between NUPEC and NRC/Sandia. The programs of each side were presented, and from these programs and possible future programs of those sites, two areas of interest for possible cooperation were put forth.

On the NRC side, the primary interest was in the testing of a pre-stress containment. In this scenario, at least what is being talked about at this time, a pre-stress containment model would be constructed at Sandia. The primary design would be by NUPEC, with review by Sandia.

MR. SIESS: You said they would design it?

MR. PARKS: That is the plan.

MR. SIESS: Why are they interested in that?

MR. PARKS: Why are they interested in a pre-stress containment?

MR. SIESS: Yes.

MR. COSTELLO: Jim Costello, from the NRC staff.

Brad has given a very good summary of tentative negotiations that have been underway ever since the last workshop in Arlington, or Rosslyn, in '88.

There is what's probably the first of the major Japanese light-water reactor safety reliability testing programs operated by NUPEC, which is focusing on severe accident questions.

MR. SIESS: Existing plants or future plants?

MR. COSTELLO: Pretty much existing.

MR. SIESS: They have got pre-stress containments?

MR. COSTELLO: Yes, Sir.

MR. SIESS: And they are interested in severe-accident phenomena?

MR. COSTELLO: Yes.

MR. SIESS: That's fascinating. I just read the other day that they told the staff they weren't interested in severe accidents; they were too low probability.

MR. COSTELLO: Well, see, there is a formal regulatory interest and there is a research interest.

The Nuclear Power Engineering Test Center was the entity organized by the Minister of International Trade and Industry, to focus on safety and reliability of light-water reactors in existing and in-the-pipeline designs.

There is one, that I know of, pre-stress containment in Japan and more in the pipeline. I forget which plant has the existing.

MR. SIESS: Their next reactor is the ABWR, and that's not pre-stress.

MR. COSTELLO: No. The last part of the generation of PWRs does have a design that is not all that similar from a Bechtel design and sufficiently close to U.S. practice that we would consider it a very useful vehicle, should they be willing to provide its design and construction as their contribution, and so, these discussions are underway.

MR. SIESS: Now, their interest in BWR vessel heads and flanges is existing or future?

MR. COSTELLO: Existing. The formal name of the

Japanese test program, which encompasses these topics, is called the containment proving tests program. Apparently, all NUPEC program are entitled "proving tests".

MR. SIESS: Like in a CEGB.

MR. COSTELLO: And they are paying for a large amount of hydrogen work, as well, as part of this program.

MR. SIESS: That's interesting.

MR. COSTELLO: So, so far, there is still mutual interest on both sides, and we will probably come a little closer to resolution if the Commission should agree to an agreement sometime later this year. We'll keep you posted.

MR. PARKS: Okay.

The penetration tests that are planned -- we just finished the discussion on those items, the equipment hatch test and the doubles test.

Okay. The remainder of this presentation just deals a little bit with the planned separate effects test. Again, these separate effects tests are aimed at generating more data to investigate liner tearing.

MR. SIESS: Let me ask something.

There has been a lot of analytical work. Dave presented some yesterday.

MR. PARKS: Right.

MR. SIESS: We cut him off on part of it.

Has the analysis looked at what happens if you change the thickness of the liner?

MR. PARKS: Not at this point, no.

MR. VON RIESEMANN: We haven't done that yet.

MR. PARKS: The only analysis that I am aware of -- in that round-robin post-test report, there is some additional analysis, other than what Sandia did.

MR. SIESS: So, it's mainly been just analyzing what was there.

MR. VON RIESEMANN: This is what Randy Weatherby started, yes, and maybe Bob Dameron might mention a few things a little later about some work they have done.

MR. PARKS: Okay.

MR. SIESS: Just offhand, what was the scale of the prototype liner thickness?

MR. VON RIESEMANN: One-sixteenth, and the insert plate was 3/16ths. So, it was 3 to 1.

MR. SIESS: Three-sixteenths for the prototype.

MR. PARKS: For the insert plate, it's 3/16ths.

MR. SIESS: Oh, I'm sorry.

MR. VON RIESEMANN: Insert, yes.

MR. PARKS: The liner is 1/16th.

MR. SIESS: One-sixteenth in the model?

MR. VON RIESEMANN: Right.

MR. SIESS: So, that's 3/8ths in the prototype.

MR. PARKS: Right.

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IMAGE EVALUATION
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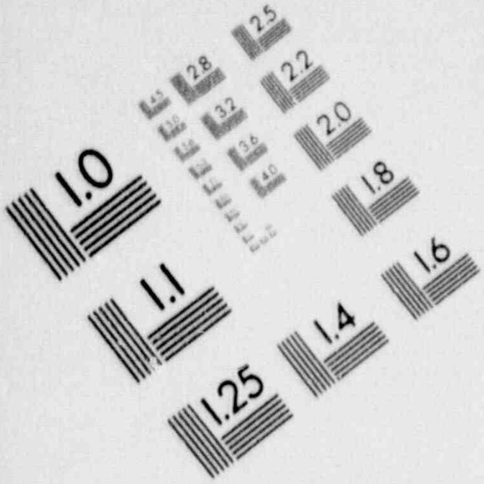

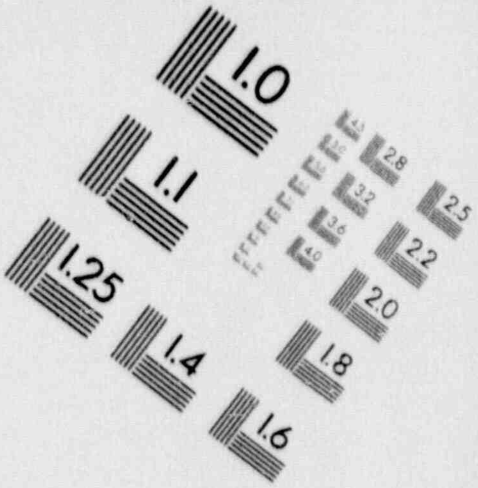

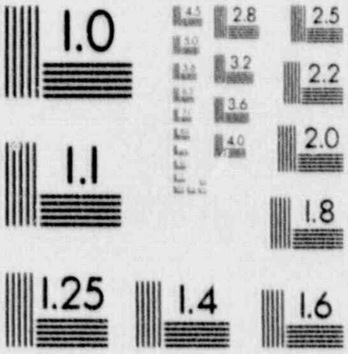

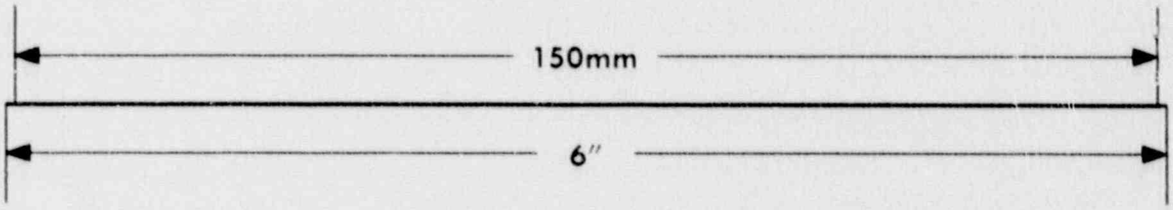

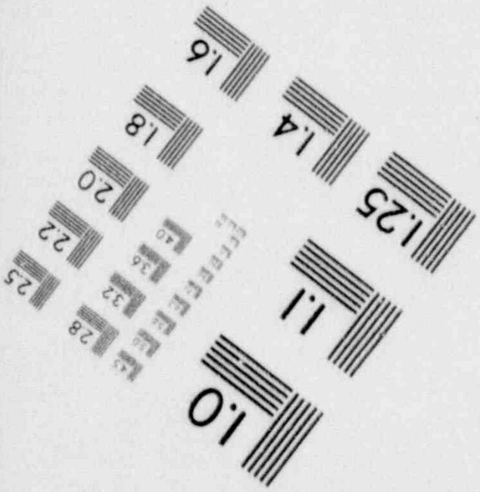

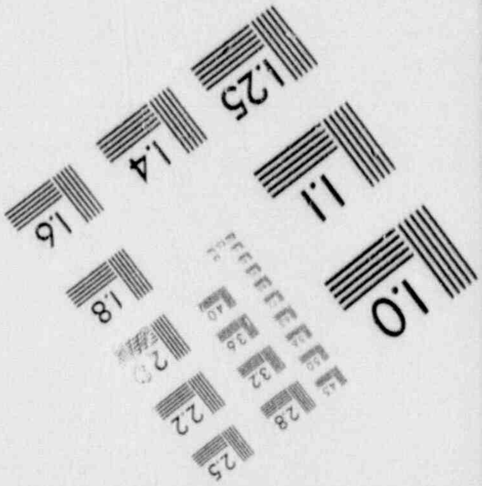

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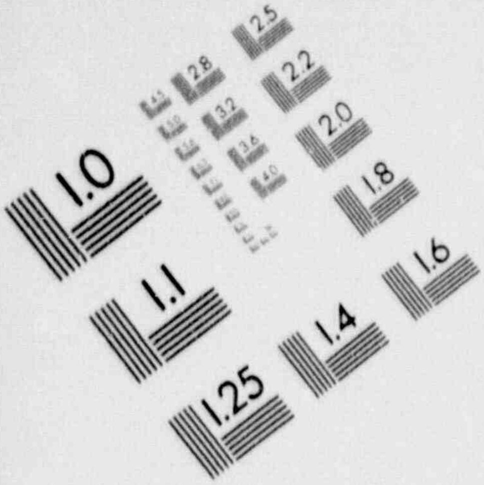

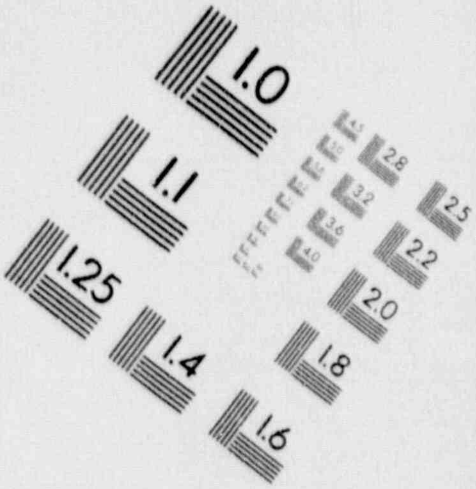

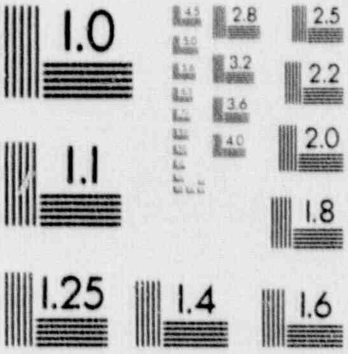

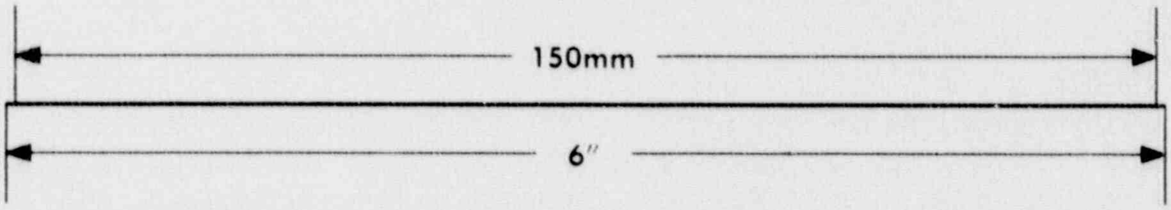

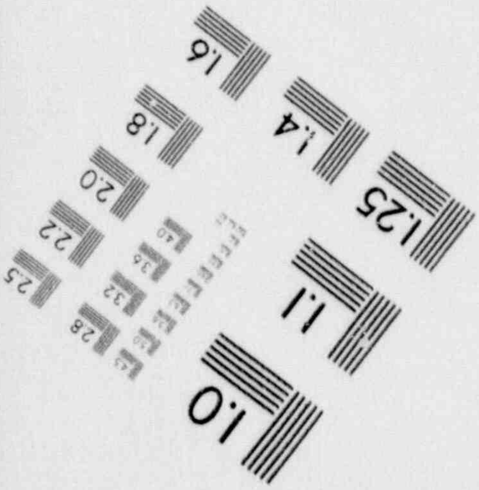

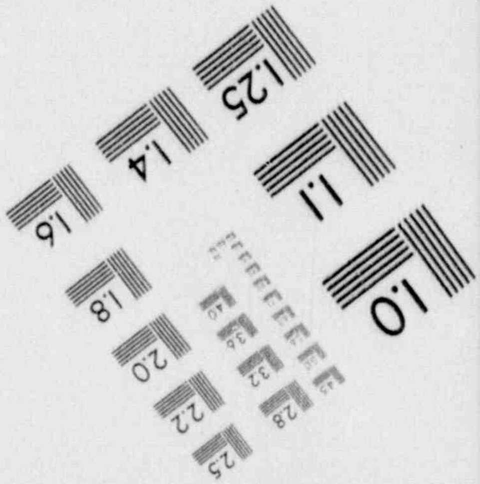

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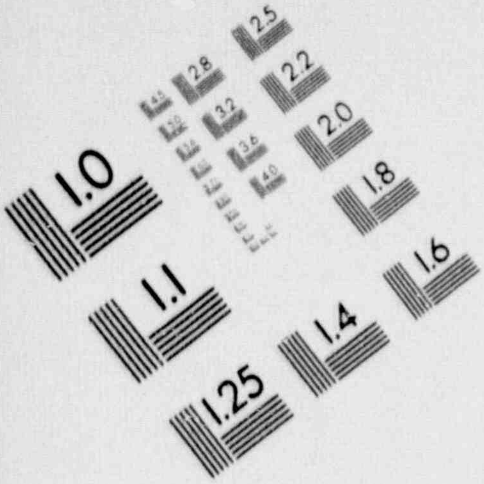

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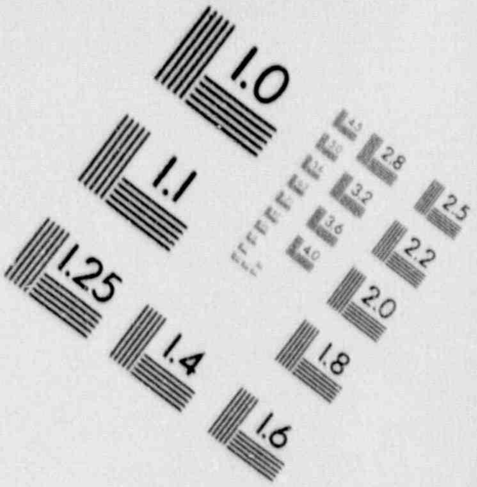

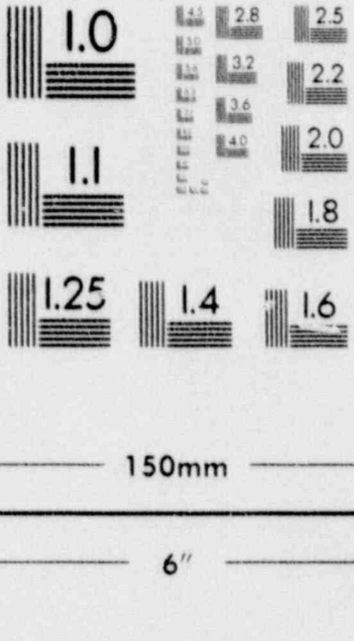

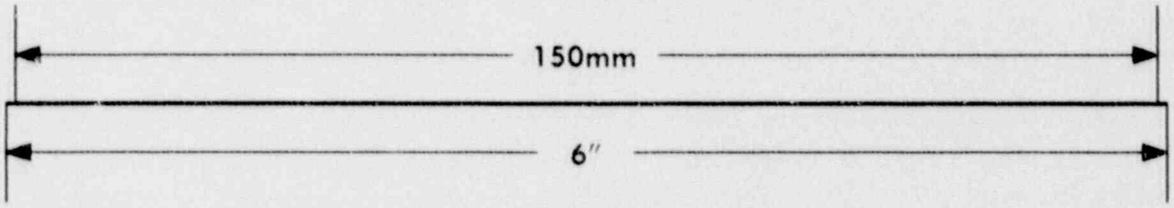

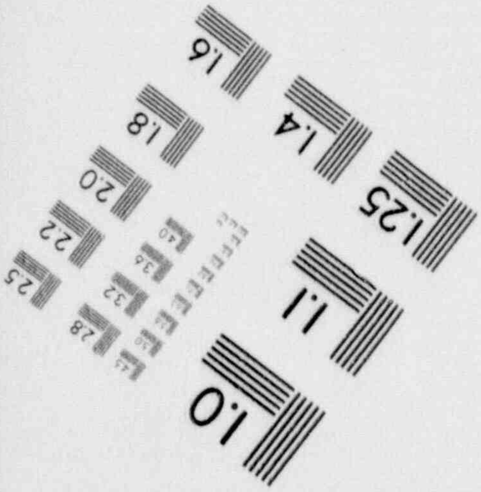

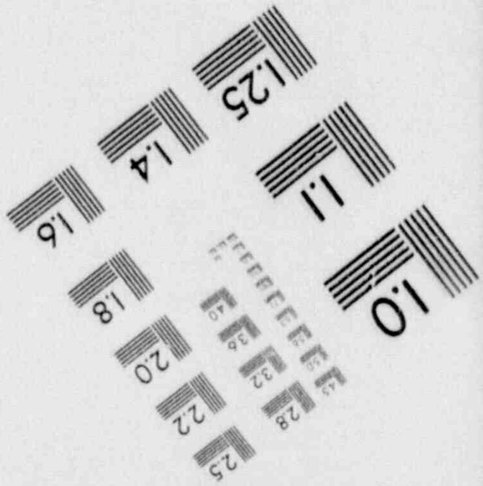

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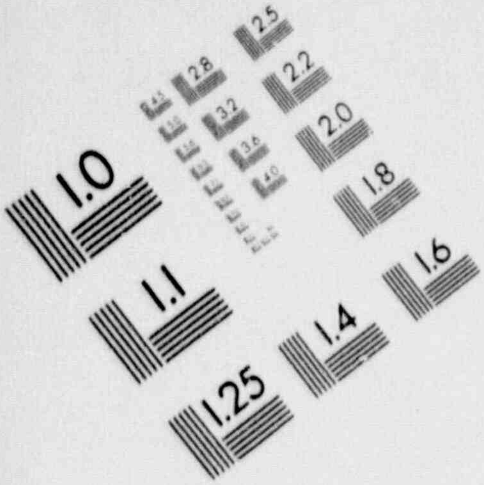

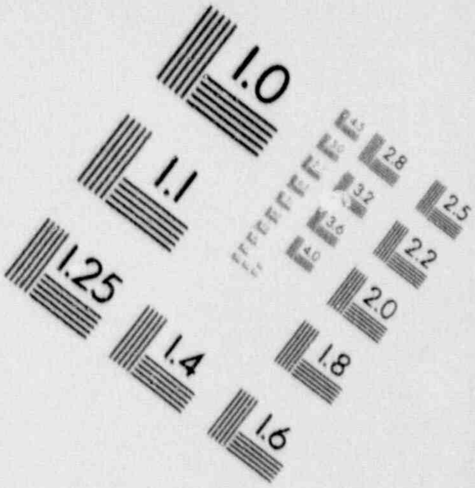

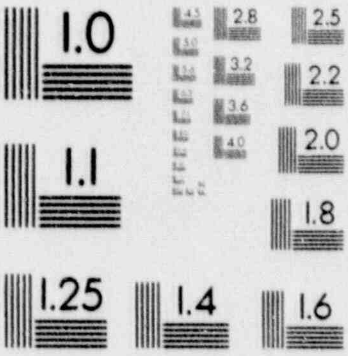

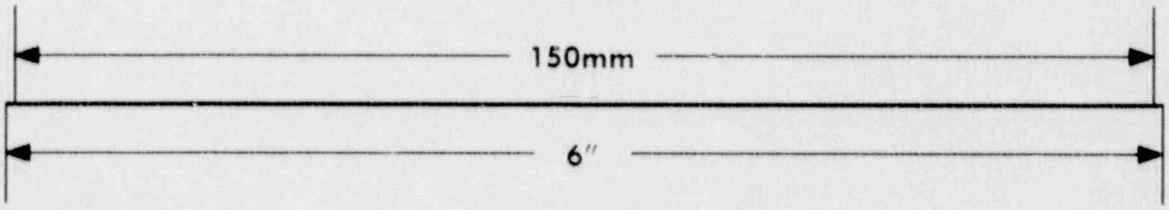

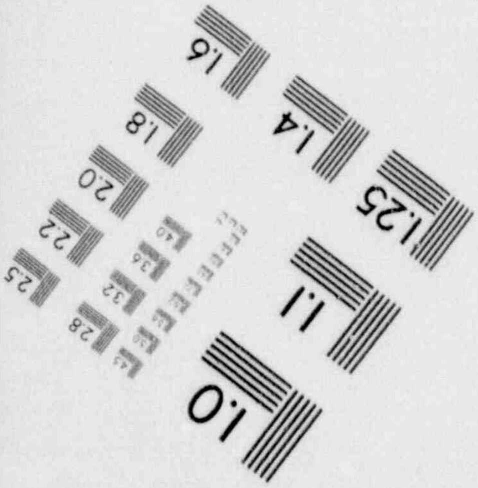

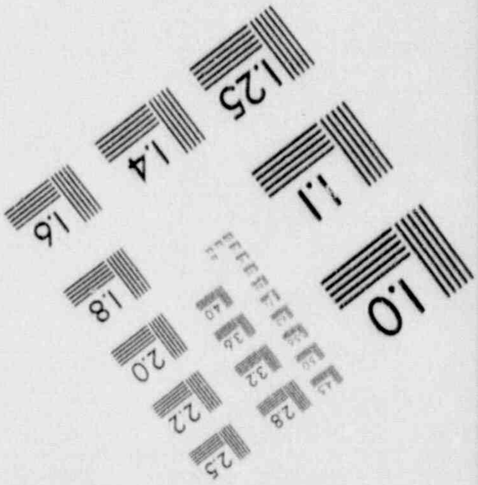

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MR. VON RIESEMANN: And liner plates, you know -- containments vary.

MR. SIESS: Yes.

MR. VON RIESEMANN: They're not always uniform. They go from a quarter-inch up to three-eights, and some may be a little thicker.

MR. SIESS: Yes.

How much do studs vary?

MR. VON RIESEMANN: The size and thickness? They do vary. There is a -- in the ASME code, they have some recommendations in there.

MR. SIESS: Are these all Nelson studs, the same welding process, or are some of them VSL or whatever it used to be?

MR. VON RIESEMANN: I'm not sure.

MR. SIESS: There were two outfits that made the stud welders, and I was wondering if that makes any difference.

MR. VON RIESEMANN: We've never done a studs -- these are Nelson studs we used. Right? Which type of studs in the containment model?

MR. HORSCHEL: They're scaled from analysis study.

Dan Horschel, Sandia National Labs.

MR. SIESS: Are they welded with automatic equipment, like Nelson?

MR. HORSCHEL: What we had was essentially a capacitive discharge welder. The stud itself would actually go into a grid. You'd have a certain voltage.

MR. SIESS: Okay. So, you don't know whether the weld in the weld heat-affected zone and so forth is the same as you get in the full-size thing with the Nelson stud.

MR. HORSCHEL: Because of the thicknesses of the liner and the diameter of the stud itself, you would assume that they would be different than the full-size, and that's part of the thing we were addressing with these separate effects tests.

MR. SIESS: Now, are all the actual containments made with Nelson studs, or are some made with -- there used to be two outfits that made studs.

MR. HORSCHEL: I have only heard the term Nelson stud.

MR. SIESS: There were two proprietary ones, because when -- did all that stuff for bridges, did it for both of them. The other one had a "K" in it. There were, at one time, two proprietary systems, and whether they still exist, I don't know, and apparently, nobody else knows right now. So, forget about it.

[Slide.]

MR. PARKS: The next slide is liner tearing mechanisms -- they presented almost the identical slide yesterday. I don't plan to go into all that much detail.

The mechanisms that led to the liner tear at least are there and also the variables that might affect minor tearing are described.

MR. SIESS: Scale?

MR. PARKS: The scale is not listed there, no.

MR. SIESS: It's listed on the next one, isn't it?

MR. PARKS: Listed on the next?

MR. SIESS: Yes. Right there.

MR. PARKS: Right. Let me go ahead. It's not listed on the list of liner tearing mechanisms, no.

The plan right now for the separate effects test --

MR. SIESS: Variables affecting phenomena.

MR. PARKS: Right, and scale is not listed on the list and it is a potential variable.

This test program as we currently have it planned is to be composed of two phases. Phase I would just be looking at the effect of liner prestress on liner tearing.

MR. SIESS: What's liner prestress?

MR. PARKS: Of the initial yielding of the liner and the liner was in a state of initial yielding which pre-loaded, if you will.

MR. SIESS: "Pre" is "pre" to the failure, not "pre" to the --

MR. PARKS: It's also "pre" -- when you get the majority of the shear loading induced into the liner from the stud, the liner is already in a yielded condition before most of the slippage occurs. We think that's why --

MR. SIESS: Do you think the sequence is important?

MR. PARKS: The sequence?

MR. SIESS: Yes. Usually in prestress you know -- it caught my eye and bothered me -- you stress the liner and then on the load of the stud.

MR. PARKS: Right. That's what we were looking at in the first place was the fact that it's initially stressed.

MR. SIESS: Suppose for some reason they decided to load the stud and stress the liner simultaneously.

MR. PARKS: Suppose we did that?

MR. SIESS: Yes -- whatever was prestressed, is that right?

MR. PARKS: Not in the way we're describing it here, no.

What we're looking at in the Phase I test, as I'll describe it in more detail in a minute, is seeing if the fact that the liner has initial yielding before the stud load is induced into the liner has an effect on the failure mode.

MR. SIESS: But why would -- I just don't see why the stud load doesn't grow at the same

MR. VON RIESEMANN: Could I possible say something --

MR. PARKS: This is a test of studs.

MR. SIESS: I'm not talking about tests now.

MR. VON RIESEMANN: Let me take a moment there. You take a hammer test, supposedly, bend them over, the stud fails or bends but the liner doesn't fail. If we do a shear test, if you will, with the studs in concrete and pull on the liner, again the studs fail. The liner doesn't fail and it's Randy Weatherby's hypothesis that you need a load in the liner combined with the bending if you will on the stud to cause a liner failure.

MR. SIESS: Yes. The implication and this is partly semantics and partly the thinking that is behind the semantics, if I do an interaction diagram where this is the liner membrane stress and this is the stud load, then I have to do it this way. First apply the liner stress, then apply the stud loads. I don't think that is what happens in the structure. I think it goes this way.

MR. VON RIESEMANN: What happened in the analysis is that the stud load, if you will, the stress concentration or strain concentration at the point of the stud only occurs after the liner has yield, not before.

In real life things are growing together but the analysis shows that initial, if you will, is not important.

MR. SIESS: Okay, so you are saying that things actually move this way --

MR. VON RIESEMANN: Right.

MR. SIESS: -- that you will get the same result if you went this way.

MR. VON RIESEMANN: Right.

MR. SIESS: And that is easier to test.

MR. VON RIESEMANN: Right, exactly.

MR. SIESS: Okay. That's bad wording up there and bad thinking.

You are going to test it by doing it sequentially and you think it is the same as if you did them simultaneously. I think it is too, provided you know what points you're going to.

MR. PARKS: Okay. Anyway, that's the effect of this initial --

MR. SIESS: But you should think in terms of an interaction.

MR. PARKS: -- the effect of this initial loading of the liner will be to hopefully determine in the Phase I test.

In the Phase II test we will be looking at a more complex specimen, as I will show again in just a second. We're trying to see which strain mechanisms are the most important and the tests will be conducted by taking one strain mechanism at a time and then adding additional mechanisms on until we can reproduce the liner tear that actually occurred in the model.

The purpose of the Phase II tests as they are now planned is just to show that our testing method can be used to reproduce that liner tear. Then beyond that, once we feel that we can comfortably reproduce that liner tear, then we can vary the different parameters such as the stud spacing, the stud diameter. We could even test different liner anchorage systems.

MR. SIESS: Now you haven't told me at what scale this is to be done.

MR. PARKS: I will when we proceed.

MR. SIESS: I'm sorry.

MR. PARKS: All at one-sixth scale at this time. We do have some full-scale tests that we will test.

MR. SIESS: I don't see any way that your tests at one-sixth scale are going to provide a link between the model and the prototype. It will provide analysis which you could apply with confidence to the prototype but it would ignore completely differences in material properties.

MR. PARKS: The plan right now is to conduct the Phase II tests at one-sixth scale to make sure we can reproduce that liner tear and then take the same --

MR. SIESS: Experimentally.

MR. PARKS: -- experimentally and analytically use -- analytical method should follow the experimental methods based on our evidence so far. Then we would take the same test concept, go up to full scale, and then do additional testing at full scale once we are confident that the method that we are using to do these tests is actually representative of what happens in a real containment.

MR. SIESS: Now suppose you go through all of that, and when you get up to full scale it doesn't work? What are you going to do?

MR. PARKS: We would have to see --

MR. SIESS: Are you going to know why it doesn't work when it doesn't work?

MR. PARKS: I hope that we do.

MR. SIESS: Well, I mean unless you think of that in advance, you are not going to have measured all the properties, heat-affected zone around that thing, all those things.

MR. PARKS: If we can generate the liner tear at one-sixth scale and it doesn't happen at full scale then that tells us that maybe that liner tear wasn't really representative of what would happen in a full size containment. That would be useful information.

MR. SIESS: But you're going to go a long ways before you get to making that comparison.

MR. PARKS: Yes. Okay, this --

MR. SIESS: Hasn't Randy made enough calculations to predict what happened in the one-sixth scale model?

MR. VON RIESEMANN: Yes, but --

MR. SIESS: Now you want to do simpler tests and see if the same analysis will hold?

MR. VON RIESEMANN: We are not trying to prove, if you will, his analysis with these tests, and then the question of scaling is the other question, obviously, and once that is done then you can say you can use that analytical method for any type of anchorage system because the goal was not only to handle the question for reinforced concrete anchorage but also prestressed liner anchorage.

MR. SIESS: You say liner but --

MR. VON RIESEMANN: As it turns out, I don't know if it is fortuitous or what, a lot of the welding on these liner anchorage the area if you will of that weld happens to be the same area as the cross-section of stud and I don't know if that was planned ahead or just a quirk.

MR. SIESS: Has anybody thought of going back and decide whether we really need those things?

MR. VON RIESEMANN: People are seeing our results and the ASME code committee start wondering about these.

MR. SIESS: I remember what we went through to get the darn studs on there and now -- on the prestress I think some of that is stiffening -- it's the form weight.

How about the temperature test on the one-sixth scale? Add that to your list.

MR. VON RIESEMANN: Right.

MR. PARKS: Listed here is a test matrix for the Phase I test. Again the ones that are planned for the immediate future, the one-sixth scale test. Other tests are planned at full scale to be conducted at a later date.

We've arbitrarily got a 1A and 1B test series. The difference would be the initial preloads to the liner.

If you look at the sketch of the Phase 1 A and B test specimens, this is basically a uniaxial test specimen. It should be relatively easy to construct and to actually do the test.

The 1A test, this block of concrete which is poured around a single layer of studs or single roll of studs, would be subjected a load by a jet. If there is no initial preload in the liner, the load would be increased until we had a failure, probably at the base of the stud.

The 1B test would be an identical specimen but we would initially apply uniaxial tension to the specimen until we got a zone of yielding around the studs and then --

MR. SIESS: I've got some test data like that. I don't know whether it would be any good at all but we tested a lot of beams that had a steel plate for reinforcement attached to the beam with studs.

MR. PARKS: On the compressed side, I guess.

MR. SIESS: Bending.

MR. PARKS: Yes.

MR. SIESS: Bottom of the beam, the steel plate was in tension and then the regions outside the third points -- I think it was third point loading -- the studs had a shear on them.

I don't know that we measured anything or not.

I'll go dig them out just for the heck of it.

MR. PARKS: Might be useful, yes.

MR. SIESS: You can't test that way because it's not determinant enough for what you are trying to do here. Now they were looking at the roofing of a reservoir with essentially a flat slab --

MR. PARKS: With the liner on the bottom?

MR. SIESS: There was a steel liner underneath it because it was aircraft fuel and they didn't want the concrete exposed to it and they decided, well, if they had the liner maybe they could use studs and leave our the rebar.

MR. PARKS: Okay, so that's the basic scope of the one-sixth scale Phase 1 test.

There are also tests planned at full scale using the same type of tests.

The 1C test would be identical to this 1B with the initial preload except that's full scale and same d/t ratio. This is stud diameter to liner thickness.

The 1D tests are just to determine how much friction you have between the concrete block and the liner. There would be no studs in that test. It's would just be pushing the block of concrete and having an external pressure applied to the liner.

MR. SIESS: Now if you are going to study the friction between concrete and steel, that is going to take a lot of tests because that depends on the surface condition, the cleanliness of the surface. It is extremely sensitive to anything that tends to separate it.

MR. PARKS: This test series should be a nondestructive test, if you will. You could apply a given external pressure load to the block and see when you measure the friction and then vary the external pressure down to a different level and determine the friction force again.

MR. SIESS: Well, there have been a lot of tests made on friction of concrete to steel and, you know, they literally depend on everything and it's extremely difficult to destroy any body and just have friction, if you are going to cast the concrete against the steel. You're going to have to move it far enough to break the bond. Be sure you do that. It's a tricky business and you are going to have very variable results so enough you have enough tests to get a statistical distribution, I think you are going to have a problem convincing anybody that the numbers you have are good.

MR. PARKS: The 1E tests are exactly the same as the 1C test, the same specimen and type of design. The only difference here would be applying an external pressure to the liner and trying to induce whatever friction that might exist between the liner and the concrete, seeing how that affects the liner tearing failure mode.

Obviously what we're doing with the external pressures, we are trying to see what effect the internal pressure in a containment has by pushing up the liner against the concrete.

MR. SIESS: Did the analyses that Weatherby made suggest that there is any frictional force in there --

MR. PARKS: In that analysis, no.

MR. SIESS: -- affecting the results?

MR. PARKS: There is no friction assumed in that analysis.

MR. SIESS: I know that but does the results of the analysis and the comparison with the tests suggest that they should or should not be friction in there?

MR. PARKS: The comparison between the analysis and the tests as you know are very good so --

MR. SIESS: Assuming no friction.

MR. PARKS: Assuming no friction.

MR. SIESS: I could conclude that there wasn't any friction, couldn't I, unless analyses have also been made assuming friction and give equally good results. Then I could assume there is friction. I could assume that that would make a difference.

MR. PARKS: Bob may be the one to address this but there is some speculation that if there was significant friction there it could reduce the amount of shear that the studs are having to pick up. Now that's the same shear that would be reduced just by the friction, would be resisted just by the friction so that's the reason for wanting to look at it but I follow what you are saying and I tend to agree with you.

MR. SIESS: I mean you can learn something from the analysis. I am not sure analyses are cheaper than tests these days. It used to be, but --

MR. DAMERON: Bob Dameron.

MR. SIESS: Better get up to a mike, Bob, or you won't be on the record. He's taking a recording. Just pick up a mike somewhere.

MR. DAMERON: I think it is appropriate for me to add a comment, because in our association with Sandia in the last couple of years, we have been sort of devil's advocate on this liner tearing phenomenon. And the record would be incomplete if it were not stated that the liner tear as a result of the stud itself is a displacement-controlled phenomenon.

You must achieve relative displacement between the liner and the head of the stud in order to cause a tear due to the stud. And Randy Weatherby's analyses were of the steel only, and they were using assumed displacement boundary conditions without modeling the concrete explicitly.

And that has been the primary source of these discussions between Sandia and ANATECH. And ANATECH has proposed a third behavior that should be considered, and that is the crimping of the liner that occurs at a major crack that must be present near these penetrations, because we have seen that the penetrations move outward, radially outward, less than the free field.

MR. SIESS: By crimping, d you mean local bending?

MR. DAMERON: Yes. Exactly.

MR. SIESS: I'm learning a lot of new words.

MR. DAMERON: If, in the test, there was not enough slippage between the concrete and the liner at that row of studs to account for the same type of boundary conditions applied in Weatherby's analysis, then there must be some other mechanism involved there.

MR. SIESS: I hear you, but I don't understand you. I'd have to study this extensively, I'm afraid.

MR. DAMERON: Okay.

MR. SIESS: But you don't think the tests are typical; is that your bottom line?

MR. DAMERON: No. I think that the test program that they have proposed is very comprehensive, and I agree with you that bringing in aspects such as friction is potentially going to require many tests, and that is a very complex subject to study.

MR. SIESS: I agree with you. But do you think that the tests as you see them would be adequate to resolve any concern as to your concept versus Randy's, of which is the proper mechanism and which is the proper representation?

MR. DAMERON: I think, coupled with supporting analysis, we can resolve it.

MR. SIESS: If we are only going to talk analyses, we are not going to resolve anything. The object is, what makes the steel crack? Well, we know what makes it crack is stress or strain, but to be able to predict the cracking, you think one set of phenomena need to be included that he doesn't? Will these tests be sufficient to tell us what has to be included in the analysis in order to predict what happens?

MR. DAMERON: Yes. I think that they have developed them in such a way that they are starting at the simple and moving toward the complex. And if it turns out that they can get the liner tears without considering this out-of-plane motion or crimping, then we will no loner be advocating that behavior.

MR. SIESS: That's good.

I have just a basic problem, that if the only way we can predict the integrity of a containment, the ability of a containment to prevent excessive leakage is by analyses that get down to that level of detail, then I have a real problem with building containments like that, or believing what anybody tells us about the leakage of a containment. If those things are important in a structure that is 200 feet tall and 150 feet wide, and built by a bunch of guys in hard hats out there messing around with concrete and vibrators and banging things around, I got a real problem predicting when that containment is going to crack.

MR. DAMERON: Okay. There are a couple of other test series that are planned here. No real difference from the other ones except that the stud diameter is smaller in these two tests.

As I mentioned, as now planned, the second phase of the tests would all be at one-sixth scale. In the first series of tests, we would only be working at the strain concentration due to the insert plate connection to the liner plate.

MR. SIESS: Which could change the thickness.

MR. DAMERON: Yes. All we are looking at is a single steel plate for the first test, unlike what is shown here.

MR. SIESS: But you have studs.

MR. DAMERON: The first 2A test has just one steel plate with the insert plate, a liner plate and then another insert plate.

MR. SIESS: And no studs, no concrete?

MR. DAMERON: Exactly. And just to see what the strain concentration, strain distribution is there. It is a very simple test to conduct, just to get a baseline on what the strain concentration is, just due to that mechanism.

MR. SIESS: How wide?

MR. DAMERON: In this case, it is eight inches.

MR. SIESS: I bet it's not that simple. But go ahead.

MR. DAMERON: Okay.

The second series of tests that we call 2B includes, here we add the studs, we add the concrete, and also add some rebar. But there is no friction model in this case, there is on external pressure applied.

And then in the final case that we would test, we would actually include friction by applying an external pressure in that region around the studs and the liner break.

For those second, the 2B and 2C tests, the specimen looks pretty much as shown here. There have been some minor revisions.

MR. SIESS: Now, when you try to test with friction, you realize that you start off with some sort of an adhesion between the concrete and the steel.

MR. DAMERON: Sure. Some sort of bond.

MR. SIESS: And so your load slip curve is not going to be a straight line that you can get a slope of as a friction coefficient.

MR. DAMERON: Not until the bond is broken.

MR. SIESS: Not until the bond is broken.

MR. DAMERON: Right.

MR. SIESS: And as long as you know that and expect it, but don't try to break that bond.

MR. DAMERON: Yes.

MR. SIESS: You are going to have to carefully define and document the surface condition of the steel, and the cleanliness of it.

MR. DAMERON: Okay.

MR. SIESS: Because you are getting in an area that just so many things affect it, that we ignore it, usually.

MR. DAMERON: As I mentioned earlier, what we hope to do here is to reproduce the type of liner tearing that actually occurred in the model.

MR. SIESS: This is all one-dimensional or two-dimensional?

MR. DAMERON: That's the uni-axial test, yes.

MR. SIESS: And that's not reproducing the model?

MR. DAMERON: Well, we kind of reproduce the liner tear that occurred in the model due to the same --

MR. SIESS: But the stress in the other direction you don't think had any effect on it?

MR. DAMERON: That is what we are postulating, that, you know, we don't have yielding in the other directions, very small elastic strain in the vertical direction. We are hoping that that is not significant.

MR. SIESS: That's reasonable.

MR. DAMERON: And so therefore, we are hoping to do the uni-axial test as a much simpler test than a bi-axial panel type test.

Again, if we can reproduce the liner tear, with this type of specimen, then we would like to go to full-scale. Then we can vary the stud diameter, stud spacing, and test different types of liner acres, as we mentioned earlier.

MR. SIESS: As you bring in these variables of stud spacing or whatever, I assume that you will first make analyses and get some idea of what the analysis predicts as the difference in load at which it will crack. Because if the analysis says it doesn't make any difference --

MR. DAMERON: No need to do the test, possibly.

MR. SIESS: It is going to be harder to interpret the tests,because there will be some normal variations.

MR. DAMERON: We plan to conduct pretest analysis of every one of these specimens.

MR. SIESS: Okay.

MR. DAMERON: And then, from what we learn from that, we will see if we need to adjust our analysis method.

I think you have the scope of the tests that we are planning. Are there any additional questions?

MR. SIESS: Are these assured now? Is this part of the program? Is this an approved program?

MR. COSTELLO: The initial testing of the fixtures is. We have not yet really gone through the whole -- Jim Costello, NRC Staff -- we really have not gone through and budgeted for this whole collection or any subset thereof. We still have a little reservation about what do you need, what is the minimum set you need to get where you have to be.

We haven't gone through a final run with our peer review panel.

MR. SIESS: I don't think you are ever going to know in advance what tests you need. The ideal way to do it is sort of do it step by step and play it by ear.

MR. COSTELLO: Yes.

MR. SIESS: But the Government doesn't always work that way.

MR. MARK: I was wondering if it is estimated how long a program you have just described?

MR. SIESS: The only thing that will pace that program is how many specimens, how many rigs, and how long they are going to cure the concrete before they go test it.

MR. PARKS: I can answer the concrete curing question. It's 28 days.

MR. SIESS: I can make it seven days and speed your program up a little bit.

MR. BENDER: How much money has been allocated so far?

MR. COSTELLO: So far, we are still looking at the final 1990 budget. We are operating right now on carryover from 1989. And we have to size this out with other options.

We have not as yet totally scoped the number of tests, and we certainly would welcome your thoughts on what constitutes the minimum that we need.

The proposals also we intend to look at in-house as well as with -- We plan to go through this in-house as well as with the peer review committee.

I know how much I am willing to spend, but I don't know how far it is going to take me at this stage.

MR. SIESS: Are you through, now? I mean, you are through, period?

MR. PARKS: Yes.

MR. SIESS: You haven't got any more presentations.

I was looking through this other handout -- thank you, Brad; you can sit down, now -- on the assessment of analytical methods, with little pictures of light bulbs and people thinking.

Did we still want to present that, somebody? Were you going to do it?

MR. VON RIESEMANN: It is up to you.

MR. SIESS: I think it would be interesting. And we have 20 minutes before Noon, and we could go a little bit after that if we wanted to before eating. Do you want to try to do it now? And we can finish up afterwards, if you want.

MR. VON RIESEMANN: With the new computer systems now, we have all little gadgets on there and it's interesting, fingers pointing and people at the side of a computer.

MR. SIESS: I notice that you only think at the beginning. After the thinking's over and it's on the computer, you don't have to think.

MR. VON RIESEMANN: I think the person at the desk has been shown to be thinking, too. I think I'd like to quote something I read in a book many years ago and I think it was Hamming's book. In fact, my days at the University of Illinois. Richard Hamming's "Numerical Methods." "The purpose of computing is not numbers, but insight."

MR. SIESS: That's what they say about PRA data.

[Laughter.]

MR. VON RIESEMANN: I feel that with analysis and let me take a few of Dave Clauss' viewgraphs -- and only a few of them -- and use them and let me just do the top half here.

The question we're really looking for is when, how and where failure occurs in the containment system and I emphasize system also. We're not just looking at the containment shell. As Dave presented this material to you, well, he gave you the handout yesterday. Let me just go over these details. Don't get too distracted by the things on the left.

One of the key things is identify the potential failure modes in the containment system so we don't overlook those. It doesn't mean that every failure mode has to be tested, if you will. There might be some appropriate analysis methods. The other thing then, too, is define the appropriate evaluation criterion and that's, I think, the key step and the hardest step.

People who do stress analysis usually like to report stresses but don't like to necessarily report, does it fail or not fail, what does it mean, and that's the key, I think, in this process.

MR. SIESS: Failure still isn't defined.

MR. VON RIESEMANN: Failure being functionality of the system. Okay, leakage.

We're not going to define it. Somebody else will define it.

MR. SIESS: We've still got that problem.

MR. VON RIESEMANN: Still have that problem. Failure as you pointed out, I think, in the first containment's workshop, is not necessarily structural failure but the function of the containment.

MR. SIESS: Failure to contain.

MR. VON RIESEMANN: Right. That's the name of the structure. The other thing I might add is the purpose of the program --

MR. SIESS: Containments have other purposes besides containing. This project is interested in the containment function of the containment, I think, primarily.

MR. VON RIESEMANN: The background of the program too was to do a limited number of experiments pretty well defined, well planned, to benchmark, if you will, the analytical methods. Not in every case. For example, in inflatable seals, nor perhaps with bellows, will they have a finite element program. It might be empirical methods, but some method to evaluate the performance of the containment.

The next step, if you will, in the analytical method, is to design a model and I think you've heard already some of the complexity, steel being somewhat easier than concrete and you can argue here, we can discuss all day the constitutive model, for example.

Once that is done, the next step of course is to calculate the response and not a minor point also is knowing the loads. Again, we are working with given temperature and pressure, say. We're not saying that's tied into any given accident scenario again. Other people will do that, or, given an accident scenario, we can do the response. It works both ways.

MR. SIESS: But Will, go back a minute. The analytical model, how complicated it has to be, depends on the failure modes that you've identified up there. If the only failure mode is gross rupture, it's a pretty simple model, isn't it?

MR. VON RIESEMANN: Yes. The method that you use depends on the failure mode and the evaluation criterion you use. Also, they're tied together. For example, the paper that we gave you this morning on Sequoyah, depending on the complexity of the model, you use different failure criterion -- strain limits, perhaps.

MR. SIESS: I think there's a little semantic confusion. We talk about failure modes. You don't really mean failure modes until you define failure and you're not going to define failure until you get down to evaluation. So, it's really behavior modes but if you've defined failure as being 10 percent leakage, now you can look for all the ways you can get 10 percent leakage but you would have defined -- your evaluation criterion would be there -- 10 percent leakage.

MR. VON RIESEMANN: Right.

MR. SIESS: Somebody would translate that into a hole size or an annulus width.

MR. VON RIESEMANN: What I think I was also meaning is if you look at the containment system, you look at various parts, okay, and again, as you mentioned, for the various parts of the containment system and I only have part of the viewgraph on here, obviously you use different methods, different techniques, okay?

Not every method needs an experiment. Now once you do the response calculations, then I guess the next difficult job is to compare and you've heard some of that yesterday, the calculated response with the evaluation criteria to make an evaluation if the thing has failed or not failed and that's in a sense the process if you will of the analysis.

I might add, I think you mentioned this yesterday or this morning, part of the reason for doing analysis is also to determine where to put instrumentation, to guide you in the response calibration of instrumentation. You try to put instrumentation where you know the least about the structure.

MR. SIESS: That's particularly true of course, when you're only going to be able to test one.

MR. VON RIESEMANN: Exactly. You always wish you could put the instrumentation on after the test, right? I'm going to skip because --

MR. SIESS: That I like. Don't skip it.

MR. VON RIESEMANN: Don't skip it? I was going to.

MR. SIESS: You got that one in color?

MR. VON RIESEMANN: Sure. I think I had it in color.

MR. SIESS: That's all right. Go ahead. I just wanted to make a comment about that slide, though.

MR. VON RIESEMANN: It's FRG. Federal Republic of Germany.

MR. SIESS: On the right?

MR. VON RIESEMANN: Yes.

MR. SIESS: Okay. Black, red, okay.

MR. VON RIESEMANN: The gold didn't come out too well.

MR. SIESS: The comment I wanted to make, it says accurate calculate structural response and I find it noteworthy that the structural response is represented up there by a plot of pressure versus displacement.

MR. VON RIESEMANN: Point well made.

MR. SIESS: Which incidentally was the only response that was calculated accurately by any of those methods, right?

MR. VON RIESEMANN: Well, the hoop strain was also done quite well, yes.

MR. SIESS: Well, hoop strain in this --

MR. VON RIESEMANN: It's directly related.

MR. SIESS: One to one in this case.

MR. VON RIESEMANN: Those who are familiar with experimental work know that it's much nicer to compare say displacement in the dynamic result than acceleration because of the variation in that.

MR. SIESS: It's integrated.

MR. VON RIESEMANN: Right.

MR. SIESS: But that's the very reason it's important is because it's integrated there.

MR. VON RIESEMANN: However, and I think here maybe we differ a bit, to check the validity of a computer program, you can sometimes measure -- match displacements fairly well -- I've seen that happen -- and have the strain field at intermediate points quite different.

MR. SIESS: Yes, but again, that gets back to what you're trying to use the analytical program for. If you're trying to use it for something that isn't sensitive to those strain fields.

MR. VON RIESEMANN: It's again the insight.

MR. SIESS: We're trying to predict when that stud will, you'll get the crack at the stud.

MR. VON RIESEMANN: Now, I'll skip with your permission to the very last slide because the other discussions in this presentation had to do with the concrete model and that has been discussed to some extent yesterday and this morning. If there are any questions, I'll try to answer those, but the summary that Dave had prepared is that computer codes that are available for calculating structural response are fairly well established.

The bigger problem is the next bullet -- I won't call it a finger -- is identifying the potential failure modes. I'll never forget years ago, people doing an analysis of a component and not including buckling in the analysis and buckling was the mode of failure.

MR. SIESS: We had a building fall down in the Hartford Arena.

MR. VON RIESEMANN: Right. So you have to include the appropriate failure modes in your analysis. Otherwise, the whole game is lost. Then, how do you take your results, if you will, and determine that you either passed or failed, if you will. What's the response mean?

A point that Dave wanted to raise, designing consistent models, if you will, for doing the analysis. The last bullet is, if you understand failure, the feeling is then we can with fairly well assurance determine at what pressure the containment integrity is preserved and obviously failure is useful for risk assessments, accident management and other activities dealing with severe accidents.

MR. SIESS: You know, we've got -- the analysis is of course a big part of this thing and it's getting a lot of attention but philosophically, we need to keep in mind that we only need analysis because we can't test everything. There are a lot of areas where the tests are better than the analysis but we can't test everything. If we really want to know how the thing is going to act, somebody said, you go ask the structure but that isn't always possible. In fact, it seldom is possible, so we do the next best thing.

We develop a mathematical model with various degrees of complexity. They used to be fairly simple ones and we seemed to do a pretty good job in those days, that represents the behavior of the structure and then we go ask it but they had to be simple because we didn't have computers. Now we can make them complicated.

MR. BENDER: I wanted to offer one addition to the points you've made. I don't disagree with any of them. One of the reasons for doing analysis is to find out how to control the failure model. You can't do that after the fact but if you do enough analysis before, you can decide where it is you'd like to have the structure fail and how.

MR. SIESS: You're talking about an ideal world, Mike.

MR. BENDER: Sure I am.

MR. SIESS: We've made an awful lot of changes to the code after the fact. We didn't effect that failure but we granted the next one.

MR. BENDER: In those simplistic days before we did it this way, pressure vessels had ruptured disks in them because we didn't want to go through the exercise of trying to figure out where the failure is. You're not going to put ruptured disks in -- until you can design the structure in such a way that it works like a ruptured disk, like a liberally thinking.

MR. SIESS: Put them in.

MR. WARD: Sounds a lot simpler to me.

MR. BENDER: Well, I'm a proponent of ruptured disks and always have been.

MR. SIESS: If you just want to pipe the effluent to Russia or China.

MR. BENDER: If the ruptured disk there, at least I can aim the effluent towards something.

[Laughter.]

MR. SIESS: I want to say something. I think that one of the real accomplishments on this project is this defining the possible failure modes and looking at them. I still think some of them are probably a lot more important than the ones you haven't touched that I think are more likely are the failure to isolate pre-existing openings and so forth which obviously --

MR. VON RIESEMANN: Other NRC programs are looking at those.

MR. SIESS: They're looking at those. They're not going to do anything about them like operating all the plants at 3 p.s.i. containment pressure which would solve a lot. There has been, I think, an excellent job and I think the approaches that have worked on some of the simpler things like the seals, the inflatable seals, are simple maybe because the tests have been easier to make -- multiple tests.

The failures aren't simple. If you wanted to do for the inflatable seals what we're doing for the containment liner, you'd be doing a finite element analysis of that inflatable seal to predict why it did what it did but you didn't see the need for it.

MR. VON RIESEMANN: We don't see the payoff, if you'll need.

MR. SIESS: Jim can have four minutes.

MR. COSTELLO: I think I'll take much less than that. Thank you very much.

I just wanted to thank the members of the committee and the consultants for their attention. These are very good occasions for us. It forces us to focus our attention at certain point in time of where we are and where we're going and we often get the benefit of additional suggestions. We thank you again very much for the opportunity.

MR. SIESS: Jim and Charlie while you're still here, I hadn't planned to bring anything before the committee because it's getting pretty specialized and I hadn't planned to write a letter but I'm wondering, we don't do our research report where we comment on the budget and we've been trying to provide individual comments as we go along and I don't see an awful lot of point in writing the Congress about the nature of this research but I think the budget parts or the commissions.

Could you provide us with the '90 and '91 -- well, '90 budget you've got, '91, you're working on, right, budget for the structural engineering research, this and the others, and anything else that we don't know about and give us some idea of the status of that and we might want to write a letter to the Commission. We're having some meetings and trying to decide whether we want to say something about this constantly decreasing research budget and this would be an opportunity to be rather specific. Can you give me that information?

MR. COSTELLO: I certainly believe we can and would perhaps in a couple of weeks be sufficient?

MR. SIESS: Well, we've got a meeting with ERIC scheduled -- our so-called research subcommittee which is a big chunk of the committee -- scheduled for February 7th which is two weeks from yesterday, I guess.

MR. COSTELLO: So you prefer to have something next week. I think we can do that.

MR. SIESS: Before or after 7th would be good enough for me, probably. I doubt if you'll be coming in. I don't think we could bring it but you know the kind of stuff we want, the kind of stuff we used to look at.

MR. COSTELLO: Is there anything else I can do?

MR. SIESS: Not right now.

MR. COSTELLO: Thank you.

MR. SIESS: You can get that Sequoyah stuff to us when it comes out. Been any reports on the Sequoyah thing?

MR. COSTELLO: Not -- the NUREG CRs aren't out yet.

MR. SIESS: We'll see those when they come out.

MR. COSTELLO: They are in draft though.

MR. SIESS: We'll return at 1 o'clock at which time we'll be talking about the Category A structure stuff. Thank you.

[Whereupon, at 11:58 a.m., the meeting recessed for lunch, to reconvene later the same day at 1:00 p.m.]

AFTERNOON SESSION

[1:10 p.m.]

MR. SIESS: The meeting will resume. Mr. Kenneally.

MR. KENNEALLY: Thank you, Professor Siess and Subcommittee members and consultants.

This afternoon, the staff would like to present two programs to the Subcommittee relating to seismic response. The first one is the Seismic Category I Structures Program; that is work that is being performed at the Los Alamos National Laboratory. The principal investigator and presenter is Dr. Charles Farrar.

The second program is one entitled the Assessment of the Effects of Structural Response on Plant Risk and Margin. That is an effort that's being performed at the Sandia National Laboratory. The principal investigator and presenter is Dr. Michael Bohn.

Both of these programs are addressing the regulatory issue of load beyond design. It could be within the context of the new seismological information that we're learning; the Charleston earthquake issue or whatever. What would happen if a plant were to have an earthquake higher than the design basis. Or it could be a subset of that, as what would happen if during some of our testing, we discover that maybe there are some unconservatisms in our analytical approach; what would be the effect on plant margin and risk.

That is principally what the second program is looking at. The Seismic Category I Structures Program is an analytical experimental effort. It's been going on for a number of years at Los Alamos. It is addressing building response of non-containment buildings.

It is investigating the changes in the design parameters; for example, damping, floor response spectra, and gross structural behavior to design basis beyond -- created from earthquake motions beyond the design basis.

It will assess the adequacy of the current analytical methods and it is being closely tied with some work from the ASCE, American Society of Civil Engineers Working Group on stiffness of concrete sheer wall structures.

That particular program started about 1980 and the final funding for that is this year. We will not be doing any funding this current Fiscal Year. There probably will be a lag on getting reports issued and the like into the early part of Fiscal Year 1991, but essentially the program is concluding.

The second one, the effects of the structural response, is strictly an analytical effort. It is addressing how some of the differences that we've observed from early Los Alamos test data on the larger than anticipated reductions in building frequency might effect the margin or a probabilistic risk assessment that had been done on a particular plant.

MR. SIESS: How about the design?

MR. KENNEALLY: We are evaluating some design conditions, yes.

MR. SIESS: How it would have effected the design.

MR. KENNEALLY: Yes. That is --

MR. SIESS: You've analyzed the assumed cracking and got different things. Would it have changed anything in the way they designed it?

MR. KENNEALLY: We are looking at that and are making notes of what are the differences in base shears, overturning moments, floor spectra that might impact additional equipment or the like. I think that's covering what you're asking me.

MR. SIESS: Okay.

MR. KENNEALLY: This is being done by reevaluating seismic probablistic risk assessments, three of them in particular. The one that will be reported this afternoon is a reevaluation of the Peach Bottom 1150 PRA.

It is also revisiting some of the design-like calculations, as we just discussed. What would be the changes in the design floor response spectra and some of the other parameters, overturning moments and base shears, the like.

That particular effort will also be concluding this Fiscal Year. We have a draft report that is out for staff review right now on the Peach Bottom analysis and we will be looking at two more plants and then hopefully at the end of the

Fiscal Year, the early part of the next Fiscal Year, be issuing a conclusive NUREG on that topic.

MR. SIESS: Some portions of the staff are reviewing applications for future plant designs, either PVA-FDA certification, whatever, the ABWR combustion.

MR. KENNEALLY: Yes.

MR. SIESS: Are they looking at the seismic analysis to see what assumptions have been made or do they know about what's going on?

MR. KENNEALLY: Know what's going on as far as the results from our program?

MR. SIESS: Yes.

MR. KENNEALLY: No. They are definitely cognizant of the results of our program. They are looking at a lot of different concepts, obviously for seismic. Some are even entertaining base isolation and concepts we haven't considered yet.

MR. SIESS: But if I talked to the ABWR reviewer, could he tell me whether GE is assuming everything is uncracked up to some shear stress or whether they take into account the possibility of cracking?

MR. KENNEALLY: I think in that light they probably haven't changed their philosophy yet. They're still using the current thinking of the staff will analyze these sections as uncracked.

MR. SIESS: Okay.

MR. KENNEALLY: I'd like to turn it now over to Chuck Farrar from Los Alamos who will be highlighting some of the more recent results from the Seismic Category I Structures Program.

MR. FARRAR: Thanks, Mike. I'm going to start this talk with a brief review of the results that were obtained in the early portion of this program. As Roger pointed out, the program started in 1980.

I will go over very quickly the results through Fiscal Year 1984, but try to spend most of the time on the testing and results that have been obtained from Fiscal Year 1985 to the present.

I think that the initial material provides a little bit of background for why we did some of the other tests. As Roger already mentioned, what we're looking at here are loads beyond design basis, particularly seismic loading of Category I structures, exclusive of containment.

The objectives of the program were, again, just the seismic response, reenforced concrete, Category I structures, other than containment; develop experimental data to look at the behavior of these structures in both the elastic and inelastic range; and, provide experimental data to validate computer codes.

We also want to investigate how the floor response spectra in these structures change as the structure goes from the elastic to inelastic range; look at how damping changes as we, again, go from the elastic to inelastic range. Then, the latter data from this -- well, actually all the data from this program is being used to support the plant risk studies that are being done by Sandia.

Because the structures that we want to test are very large and because we want to test them into the non-linear region, we really had to go to scale models as a practical means of testing the structure.

The program began by testing one-thirtieth scale and we say one-thirtieth scale based on the wall thickness of the shear wall. They are one-inch thick walls. Those structures were tested both statically and dynamically.

We also tested the scale models of idealized diesel generator buildings and auxiliary buildings. These structures were ranged from, I guess, one-tenth scale to one-forty second scale. We tested different sized models so we could look at scalability.

These structures we started to test with simulated seismic inputs on shake tables. Just a quick look at the -- this will be one of the isolated -- a two-story isolated shear wall that was tested early on in the program.

You can see that we have weight added to the structure for similitude requirements. The structure is actually placed inside some guide, so it would shake in the plane of the shear wall. I should say should respond in the plane of the shear wall.

We then have a one-story diesel generator building model, idealized because we don't put doors in, we don't put any kind of penetrations that would be in a real structure in these models.

This, again, is a one-thirtieth scale model. It's about 18 inches long by about ten inches deep, one-inch wall thickness.

We tested 2-story, 1/30th scale diesel generator buildings very similar to the previous one.

MR. SIESS: Give us an idea -- I can't see the calendar, but I wish I could -- keep us a running timeframe here, because we started way back, and I'm not objecting, because we need to recall some of this.

MR. FARRAR: The isolated shear walls were about FY'82 when those were being done. These diesel generator buildings and the auxillary buildings that I will show in a second are about FY'83-84 timeframe.

This starts to get into some of the larger structures. This is the 1/10th scale diesel generator building model. This is at the Construction Engineering Research Laboratory in Champagne. This is about a five foot high model now.

MR. SIESS: You ought to put somebody in those pictures to show the scale.

MR. FARRAR: Well, in the next one, we have somebody in the picture. We have the high priced consultant on the project in there.

MR. SIESS: I used to get fussed at for that.

MR. FARRAR: In fact, I'm standing right over here, so you can get a scale with this one. This is one of the largest three dimensional structures that we tested. This is a 3-story auxillary building model.

Again, we're testing at the construction facility. The table is 12 feet by 12 feet there, so you can get an idea of the structure that we're testing, in addition to having the people in this one.

MR. MARK: Did you think that you got any specific information on the matter of scaling?

MR. FARRAR: Yes, we've got a lot of information on the matter. That will be something that I will develop as we go along.

MR. MARK: Okay, that's fine.

MR. FARRAR: That was the purpose for testing a 1/10th and then a 1/30th scale diesel generator building model. Then they are a one third scale model -- the 1/30th scale is a one third scale model to ten, and we can look at similitude then.

MR. SIESS: He's got a slide coming up.

MR. FARRAR: From the early test results, what we found was that it turns out these structures have a lot of reserve margin. When we scale the response to a prototype structure, they won't fail until we get excitation levels above 2 Gs, which I think is bigger than most credible earthquakes that we would consider.

MR. SIESS: That's 2 Gs ground acceleration.

MR. FARRAR: Right, but one of the things that we saw was that the stiffness of the structure -- and this is now stiffness measured both statically and then stiffness that's inferred from frequency measurements dynamically -- goes down by a factor of as much as four below what the theory would predict. That would be an uncracked cross section analysis using the strength of materials principles which, according to our technical review group, is a method that the AE firms used to design these structures.

MR. SIESS: You couldn't get it down that far by any method of analysis without cracking it; could you?

MR. FARRAR: Right.

MR. SIESS: To get that, you've got to have a crack. All the uncertainties about the end walls are there, but they don't make that much difference.

MR. FARRAR: No, they don't. In a sense, an end wall acting as a T-beam, looking at the contribution -- it really only contributes the bending stiffness of the structure and these things are getting their stiffness all from the shear resistance.

MR. SIESS: Let me interrupt you to ask on question. Has anybody ever gone out to a four or five or six nuclear power plants and walked through the diesel generator building and maybe the service water pump aux building -- anything that would fit this category, and looked for cracks in the walls?

MR. FARRAR: We've discussed that with the technical review group, and they seem to be divided on that issue. Some claim that if you look at a structure, you'll see visible cracking in the in situ condition. Others claim that these structures are not cracked in their initial condition.

MR. SIESS: They've actually looked at them?

MR. FARRAR: They claim that they have.

MR. SIESS: And they know a crack when they see one?

MR. FARRAR: Pardon me?

MR. SIESS: They know a crack when they see one?

MR. FARRAR: I can't judge whether they know a crack when they see one, but they claim that -- again, it's mixed, and we get further on in the presentation when we talk about the interaction with the ACS working group, we get about the same response from them. Those are people who --

MR. SIESS: That's strange that --

MR. FARRAR: Those are people who are involved in the design of these structures.

MR. SIESS: I don't care what they were involved with. The question is; have they looked at them? If somebody says, I've looked at these buildings and I haven't seen any cracks and somebody else says, I've looked at them and I have seen cracks; the first question is, are they looking at the same building?

MR. FARRAR: Right.

MR. SIESS: The second is; do you know how to look for cracks? Some cracks you have to wet the wall down to find them, and so forth. A crack doesn't have to be big enough to see from here.

MR. FARRAR: Well, I would claim that if the crack -- from the testing I have done, you can have the crack appear and when you take the load off the structure that cause that crack, you can not go back and find that crack.

MR. SIESS: Well, I can give you evidence from laboratory tests where cracks in the fluctual member sufficient to produce, in effect, a hinge, almost a zero a moment hinge, it could not be detected with microscope. They went to elaborate procedures to locate the crack to narrow it down to a half inch and then go in with a high powered microscope and again, until you put some load on it, you couldn't see it.

MR. FARRAR: I agree.

MR. SIESS: I've also looked at some building and I haven't seen very many that didn't have cracks.

MR. FARRAR: All right, one of the other issues that we looked at with these is the scale-ability issue. We were able to demonstrate the scale-ability of the dynamic properties of the structure, but we have to keep in mind that these structures that we tested at this point were all made with microconcrete and microconcrete is very susceptible to curing cracks during -- or shrinkage cracks during the curing process.

MR. SIESS: They would have to be; they're cured longer.

MR. FARRAR: Pardon me?

MR. SIESS: They're cured longer.

MR. FARRAR: Is it cured longer or cured shorter.

MR. SIESS: You get the tensile strength before you let it dry out; that's one trick. That's what I always did with my microconcrete. I kept it wet as long as I could and let the tensile strength come up before I let it dry out and get the shrinkage stresses on it. It worked reasonably well.

MR. FARRAR: Okay, now unfortunately, a lot of this was before I was on the program, so I'm not sure what they did in the curing.

MR. SIESS: In the very early stages, I raised the question of that and suggested that they test some specimens wet.

MR. FARRAR: They have tested some wet.

MR. SIESS: They did, and I think they still got the same results.

MR. FARRAR: Right.

MR. KENNEALLY: That's correct. They didn't see a significant difference.

MR. SIESS: No. That got wiped out real quick, I think.

MR. FARRAR: This slide addresses the scale-ability issue. What we've taken is the resident frequencies of both the 1/30 and 1/10th scale diesel generator building and now scaled them to the prototype structure. As you can see, both structures predict the same frequencies for the prototype and they predict them well into the inelastic range or the non-linear range, because as we start to get a dropoff in the resident frequency, that implies that the structure is being damaged.

We have scaled the acceleration levels also to a prototype. Again, you can see that failure is above 2-Gs. We've also looked similitude of the damping and it looks like that there is no distortion in the damping between the model and prototype.

If the damping mechanism is historetic, that's what it would turn out to be. If you went through the similitude laws, it would show that there should be no distortion.

Again, we have the actual G-levels and then the scaled to the prototype G-levels. At the end of --

MR. SIESS: Did the shake tests at HDR have any instrumentation on the structure?

MR. FARRAR: They had instrumentation, but that's a tall --

MR. SIESS: I know it's an oddball, but it's at least full size.

MR. FARRAR: Yes, I believe they do have instrumentation on the structure, but we felt that the geometry of the structure wasn't representative of the type of types of structures.

MR. SIESS: You couldn't make any direct comparisons. The only thing you could do if you're really worried about scaling, is now make a model of that.

MR. FARRAR: Right, that's --

MR. KENNEALLY: I don't think I'd want to try.

MR. FARRAR: That's too complicated a structure, I think. If you want to get the interior walls and all of that to -- I think that's a little bit beyond our capabilities.

We'll further address the scaling issue with some more recent tests.

MR. SIESS: I've got no problem with scaling.

MR. FARRAR: At the end of FY '84, we had tested 23 structures. We had tested them both statically and dynamically. The technical review group was most concerned about this reduced stiffness because it is higher than what the analysts would use in the design of these structures, and they were -- the problem here also is that these stiffness reductions are at very low load levels. The lowest excitation that we could put in on a shake table and control the shake table where we're seeing this reduction in stiffness, is well below a nominal stress level of 50 PSI.

MR. SIESS: When you say "reduction in stiffness," can I equate that to cracking?

MR. FARRAR: Yes.

MR. SIESS: Okay, you're seeing cracking.

MR. FARRAR: Let me rephrase that. If you're saying that we're seeing cracking, no, we are not seeing cracking.

MR. SIESS: You are seeing the consequences of cracking.

MR. FARRAR: We're seeing the degradation and the resident frequency of the structure.

MR. SIESS: That could only be attributed to cracking?

MR. FARRAR: Yes. No, only attributed to cracking, or possibly that we're not accounting for the boundary conditions during the test properly.

MR. SIESS: Is that enough to account for the reductions you're seeing?

MR. FARRAR: I think we'll see -- when I get further down in the presentation where there are our more recent tests, I think that we can lend a lot of evidence to, yes, that possibly, not accounting for the boundary conditions, or problems due to induced stresses due to the way we mount the structure, can cause a lot of this.

MR. SIESS: You can't account for what's happening without having to assume cracking?

MR. FARRAR: Yes.

MR. SIESS: Okay, that's what I wanted to know.

MR. FARRAR: Okay, all right, so the technical review group is much concerned about this reduced stiffness issue, and again, they aren't so concerned about the margins, because these things have shown that they have plenty of reserve margin.

MR. SIESS: In other words -- you haven't mentioned it, but wouldn't it be proper to say that they're really not concerned about the structure, but they might be concerned about the equipment that's on the structure, the way it was qualified and so forth?

MR. FARRAR: Exactly. The floor response spectra that that equipment was designed to, may be improper.

MR. SIESS: That's not my concern. I don't care whether the floor response spectra were proper or not. I'm concerned with how the equipment was qualified; whether the equipment would be qualified for the spectra I would now get.

It's entirely possible that it is.

MR. FARRAR: I agree. Yes, I guess that's a better way of stating it.

MR. SIESS: That's what Sandia was going to be doing, looking at what changes you would get if you assumed cracking, say, and then look to see to what extent the equipment qualification process would take those into account.

MR. FARRAR: That's correct. I just threw up one slide here to show who the members were because the technical review group has had quite an influence on the direction of this program. So, to let you know who these people are -- okay, so again, at this point, the technical review group is focusing on this reduced stiffness issue.

A Category 1 structure that's designed based on an uncracked cross section analysis, again, the stiffness reductions that we're looking for are not accounted for in the design. The plant equipment then could have been designed to -- again, we're getting into this --

MR. SIESS: You really mean that they were not accounted for in the analysis.

MR. FARRAR: In the analysis, that's right.

MR. SIESS: Whether the final design would account for them, you don't know yet.

MR. FARRAR: Right, okay. Then the other problem is --

MR. SIESS: The same thing here, the equipment is not designed for a spectra; is it?

MR. FARRAR: It's my understanding --

MR. SIESS: You don't go out and tell somebody; I want a pump that will --

MR. FARRAR: No, the analyst would come up with a spectra for the particular site.

MR. SIESS: Then you would get a pump and send it down to Wylie Labs and prove that it will operate when subjected to that?

MR. FARRAR: Yes.

MR. SIESS: So it's been qualified -- it's an EQ, equipment qualification issue.

MR. FARRAR: I've used the wrong terminology.

MR. SIESS: It makes a difference to what I hear next.

MR. FARRAR: Then the other problem with the reduced stiffness issue is that generally these structures have -- you know, because they're very short and squat, they have fairly high resonant frequencies. The reduced stiffness will then shift the resonant frequency of the structure down into the area where an earthquake dumps more energy and potentially would cause more damage.

MR. SIESS: Now, what does it do to damping?

MR. FARRAR: The reduced stiffness?

MR. SIESS: Well, the phenomena that lead to the reduced stiffness.

MR. FARRAR: We have seen that the damping stays fairly constant until you're at a pretty high acceleration level. If we look at that previous slide, --

MR. SIESS: I would expect cracked concrete to have a higher damping rate than uncracked concrete.

MR. FARRAR: I have a slide further on that addresses damping specifically, and as it turns out, that would have been my feeling as well. It stays within a fairly narrow band, up until you get very high stress levels above what we would expect in an SSE.

MR. SIESS: High stress in what, steel or concrete?

MR. FARRAR: Concrete -- well, high stress in terms of like a nominal base shear stress.

MR. SIESS: Now, that second conclusion; as I remember several years ago, there was a conclusion that the reduced stiffness really didn't mean much for the structure.

MR. FARRAR: Right, as far as collapse of the structure. It doesn't seem to --

MR. SIESS: And it still doesn't, even if it puts it down into the higher energy range?

MR. FARRAR: Yes. We've never been able to actually have a structure fall apart. When we say failure of the structure, we're talking about that we can see fractured reinforcement in it.

MR. SIESS: You're not going to see fractured reinforcement in anything.

MR. FARRAR: But we cannot make a structure fall over.

MR. KENNEALLY: In the next presentation, you'll see some of the more quantitative results that would indicate what happens on this difference in frequency.

MR. SIESS: That's what I'm trying to build up to in my mind here.

MR. FARRAR: All right, so, one of the things that, because of this emphasis on the reduced stiffness issue, what are the causes of it? Is there something with microconcrete that it's behaving differently than conventional concrete or just being more susceptible to shrinkage cracking beforehand?

MR. SIESS: What, in your mind, makes microconcrete micro?

MR. FARRAR: What we have used is No. 4 sand or smaller.

MR. SIESS: Okay, so, if I have an actual structure with some 12-inch walls, with inch, inch and half maximum size aggregate and somewhere else, I've got something with 2-3 foot thick walls where somebody decided they could save a little money by going to 2-inch aggregate, would you expect to look for a difference there?

MR. FARRAR: Based on the stuff I'm going to talk about later, no. We've done studies to look at the different size aggregates, similar structures made with different size aggregate, and see if that really is a possibility.

MR. SIESS: Okay, so you're really thinking in terms of maximum aggregate size rather than words like microconcrete which don't --

MR. FARRAR: Yes, maximum aggregate size.

MR. SIESS: I just wanted to get clear how you're looking at it.

MR. FARRAR: Okay, now we have the problem particularly with these structures that were taken to CERL in the early part of this program. You know, was there a damage incurred in the shipping process?

We built them at Los Alamos, put them on a flatbed truck and ship them out to Illinois, which is about a thousand miles away, I think. You know, can they be damaged in the shipping?

MR. SIESS: The damage being nothing more than cracking?

MR. FARRAR: Cracking, in this case, yes.

MR. SIESS: That's why I asked earlier if anybody has gone out and looked at actual buildings. You don't have to have an earthquake to have cracking.

MR. FARRAR: Yes, and then again, I think the issue that's going to come up more is; what were the actual boundary conditions during the test, and when we compared with theory, are we comparing with -- is the theory really what predicting -- or similar to what we have in an actual test?

MR. SIESS: You didn't put something like a Redhead meter on the truck with that specimen to see if --

MR. FARRAR: No.

MR. SIESS: -- what kind of G's it got?

MR. FARRAR: No, we didn't.

MR. SIESS: In retrospect, that might have been interesting.

MR. FARRAR: It would have been a good idea and also, when we get into what we're going to call the TRG series of tests, one of the structures that we built at Los Alamos and then shipped to CERL, there was visible cracking in that structure.

MR. SIESS: I think I saw that one.

MR. FARRAR: Yes.

MR. SIESS: It was at the bottom.

MR. FARRAR: Right, exactly, at the base of the structure.

So, the current program emphasis -- and when I say current, I'm talking now about from FY '85 on -- was to -- both the TRG and the NRC staff felt a need to resolve this reduced stiffness issue.

The technical review group then proposed an ideal test structure geometry to look at this reduced stiffness issue and at the same time, we started interacting with the ASCE Dynamic Analysis Subcommittee of the Nuclear Structures and Materials Committee, and they formed a working group to investigate this reduced stiffness issue.

MR. SIESS: How many of your advisory committee members are on that working group?

MR. FARRAR: Bob Kennedy and John Stevenson are on that.

MR. SIESS: Then they called Sozen in as a consultant; didn't they?

MR. FARRAR: Yes.

MR. SIESS: It's hard to get a peer review in this business; isn't it?

MR. FARRAR: Yes. This is the structure that they suggested we start using to -- the structure of the geometry to start looking at the reduced stiffness issue. They put a bunch of -- actually, they didn't specify this configuration per se. They gave us a bunch of design criteria. The design criteria was that they wanted a structure made of what I am going to call conventional concrete which was with 3/4 inch aggregate or larger, use conventional reinforcement.

Before, we had used wire mesh in a lot of the structures or scaled reinforcement that was available from PCA.

MR. SIESS: You had wire mesh in the very early
little ones?

MR. FARRAR: Yes.

MR. SIESS: Did you use it in anything bigger than
that?

MR. FARRAR: That was the half inch square hardware
cloth that we used in those small models.

MR. SIESS: That's not what you meant by wire?

MR. FARRAR: No, that is what I meant by wire mesh.
Later on, we're going to use wire mesh that's typical of what you'd put in a sidewalk for reinforcement in some of our later --

MR. SIESS: Wire fabric.

MR. FARRAR: Wire fabric, I guess, would be the term that they would use. They wanted 4-inch minimum wall thickness. They wanted the resonant frequency below 30 Hertz. They wanted uncracked cross-section strength of material analysis.

That's why we have these large steel plates on top. It turns out that those plates helped get the normal stresses in these structures more to what they would be in a prototypical plant.

MR. SIESS: Now, the early big tests at CERL were
boxes; weren't they?

MR. FARRAR: Right.

MR. SIESS: This, now, is T-shaped?

MR. FARRAR: Well, an I-cross section.

MR. SIESS: Did they dictate that?

MR. FARRAR: No.

MR. SIESS: You just did it because you wanted to see both sides of the wall?

MR. FARRAR: If we put two walls in and got that 4 inch wall thickness, the structure becomes so stiff that the frequency characteristics of the CERL table would not allow us to test it there. We wanted to put those on to try and help any out of plane motion of the shear wall.

MR. SIESS: You bring in the question of how much of the flange is acting with the wall, but that's --

MR. FARRAR: Yes. We'll address that issue later on. Everything is later on here.

MR. SIESS: I didn't know whether it was done deliberately to bring that in or not.

MR. FARRAR: No. As it turns out, that was one thing that --

MR. SIESS: Is that one of the boundary conditions you're talking about?

MR. FARRAR: No. I'm talking about the boundary condition of -- what's the fixity condition, essentially, when we test it.

MR. SIESS: Okay. Fine.

MR. FARRAR: As it turns out -- I'll jump ahead in just a second here. Because we have these -- this is 12 inches of steel bolted to the top of the structure. In addition, we have a thick concrete slab on top. All that stuff held together tends to make that plane section remain plane and make these end walls fully effective. We can see that in the static testing of structures that we've done like this.

So this is the general geometry that we came up with based on their design criteria.

MR. SIESS: It puts a pretty good compression load on that wall, too, doesn't it?

MR. FARRAR: I think it gets up to about 40 psi.

MR. SIESS: That's all?

MR. FARRAR: Yes.

MR. SIESS: Okay. That's minor. It sure eliminates the warping, doesn't it?

MR. FARRAR: Yes. In the TRG series of tests, we tested 15 structures. The structures were made out of -- again, I'll use the term conventional concrete. Actually, we looked at three-eights inch aggregate and three-quarter aggregate structures and then also the micro-concrete; again, which is No. 4 or smaller sand.

The structures were tested statically, some of the structures, and then some of the structures were tested dynamically. We do that two ways. We do what we refer to as experimental modal analysis. That is where we put the structures on air bearings to simulate free boundary conditions.

We hook up a small shaker, drive it with a random signal, measure acceleration response at a variety of location that are indicative of the structure's motion, and then we calculate the frequency response functions for each of those points, and in the frequency, the main curve fit, a parametric form of a one degree of freedom equation to the frequency response function to back out the modal parameters.

MR. SIESS: I want to ask you a completely irrelevant question, but it was intriguing me yesterday. Do you make these slides on a computer?

MR. FARRAR: Yes.

MR. SIESS: Do you have a spell-checker?

MR. FARRAR: I doubt it. What did I spell wrong there?

MR. SIESS: That's not important. But yesterday there were about four slides with spelling errors and we were sitting here debating whether they had a spell-checker for making slides.

MR. FARRAR: They have a spell-checker, but I learned to use that computer two days ago to make these slides. So making the slides was as far as I got along.

MR. SIESS: Well, typos on slides have been with us for as long as we've had slides, I guess. But it just occurred to me now that we've got spell-checkers to do -- experimental is the error in the fourth line.

MR. FARRAR: You're right.

MR. SIESS: But that's unimportant. They represent minor distractions to the audience. That's all.

MR. FARRAR: This shows now the different geometries that we tested. What I'm going to discuss now is some of these tests can be put into certain groups with the -- where each group of tests had a specific purpose, and I'll now address those groups of tests within this overall TRG sequence.

The first group of tests was actually just really two tests; TRG-1 and TRG-3. TRG-3 is the structure that I put up there when I showed you what we came up with based on the Technical Review Group's design criteria. That structure was made, again, with three-quarter inch aggregate.

TRG-1 was a one-quarter scale model of it, made with micro-concrete. The purpose of these tests was to determine if a conventional concrete structure would exhibit the same reduced stiffness as we had observed with all the previous micro-concrete structures. We were also trying to look at the scalability between the micro-concrete and the conventional concrete.

The tests that we performed were, again, the experimental modal analysis. Then we did static monotonic loading to a very low level where we tried to keep below 40 psi principal tensile stress. Then we did a simulated seismic excitation on shake tables.

MR. SIESS: Now, to the best of your knowledge, there are no cracks in that specimen to begin with.

MR. FARRAR: On the large TRG-3 structure, that's the one where we shipped to Champaign before it was simulated, seismic tested, and I could see cracks in the base of that structure.

MR. SIESS: But the other one, the small one.

MR. FARRAR: The small one, there are shrinkage cracks, small shrinkage cracks that you can see once the forms are pulled off. Again, that one would only have a one-inch thick wall. In those walls, you can't tell if those cracks go all the way through. It's not apparent.

MR. SIESS: Did you do any dye injection to look at it --

MR. FARRAR: No, we did not. On the larger structures, we did ultrasonic inspection to see -- to try and determine cracks or voids within the structure.

MR. SIESS: If you wanted to know where a crack goes, you just dump some dye in it and when you get through, you break it open and look.

MR. FARRAR: I see. No, we did not do that.

MR. SIESS: But you did look to see if you could see cracks.

MR. FARRAR: Yes.

MR. SIESS: Did you wet it and allow it to dry out and look at the wet concrete?

MR. FARRAR: We looked at it when we pulled the forms off. When we pulled the forms off, the structure was still wet.

MR. SIESS: That's one way, yes.

MR. FARRAR: After we got done shipping it, of course, the forms had been off for a while, but the cracks were large enough that you could actually see.

MR. SIESS: Now, in the lab, the people are using a dye and a fluorescent light technique.

MR. FARRAR: That doesn't just get into the voids and you don't see --

MR. SIESS: No. That's the way he looks for cracks all the time. It's the not the way I did, but then things have gotten better since I quit testing stuff. We used to do it with a flashlight. There are ways. This fluorescent dye finds cracks that you wouldn't expect to find otherwise.

MR. FARRAR: To give you an idea, this is the TRG-3 structure, the large structure, again on the shake table at the Construction Engineering Research Laboratory.

Again, you can see one of the engineers over here, a fairly large structure. In fact, the largest structure they've ever put on that shake table according to the operators. What it turns out when you test one of these with this much mass up at top and it's a bi-axial table, and we're only trying to excite in one direction, that you get a lot of problems with overturning moment.

MR. SIESS: You must be their best customer.

MR. FARRAR: We were up until a while ago.

MR. SIESS: Only customer.

MR. FARRAR: We never seem to have a problem getting on the table there. The results that we got from this showed that the conventional concrete structure actually showed more stiffness reduction during the simulated seismic test than we had observed with the micro-concrete structures and the micro-concrete model here.

We could demonstrate scalability during the low level static test and during the experimental modal analysis. But, again, this question about was the structure damaged during the shipping, we know it was. We saw cracks in this structure. Really, leave the issue of scalability still a question.

MR. SIESS: I wish you would quit saying damaged. To me, a cracked concrete structure is a perfectly normal --

MR. FARRAR: I see your point, yes.

MR. SIESS: I don't consider cracking damage to reenforced concrete and I don't think you should, or we're going to mislead the public about the safety of our installations.

MR. FARRAR: Okay.

MR. SIESS: I'd hate to have them going around worrying about every time they see a crack in a concrete wall.

MR. FARRAR: To show where this data is fit in with our other tests, all the structures down here, the white dots without any box around them are previous static tests on the micro-concrete shear walls and diesel generator buildings.

The ones with squares around them are from the dynamic tests. Again, in the dynamic tests, we have to infer stiffness from resin infrequency measurements. During a low level static test and the modal test --

MR. SIESS: Wait a minute. You're going a little too fast. Let's see what's plotted there first.

MR. FARRAR: Okay. We have the normalized stiffness. By normalized stiffness, we take the measured stiffness, divide it by the theoretical stiffness based on strength of the materials --

MR. SIESS: Is the shear stiffness the G/J?

MR. FARRAR: Yes. We do actually put a bending component in there, but it's insignificant to --

MR. SIESS: Horizontally, these are just different grades of concrete.

MR. FARRAR: Right. One of the Technical Review Group members, early in --

MR. SIESS: Whether there was a --

MR. FARRAR: Yes. An effect due to the -- yes. So we started out plotting and I guess at this point it's just a nice way to spread the data out so you can see it all. When we did the initial low level test, both the experimental modal analysis and the static, we got pretty good agreement with theory -- I shouldn't say pretty good. We got better than we've ever gotten before.

MR. SIESS: I still haven't -- you're going a little too -- there's too much on there for me to absorb. You did something I think you can correct. You explained the differences in the models back when I didn't have the slightest idea what had bent. Now, when I'm looking at the figure, I'd like to know which models are what.

MR. FARRAR: Let's start over here.

MR. SIESS: That batch down along the .25 line are what?

MR. FARRAR: Those are the diesel generator building models that we tested in the first --

MR. SIESS: These are static, dynamic scale?

MR. FARRAR: If they have the box around them, they would be dynamic tests.

MR. SIESS: Okay.

MR. FARRAR: Simulated seismic.

MR. SIESS: What distinguishes them from the ones that are up near the top?

MR. FARRAR: The ones up near the top, these are the static tests and experimental modal analysis tests.

MR. SIESS: Different kinds of testing.

MR. FARRAR: Yes.

MR. SIESS: And different ways of calculating the stiffness.

MR. FARRAR: Right. Well, no.

MR. KENNEALLY: Different models.

MR. FARRAR: In the static test, in theory, we're measuring stiffness directly.

MR. SIESS: Okay. That was one you said. Then you said differential modal analysis.

MR. FARRAR: The experimental modal analysis is different than a shake table test, because it's a -- again, we have a free boundary condition or we simulate that with air bearings.

MR. SIESS: That's the one that you supported up there.

MR. FARRAR: Yes.

MR. SIESS: So it has different boundary conditions.

MR. FARRAR: Yes. In a sense, we take boundary condition problems out of the --

MR. SIESS: And it's not static. You vibrate it and --

MR. FARRAR: You vibrate it, but at a very low level.

MR. SIESS: Yes. Okay. Now, which is which up there?

MR. FARRAR: The box here are --

MR. SIESS: You've got more than I've got here.

MR. FARRAR: I know. I didn't have a copy of this that I could reproduce. This was the closest that I had.

MR. SIESS: The bottom with the red dots in them are?

MR. FARRAR: Okay. This would be the TRG-3 structure over here. This is the static test. The low level, where we didn't exceed 40 psi principal tensile stress.

MR. SIESS: And you still came out at seven. All right.

MR. FARRAR: Yes. This would be the experimental modal test.

MR. SIESS: Okay.

MR. FARRAR: It gave about the same results.

MR. SIESS: Are those different from the ones at .25? In what way?

MR. FARRAR: The static tests, the ones at .25 -- now, this would be the same structure at .25. This is a simulated seismic excitation. We have it mounted on a shake table.

MR. SIESS: Yes.

MR. FARRAR: So the sequence of tests --

MR. SIESS: If I made a static test, I've got a 25 percent reduction in stiffness. If I made a dynamic test, I got a 75.

MR. FARRAR: Yes. And that's why we think that there --

MR. SIESS: That's what that's telling me?

MR. FARRAR: Yes.

MR. SIESS: What about the ones that don't show any reduction in stiffness?

MR. FARRAR: Those were some other investigators' results that were at, again, very low load levels, that they got very good agreement with theory. But then when they got up to higher load levels, they got reductions consistent with what we had measured.

MR. SIESS: If I look at the 25 percent reduction -- I may be pushing you ahead, but please try to answer it. Can you explain that in terms of your boundary conditions on an uncracked specimen?

MR. FARRAR: At this point in the testing, this would be like at the end of Fiscal Year 1986, roughly, when this was done.

MR. SIESS: I'm talking about now.

MR. FARRAR: Now, yes. I think I can.

MR. SIESS: Now, the 25 percent, the bottom batch.

MR. FARRAR: Yes.

MR. SIESS: Can you explain those without having to
assume cracking?

MR. FARRAR: Yes. Well, I thought that's what you
were just asking.

MR. SIESS: No. We talked about the top batch.
Forget about the ones on the top up there.

MR. FARRAR. Okay. This group right here.

MR. SIESS: Two sets; 25 percent reduction and 75
percent reduction. First I asked about the 25.

MR. FARRAR: I would have a harder time explaining
that as opposed to this.

MR. SIESS: I said 25 -- let me use the numbers on
there.

MR. FARRAR: Sure.

MR. SIESS: 75 percent is the upper, 25 percent is
the lower. I won't talk about the reduction. The 75 percent
you could explain.

MR. FARRAR: No. The 75 percent, you mean this data
here.

MR. SIESS: Right there.

MR. FARRAR: I would have probably a tougher time
explaining why that didn't come in theory now than I would
having -- I think I can have a much better explanation for why
this didn't come in theory. This data here came in so low.

MR. SIESS: You're using terms I don't understand.

I'm going to rephrase it to be sure you understand.

MR. FARRAR: Okay.

MR. SIESS: At the 25 percent level.

MR. FARRAR: Yes.

MR. SIESS: Can you explain that reduction in stiffness without having to assume cracking?

MR. FARRAR: Yes.

MR. SIESS: At the 75 percent level, can you explain that without having to assume cracking?

MR. FARRAR: No.

MR. SIESS: That I can't understand.

MR. FARRAR: I understand why you can't understand that. That doesn't make sense.

MR. SIESS: It doesn't.

MR. FARRAR: The reason I -- what I would assume why in the static tests, I think it was limitations on the instrumentation that we used there, but I don't have any way of verifying that at this point.

What I do have is tests with more now -- with more refined instrumentation where, at those low load levels, I will get very good agreement with theory. So that these values would be --

MR. SIESS: I'm not sure that those values exist.

MR. FARRAR: Correct.

MR. SIESS: Then the $64 question. Do you think you can explain that 25 percent value in terms of boundary conditions?

MR. FARRAR: Yes, I do.

MR. SIESS: That's great.

MR. FARRAR: And I'll go into that. That's what -- all right.

All right. So, now, we have a next group of -- we presented these results to the technical review group. At this point, the technical review group now was convinced that this 25-percent reduction -- or this stiffness reduction down to 25 percent was real, because they saw it on a conventional concrete structure. All right?

So, what they said, now, what they wanted to was, now let's look and see if we can come up with a method or do a series of tests to find out this reduction in stiffness as a function of the aspect ratio of the shear wall and the percent reinforcement in the shear wall. All right?

So, this next group of tests -- they proposed -- they came and said they want to do this and asked us to come up with a test matrix to look at these.

So, we came up with a text matrix, and then, at the next TRG meeting, they decided that that next matrix was too costly, that we shouldn't do it, and we had already built the TRG-4 structure and were a ways into the construction of the TRG-5 structure.

So, they suggested, well we'll do one more in addition to that, and that would, you know, suffice for what they wanted to look at.

Now, again, the tests that were done on this group of structures was the experimental modal analysis, because again, that gives us a good way of looking at the dynamic properties of these structures without introducing much damage, and then we did static cyclic loading to failure.

This was the test matrix that had been originally proposed. They gave us the limits, showing that the percentage of reinforcement went from about .25 to 1 percent, by area, and that the aspect ratio was about .25 to 1.

As you can see, most of our tests were right in the middle of that in terms of percentage. The aspect ratios varied -- our previous tests on the micro-concrete models, and let's see, the TRG-4 would be this point A up here. It would have an aspect ratio of 1 and .25 percent reinforcement.

They also wanted us to test statically a structure like the TRG-3 one that we had tested on the shake table and found -- that was the first conventional concrete structure that we saw the reductions in stiffness, and then --

MR. SIESS: They were going through all of this to try to find something that would test without a reduction in stiffness?

MR. FARRAR: At this point, they believe the reduction in stiffness, and what they want to do is now find --

MR. SIESS: They didn't believe it would be true in real life for real structures.

MR. FARRAR: Yes. But when we tested that conventional concrete structure, you know, even with all the questions about it, I think they were more convinced, then, when they saw the same reductions in stiffness.

MR. SIESS: They're easily convinced, because until you test a real structure --

MR. FARRAR: We haven't demonstrated --

MR. SIESS: You haven't demonstrated anything --

MR. FARRAR: Right.

MR. SIESS: -- to me.

MR. FARRAR: I agree.

MR. SIESS: Yes. And that's why, 5 years ago, I suggested we stop all of this nonsense and try to find out what difference it made.

MR. FARRAR: I think that's what the Sandia program is --

MR. SIESS: I know.

MR. FARRAR: -- to try and figure out what difference it makes.

MR. SIESS: I have been waiting 5 years for it.

You know, if we have to know how stiff the real structure is in order to protect the health and safety of the public, we are in trouble, because I would hate to go before a hearing board or a court and prove what the stiffness was for the diesel generator building at any plant.

MR. FARRAR: I don't think that that would be an easy thing to prove.

MR. SIESS: Well, I could do it, but I might destroy it in the process.

MR. FARRAR: For the larger structures, this is what we do during the experimental modal analysis. You can see the shaker over here that we hook up to a plate that's dental-cemented on to the structure.

We had to lift it up with these nylon straps to put air bearings underneath it. Those air bearings are deflated at this point.

They get pumped up and simulate the free boundary condition, and the reason that we simulate the free boundary conditions, rather than bolt it down, is that when we do the test, if we bolt it down, we vibrate the stand, as well, and here, we can get the most direct comparison with, like, a finite element analysis of the structure.

MR. SIESS: To me, it would be awfully difficult to do that for the diesel generator building.

MR. FARRAR: But that's, in a sense, what they did at HDR, with that -- mass shaker.

MR. SIESS: I know, but we haven't got HDR.

MR. FARRAR: I know, but you have the potential to destroy the structure when you use something like they have in HDR.

The only thing that I have seen is they have looked at the vibration due to ambient wind, and I don't know if, on these short, stiff structures, whether that's a realistic option. I've seen it on high-rise structures.

But anyway, this just gives another view of what we do, how we hook the shaker up and test the structure.

MR. SIESS: What was CDTR structure?

MR. COSTELLO: It was a containment for the --

MR. SIESS: What was the structure? Concrete structure, steel structure?

MR. COSTELLO: Concrete.

MR. SIESS: They did some ambients on that, didn't they?

MR. COSTELLO: Oh, yes.

MR. SIESS: Somebody ought to look at that. Ambient will tell you something.

MR. COSTELLO: They also did shaker tests.

MR. SIESS: They did shaker tests, too?

Go ahead.

MR. FARRAR: Okay.

We also did the static load testing, and this is the load frame that we built, and it shows the structure in there.

You could see the load frame in the back there of that previous structure that showed the modal testing.

I'd like to point out that we put the gauges -- put relative displacement gauges on here, the diagonal one so we can -- diagonal and vertical gauges so we can separate out the shear and bending components of stiffness.

Again, we left -- even though we didn't need it for static testing, we left the steel plates on to get the normal stresses up with a level that would be typical of this type of structure.

I should point out one other thing that will come in with this issue about the boundary conditions -- is that this structure was poured in place on the load frame that it was going to be tested on. All right?

The only movement that it saw was the lifting up to put those air bearings underneath and putting back down, and it was put down in the same place, because during the pouring of the model, these bolts were holding it in place.

So, in a sense, we get a very good match to the surface that it is going to be tested on. Okay?

The load cycle that the structure saw looked something like this. We started out at -- the first level that we test at corresponded to 50 psi nominal base shear. We went to 100 psi, 200, 300, and actually, we never got to 300, because it failed before we got there.

MR. SIESS: You didn't get to load step 300?

MR. FARRAR: The load level, 300 psi nominal shear.

MR. SIESS: Oh, I'm sorry. I was looking at --

MR. FARRAR: I actually plotted them in terms of force, but --

MR. SIESS: Okay.

MR. FARRAR: We were trying to go -- at this level, go to 300 psi nominal shear, and it failed.

MR. SIESS: How did it fail?

MR. FARRAR: We opened up large cracks, both shear cracks and inflectual cracks through the end walls.

MR. SIESS: Got any pictures?

MR. FARRAR: Not of that one. I have pictures of another one.

MR. SIESS: Okay.

MR. FARRAR: So, the results that we got in this case was that -- the experimental modal analysis, the results agreed almost exactly with theory, or with finite element analysis.

MR. SIESS: What aspect -- what agreed with what?

MR. FARRAR: The TRG-4, -5, -6 structures --

MR. SIESS: You had to compare some quantity.

MR. FARRAR: All right. The resident frequencies identified by the modal analysis.

MR. SIESS: Okay.

MR. FARRAR: And then, the modal analysis, the software packages that are available today, you can actually look at the animations of the mode shape, as well, and the mode shapes also compared with what the finite element theory would predict.

The total stiffness and -- actually, when we separated the shear and bending components of stiffness, again, agreed almost exactly with their strength of materials theoretical counterpart, and as it turns out, then, the results that we got from these structures are starting to contradict what we found earlier in the program.

MR. SIESS: Why do you say -- they're different.

MR. FARRAR: Different. All right. Well, we see -- at similar stress levels, we see nowhere near the reduction in stiffness.

MR. SIESS: That's different. That doesn't contradict it.

MR. FARRAR: Okay. Different would be a --

MR. SIESS: Because something is different in the tests.

MR. FARRAR: All right.

MR. SIESS: Something is different somewhere.

MR. FARRAR: Right. All right. That would be a better way of --

MR. SIESS: Contradict would mean that what you got before was wrong. What you got before was perfectly right for what you tested.

MR. FARRAR: Okay.

MR. SIESS: But what you tested was different than what you tested here.

MR. FARRAR: Correct.

All right. The first figure shows the results of the experimental modal analysis.

If you look down at the bottom, I have the resonant frequencies for -- I guess it's the first six modes from the TRG-4 structure, compared with the resonant frequencies from a finite element calculation, and they agreed pretty well, and the mode shapes are similar, as well.

Then we have the -- now, we did the static cyclic testing.

MR. SIESS: Now, that's what you had back here?

MR. FARRAR: Yes. That's the response of the structure to that loading, and what happened was we got several -- the first five cycles there, where it's at 50 psi nominal base shear and 100 psi, we got almost exact agreement with the strength of materials theory.

While we were going to the 200 psi, and in this case, we loaded down in the negative direction here first, the structure cracked, and then, when we loaded in the opposite direction, you can see it cracked again on the first cycle, trying to go to 200 psi.

The subsequent cycles to 200 psi, it went back to behaving as a linear structure, but now with a reduced stiffness, almost a factor of 2 reduction in stiffness after we introduced what I would call a structural crack, as opposed to something that might have been there from curing.

And then, when we tried to go to 300 psi, the structure --

MR. SIESS: Okay. That's what -- you went this way.

MR. FARRAR: Yes, first.

MR. SIESS: And then you got into yield, I presume.

MR. FARRAR: Yes.

MR. SIESS: Yield of the rebar.

MR. FARRAR: Yes.

MR. SIESS: Okay.

MR. FARRAR: And then -- and in fact, now, we're getting to a point -- we were controlling our load during this test, and we couldn't keep pumping enough hydraulic fluid into the actuator to keep up with the deformation of the structure when you're out in this range here, and so, that's what we decided to call failure, and then we unloaded, and the same thing happened in the other direction.

We did one low-level test after we had essentially failed it, and that's what this final loop is down here.

MR. SIESS: That was about an inch?

MR. FARRAR: The total deformation?

MR. SIESS: Yes.

MR. FARRAR: A little bit more than an inch, yes.

MR. SIESS: If you use a mechanical jack, you wouldn't have the problem. You were doing static tests, weren't you?

MR. FARRAR: Yes.

MR. SIESS: Okay.

MR. FARRAR: Going back to the difference in result, well, first of all, just a comparison of the stiffnesses that we measured as compared with the stiffnesses in strength of materials, and when I say strength of materials, we apply Castigerano's Theorem to the section to ge the relative displacements of that field covered by the relative displacement gauges.

MR. SIESS: Assuming shear on them.

MR. FARRAR: No, we have a bending component in there, too.

MR. SIESS: Did you take that into account?

MR. FARRAR: Yes, for the total stiffness.

MR. SIESS: Okay.

MR. FARRAR: Again, then we separated that total into a bending and shear component.

MR. SIESS: Okay. Now, pounds per inch. Is that the right units for stiffness? It's not the right stiffness for J.

MR. FARRAR: F equals K-delta, right?

MR. SIESS: Okay.

MR. FARRAR: So stiffness would be pounds per inch.

MR. SIESS: For cross-sectional property, this is the models, this is for the whole model?

MR. FARRAR: Yes.

MR. SIESS: Okay.

MR. FARRAR: So as it turns out, the way we evaluated the strength of material stiffness, we took four different cases -- one where we considered those end walls fully effective; one where we used ACI T-beam criteria; one where we neglected the end walls altogether; and then this ASCE 486 design code for nuclear structures has a different criterion for how much you take into account -- and looked at all four of them.

This is taking the wall, end wall fully effective, in resisting bending. And this was consistent with all three tests.

MR. SIESS: That's interesting.

MR. FARRAR: But again, I think that is a function of the geometry of the test structures that we're looking at.

To emphasize the difference in results, during the static tests --

MR. SIESS: Incidentally, the code T-beam effective widths, to the extent that they are, are based on anything, are based on pure flexure behavior.

MR. FARRAR: Right. They are not shear. I realize that.

MR. SIESS: Yes. If they worked for shear, it would just be an interesting coincidence. But they don't even work for flexure, either.

MR. FARRAR: Okay.

MR. SIESS: If you look at what the Europeans do, where it depends on the load, the spacing, the beams and so forth, is it much more elaborate representation of test data. ACI is very crude.

MR. FARRAR: But we felt that those are the only criteria around that --

MR. SIESS: It is between zero and one.

MR. FARRAR: Yes.

MR. SIESS: Okay. If you want something in between zero and one you can describe, that is as good as anything.

MR. FARRAR: So from those static tests, up until we produced again what I call first structural cracking, which occurred at a nominal base shear stress of 130 psi or a maximum principal tensile stress of 171 psi.

MR. SIESS: Okay. That is not unreasonable.

MR. FARRAR: No. We will get an idea of how those relate to OBE and SSE levels a little bit later.

MR. SIESS: I was just thinking of tensile strength.

MR. FARRAR: Yes. It turned out they failed a little bit lower at tensile strength than what I would have thought. And I don't have a good explanation.1

MR. SIESS: Did you have any measures of the tensile strength of the concrete --

MR. FARRAR: Yes, I do.

MR. SIESS: -- split cylinder?

MR. FARRAR: Yes. That was up a little higher. So that was up around 300 psi.

MR. SIESS: Yes, it would be. Usually you don't get the theoretical.

MR. FARRAR: Okay.

MR. SIESS: There are too many things in there that can produce little residual stresses or weaknesses. And it is awfully hard to get the real tensile strength developed.

Go ahead.

MR. FARRAR: All right. And then as compared with when we did the seismic test on TRG-3, the stiffness was 25 percent of theory. This is now again that conventional concrete structure that we tested on the shake table. During the first seismic pulse, it is a nominal base shear stress of 91 psi and a principal tensile stress of 92 psi. And it was behaving quite a bit differently.

MR. SIESS: It was the same geometry?

MR. FARRAR: Same geometry.

MR. SIESS: Not the same support conditions.

MR. FARRAR: Same -- well, yes. But the one, I think the difference is that the one was built right on the fixture that it was going to be tested to. So that when we cast the concrete, there was nothing between it and the steel that it was going to be tested to.

It conformed right with the base. This other one, the one that we shipped to Champagne, was built here.

MR. SIESS: But you can explain the difference between these two without cracking?

MR. FARRAR: I think so.

MR. SIESS: Yes. And you said in terms of boundary conditions.

MR. FARRAR: All right. I can give you my rough idea, and I will give the data later on. What I support is that I think if that structure is not flat on the bottom, when you put it on a surface that is flat and put large torques on the bolts, you start to introduce high initial stress condition there prior to any test, as you deform the structure, to try and make it mate up with the surface that you are bolting it to.

Now, what is going to happen is, later on in some tests that we will get into next, is that the first structure we tried the next sequence, we did the same as before, when we put it on a shake table. We just tried to bolt it directly to the table.

We saw that, when we started to put it on, you could feel that it rocked. When we bolted it down, you could see that it cracked.

MR. SIESS: What you are saying here is that your boundary condition led to an early cracking.

MR. FARRAR: Yes.

MR. SIESS: And the cracking led to an early --

MR. FARRAR: Reduction in stiffness.

MR. SIESS: But when I asked you whether you could explain that reduction in stiffness without cracking, you said yes.

MR. FARRAR: You are correct. That's wrong. I cannot.

MR. SIESS: It still takes cracking --

MR. FARRAR: Yes.

MR. SIESS: -- to get that stiffness down to 25 percent.

MR. FARRAR: It is just how is that cracking entered into --

MR. SIESS: -- by geometry?

MR. FARRAR: Yes.

MR. SIESS: Okay.

MR. FARRAR: That's correct.

The other information that we can get from the static tests is we can look at hysteretic energy loss, and from the hysteretic energy loss, we can come up with an estimate of a damping value. The way we do that is take the energy loss due to the hysteretic energy loss and then equate that to the energy loss caused by a viscous damper during steady state excitation.

And this is an example from the first 50 psi load cycle, from that TRG-4 structure.

All right. So after this last set of three large structures that we tested statically, we started on a series of tests that were to help the people at Sandia, Mike Bohn, do it with their risk study of this reduced stiffness phenomenon. And what they needed -- Maybe I should take a step back.

In all our previous seismic testing, what we do is we start at a low excitation level and just monotonically step, you know, test one right after the other at higher and higher levels. And Mike wanted data on what if we just take a structure and hit it with one high level excitation.

MR. SIESS: What the heck has that got to do with the equipment qualification?

MR. FARRAR: I'm not sure, to be honest with you.

MR. SIESS: Okay.

MR. BOHN: It was a major question, in that all the later test were a series of step tests. An earthquake is just a one-pulse -'-

MR. SIESS: That has nothing whatsoever to do with the analysis and design of nuclear power plants.

MR. BOHN: I think it does, in designing for --

MR. SIESS: Just save it until later.

MR. BOHN: All right.

MR. SIESS: I'm sure you did what they asked you to do.

MR. FARRAR: So this last set of tests was to provide information about cumulative damage effects. That is what I am referring to, where we test at one level, one initial high level. And then we used these tests also to further address the scaleability issue. All these subsequent models were one-third scale models of the TRG-4 structure. That is the one that I just showed the static response for.

MR. SIESS: Now, a few slides back, you had concluded that these things were quite strong, it took a much larger 2-G earthquake to cause damage. And now you are re-examining that for cumulative damage effects? Is that what I am seeing?

MR. FARRAR: We said that the 2-G level was to cause failure of the structure. We had to get above, the structures were not in danger of collapse at levels up to 2 Gs. What we are looking at now is, we want to see, let's say if we hit a structure -- I'm just going to throw out random numbers -- with 1 G, and we see a certain degradation in the frequency, the resonant frequency of the structure, because of damage, will that be the same as if we had hit that structure with a .25 G, or .5 G?

MR. SIESS: Wny are you interested in the degradation of the resident frequency?

MR. FARRAR: That information was to support the risk assessment.

MR. SIESS: It has nothing to do with the strength --

MR. FARRAR: No.

MR. SIESS: -- of the structure?

MR. FARRAR: This was to provide information for the risk assessment.

MR. SIESS: Okay.

MR. FARRAR: All right. So again we did the experimental analysis; we did the static cyclic testing on some of these structures and then we did simulated seismic testing.

Some of these one-third scale models were made of microconcrete and others were made with 3/8ths inch aggregate.

One of the other things that our people on our technical review group asked is if we can come up with better ways to measure stiffness directly during a dynamic test as opposed to having to infer it from resident frequency measurements.

That requires us to measure the force that the structure sees and also to measure directly displacement, where normally we would measure acceleration response.

The method that we came up with for doing that is to mount strain gauges on the structure. And these correspond to those gauges that I showed during the static test. But now we mount these strain gauges in a series and wire them in series so they act as one long strain gauge. And we did that in a sense just to match the relative displacement gauges that we had during the static tests.

MR. SIESS: So are you just measuring the displacements?

MR. FARRAR: Yes. So it's to get a relative displacement measurement.

MR. SIESS: And where are you getting the force?

MR. FARRAR: The force we still have to estimate just from an inertial force, measure the acceleration -- you see we have accelerometers up at the top here -- of that top mass, and say that that, just use an F equals MA to get the force.

MR. SIESS: Now, why do you need to know all this?

MR. FARRAR: This was, our technical review group wanted us to look, because we were inferring frequency, stiffness reduction from frequency measurements, they wanted us to actually measure stiffness.

MR. SIESS: But the reason you want to know the stiffness is to get the frequency.

MR. FARRAR: That's right.

MR. SIESS: And they object to getting the stiffness from the frequency to get the frequency from the stiffness? If

I hear what I'm hearing, it is absolutely ridiculous. You are measuring frequency, that is what we are concerned with.

MR. FARRAR: I agree.

MR. SIESS: And they want you to measure stiffness so they can calculate the frequency.

MR. FARRAR: I agree.

MR. SIESS: Okay. But you got to do what they ask you. All right.

MR. KENNEALLY: The results, they were really very happy with the displacements that we were able to get from the static testing, and they tried to get a correlation in there.

MR. FARRAR: All right. The thing that we started, this is now the series --

MR. SIESS: I am disappointed in your technical review group.

MR. FARRAR: This is, it was with the first structure in this series that we noticed now the problems with the base where we would have any irregularity. What we did in all these structures to take that problem out was to put a layer of plaster of paris down. First of all, we had to bolt an aluminum plate to the shake table and then put a layer of plaster of paris, so it would mold to the bottom of the structure, and in a sense, take up the gaps.

Again, we did the experimental modal analysis. This one actually shows all the points that we measure acceleration response at during a test.

Again, the structure is sitting on the air bearings to simulate free boundary conditions.

We then did the static cyclic testing of some of these structures as well. This was again at the TRG's request because they felt that if we got agreement with theory with those large structures, to show scaleability, we should be able to get agreement with theory with these smaller ones.

And to verify that that strain gauge instrumentation scheme would work, in these static tests we mounted the strain gauges right in back of the displacement gauge.

MR. SIESS: Why don't we just move ahead a little bit to the results?

MR. FARRAR: Okay.

MR. SIESS: I think we've heard so much on that I don't know what to look for.

MR. FARRAR: All right.

MR. SIESS: And we can ask you questions.

MR. FARRAR: This shows, this is the test sequence that we used to look at the cumulative damage effects.

Again, like the first structure, we test as we normally have, just incrementing up in the peak acceleration levels during the excitation, and then the second test we would jump in at the second level and then by looking down one of the columns we can look at cumulative damage effects.

MR. SIESS: I like the last column best.

MR. FARRAR: The question marks were there because of whether we could reach the acceleration levels on the shake table without reaching shake table limits.

MR. SIESS: Could you?

MR. FARRAR: Yes. But we can't reproduce the signal then. We can get high acceleration levels, but we can't reproduce the command signal that we're using.

This shows the results that were obtained from all this testing on the TRG structures, to TRG-4. If we look, first of all, let's look at TRG-4. At 50 psi and 100 psi, we got theory. When we started to go to 200 psi, we cracked the structure. And then the stiffness drops off.

Same thing with TRG-5. All these points right here, there are five points stacked up on top of each other. Those are essentially all at one. But I had no way of showing five different structures at one.

MR. SIESS: Which one?

MR. FARRAR: These ones right here and right here.

MR. SIESS: Okay.

MR. FARRAR: They are really all at one. And the same with the two out here.

MR. SIESS: If you didn't know anything at all, where would you expect it to crack?

MR. FARRAR: For that one, for the TRG-4 I would have expected a little bit higher, like at 200 psi. And we cracked on the way to 200 psi. That would be just looking like at an ACI design criteria.

MR. SIESS: Let's see. TRG-4 is?

MR. FARRAR: The black square. All right. So 50 and 100, it was right at 50, and then it dropped off.

MR. SIESS: Now, should we be looking at the individual ones or is it the aggregate? I just see points. I don't see any lines drawn to tell me what to look at.

MR. FARRAR: Okay.

MR. SIESS: And I am looking just at the overall rate.

MR. FARRAR: All right. Let's look at the overall picture.

The overall picture would say that up to about 100 psi, either microconcrete structures, conventional concrete structures, 3/8ths-inch aggregate structures, structures that were tested statically, structures that were tested dynamically, either experimental modal analysis or simulated seismic, the most reduction stiffness that you are going to see is about 30 percent. And that corresponds to about an OBE level.

MR. SIESS: And that doesn't require any assumption about cracking in the concrete to explain? That is just within a scatter band you would expect from the material, or what?

MR. FARRAR: I think maybe a little bit of, a little bit more than a scatter band for material. I'm not sure that we have a perfectly fixed-base condition during that. The ones that are low here are dynamic tests on the shake table. And from accelerometer readings that we have at the base of the structure, those structures do not get excited only in the horizontal direction. There is --

MR. SIESS: You are comfortable that up to those levels it is probably not cracked --

MR. FARRAR: Yes.

MR. SIESS: -- and the reductions are due to other --

MR. FARRAR: Yes.

MR. SIESS: -- test phenomena?

MR. FARRAR: Yes.

MR. SIESS: Or something else. Okay.

MR. FARRAR: Now, when we start getting up about OBE level, I think we start to see the structures are starting to show cracking. And then the stiffnesses are dropping off. And again, static and dynamic test data is overlaying and microconcrete and conventional concrete data is overlaying. You can't distinguish between the two.

MR. SIESS: Now, what about those four little fellows down at the end?

MR. FARRAR: All right. I think that those are all dynamic test on the shake table. Again, we are not really reproducing the seismic input that we tried to. I mean, we are just hitting them with as big a shot as we can.

MR. SIESS: And you think those stiffnesses are --

MR. FARRAR: Those stiffnesses are associated with visible cracking and some of them are fracture of the rebar during the test.

MR. SIESS: You actually broke rebar?

MR. FARRAR: Yes. Actually big chunks of concrete flying. These are now the smaller models, so that we can do that more reasonably than the big ones, and we can break the reinforcement in those tests.

MR. SIESS: You said reinforcement that time. You said rebar before.

MR. FARRAR: All right. These structures where we broke the reinforcement were using the wire mesh or the welded wire fabric you would put in it, and I don't believe that has the ductility.

MR. SIESS: Not by a long shot.

MR. FARRAR: And we see that in that the failure mechanism in these structures is not the failure mechanism in the static structures that are made with conventional reinforcement.

The other information we got from that is damping information.

Again we evaluated damping in a variety of methods.

We've done it during the dynamic tests, both in the time domain and the frequency domain and then we have it from hysteretic energy loss in the static tests.

There's certainly more scatter but, well, I put in there the horizontal lines, the SSE lines and the OBE. Those are the ones that Reg Guide 161 specifies, 4 percent for OBE and 7 percent for SSE.

MR. SIESS: That is why OBE governs.

MR. FARRAR: Pardon me?

MR. SIESS: That's why OBE governs the design.

MR. FARRAR: And if you see the stress levels that correspond to that, we haven't really gotten -- we haven't been able to measure damping values as high as what is specified.

Now there's a lot of assumptions that go into the measurement of damping values.

I think it is a harder parameter to measure but this is the data that we have obtained.

We have to get fairly far out before we start to see significant increases in the damping. Again this is all equivalent viscous damping that we evaluate this in terms of.

We are still in the process of reducing the data to look at measuring the displacements directly in the dynamic tests.

MR. SIESS: Is Sandia looking at the significance of the reduced damping as well as the reduced thickness?

MR. FARRAR: I doubt it but I guess Mike will have to address that when he talks.

MR. BOHN: The answer is no, because that data shows on full-scale structures up to about a g there is no effect on damping --

MR. SIESS: Do we have test data on full-scale structures up to a g?

MR. BOHN: No. These are -- I am taking, the only thing I'm using are Chuck's data here. What the early data plus this data implied is that up to about a g the damping is pretty much constant.

MR. SIESS: Do you see that on this plot?

MR. FARRAR: On this plot a g would, 1g on these tests, for the dynamic tests, would correspond to about somewhere in the 80 psi nominal base shear stress region.

MR. SIESS: On that scale?

MR. FARRAR: Yes, for these tests -- on this plot the structures that are plotted at about 80 psi on these scale models, they saw about 1g acceleration, 1 to 2g, in that range.

MR. SIESS: I just am hearing words that don't make any sense to me at all.

One of you is telling me that there are damping factors that come nowhere near as high as the Reg Guide 161 damping factors at the OBE, which would seem to be important, as important, and somebody else is saying that up to 1g, which is in the neighborhood of five to six times the OBE there is no change in the damping factors.

MR. FARRAR: He means change -- if they are all here at 4 that it has to get up past 1g before they are jumping up like in this ten percent range.

MR. SIESS: But I am concerned about the low ones.

MR. FARRAR: I agree that there's a -- from this data it appears that those, from my standpoint that those damping values are not conservative.

MR. SIESS: Yes, that's what I heard.

MR. BOHN: In our calculations we use 7 percent for the concrete structures -- 5 to 7 percent -- but we didn't have it vary with g level.

MR. SIESS: How about using 2 percent?

MR. BOHN: We didn't --

MR. SIESS: I know you didn't but I am a regulator and I look at these tests and I say they'll get nowhere near 4 percent damping at the OBE. I know that the OBE governs the design, not the SSE and I say you mean we've been building these things out there for 4 percent damping or 5 percent damping when you can only get 2? Now go back and tell me are we in trouble?

MR. FARRAR: Let's look. When we say 4, if we took the average of this data right here it would probably be around 4 percent but there are significant --

MR. SIESS: I don't find many people in the NRC that are willing to take averages. They generally look at the lower values.

MR. FARRAR: Well, if they look at the lower, then you're right.

MR. SIESS: If you are going to worry about this you look at the low ones. You know, if I wanted to look at the high ones, I wouldn't worry, so one thing people are concerned about or think they are is that we have got stiffnesses at one-fourth were assumed in the analysis and somebody got floor spectra and somebody might have gone out and bought a pump --

MR. FARRAR: I understand.

MR. SIESS: What's more, we've got damping factors that are wrong and if we'd put in 2 percent damping we might have gotten a completely different answer and how does that affect us?

Those are the questions that I think the regulator asks as a result of these tests. Maybe the answer is that these aren't worth a darn for damping.

MR. FARRAR: Well, that might be. I'd like to have somebody point out in my tests where they had gone wrong but there's a lot of assumptions that go into evaluating damping.

MR. SIESS: There are a lot of assumptions that go into analyzing the structure too, and that's the ones we're looking at.

I don't for a minute believe that the dynamic analysis that somebody made is the proper one.

MR. FARRAR: Okay.

MR. SIESS: I mean it's a very complex process. They make a lot of assumptions but there are certain assumptions they have made that apparently may not be correct.

The question is, does it make any difference? Are we going to have pumps fail? Are we going to have pipes fail? Are we going to have relays that don't work or something like that? That's the question. Does it make any difference?

I thought that was the question we were going to get answers from from somebody in the project.

MR. FARRAR: Whether it makes a difference I think is more Mike's --

MR. SIESS: But not if he doesn't look at the effect of damping.

MR. FARRAR: I understand.

MR. KENNEALLY: Before we leave that slide, one thing though, this series has just finished and the final report that's describing it with information like damping presented the way we're seeing it here, has not been circulated to the licensing staff so they have not been able to focus on that issue yet, Professor Siess.

MR. SIESS: They may never, for all I know.

MR. BOHN: Dr. Siess, this is Mike Bohn. In our calculations we did both probabilistic as well as deterministic. For probabilistic we used damping levels of about 7 percent. For the deterministic we used what was specified in the FSAR, which is about 5 percent, so you will see that difference there.

MR. SIESS: I'll wait.

MR. FARRAR: Again, just briefly to look at how our method of measuring stiffness or displacements directly, this is from comparing the displacements from the static gauges to the ones from the strain gauges. The dotted line is a strain gauge. The solid line is the displacement gauge and we think we are getting very good agreement but again we are still in the process of reducing that data.

MR. SIESS: I'm sorry. I'm looking at the next slide. Go ahead.

MR. FARRAR: Okay. So now, what are the conclusions or the results from all this testing of these TRG structures?

We don't feel that the reduction in stiffness is anywhere near as high as was initially reported in this program.

MR. SIESS: In these tests? In these specimens?

MR. FARRAR: Yes. Well, results from this TRG test sequence is what --

MR. SIESS: Okay.

MR. FARRAR: -- and reduction in stiffness from 4 was probably related in the previous test to both damage during shipping and then the boundary conditions. By boundary conditions I mean the stresses that we induced by mounting these structures to their test rigs.

Currently it appears that up to the OBE level at most the stiffness reduction would be about 30 percent.

MR. SIESS: What the slide says will be -- now I don't know what that refers to.

MR. FARRAR: All right.

MR. SIESS: Does that mean --

MR. FARRAR: The wording there should be from the tests that we have done it appears that the stiffness reduction is 70 percent -- 70 percent of theory that works.

MR. SIESS: If you have a structure that is not cracked, that the stiffness wouldn't be more than -- and it's not 70 percent. The reduction is 30 percent, you mean?

MR. FARRAR: Right, 30 percent. Excuse me. I misworded that.

MR. SIESS: That would be if it didn't crack.

MR. FARRAR: Yes, if there was not cracking there from -- like differential settlement had not occurred and if differential settlement could have caused --

MR. SIESS: It cracked, period.

MR. FARRAR: -- caused cracking and this would not apply.

MR. SIESS: That's right, or anything else.

MR. FARRAR: Right.

MR. SIESS: Okay.

MR. FARRAR: We feel that we have established the scalability of microconcrete response to conventional concrete at least in the elastic range.

I have a few more slides that go into that a little bit more in detail following and from those tests we saw no cumulative damage effects.

If we can go back just for a second to the one that showed the stiffness as a function of the nominal stress level.

[Slide.]

MR. FARRAR: If we look at the structure labelled TRG-11, okay, what we did when we had the experimental modal analysis, which is essentially at zero stress level, because we have free boundary conditions and are putting in a very low random input, we got above theory and when we got similar results -- when we do any of these seismic tests we first have to put a random signal into the structure to allow the control system for the shake table to get the information about the compliance of the structure it needs to operate the table. We again got very close to theory.

We now hit it with one fairly high level pulse. This was about a 5g base excitation and we got a reduction -- well, you can see where the value came out.

If we look at TRG-10, similar structure, again we did the low level tests but then we had hit this one with a series of tests here before and when we get about 5g's on that one it comes out almost the exact same as the one that just saw one 5g pulse. That result was consistent with the other structures.

MR. SIESS: Now here, I guess, if I take structures like these, shear wall type structures, and subject them to either static or dynamic loading, and if I load them high enough they'll crack and I'll get a significant reduction of stiffness.

MR. FARRAR: Correct.

MR. SIESS: At the lower loads, they may not be cracked.

MR. FARRAR: Correct.

MR. SIESS: And if they are not cracked there is probably not much reduction in stiffness once you correct for geometry problems and some of the test problems.

MR. FARRAR: Correct.

MR. SIESS: The concern that came up earlier that you thought you were seeing cracking at low loads, you were seeing cracking at low loads but the cracking was not due to the loads you applied knowingly --

MR. FARRAR: Correct.

MR. SIESS: -- it was due to some things that had been, let's say, residual or initial stresses that had been put in while the cracks were present due to other kind of damage.

MR. FARRAR: If I had to summarize that reduced stiffness issue, that would be the summary right there.

MR. SIESS: No. The reduced stiffness issue as it applies to these types of structures is that if you are going to predict their behavior, either their deformation or their frequency or so forth, analytically, if they are uncracked you can predict it using the assumptions --

MR. FARRAR: Or plastic analysis.

MR. SIESS: -- or plastic analysis. If they are cracked you are going to have to use a different set f assumptions.

MR. FARRAR: Correct.

MR. SIESS: And whether they are correct or not probably has no relation whatsoever to the load on them.

MR. FARRAR: Well, because of environmental conditions.

MR. SIESS: -- a separate issue. A diesel generator building could start out absolutely uncracked and then up to an SSE it would probably stay uncracked, but if it was cracked from other reasons to begin with --

MR. FARRAR: Then you have to use some other assumptions in your analysis for that.

MR. SIESS: And it will make no difference to the building because it would behave just about as well in one case as in the other. Some of the things in it might not.

MR. FARRAR: Yes, correct. That's pretty much the stiffness issue in a nutshell.

MR. SIESS: There are two stiffness issues. One relates to real buildings and one relates to your tests.

MR. FARRAR: I agree.

MR. SIESS: And you have decided that there is nothing unusual about dynamic testing that makes things crack earlier?

MR. FARRAR: Yes.

MR. SIESS: Or even the models that made them crack earlier.

MR. FARRAR: Well, I can tell you that some of the -- the last group of structures that were microconcrete models I could see visible shrinkage cracks in the structure before I put it on the table. That's just from the curing effect.

Those structures still come out within 75 percent or within -- only with a 25 percent at most reduction of stiffness until we get up above the OBE levels.

MR. SIESS: Yes, but if you have got a shear wall and you've got a couple of cracks in it, that's one thing. If you crack it due to load, not due to shrinkage, you're going to -- the shear stresses in that, the testing stresses in that were all from load, are uniform. Now you have a lot of cracks. That will take you way down but the shrinkage cracks, if there's only a couple of them --

MR. FARRAR: As it turned out in these structures the shrinkage cracks were almost all in those end-walls as well so that again I think is the reason that we didn't see very much --

MR. SIESS: Stress cracks are likely to be very pervasive.

MR. FARRAR: Yes.

MR. SIESS: Since everything cracks at once, you know, you'll have -- shrinkage cracks can be pretty pervasive too but they don't have to be.

MR. FARRAR: Yes.

MR. SIESS: Settlement cracks -- all the settlement cracks I have ever seen were pretty general.

MR. FARRAR: Right. Okay, I thought in the last few slides here I'm just going to summarize where we stood on the similitude and the interaction we have had with the ASCE.

From the experimental modal analyses we have been able to demonstrate that the similitude and the dynamic properties of the structures and by that dynamic properties the resident frequencies, the mode shapes, the modal damping, we have been able to show from microconcrete to 3/8ths inch aggregate concrete structures of the same geometry scale factors, one, and we've been able to demonstrate scalability from microconcrete and 3/8th inch concrete to use conventional

-- that means 3/4ths inch concrete in this case where the scale factor was 3, wherein essentially those microconcrete and 3/8ths were one-third scale model of the TRG-4 structure.

MR. SIESS: But now if you are going to make an analysis, have you got any reason to think that your analysis won't apply to full-size structures is just because the aggregate is larger?

MR. FARRAR: My personal opinion is no, but I don't think we have a seismic test that will show that.

MR. SIESS: You don't analyze down to the aggregate size.

MR. FARRAR: No. We analyze as a continuum.

MR. SIESS: Okay, so why would -- if had an effect of the aggregate what kind of an effect would you expect it to be?

MR. FARRAR: The only thing at this point that I could see is just a different curing, things that would happen in the curing process.

MR. SIESS: But those have nothing to do with the -- those are just the property of the material that could be factored in.

MR. FARRAR: Right.

MR. SIESS: You might put in a lower tensile strength, a different ratio from strength to modulus or something like that. That's not a scaling effect. That's just different material.

MR. FARRAR: No, you're right, but at this point we don't have a test that would verify that.

MR. SIESS: And you are not going to, because you don't need one.

MR. FARRAR: Again showing the scaling of the results, this is the measured dynamic properties from an experimental modal analysis on the TRG-4.

This is the measured properties, the measured modal frequencies scaled and this R value is what you were just talking about.

The modulus of the two materials comes out different so you have to account for [illegible] in the scaling and when you account for that and apply [illegible] scale factor of 3 we get very good agreement in the mod[illegible]equencies -- good agreement in the elastic range but w[illegible] we find is that the failure mechanisms are different because we're using welded wire fabric rebar which has a lot different ductility than the conventional rebar.

Again, we've been able to -- the microconcrete to the 3/8th inch aggregate we've shown over the entire load range because they had the same reinforcement in them, in this last group of TRG structures and the micro and 3/8ths, the conventional -- well, we'll only again be able to show that in the elastic range.

MR. SIESS: You must have tension tests on the reinforcement materials; don't you?

MR. FARRAR: We have on some of them. The more recent ones we have not tested at this point. We have the material so we can do that and get the --

MR. SIESS: We don't have to speculate about the difference.

MR. FARRAR: Right. Well, if you just look at the ASTM standards for the two different materials, you see that there's quite a difference.

MR. SIESS: On normal welded -- we had welded wire fabric in quarter scale slab models which when we carried the tests to very large deformations just zipped open.

MR. FARRAR: That's essentially what happened. We got a very abrupt failure with the welded wire fabric as opposed to a much more -- very little warning of failure and complete failure all at once.

MR. SIESS: We had plenty of warning but when it went -- bang.

MR. FARRAR: Yes. All right. For the seismic excitation, we've been able to -- we don't have I think a reliable -- the large structure that we tested at Searle that was made of conventional concrete, I think there's too many questions about its initial condition before we even put it on the shake table due to the damage in shipping. We have been able to show similitude --

MR. SIESS: Probably very typical of a real building.

MR. FARRAR: Of a real building? Then we also -- but we also get into that problem of mounting it on the table, what kind of initial stresses we induce. We have been able to show similitude between the microconcrete and 3/8ths inch aggregate structures of the same size.

All right. To conclude here, I'm going to talk about the interaction of all this work with the ASCE. Currently we're involved with two ASCE working groups. Both groups are part of the dynamic analysis subcommittee of the Nuclear Structures and Materials Committee. This is all under the structural division. There's a shear wall stiffness working group and a structural capacity and I should have failure mode working group there. I left the word "mode" out.

One of the things that this does is it provides additional peer review other than our technical review group's review. These committees also provide a way to disseminate the information developed under NRC research to people in the field who actually are going to use this information. The working group on shear wall stiffness is currently in the process of completing a position paper on what shear wall stiffness should be and how you should use it, how you should account for it in analysis.

The current position right now is that at nominal stress levels below 100 p.s.i. or at o.b.e. levels and below, there will let the response specter broadening specified by the NRC account for variations in stiffness.

MR. SIESS: Is the Sandia study going to confirm that conclusion?

MR. FARRAR: I don't know.

MR. SIESS: Confirm or deny?

MR. FARRAR: I don't know enough of the results. I'm not familiar enough --

MR. SIESS: I didn't mean confirm. I meant examine it, shall I say?

MR. FARRAR: I'll have to let Mike discuss that when he gets up here. Then, above the o.b.e. level, essentially they want to do two analyses, one looking at -- in a sense, bound the problem, look at -- that their stiffness hasn't degraded which some of our tests show but the vast majority show that stiffness is degrading.

MR. SIESS: The theory being uncracked.

MR. FARRAR: Uncracked, yes, in the strength of the material.

MR. SIESS: Why not cracked and uncracked?

MR. FARRAR: I'd assume -- this is a pure assumption. I don't know why not cracked and uncracked. I assume because they think it's a lot easier just to take a cracked and uncracked analysis and take 50 percent of the value rather than do a --

MR. SIESS: But that's about twice as stiff as the uncracked from your test.

MR. FARRAR: Right.

MR. SIESS: In other words, why not 100 percent and 25 percent?

MR. FARRAR: Because I think they felt if we went back to that plot, the data showed 50 percent up to the SSE levels, anyway. It was a more realistic value.

MR. SIESS: For cracked wells?

MR. FARRAR: Yes.

MR. SIESS: Okay, and the 25 percent that you got in all the earlier tests? Those are fictitious?

MR. FARRAR: I would have to look at their -- I would have to be a lot more familiar with their tests. What you can get out of the literature doesn't address issues like how do we bolt this thing down.

MR. SIESS: I'm not talking about how you bolted it down. I'm talking about is it cracked or uncracked.

MR. FARRAR: I'd assume it's cracked.

MR. SIESS: I thought you had plenty of figures to show me that for a cracked section, you were getting 25 percent of the original stiffness; am I wrong? That was a conclusion five years ago. You had plots five years ago showing Sozen's data, the Japanese data, all agreeing with your data, 25 percent for -- section.

MR. FARRAR: First of all, as we discussed, I think --

MR. SIESS: It's a function of the steel ratio, of course.

MR. FARRAR: I feel that the 25 percent reduction that we have measured is due to other conditions than the loading that we put on it, than the seismic loads that we put on the structures initially, that that was a result more of how we fixed the structure to the test apparatus.

MR. SIESS: It wasn't a true measure of the stiffness.

MR. FARRAR: Yes. Now, I cannot comment on Sozen's or Umamura's or the other data, because I don't know enough of their test conditions.

MR. SIESS: Well, at one time, somebody on this project did.

MR. FARRAR: They -- based on what they got out of the literature which does not go into the details of how you do the testing enough --

MR. SIESS: Well, Sozen's on your technical review group. Somebody could ask him.

MR. BOHN: Dr. Siess, I will show a slide that shows all those early data sources and what they're implying now. We can revisit that question then.

MR. FARRAR: I will say that I will think that Sozen would claim 25 percent is the correct limit down at the bottom. This would be as of the last time I talked with him which would be several years ago, that he would --

MR. SIESS: Now I am confused.

MR. FARRAR: Okay.

MR. SIESS: I just thought you had data on this project --

MR. FARRAR: That is data we can get out of the literature --

MR. SIESS: No, not out of the literature. Data from this project showing reductions down to 25 percent consistently for cracking. You're getting what you thought was early cracking, got all excited about it. Now, I'm not sure what we're talking about at all. I think we might as well go ahead. I'm not going to understand this and I don't really have to. See, I'm looking at a plot right there.

MR. FARRAR: Those are the ones where I feel that we did not -- when we bolted those structures to -- either when we shipped them or we bolted them to the test facility, that we were inducing stresses such that when we hit it with the first seismic excitation, we were damaging the structure.

MR. SIESS: By damaging, you mean cracking.

MR. FARRAR: Cracking, yes.

MR. SIESS: Yes, and I'm saying that a cracked structure should have a stiffness of about 25 percent of the uncracked structure.

MR. FARRAR: I disagree with that from the test results that we've gotten. You can say that that's true for these.

MR. SIESS: You said that you've got a figure here showing dozens of tests at 25 percent.

MR. FARRAR: Correct.

MR. SIESS: Now why is it 25 percent?

MR. FARRAR: I believe it's 25 percent because either they were cracked before we put them on the shake table from shipping or that when we put them on the shake --

MR. SIESS: Let's take it one at a time.

MR. FARRAR: Okay.

MR. SIESS: If it was cracked before you put it on the shake table and the cracking caused the reduction to 25 percent.

MR. FARRAR: Right.

MR. SIESS: Thank you. That's all I've ever said. Now, I go back and look at the ASCE committee's recommendations and I said, why wouldn't they not say -- make two assumptions, cracked and uncracked, and uncracked would be about 25 percent. It'll vary depending on how much steel you've got in there.

MR. FARRAR: They felt that uncracked would be 50 percent.

MR. SIESS: Why. That's calculatable. I'm not taking -- if I've got no reinforcement, uncracked -- cracked will be 100 percent.

MR. FARRAR: That's right.

MR. SIESS: Or zero, however you want to look at it. If I've got a tremendous amount of steel, I can probably only drop it 10 or 15 percent when I crack it and I'm not saying 100 percent of theory and 50 percent of theory. I'm saying why not take cracked and uncracked? You're saying the 50 percent you think corresponds to a cracked section. I don't think it is but maybe typical for these structures. I don't know. That's what I'd expect the Sandia analysis to tell me if necessary.

MR. FARRAR: If we would go back and look at this plot, what they are saying is once we get up in this range here --

MR. SIESS: That's when it's cracked due to stress.

MR. FARRAR: Right.

MR. SIESS: I'm not talking about cracked due to stress.

MR. FARRAR: But that's all that they are talking about.

MR. SIESS: Ah, they're not going to entertain the idea that it might be cracked for some other reason.

MR. FARRAR: Correct. This is looking just at --

MR. SIESS: Now I know their thinking. Okay.

MR. FARRAR: Okay.

MR. SIESS: I think it's -- I don't know whether it makes any difference anyway because I don't know what the effect is on the equipment qualification.

MR. FARRAR: All right. The other working group that we are associated with and working with is the Structural Capacity and again I should have put Failure Mode Working Group. What this group is trying to do is put together in one document all the experimental data on shear walls as well as some other structural components and experimental data as well as experience data. I don't think they have very much experience data when it comes to shear walls in nuclear power plants and show how this information is used in PRA and margin studies and then identify areas where they think more -- where we're having to rely strictly on analysis and don't have experimental data to back it up.

MR. SIESS: There's a fair amount of data on the behavior of actual shear walls in non-nuclear structures and seismic. Some of the people on your technical review group and I'm sure some of the people on the ASCE committee have gone out and looked at -- the Chilean earthquake was pretty well investigated by a group that back-calculated a lot of things, tests made in the laboratory and Chile in building design was nearly all shear wall.

MR. FARRAR: But low rise like these -- low rise as the structures are here.

MR. SIESS: Well, not necessarily.

MR. FARRAR: When I say shear wall, I could mean, you know, a 16-story building.

MR. SIESS: Shear walls are shear walls. They respond differently but they probably crack the same. I mean the cracking is a local -- relatively local thing. It depends on what you're looking at. If you want to know how diesel generator buildings behave in an earthquake, the answer's no.

MR. FARRAR: I think that's more what their approach -- this is really approaching for nuclear structures --

MR. SIESS: I hope we never know but we might be lucky and get a big earthquake somewhere.

MR. FARRAR: My last slide here is what we're doing to conclude the program right now. Again, there's the other aspect of this program that it's looking at the risk significance of stiffness reduction and Mike Bohn's going to talk about that next. We'll conclude -- the testing for this program is over at this point. We'll conclude it by issuing topical reports on the different issues for this program.

MR. SIESS: Thank you. Any other questions?

Okay, let's take a few minutes break.

[Recess.]

MR. BOHN: Does everybody have a copy of the handouts?

MR. SIESS: Yes, I think so, Mike.

MR. BOHN: I'm glad I don't have one of these suits that has the breast-pocket sewn together.

Okay. I'm Mike Bohn, from Sandia Labs, and I will be talking about the program that Chuck referred to in terms of determining the implications of these softening stiffness structures on risk and on deterministic-type calculations, and so, what I'd like to do, if it's all right with you, is turn to the back of the packet.

MR. SIESS: Good place to start.

MR. BOHN: We'll start in the very back, and this is -- about the fourth slide back "Deterministic Impact Assessment". I want to go right to the question you have asked twice. It should be the fourth or fifth back. Maybe it's the sixth slide back.

Now, the question that has been raised several times by Dr. Siess is one of the aspects of this program should be to look at the potential change in our view of equipment qualification that might be implied by structures having less stiffness than was used in the calculation of the in-floor response spectra.

In the design process using -- either at the OBE or SSE, they calculate in-floor spectra by rules presented in the FSAR, and then, those spectra are broadened and enveloped and used to provide the seismic qualification table response spectra.

MR. SIESS: What was the term you were using? In-floor?

MR. BOHN: In-structure floor response spectra.

MR. SIESS: Oh, the in-structure floor. Is there an ex-structure?

MR. BOHN: No. That's just the terminology they use.

MR. SIESS: Okay.

MR. BOHN: So, as part of this work, which I will describe the basis of it, we did look at a design-type calculation for the one power plant that I'm reporting on today, which is the Peach Bottom boiling water reactor. This was also one of the two plants studied in the NUREG-1150 program, for which external events were considered.

So, a design-type calculation meant we did a calculation of the structure response at the SSE, .12-g. We included both the original stiffness, as in a strength of materials type calculation, and a degraded stiffness, as implied by Chuck's data that he has been reporting on.

MR. SIESS: Just a flat percentage?

MR. BOHN: Since we are dealing with only one acceleration level, it was a flat percentage. It was about .56 times the initial stiffness.

MR. SIESS: Okay. Roughly half.

MR. BOHN: Roughly half, yes. Okay?

So, one of the things that happens, as he describes, is in softening the structure, you push the structure frequencies, or some of them, down in the amplified acceleration region, and so, what happens then is your net shears in moments at each floor slab have some increase.

These are the five structures that played a critical role in the PRA, and these are the safety-related structures at Peach Bottom. What I have shown is the maximum increase in shear and in moment, based on including the softening effect.

So, we see, for Peach Bottom, which has a relatively low SSE for its location -- .12-g for Peach Bottom is pretty low -- we are seeing somewhere -- a 20- or 25-percent increase in loads.

MR. SIESS: Now, do you consider that significant?

MR. BOHN: Given the capacity of the structures, no.

MR. SIESS: The structures just have an extra margin because of the way they are built?

MR. BOHN: When we did the deterministic correction, the best-estimate evaluation of the capacity for Peach Bottom, with the exception of the emergency cooling tower, they all had median capacities of about 1 1/2 g's.

MR. SIESS: In other words, these structures are not designed for seismic loads. They are designed and then somebody calculates the seismic --

MR. BOHN: That's correct, and often times -- for example, 18 inches is considered a minimum wall thickness for a nuclear power plant, both from tornado considerations as well as others. They won't build a wall that's 12 inches anymore, for example. So, yes, there is considerable margin.

One exception is the emergency cooling tower. That had a capacity of about half a g. Still, it's well over the SSE, but it's lower.

So, that gives you an idea of what the impact is.

MR. SIESS: So, a structure itself, it's a no-never-mind.

MR. BOHN: It seems to be for this plant.

MR. SIESS: Well, I think that's probably going to be true.

MR. BOHN: I think that's true, also.

MR. SIESS: You might find some element, like that emergency cooling tower or something.

MR. BOHN: Now, turning to the spectra, what I have shown here is a spectra of plots, spectral acceleration versus frequency, in hertz.

The solid curve is a ground-motion spectra. This is the input to the entire analysis. And then I show the large dashed line, as I have shown here. This is with the original stiffness, 1.0 times K, and that's the nomenclature I have used all the way through.

MR. SIESS: Okay. This is for the rad waste turbine building.

MR. BOHN: This is an important structure.

This is the small dashed line. This is the calculation of the spectra based on the .56 times the initial structure. So, this has the softening for the SSE.

MR. SIESS: Are you going to justify four significant figures in that number?

MR. BOHN: This was plotted by my contractors, EQE, and they wanted to give me my money's worth. Of course not. It's a novelty to have.

And so, the rad waste turbine building is important, because it has the control room, it has the emergency buses, it has the cable spreading room, and emergency switch gear room, and it plays quite an important role. So, many of the important pieces of electrical equipment which would be qualified are in this structure.

This is reasonably high up. This is the elevation of the emergency switch gear room, by the way. So, this is an elevation that does -- where important equipment is located.

So, what you see is -- this is the original spectra, which presumably is very close to what they envelope to get an equipment qualification test response spectra. This is the same spectra when we do the calculation including stiffness reduction.

MR. SIESS: Now, the ASCE Committee says, you know, up to some level, simply the broadening is going to take care of this, and I don't see --

MR. BOHN: No. We would not agree with that, based on these results.

MR. SIESS: I could broaden that thing forever and it's not going to pick up a peak that extends over about a frequency factor of about 6.

MR. BOHN: That is correct, and I would also like to point out that these are based on 5-percent damping. Often times, equipment is specified at lower damping than that when they do the qualification.

MR. SIESS: Now, if you did the one, say, for 5 percent and the .56 for 7 percent, they wouldn't be as far apart.

MR. BOHN: No, but no equipment is tested at 7 percent.

MR. SIESS: I'm not talking about equipment now. I'm talking about the structure damping.

MR. BOHN: Okay.

MR. SIESS: That's structure damping.

MR. BOHN: No. This damping is the --

MR. SIESS: Can't be the equipment damping.

MR. BOHN: This is the damping at which the spectra are calculated.

MR. SIESS: Yes, but that's damping in the structure.

MR. BOHN: The structure damping was about 5 percent, in this case.

MR. SIESS: It was not in the equipment here yet.

MR. BOHN: This is the equipment damping here. The damping that went into the calculation of these spectra was the concrete damping, and that was, independently, 5 percent for the deterministic calculations, as per the FSAR.

MR. SIESS: I guess I am confused. Floor spectra, I thought, were the spectra for the floor at that point in the building, with maybe just the mass of the equipment added.

MR. BOHN: There is no equipment involved here.

MR. SIESS: Then why is the equipment damping an element?

MR. BOHN: You see, all this is is I have calculated for a certain floor-slab mass in the structural model. I have calculated a time history.

MR. SIESS: Yes.

MR. BOHN: And going into that time history calculation was an assumption on structure damping.

MR. SIESS: Okay.

MR. BOHN: Now, given the time history, I can compute this spectra at any damping I want.

MR. SIESS: You have taken a full internal history, and then you have computed a spectra --

MR. BOHN: Yes.

MR. SIESS: -- for something sitting on that at a particular damping.

MR. BOHN: Right. And I can choose whatever I want to process that time history.

MR. SIESS: Okay.

MR. BOHN: I can't change the time history.

MR. SIESS: What I was saying was that the time history could have been computed at two time histories. The one with cracking could have been for a higher damping. It would make sense. I don't think the code of the Reg Guide allows it, but you would think that cracked concrete would have a little higher damping than un-cracked.

MR. BOHN: In this case, since we are doing a design-type spectra, I used the same damping, and the damping is prescribed by the FSAR.

MR. SIESS: But since you didn't use the same cracking, you could still be consistent.

MR. BOHN: That is correct.

MR. SIESS: In analysis for use in design, I could say if it's cracked, it's low damping. I mean if it's un-cracked, it's low damping, high stiffness. When it's cracked the stiffness goes down, but the damping goes up.

MR. BOHN: I understand what you're saying.

MR. SIESS: I think that's the right direction, although the tests don't justify some of it.

MR. BOHN: That's correct, and it was on the basis of his tests that we chose not to change the damping --

MR. SIESS: Yes. Okay. That's all right.

MR. BOHN: -- for the deterministic calculations.

MR. SIESS: Now, I'm a designer, and I now have this, and I want to put some equipment there.

MR. BOHN: Okay.

MR. SIESS: And I have got some equipment that has, what 8 hertz natural period?

MR. BOHN: That's a typical value for a switch gear, yes.

MR. SIESS: And it's got to be qualified, right?

MR. BOHN: Right. So, you're operating right about there in the spectra.

MR. SIESS: All right. Now, if I just had the lower curve, I'd qualify it, say, for what? I'd say that would be qualified for 6/10ths-g?

MR. BOHN: Right about like that, yes.

MR. SIESS: What would they do when the qualified it for 6/10ths-g?

MR. BOHN: They would take the broadened spectra, they would envelope it like this. They would take that broadened spectra and generate an artificial time history, and that artificial time history would then be fed into the table. They would mount the equipment on a test table, and that artificial time history would be fed into it.

They would, first of all, do several tests at OBE, followed by one test at the SSE. That's what the 324-IEEE standards require that they qualified this equipment to.

MR. SIESS: That's what IEEE requires.

MR. BOHN: That's typically what they use, though.

MR. SIESS: What about for other components, where they do sine sweeps and --

MR. BOHN: Well, often times, they do do very small sine sweeps to establish --

MR. SIESS: No. This is a switch gear unit, Model 1403 from Company XYZ, and I go up one story now, and it's .1-.2 g.

MR. BOHN: Right. For the floor spectra on which it's mounted.

MR. SIESS: Okay. But now, are they going to put a label on it saying this one was tested to 6/10ths and the other one was tested to 1.2, or are they going to test them both to 1.2, or are they going to test one of them to 1.2 and sell me both of them? How do they do this?

MR. BOHN: They are going to do what you just said. They are going to probably pick the highest spectra that applies to that class of equipment, test one of them. If something falls out, they're going to put it back in and tape it on. Then they are going to test it according to the four OBE's and one SSE, monitor the equipment, and if it passes, they all pass.

MR. SIESS: It would be the highest spectrum for any plant that might be buying one from them.

MR. BOHN: No. They did that for -- Westinghouse defined a class of equipment called high-seismic-zone equipment, and for that class, the specifically went and tried to pick the highest spectra that they thought they could sell in the western United States, but if you didn't specify high-seismic-zone equipment -- that is, you were dealing east-coast plant -- then they tested it just to whatever the floor spectra was, enveloped. They do not try to generically qualify them.

Now, I am told that the only difference between the two is the paper trail, but I can't verify that.

MR. SIESS: Well, when EPRI put together these --

MR. BOHN: Generic equipment response, or GERs.

MR. SIESS: They did it by specific pieces of equipment, didn't they, that had probably been qualified for different plants, and they looked to see which one was the envelope or something. Am I right?

MR. BOHN: Yes. What they did was they looked at the equipment that showed up in the past-earthquake-experience database, and they tried to estimate the type of earthquake and the spectra that each one had been seeing.

MR. SIESS: Well, they also looked at test data.

MR. BOHN: And they also looked at some test data, also, but you see, the test data is also just -- its pass but not fail data, typically.

MR. SIESS: Yes, but again, if somebody got together -- if EPRI could locate all the test data for a particular type of switch gear and find somebody had used the high value -- I qualified mine only for 6/10ths-g, but the guy over there qualified his for 1.2. Now, I look at this and say gee, well, I am probably home-free. That's what I am getting at.

MR. BOHN: Well, the generic equipment response spectra is basically a lower band, if you will.

MR. SIESS: The lowest that has been tested, yes. Okay.

MR. BOHN: It's the lower band, above which they have seen some failures and below which they haven't.

MR. SIESS: I wouldn't capture what I was looking for from that.

MR. BOHN: Not exactly.

MR. SIESS: Okay.

So, deterministically, in this particular case, the switch gear, if it were qualified to IEEE, that case would not be qualified for the new spectrum.

MR. BOHN: That would be the inference, yes. It doesn't mean it would fail. It just means it wasn't qualified.

MR. SIESS: And if there were another piece of switch gear like it that had been qualified higher, you might not even be able to find out. No, there must be a paper trail on that.

MR. BOHN: I would think that what a plant would do, given this question, is they would go back to Wylie and say we have questions about whether or not our required response spectra were high enough. Have you tested others for other plants that had a higher SSE? In the case of Peach Bottom, you see, it's only .12, and we have other east-coast plants that go up to .25, and so, they would immediately go to that as a data source and see if they could say the same piece of equipment had been qualified for a different spectrum.

MR. SIESS: Or you might have the same thing at a higher level.

How typical is this?

MR. BOHN: In terms of --

MR. SIESS: What you have looked at.

MR. BOHN: It's fairly typical.

See, here is another one from a different building. This is the crib house. The important equipment in that, now, in this case, are 7-hertz vertical long-shaft water pumps -- service water pumps, in effect.

MR. SIESS: They're a problem anyway.

MR. BOHN: They're a problem anyway. Right. Depending on how frequently the spiders are located along the shaft.

MR. SIESS: Yes.

MR. BOHN: So, they're roughly a 7-hertz piece of equipment, and so, here, you have exactly the same thing.

Here again is the ground-motion spectra. Here is the original spectra, and in this case, this is the degraded stiffness spectra.

If we're talking about 7-hertz, that puts us right about there. So, the difference is between this point here and that point there, almost a factor of 2.

MR. SIESS: If that's the equipment spectra, why wouldn't it peak over near the 7 hertz?

MR. BOHN: This has nothing to do with the piece of equipment. This is only the floor spectra.

MR. SIESS: Okay. The floor spectra with the damping for the equipment.

MR. BOHN: It comes from the time history.

MR. SIESS: The damping is for the equipment.

MR. BOHN: The damping is the damping at which I calculate the spectra. So, if I am qualifying at 5-percent damping, I have to compute the spectra.

MR. SIESS: Okay.

MR. BOHN: If I specify a test-response spectra, I have to tell them the shape of the spectra as well as the equipment damping, which is, as you say, the 5 percent.

For example, piping, of course, has lower damping levels when they look at it. Electrical equipment tend to be around 4 or 5 percent.

Earlier, they used lower values, of course.

MR. SIESS: If I appear to be stupid, it's because I am on this stuff.

You talked about getting a time history.

MR. BOHN: For the table motion, they have to specify a control motion. What's the right word, Chuck? The test-response spectra.

MR. SIESS: But you were explaining to me why these spectra depend on the equipment damping.

MR. BOHN: These do because these come just from a time history. I do my structural analysis -- does anyone have a fatter magic marker?

MR. SIESS: The first solid line is the floor spectra.

MR. BOHN: No. The solid line is just the ground-motion spectra. That's the earthquake -- might be NUREG-0098 --

MR. SIESS: Okay. That's the ground-motion spectrum. Okay. Forget about that.

Putting that ground-motion on the two structures, I then get the other two curves.

MR. BOHN: Right. This at elevation 130-feet.

MR. SIESS: And these are floor spectra.

MR. BOHN: Right, for slabs above the ground.

MR. SIESS: Okay.

MR. BOHN: And I have calculated them by doing a lumped mass model, dynamic analysis, including --

MR. SIESS: Where does the equipment damping come in?

MR. BOHN: Well, what I get from the structural analysis is a time history.

MR. SIESS: Okay. That's what you get from the structural analysis.

MR. BOHN: Now, I can process that time history to generate spectra at any damping level.

MR. SIESS: Okay.

MR. BOHN: I have to choose the damping level appropriate to the piece of equipment I am qualifying.

MR. SEISS: I've heard it before, I just forgot it.

MR. BOHN: No, it's a very confusing thing. Then when you get into testing, you have differences between the required response spectra, the table spectra, and what actually came out.

Anyway the bottom line is, even at the SSC level, we see that the equipment spectra that you might specify from these two curves are maybe a factor or two different. So it is a significant difference in terms of equipment qualification.

MR. SEISS: Physically, what has caused this? As we reduce the stiffness, we change the frequency.

MR. BOHN: Of the structure.

MR. SEISS: Now, that should cause -- should that, in itself, cause a shift in the spectra?

MR. BOHN: Yes. And I will show you plots like that if we go back to the first part. I will show you exactly that sort of thing. But the answer is yes.

Where the effect comes in is the Crib House had frequencies at ten and 14 hertz, the two lowest horizontal. When you degraded them, they dropped down in the eight or nine hertz range, and we're pushing the structure into the amplified region of the ground motion spectra.

MR. SEISS: And that's what's amplifying these spectra?

MR. BOHN: You bet. And it does shift, and it does amplify.

MR. SEISS: It doesn't effect the structure, but it effects what's on it?

MR. BOHN: Well, it effects the structure in terms of those wall loads that I showed you. Those are a smaller effect.

MR. SEISS: Okay. Very interesting.

MR. BOHN: So, turning back, and I'll go through this either as fast or as slow as you want.

MR. SEISS: This is a Peachbottom --

MR. BOHN: This is a Peachbottom BWR. So the overall objectives of the program obviously were to look at the effect of this stiffening, both from two points, both from a probablistic sense, because we can jack these flow response spectra all around, and it may have very little to do with risk. If critical components are, for example, very rigid and sensitive only to ZPA, they're not going to change much, and also to look at the design type calculations which I just showed you.

MR. SEISS: The regulatory process has no way of looking at it any way but deterministically.

MR. BOHN: That's correct, but in the context of an IPE, it was our hope that by looking at it from a probablistic point of view, if these questions were raised, they might be able to buy them some relief, if you will.

MR. SEISS: This is something that could come within the scope of the IPE.

MR. BOHN: Yes, because that is one option, that they can do a PRA for a seismic rather than a margins.

MR. SEISS: They're not going to do a PRA for a seismic.

MR. BOHN: Well, if -- I agree with you, in general.

MR. SEISS: They probably could do it just as well wit the margins.

MR. BOHN: Pardon?

MR. SEISS: They probably could do it just as well with the margins. I think the margin study showed that the equipment was probably acceptable.

MR. BOHN: Depending on how they do that, it's possible.

MR. SEISS: I think so.

MR. BOHN: Okay. This is the data that Chuck developed. This is some of his earlier CERL data. I just thought I'd show you the basic data that we started to work with on this project. This is the same structure, and I know it's a little busy, but it's the same structure at three different acceleration levels: .26g, 1g, and 1.96g. You can see how the structure frequencies are shifting down as you go to higher accelerations. This is just, from a specter viewpoint, the sort of thing Chuck was showing you already.

In general, the effect of decreasing the fixed-base structure frequencies. I emphasize fixed-based here because all of his tests are modelling a fixed-base situation.

It effects the overall building response do to the earthquakes, including soil structure interaction. As I've shown you, it effects the wall shear and moment load somewhat. It certainly effects the forward slab accelerations and the spectral accelerations. So those are the four quantities that we want to look at.

Now, one of the things that you mentioned was the UNEMUR data and Dr. Sozen's data. I took this pair of figures right out of the draft ASCE working group report that Chuck referred to. My understanding is that Dr. Sozen went back and reevaluated all his sources on degradation of stiffness, both in the US and Japanese data that he had access to.

Now, the top plot is sort of a histogram showing the number of occurrences, and then the ratio of stiffness to calculated stiffness. So if they are exactly the same, we would be talking about a number right about here. So any of these occurrences below here show a measure degrade stiffness.

MR. SEISS: What about the ones above there?

MR. BOHN: I guess it shows they get better.

MR. SEISS: Either that, or they're just cast outs on the whole process.

[Laughter.]

MR. BOHN: Well, I won't justify these; I'm just going to present it.

The reason I do this is to show that, besides the LASL test, there were quite a few other sources of data that showed the same type of effect.

MR. SEISS: That's what we told them when they first got it.

MR. BOHN: Probably.

MR. SEISS: Well, it looks like --

MR. BOHN: And if you plot the Japanese data on the second one, here on the ordinance, we have measured stiffness divided by calculated stiffness. So, again there is one. And you see most of them, of course, show a reduction, as you would expect, below my pointer. But the reductions -- here is .4 -- most of them are above .4 in terms of stiffness.

MR. SEISS: These are different sets of tasks?

MR. BOHN: Different sets of data. There's the UNEMUR data, etcetera.

MR. SEISS: They're not all Japanese, I don't think.

MR. BOHN: So that just shows something besides the LASL data in terms of what has been measured. I do not know the details of the test, or how he processes, or whatever it is, but that plus the LASL data is what the ASCE committee is working with, and that's on which they're making their recommendations.

MR. SEISS: I guess it's hard to get a real strong case for 50 percent out of either one of them.

MR. BOHN: They're probably not 25 percent, either.

MR. SEISS: No.

MR. BOHN: Twenty-five percent would be down here.

MR. SEISS: Twenty-five percent won't be too far off the mean on that plot. If I threw out everything above one --

MR. BOHN: Here?

MR. SEISS: Yes. If I leave all the one stuff in there, the mean's probably going to get over at about seven-tenths. The median will be around 55 or 60 or something like that.

MR. BOHN: Yes.

MR. SEISS: But if I throw out everything above one, the median will get down to about .3 or .4.

MR. BOHN: Somewhere there.

MR. SEISS: Yes. With a lot of scatter. I don't know what else you can do at design stage.

MR. BOHN: So that's another --

MR. SEISS: Even if you knew what the scatter would do --

MR. BOHN: Okay. Now, what I'll show here is the --

MR. SEISS: It's all shear wall stuff, right?

MR. BOHN: This is only shear wall.

MR. SEISS: Okay.

MR. BOHN: These are the steps we went through to look at the probablistic calculations. Now here, all we're doing is we're repeating the NUREG-1150 seismic PRA, and since we did that, it was relatively easy to do. We are incorporating the stiffening effect, which means we had to go back and recompute all the structural dynamic time history calculations with several modified stiffness values.

So, in the general process, we --

MR. SEISS: What is the range of modified stiffness?

MR. BOHN: Well, I'll address that in a minute.

MR. SEISS: That's an important question.

MR. BOHN: First of all, we chose the seismic PRAs.

The ones we're going to look at are, at this point, Zion, for sure; Peachbottom, which we have looked at; and then probably Maine Yankee. Maine Yankee is a rock PWR; Zion is a soil PWR.

MR. SEISS: And, of course, Maine Yankee's been through the margin study.

MR. BOHN: That's correct.

MR. SEISS: And you have a chance to see whether the margins would help. You could compare the PRA versus the margin.

MR. BOHN: That's exactly right.

MR. SEISS: Okay.

MR. BOHN: That would be the first comparison in that process that's been made. I think that's a very valuable byproduct to come out of this. In addition, we can save a little money, we hope, because they have evidently generated some best-estimate structure models that they have been using for their sort of licensing recalculations.

MR. SEISS: Good.

MR. BOHN: Okay. Then we have to recompute all the structure responses. That is, we use a time history dynamic analysis, and now we include the reduced stiffness effect.

The way we include them is effectively a factor on -- let's call it the frequency, which translates to a factor on the modulous of concrete, in effect.

MR. SEISS: Okay.

MR. BOHN: That's mechanically how it gets in. Then we also re-evaluate the capacity of the structure, then, of course, recompute the floor spectra. Now we're talking -- all of this is best estimate, now, not design. The first few slides we talked about were design type; this is best estimate.

So now we recompute best-estimate floor spectra for all the critical components that played a role in the PRA, and reevaluate their fragilities and their failure probabilities, then recompute all the accident sequences and uncertainties, an get an estimate of the change in risk.

Now, our process for computing structure response, both in the original 1150 and here, we used time histories.

MR. SEISS: Excuse me. How far have you gotten on that?

MR. BOHN: I went through that. Do you have a question on it?

MR. SEISS: Have you done all of the calculations on the analyses?

MR. BOHN: Yes. I'll report them here.

MR. SEISS: Have you found any plant-unique vulnerabilities?

MR. BOHN: There is a vulnerability, I think, that's present in a number of Mark I BWRs.

MR. SEISS: That's not plant unique.

MR. BOHN: No. The plant specific vulnerability here that was the emergency switch gear were probably -- the 4KV switch gear I'm talking about --

MR. SEISS: No, I'm talking about due to this, due to the stiffness change. Are you talking about the whole PRA?

MR. BOHN: Well, this just enhances whatever weaknesses we found in terms of component capacity.

MR. SEISS: Okay.

MR. BOHN: It doesn't change the fact that the anchorage is the same in both cases.

MR. SEISS: All right. Fine.

MR. BOHN: The anchorage was a bit weak in the 4KV switch gear, which are critical items.

MR. SEISS: Okay.

MR. BOHN: Since both your diesel generator power as well as off-site power go through those 4KV.

MR. SEISS: When you say the anchorage was weak, you really mean it was weak. You don't mean they left some bolts out.

MR. BOHN: I mean that they used what I consider fairly inadequate Phillips weld to anchor it rather than a proper bolted installation. Now, the capacities that I calculated were above the SSC -- we're not dealing with a licensing issue here -- but the margin above the SSC was less than a plant that had good anchors, and good bolts, and steel in the --

MR. SEISS: And that would be aggravated by this?

MR. BOHN: Yes, it would be.

Well, the point I want to make here is the way we do things, we actually go back and do time history analysis for everything, which means we use time histories as input. So for Peachbottom -- it's a rock site -- we used ten recorded real earthquake time histories, and then scaled them according to the PGA we wanted to analyze.

When we performed ten time history analyses of all the structures, including SSI -- of course, in this case, we really didn't have SSI other than some radiation damping, which was included. These slides are somewhat general.

Now, in reducing the building natural frequencies, of course we used the LASL test. This is a plot that Chuck already showed earlier. It shows the first mode frequency for the CERL test as a function of peak acceleration during the test, and it shows, for two different scales, the ten and 30 scale, that there is roughly a linear relationship between the degradation in the first mode frequency and peak acceleration. That was the basic data we had to work with early in the program, and he's described that.

So we put a simple model together which said that we have a static reduction of about 60 percent, and then a term of further degradation of about 20 percent per GPGA, and this fit most all of the early data quite well. We did a least square sort of thing, but when all was said and done, you know, I didn't carry, like you say, four decimal places. I think it was 1.97, or something like that. Anyway, this is the model I used.

Then, using this model, we degraded the natural frequencies. We did an eigen value analysis of each structure, and then we used this to reduce the natural frequencies.

MR. SEISS: Of all of them?

MR. BOHN: Of all of them. And that is an unresolved question: What do you do with higher modes, because we have no data right now.

MR. SEISS: Well, I was thinking what happens if some places crack and some don't?

MR. BOHN: Which is highly likely. You're probably going to get your cracking on your base, for starters, anyway.

MR. SEISS: And does it make any difference? I mean, would it be worse than assuming that you have a uniform reduction on stiffness.

MR. BOHN: We haven't studied that question, per se. The only analyses that we could look at are the analyses currently being done for Diablo Canyon, where they did an analysis, a non-linear analysis of one structure with a degrading model, and they found most of the cracking was on the lower floors, of course.

MR. SEISS: Yes.

MR. BOHN: Overall, it didn't seem to make that much difference.

MR. SEISS: I have no feel for this at all, but if I assume that this has been -- I have reduced stiffness here, but not here, or let's say I've got a multi-story building, and I assume I have reduced stiffness on the lower floor, but now I stay uncracked on the upper floors, am I likely to then magnify something -- to get something worse than if I assume it's degraded everywhere?

MR. BOHN: I don't know the answer to that question. However, if we say that there is a static reduction, an as-built reduction due to residual stresses or whatever mechanism, then that, presumably, would be the same for all floors, whereas --

MR. SEISS: That's an assumption, you see. That's just -- that might be wrong, too.

MR. BOHN: It might be.

MR. SEISS: Yes.

MR. BOHN: But I'm just saying, we have sort of looked at this as sort of an as-built stiffness reduction due to whatever mechanism that's in there.

MR. SEISS: Yes. If I'm looking for the worse reduction I can get, that's reasonable, but now the question is, does assuming everything is reduced lead to the worst case as far as equipment, or whatever?

MR. BOHN: Okay. In this particular case, the answer is no because the fundamental mode in terms of mass participation was up around 80 or 90 percent for most of these structures. So whatever we did on the higher modes probably wouldn't effect the final answer. That doesn't answer your question; that just says, mechanically, it wouldn't make much difference in this calculation.

MR. SEISS: Okay.

MR. BOHN: The participation factors are -- but what you've raised is a question that we don't understand, that is, how best to incorporate this effect into a dynamic model.

MR. SEISS: Yes. Of course, it can get ridiculous if, say, I have to take every possible combination of fully cracked, intermediate cracked, cracked, and find out which one is worse.

MR. BOHN: That's why I wanted to emphasize that how we put this in was effectively a degradation of E.

MR. SEISS: Yes.

MR. BOHN: That's how we put it in.

I'm sorry, do you have a question?

MR. AMIN: This is a question on just the way the analysis was made to clarify for myself. Mo Amin from Sargent & Lundy. Did you change the stiffness, or did you change the calculated frequency?

MR. BOHN: We did an eigen value analysis on the original structure in terms of an original Young's Modulus, original dimensions. Then, from then on, we did a modal analysis, but we did a modal analysis and reduced the natural frequencies.

MR. AMIN: In other words, you computed your natural frequencies with the non-reduced stiffness, and then you reduced those frequencies according to this equation?

MR. BOHN: Now, let me thing.

MR AMIN: I know when you went to do eigen value, you should have picked up the reduced frequencies.

MR. BOHN: If we had modelled it as a Young's Modulus reduction.

MR. AMIN: If you are saying that the structure has degraded, irrespective of the --

MR. BOHN: I think what we did was we did the stiffness. We did Young's Modules. I'm sorry. I said something wrong. This came up when we were specifying to our contractor who did these analyses and my recollection is we finally went back and calculated stiffness changes, which was meant to be a factor on Young's Modules.

MR. SIESS: Who was your contractor?

MR. BOHN: EQE on this, because they did our dynamic analysis for the 1150. So that's how we incorporated it. We initially looked at doing it the other way and that's why I was sort of hesitant and said it wrong. But we ended up doing reduction in Young's Modules as the mechanism.

So other than the building response changes, everything else was pretty much the same. That is, we used the same hazard curve, same set of uncertainty about it, same event trees and fault trees. We used the same component, random failure rates, the same fragility characterizations for all the equipment.

The equipment capacity, in terms of its anchorage failure, is not effected by this at all. The building fragilities were changed somewhat because in the process of developing building fragilities, you have to know what the natural frequency of the building is. So that was effected slightly. Those weren't large effects.

Lastly, we did a full Monte Carlo analysis on core damage frequency, both with and without the stiffness reduction. Now, the point -- well, let me get to it here in a second.

I think I mentioned most of these points. Relative to Peach Bottom, it is a rock site. We used a ground motion spectra that was characteristic. In other words, the ten times history that we used had a mean spectra that was very close to a rod band 0098 mean spectra. So the time histories we felt were appropriate in that context.

The hazard curve came from the Lawrence Livermore Eastern Seismic Hazard Characterization Program and then the building fragilities were site-specific. Component fragilities, we used both the generic database and, for a number of components that had less margin than others, we did fragility calculations.

MR. SIESS: These are the parameters that effect the PRA outcome.

MR. BOHN: Yes.

MR. SIESS: Not the comparison.

MR. BOHN: That is correct. This, just for reference, shows you the median of the ten time histories that we used doing the analysis, as compared with a broad band seed and idris type spectra. Just showing that the histories we picked were consistent with the rock site.

This just shows the hazard curve, specified by Lawrence Livermore's Eastern Seismic Characterization Program. It's a family of curves at 15 percentile median and 8th, and the dashed line is a mean.

Now, one of the things I want to point out is in doing a risk assessment as contrasted to a deterministic analysis, you are evaluating the plant response for the entire range of the hazard curve.

So we do these calculations at each particular increment of PGA and that means at each different PGA level, we have a different effective stiffening. So we're not just using one level of stiffening-softening. For the higher levels, we use what's implied for very high earthquakes.

MR. SIESS: That's why you had the PGA variable in your frequency reduction.

MR. BOHN: Right. Exactly. Now, I've already mentioned the five structures that play an important role. We discussed those earlier. You also find out that the critical components that are playing a role in the PRA, these are the dominant components contributing to risk; ceramic insulators in the yard, the emergency service water pumps. These are the vertical shaft pumps I was discussing earlier down in the crib house and one of them is up in the emergency cooling tower base.

The diesel generator day tank played a role and here are the four kv busses which I mentioned had less margin.

MR. SIESS: The ceramic insulator would not be effected by any structural --

MR. BOHN: That's correct.

MR. SIESS: The pumps probably not?

MR. BOHN: These got very much effected, and I'll show you.

MR. SIESS: Because there was that much concrete between the ground and the pump?

MR. BOHN: That's correct. The natural frequency of the crib house was around ten. It got pushed down around eight and --

MR. SIESS: And these things are above --

MR. BOHN: Sensitive to seen hertz.

MR. SIESS: They're not sitting on the floor of the crib house?

MR. BOHN: No. They're not in the foundation. They're up on one of the floors above. It's only a three --

MR. SIESS: How about the day tank? That's in a building, isn't it?

MR. BOHN: That's in a building, but it's, again --

MR. SIESS: Low down.

MR. BOHN: Pretty low down, yes. This was not effected much by --

MR. SIESS: And the five kv busses are on that building you looked at to begin with.

MR. BOHN: Yes. They're up high in the rad waste turbine building, right below the control room.

MR. SIESS: Very interesting.

MR. BOHN: So they did see an effect. I'll show you that effect in a second. This shows you -- for the reactor building, I just wanted to show -- this is the type of model that we got from the architect engineer. In other words, this provides the dimensions. It's a lump mass type overall model of the reactor building. These are the internals here. That's the outside structure.

MR. SIESS: The reactor building.

MR. BOHN: Yes.

MR. SIESS: This is a BWR.

MR. BOHN: It's a BWR.

MR. SIESS: So that building is concrete up to a certain point and then steel?

MR. BOHN: Yes. But effectively, this is from a PRA viewpoint, there's a -- the sheetmetal part of it is negligible above this.

MR. SIESS: Okay. This is just the concrete part.

MR. BOHN: This is just the concrete, yes. So this just shows you the degree of resolution of the model. This was the model that was used to design the structure originally. We just made it into a best estimate model.

Now, here are the basic fundamental natural frequencies. This shows the lowest natural frequencies with no stiffness degradation. So the lowest frequency is about seven hertz and that contributed 69 percent mass participation factor.

And here's another seven hertz, 70 percent. Then it goes up to 20 and then higher, of course. But what you see is that the two lowest modes are dominating the response.

Now, when I go to a .6 k, that is a 40 percent reduction. The frequency drops to 4.8 hertz from seven hertz. When I go to an 80 percent reduction, it drops down to 3.2 hertz, let's say. But, again, the lowest modes still are dominating the response.

This just shows you, from an item value extraction viewpoint, if you change the stiffness, how does the fundamental frequency shift. Now, an interesting point that -- well, I'll show you on the next slide. This is going from seven to four -- this is 4.8. Going from seven to five doesn't make much difference because you're already in the amplified acceleration region.

So the reactor building, in terms of changes, didn't play much of a role. Here are the spectra. Again, these spectra are computed at five percent damping. Again, the solid line is the free field. This is pretty low in the structure. This is the first floor above the foundation.

All of the spectra are for one elevation. I have three plots, and I know you can't distinguish between the two, but it's not important here.

The solid line is the free field. Here is an 80 percent stiffness reduction, a 40 percent reduction, and no reduction. And you see they are all pretty much the same.

Two reasons; it's low in the structure and the shift in the natural frequencies -- you see it shifted it from about seven here to five. In terms of the ground motion spectra, the solid curve, it's a no, nevermind.

If you go a little higher in the structure, though, you do see a little more of an effect. This is quite high in the structure. This is about the top -- equivalent to the top of the control room in the other building. So this is quite high, 165. Now you see the shifting that you were referring to earlier.

Again, this is the free field. This is the original calculated structure with no stiffness in it. Then the next two progressively go up as you saw from the structure.

So what we did is we did two -- we'll call it three complete structural dynamic calculations at no stiffness reduction, 40 percent reduction, and 80 percent reduction. But it really didn't play much role in the final analysis because high up in the reactor building, there is really nothing that plays a role in the PRA.

Now, the ones that were really effected are the crib house with the service water pumps and the emergency cooling tower because it has the third redundant service water pump.

These were effected and the reason is both of these structures had fundamental natural frequencies in the ten-to-fourteen hertz with no reduction. So then you reduce those down in the eight-to-ten or lower range. You're down in the amplified region of the ground motion spectra, and those were critical.

MR. SIESS: What's the emergency cooling tower, forced draft in a shear wall type structure?

MR. BOHN: It's a lower box type structure than with the tower above it.

MR. SIESS: Forced draft or natural draft?

MR. BOHN: I don't know. From our viewpoint, it was only the pump and the reservoir that --

MR. SIESS: This is just emergency. It would have to be -- I don't think I've ever seen a natural draft for emergency.

MR. BOHN: So the crib house, which is officially called the circulating water pump house. Here is the ground motion spectra input. Here is the original spectra. You can see as you progressively saw from the structure, you get a major increase in the peak spectra.

MR. SIESS: Is this a multi-story structure or just --

MR. BOHN: The crib house has about three slabs, effectively three slabs. Yes. A foundation and then two and a roof.

MR. SIESS: They vary quite a bit.

MR. BOHN: Now, the piece of equipment, the pumps that were in there had a nominal frequency of about seven hertz. So we were dealing with this part of the spectra. So you can see that you've changed the input that the pump sees dramatically.

MR. SIESS: Now, when you get down as low as 20 percent of the original stiffness, that is a pretty well cracked-up structure, isn't it?

MR. BOHN: That's true.

MR. SIESS: And doesn't the damping enter into that somewhere?

MR. BOHN: The way we treat damping probabilistically is we start off with a cracked concrete damping of about seven percent. And for low earthquake levels, that's what we use. And then at the very high earthquake levels, we go up to maybe ten. We have a linear function that we use.

MR. SIESS: But not as a function of the cracking.

MR. BOHN: That's right, because we --

MR. SIESS: When you get down to 20 percent, you would be at the high earthquake level.

MR. BOHN: That's correct.

MR. SIESS: And that's not reflected in any of this.

MR. BOHN: Specifically, no, because of the fact it really wasn't in Chuck's data.

MR. SIESS: When I look at his curves for getting up to the large deformations, the loops got awful and to get down to two-tenths on the stiffness, you had to be pretty well cracked but not yielded, I guess. You don't think you reach yield anywhere in here. See, this is done two-tenths for the whole thing.

MR. BOHN: I would say that you would probably have some yielding to get down there. Again, we've got to be clear here. That doesn't necessarily say -- I would have to go back and look at the figures -- that necessarily this particular one ever got used.

What we do is from these three data points --

MR. SIESS: In your PRA.

MR. BOHN: Yes.

MR. SIESS: Okay.

MR. BOHN: We do three complete structural analyses, just to jump ahead one slide, and then what I need is I need a model for any given response; say the spectral acceleration for the top floor. I need that as a function of peak acceleration, so I have to build a model. And I build the model by doing structural analyses at a low PGA and at a high PGA, and that gives me enough information to construct a model.

Then, as I integrate over the hazard curve, I take various points off of this depending on how high I need to go. I guess what I'm really trying to say is that we might have gotten down to 20 percent reduction, but the probability of the earthquake at that point was so small it probably didn't play any role.

The point you're making is a very valid one. How can you really model a structure that has one-fifth of its stiffness without increasing damping? My only answer --

MR. SIESS: In the deterministic case, we can do it.

MR. BOHN: I can build it in very easily. That's no problem.

MR. SIESS: Would it make any difference?

MR. BOHN: Well, already we're going from seven to ten percent as we go from the SSE up to about one g. That seemed to be within the scatter of what he's doing already. I think the damping is not the major effect here overall, though.

MR. AMIN: Mo Amin from Sergeant & Lundy.

When you do the structural analysis, do you make a linear structural analysis?

MR. BOHN: Yes.

MR. AMIN: You are not changing the effect of the material degradation.

MR. BOHN: We do a calculation at several different levels. We repeat the dynamic analysis. And at the higher PGA levels, we use a little more damping. It's sort of the quasi-static approach. But we didn't vary it all that much. But we used what we thought were best estimate damping values, starting at about 7 percent at the SSE and going up maybe to 10 percent at a 1-G PGA. God knows what happens at 1-G, right?

Okay. Anyway, we do have to build that sort of model.

Let's see. I showed you the crib house. Here is the corresponding curve for the emergency cooling tower. It just follows the crib house slide.

Again, we see substantial, in looking at a case with a 40 percent and 80 percent reduction in stiffness, we see very substantial changes, as you would expect, in terms of response spectra, because we pushed that structure down in the amplified acceleration region.

MR. SIESS: What is the scale on acceleration here?

MR. BOHN: Here? Let's see.

MR. SIESS: It says times 100.

MR. BOHN: In terms of feet per second squared.

MR. SIESS: What?

MR. BOHN: In terms of feet per second squared. And then this is times 100. So this would be 30. So this would be about a G there.

MR. SIESS: Why do you keep confusing us by not putting accelerations and Gs, anyway?

MR. BOHN: I apologize. I think that is a good point.

MR. SIESS: So you would have about a G, it would be about 32 there.

MR. BOHN: Yes.

MR. SIESS: Okay. First I thought those were Gs over there and I said they looked awful low compared to high up in structure. But 2 isn't low.

MR. BOHN: And again I show you this because this also has a vertical pump associated with it that has about a 7 hertz frequency in it. It does get affected substantially.

Now, if you were interested in seeing it, this is a somewhat busy table that is in the report. By the way, we have a draft report, as Roger mentioned, that NRC has, that is available, if you want to have something to read on this calculation.

This is the free field. All these are free field responses. This is the radwaste turbine building, reactor building, diesel generator turbine building, crib house, emergency cooling tower.

So I have several responses. By response, I am referring to a specific spectral acceleration at a specific elevation, in a specific building.

So for example in the radwaste turbine building, I have certain pieces of equipment up at 165 that are electrical cabinets that have their frequencies in the 5 to 10 hertz range.

That is Response Number 11. It says that it sees 3.3 times PGA with no stiffness reduction. It sees a factor of 4 times PGA with a 40 percent reduction, and then drops back down to 3.9 with an 80 percent.

So here is a case where you have actually pushed the structure frequency over the hump, if you will.

Now, if you look down this column, the bigger these numbers get, the more the effect of stiffening is. If you look down here, here we are at the crib house again. It started off with 2.4 times PGA. But then we reduced it 40 percent in stiffness, it went up to a multiple of almost 3-1/2. If we reduced the stiffness by 80 percent, it went up to a factor of 5.

This is just putting in numbers which you saw on the spectra on the last slide.

MR. SIESS: We are getting up to roughly doubling.

MR. BOHN: Yes. This says here that except very high in the radwaste turbine building, there hasn't been too much amplification. It says in the reactor building, there has been very little amplification at all.

So one of the overall conclusions you come to from this analysis is it is very much building-and-spectra-specific. You know, if I can show an effect here, it may be different at other plants. It would be different.

MR. SIESS: Plant-unique.

MR. BOHN: You bet.

So the next slide just summarizes, and obviously, this just shows the 22 accident sequences that were in the original PRA. And this shows the original calculation, now in terms of frequency per year. This shows the same frequency with the stiffening effect in it.

And the major change is a station blackout sequence, T1-33. And here we went from 3.7 times 10 to the minus 5 frequency up to about 8. So we've seen a little more than a doubling of that particular sequence.

Overall, this is the total core damage frequency.

MR. SIESS: Now, this is after you've gone through all your fragilities?

MR. BOHN: Yes. And integrated and done all the accident sequences.

MR. SIESS: That was fragility curve you had?

MR. BOHN: That is correct. We are in probability space now. And the bottom line is the risk, overall, that we would maybe compare with the safety goal, went from 7.6 to the minus 5 up to 1.2 to the minus 5. In other words, roughly a doubling.

MR. SIESS: Yes.

MR. BOHN: Which at risk base is not.

MR. SIESS: Tell us the piece of equipment?

MR. BOHN: It is those three service water pumps. Two in the crib house and one in the emergency cooling tower. And that is the sort of weakness I'm referring to. They have four diesel generators.

MR. SIESS: Are all of those along --

MR. BOHN: All of them are along shaft pumps. And --

MR. SIESS: Those were identified as a problem.

MR. BOHN: That's correct. In fact, they couldn't even come up with GERs for those.

MR. SIESS: Yes.

MR. BOHN: See, we have four diesel generators, which says a lot of redundancy, but they all comes from three pumps, two of which are identical, sitting side by side, so you have no redundancy there.

MR. SIESS: Yes.

MR. BOHN: So in fact, the fact that the spectra changed significantly, pushed their failure probabilities up.

MR. SIESS: Now, we don't, do we really know what those pumps are good for? Were any ever tested?

MR. BOHN: No. It is based on calculations of spacing and how much free space you have in the rotational bearings.

MR. SIESS: What happens to them in an earthquake? They bang themselves against something?

MR. BOHN: Yes.

MR. SIESS: The long shaft moves enough to disconnect it?

MR. BOHN: Take up the free space that is allowed. Yes, there have been no full scale tests. I mean, they are 50 feet long, typically.

MR. SIESS: I know.

MR. BOHN: By the same token, however, even without earthquakes happening, they fail often enough. They are a problem item, sort of.

MR. SIESS: Yes.

MR. BOHN: Anyway, so the point is, even with all the changes and different failures --=

MR. SIESS: Let's take a nonseismic PRA. Are they any contributor to risk there? They wouldn't be, would they?

MR. BOHN: Not particularly.

MR. SIESS: There they are probably working.

MR. BOHN: Because there they would assume the failures are independent. Whereas in the seismic case, we say they are highly correlated. They are side by side and identical.

MR. SIESS: Yes.

MR. BOHN: No. Those have not shown up as major contributors in internal event PRAs, because of the independency arguments.

So, you can look at these numbers two ways. You can say from a risk perspective, a doubling of risk, that is small potatoes. Or, if you put a safety goal hat on and say, uh-oh, I've pushed myself up over 10 too the minus 4, you might say well, in risk-base, maybe I pushed myself over a threshold that I don't want to be over.

So you can look at it in either of two ways, depending on how you want to interpret or utilize a safety-goal-type argument.

MR. SIESS: And as for the original, at T1-33 accounts for about half the total risk?

MR. BOHN: Yes.

MR. SIESS: And from there it goes up to accounting for about --

MR. BOHN: 60 percent or something like that.

MR. SIESS: -- 60 or 70 percent, yes.

Now, Al just pointed out that ALOCA 30 up there.

MR. BOHN: Yes. That is a very good point.

MR. SIESS: It also helps a little bit.

MR. BOHN: Now, the significance of that is that in terms of early fatalities, this will be the sequence that is contributing to early fatalities. So even though the sequence is quite a bit smaller, those increments could still be important not in terms of core damage, but in terms of the calculation of early and latent fatalities. And that is a very good point.

MR. SIESS: That is about a 50 percent increase.

MR. BOHN: Right. So you cannot just look at the bottom line core damage frequency.

MR. SIESS: Now, if you looked at this thing strictly deterministically, you might be able to find out that that -- Now, those pumps are not going to come out any better deterministically than they come out here.

MR. BOHN: They'll come out worse.

MR. SIESS: I was thinking about the switch gear. You might be able to find that the switch gear was actually qualified to a higher level. But if you look at it from this point of view, you've got a fragility curve for the switch gear, and it is a fragility curve for the switch gear.

MR. BOHN: That is correct.

MR. SIESS: It is not going to change.

MR. BOHN: That's right.

MR. SIESS: It may not be right. But that's true of any of them.

MR. BOHN: That is correct.

MR. SIESS: Okay. I'm trying to see whether I got a different answer probabilistically than deterministically. And in terms of this, I wouldn't. I would have to do something about those pumps. And I might have to do something about those pumps, anyway.

MR. BOHN: Well, one way to look at this is you could look at all the components for which their deterministic spectra got pushed up, but still didn't play any role in the risk.

MR. SIESS: Yes.

MR. BOHN: And then you might say well, it didn't affect the risk any at all, even though we doubled the EQ spectra.

MR. SIESS: On what basis would those pumps have ben qualified for the original?

MR. BOHN: By analysis.

MR. SIESS: And that analysis is the same analysis that would have been the basis for your fragility curve?

MR. BOHN: That is correct.

MR. SIESS: So if I gave this to a designer and said now can you qualify those pumps for twice that G value, and he said no, --

MR. BOHN: What he would say is, we'll double the number of spacers, spiders.

MR. SIESS: And then he would fix them?

MR. BOHN: He'd fix it. He'd change the spacer, cut it way down.

MR. SIESS: Well, if you put that fix in the fragility curve, it would fix it here, too.

MR. BOHN: Yes. It should increase its capacity to seismic shaking.

MR. SIESS: So the designer would have picked up on something.

MR. BOHN: You see, in the history of these vertical water pumps, they used to have spacing greater than 18 feet or something, and then they started getting a lot of failures just due to normal operation. So then they started making them with 12-foot spacers and that seems to be where they are. And I forget exactly the number, but it's less than 15, that's the cutoff.

And so presumably, they could just add more spacers and get whatever capacity they needed.

MR. SIESS: Yes.

MR. BOHN: In inertial sense.

MR. SIESS: Now, the switch gear in the turbine building is not a contributor, significantly?

MR. BOHN: It is a contributor, but --

MR. SIESS: Is it in there?

MR. BOHN: It is definitely in there. But it is in what is left after the 8 to the minus 5.

MR. SIESS: Do you know which one of those it is?

MR. BOHN: Well, it actually enters into a number of different sequences and it enters into this sequence also.

MR. SIESS: I see.

MR. BOHN: Because if those fail, you get a station blackout. If the three pumps fail, you also get a station blackout. So it is in here, as well as several others.

MR. SIESS: Okay. So that is in there on the basis of a fragility curve.

MR. BOHN: Yes. Looking at the sizes of the welds that were used to hold the cabinet down.

MR. SIESS: And if I went back and looked at it deterministically and said oh, yes, it was qualified ot that level, that would say there is no difference, but your fragility curve would say there is.

MR. BOHN: I would bet you dollars to doughnuts that when they tested that they didn't use fillet welds to attach it to the table. They bolted it to the table. And knowing the way Wylie does things, they probably put an I-beam in there and bolted the thing down against the flange.

Just the way the seismically qualify it is not necessarily the same, it should be, but it is not necessarily the same as in the field.

MR. SIESS: They get fussed at an awful lot for not being the same.

MR. BOHN: But we see it.

MR. SIESS: But anchorage is not looked at as much as other things.

MR. BOHN: The anchorage is often a field installation.

MR. SIESS: I know it. And it's the most important thing.

MR. BOHN: You get the impression that sometimes these fillet welds are sort of viewed as installation welds more than trying to achieve some seismic capacity, because you understand of course that Peach Bottom is a very old plant. They weren't worried about earthquakes back then, and you know, put a bunch of fillet welds, and just four fillet welds and install it, and that was okay.

MR. SIESS: And these are fillet welds on the bottom?

MR. BOHN: That's correct.

MR. SIESS: They ought to brace them at the top. But then we wouldn't have to worry about them.

But the point is that I'm looking at the difference between the deterministic and the probabilistic approach here, you see. And we are still designing these things deterministically, we are still regulating them deterministically, and we have having a problem figuring out how to factor the risk into it.

MR. BOHN: Let me pull out one more slide.

MR. SIESS: We were up to where you started.

MR. BOHN: We are almost done, yes.

MR. SIESS: We are up to where you started, if you want to go back over some of it.

MR. BOHN: No. I will just show you a slide from the original. As part of the PRA process -- yes. This has nothing to do with the calculation of reduced stiffness. This is from the original PRA for Peach Bottom. As part of the PRA process, for each component, you can set its capacity to infinity. In other words, you can say one at a time, each component doesn't fail and then figure out what it does to the bottom line risk. This says that ceramic insulators are the most important.

MR. SIESS: You're going to have to back off and point this to me.

MR. BOHN: Excuse me. Ceramic insulators are the most important followed by the emergency service water pumps.

This reduction is how much the total core damage frequency would drop if that component never ever failed no matter how big the earthquake. Diesel generator day tank, turbine building, ECT structure for KB buses. Now, let us say that we have this and we do in our report for each component with and without the softening.

Now from a deterministic point of view, we might find for example this particular component, its amplified or increased spectra vastly exceeded what it was qualified to but then we might look at a calculation like this and say it doesn't make any difference. Even if it fails it doesn't make any difference.

MR. SIESS: It did make a difference.

MR. BOHN: Well, I'm just saying in general.

MR. SIESS: Well now why then if that's true did it make as much difference as it did.

MR. BOHN: This is the -- the pumps that we're talking about is these pumps up here.

MR. SIESS: Oh, I'm sorry.

MR. BOHN: This is the vessel supports -- reactor vessel supports.

MR. SIESS: I confused myself. Okay.

MR. BOHN: All I'm just trying to say is one can use the probabilistic best estimate numbers to say well, yes, there are differences when we recompute spectra but most of them don't matter a hill of beans.

MR. SIESS: Now let me phrase something a little differently. Suppose at Peach Bottom they had designed that plant based on an analysis with reduced stiffness. How much different would the plant have been and how much difference would it have made in the core melt probability?

MR. BOHN: I don't think I can answer that.

MR. SIESS: If they had designed it that way, they would have found that those pumps weren't good enough. So they'd have done something about it.

MR. BOHN: Yes.

MR. SIESS: Now you'd have had to use a different fragility curve.

MR. BOHN: That's correct.

MR. SIESS: So I think it would have shown up.

MR. BOHN: It would have affected it, yes.

MR. SIESS: Now the thing is, I'm not sure that anything else would have been changed, you see. It may be that the 4 KV buses of the switch gear and the turbine building would have ended up exactly as it is now.

MR. BOHN: Yes, if that component -- if they could qualify that to the enhanced spectra. If they couldn't, they might have to somehow specify a better piece of equipment.

MR. SIESS: Yes, but they might not have any better welds.

MR. BOHN: Well that's true.

MR. SIESS: They could have put a lot better piece of equipment in there but if they didn't attach it to the floor --

MR. BOHN: You can get virtually any capacity on anchorage you want from a practical viewpoint.

MR. SIESS: But your fragility curves assume they don't get it all the time. The fragility -- a range of --

MR. BOHN: But the median level should be what we calculate when we look at the weld size, effective shear error and the weld strength.

MR. SIESS: So your fragility curve for the switch gear would be based on what you actually saw in the way of welds plus or minus, in other words, a distribution curve.

MR. BOHN: That's correct.

MR. SIESS: The thing is, those welds may have been calculated for a seismic overturning and they may not have been. They may not have changed at all. We don't know.

MR. BOHN: That's correct.

MR. SIESS: That's speculation. The pumps are a clearcut case and there probably are some other pieces of equipment where the qualification is on the -- important feature, right, of the design.

It's like when you looked at the buildings. The design is not based on the earthquake.

MR. BOHN: Piping also, for that matter.

MR. SIESS: I mean you put 18-inch walls in and put 2 percent -- 1 percent rebar arbitrarily, you know, then you calculate what it will take but you don't design it right up to that limit and there must be other pieces of equipment like that.

MR. BOHN: Yes.

MR. SIESS: This is very interesting.

MR. BOHN: This is just one plant and as Roger mentioned, we're looking at Rocksite VWR, hopefully Maine Yankee, and then Zion for sure. Now the reason that we're focusing on Zion is because all of this has so far been Rocksite or fixed base analysis.

Now when we put a soft soil in it, it's likely -- it's not clear exactly what's going to happen but it could be the structures are almost rotating as rigid bodies because of the soft soil and it will probably change this effect in some fashion. Now, most people argue and Bob Kennedy argues for a deep, soft soil site, it'll reduce this effect considerably.

MR. SIESS: You know --

MR. BOHN: However, for Zion, Zion has 110 foot of overconsolidated glacial till with an actual frequency of the soil column of about 5 hertz. So if we put structures down around 5 hertz, there we're going to get a resonance and I could see it amplifying the effect but I agree with Bob for deep soil site, it ought to decrease it.

MR. SIESS: Now, the pumps, if that pump weren't in here, we didn't have these deep pumps, there'd be very little effect.

MR. BOHN: Let's see. If the pumps weren't in.

MR. SIESS: I mean, you know, we had a particular vulnerability --

MR. BOHN: You'd decrease the total core damage by 30 percent -- 31 percent here.

MR. SIESS: No, I mean --

MR. BOHN: We would still see it on effect due to the 4 KB buses. They were amplified considerably.

MR. SIESS: Yes.

MR. BOHN: That would be the major effect we would see. Overall, the total bottom line number would drop.

MR. SIESS: But the thing is, there's a good possibility that the 4 KB buses would have been the same thing no matter how you analyzed it and it's just the difference between your fragility curve and -- if they didn't design the welds for seismic, that fragility curve wouldn't have changed.

MR. BOHN: That's probably correct.

MR. SIESS: You know, the pumps are a plant specific vulnerability but they're not a plant unique vulnerability unless somebody's using unique differently. I think what the Commission has meant were plant specific. To be plant unique means there's only one plant out there that has it and I don't think that's what they meant. I guess I'm not sure what they meant.

Okay, now Zion will be different. Well, different kinds of equipment because it's a PWR.

MR. BOHN: Yes, but it still has six emergency service water pumps of the same design.

MR. SIESS: Yes, and they were a problem because the roof was going to fail all six of them which I never believed.

MR. BOHN: But that was the fact they had all these diagrams that they hadn't designed.

MR. SIESS: -- never calculated how the roof was going to fail. They just calculated the stress and said at that stress, we don't know what'll happen.

MR. BOHN: We calculated that the parapet that held it in would fail, the lip.

MR. SIESS: I know.

MR. BOHN: Whether it's right or not, we did the best we could but that was the mode of failure. You had to assume a diagonal crack across the plate.

MR. SIESS: I can't believe an earthquake would drop that roof on six pumps uniformly.

MR. BOHN: That is an assumption. You're correct.

MR. SIESS: Only one of them stated.

MR. BOHN: That's correct.

MR. SIESS: It got to be pretty -- but -- Zion will be different, different soil conditions. So starting at the beginning you'll get different things. It's a different lay out. There'll be some differences -- the plant and so forth.

MR. BOHN: The other thing about Zion is that we have piping models for the reactor coolant system and the aux feed water system and the licensing staff has requested for one of these three pumps we give consideration to seeing what the effect on piping calculated moments and shears given the degraded stiffnessing effect.

Now from a costwise, we don't want to just go back and start building piping models. So we picked Zion because we had already done those as part of the safety margins program. We can do that fairly effectively.

MR. WYLIE: The ceramic insulated basically is the loss of off-site power; isn't it?

MR. BOHN: Right.

MR. WYLIE: The effects of the blackout room depending on how that's implemented, with the -- turbine or whatever, would affect this, I would think.

MR. BOHN: Yes, it could. It would depend on the capacity of the turbine. You know, we tried to look at Black Start turbines at some plants. We find that their capacity is so low that they're almost not worth considering.

MR. SIESS: Well, the staff's talking about an alternate source of AC power large enough to run one train of equipment.

MR. BOHN: That would definitely decrease the seismic risk at any plant because always these station loss of off-site power sequences are always the dominant ones in seismic.

MR. SIESS: This is what I just read as a requirement for evolutionary LWRs.

MR. BOHN: That would make sense and that approach has been used at several DOE facilities. In fact, at the HIFR, high intensity flux reactor at Oak Ridge, they have military generators, two of them truck-mounted, several miles from the site at two alternate routes so they can run out and bring them in. They've essentially got a big plug on the outside of their switch gear building and they have the cables and they'll drive the thing in with a two-by-four truck and plug that thing in the wall and they've got their power.

MR. SIESS: They park them somewhere where they won't be affected by the earthquake.

MR. BOHN: That was the theory, yes, and they have two of them by two different routes.

MR. SIESS: That's what Yankee Rowe was going to do. They were going to put a generator on a truck and put it in a seismically designed garage and then haul it out. That's right. This was the Yankee. That was their fix.

MR. BOHN: That's a valid approach.

MR. SIESS: The French have got this trailer they bring up and hook into an existing switch gear building to take care of post-accident. That's not the -- but we could never get away with a generator on a truck parked over there somewhere for a nuclear power plant. That's not going to meet all the requirements.

MR. BOHN: You know, one plant that we talked to that had very short battery life given station blackout, they had actually gone to the point of figuring out how many jump start cables and how many cars would you have to have in a lot to feed in enough battery power to give you your control power but they thought about it. They'd actually made that calculation. I was impressed.

MR. SIESS: The thing that bothers me is they'll think about that but nobody will think about a way to get the power from Unit II over to Unit I when both of them didn't fail. We do that now but, you know, that other scheme, how do you get a Category I truck?

MR. BOHN: Well, another way to look at it is every donkey engine in a railway switch yard gets jammed and beat around I'm sure in excess of what an earthquake can do to it and those things perform every day.

MR. SIESS: That's right, but ceramic insulators won't. You don't have to have them. Japanese don't use them.

MR. BOHN: Is that right?

MR. SIESS: Saw that picture where they had all their lines and --

MR. WYLIE: Yeah, but on the end of those pipes, there's a bushing.

MR. SIESS: But that's easier to design than an 8-foot stack of ceramic insulators.

Okay. You're through with Peach Bottom. You're working on Zion.

MR. BOHN: Yes. The other two basically together.

MR. SIESS: Nobody's figured what we're going to do with it or how, except the possibility that this is something to be thought about in the IPE.

MR. BOHN: That's possible. I don't know.

MR. KENNEALLY: Insights for IPE and also the evaluation of the potential safety effect that licensing would need if there was anything to come out of this difference reduction. They're the ones that have to make that decision and so we developed this program to try and give them those insights.

MR. SIESS: I think it could be handled in the margins thing but I'm not quite sure how.

MR. BOHN: It could if they did time history analyses, I believe. If they just use the engineering factor approach to pick a load and gear it, then it's not so clear how it could be handled.

MR. SIESS: I think we're through. Okay, let's say meeting's adjourned.

[Whereupon, at 4:45 p.m., the meeting adjourned.]

REPORTER'S CERTIFICATE

This is to certify that the attached proceedings before the United States Nuclear Regulatory Commission

in the matter of:

NAME OF PROCEEDING: ACRS Subcommittee On Structural Engineering

DOCKET NUMBER:

PLACE OF PROCEEDING: Albuquerque, New Mexico

were held as herein appears, and that this is the original transcript thereof for the file of the United States Nuclear Regulatory Commission taken by me and thereafter reduced to typewriting by me or under the direction of the court reporting company, and that the transcript is a true and accurate record of the foregoing proceedings.

Jon Hundley

JON HUNDLEY
Official Reporter
Ann Riley & Associates, Ltd.

FUTURE PLANS OF THE CONTAINMENT INTEGRITY PROGRAMS

M. Brad Parks
Containment Technology Division
Sandia National Laboratories

ACRS Subcommittee Meeting
January 24-25, 1990

PLANNED TEST PROGRAMS

- SEPARATE EFFECTS TESTS - LINER TEARING
- RETEST OF 1/6-SCALE MODEL
- POTENTIAL COOPERATIVE AGREEMENT WITH NUCLEAR POWER ENGINEERING TEST CENTER (NUPEC)

PRIMARY INTEREST:	NRC - PRESTRESSED CONTAINMENT NUPEC - BWR VESSEL HEADS AND FLANGES

- PENETRATION TESTS

  1/6-SCALE MODEL UNSEATING EQUIPMENT HATCH
  BELLOWS

SEPARATE EFFECTS TESTS
LINER TEARING

PURPOSE:

TO DEVELOP A COMPREHENSIVE ANALYSIS METHOD TO PREDICT LINER TEARING IN REINFORCED AND PRESTRESSED CONCRETE CONTAINMENTS.

LINER TEARING MECHANISMS

1) Membrane plasticity

2) Possible strain mechanisms

   - In-plane plastic state as an initial condition
   - Stiffness discontinuity from insert plate
   - Localized shear lag from studs
   - Bending due to crimping

3) Variables affecting phenomenon

   - Stud/liner geometry (d/t)
   - Stud concrete interaction (stud length)
   - Stud/insert plate (location, shape)
   - Stud spacing
   - Liner material properties
   - Liner-concrete friction

GOALS OF THE TESTING PROGRAM

PHASE I	**To determine the effect of liner prestress on the performance of the liner when the studs are loaded in shear.**
PHASE II	**To provide a link between the 1:6 scale containment model and full scale tests and to determine the most important strain concentration mechanisms.**

TEST MATRIX FOR PHASE 1 TESTING.

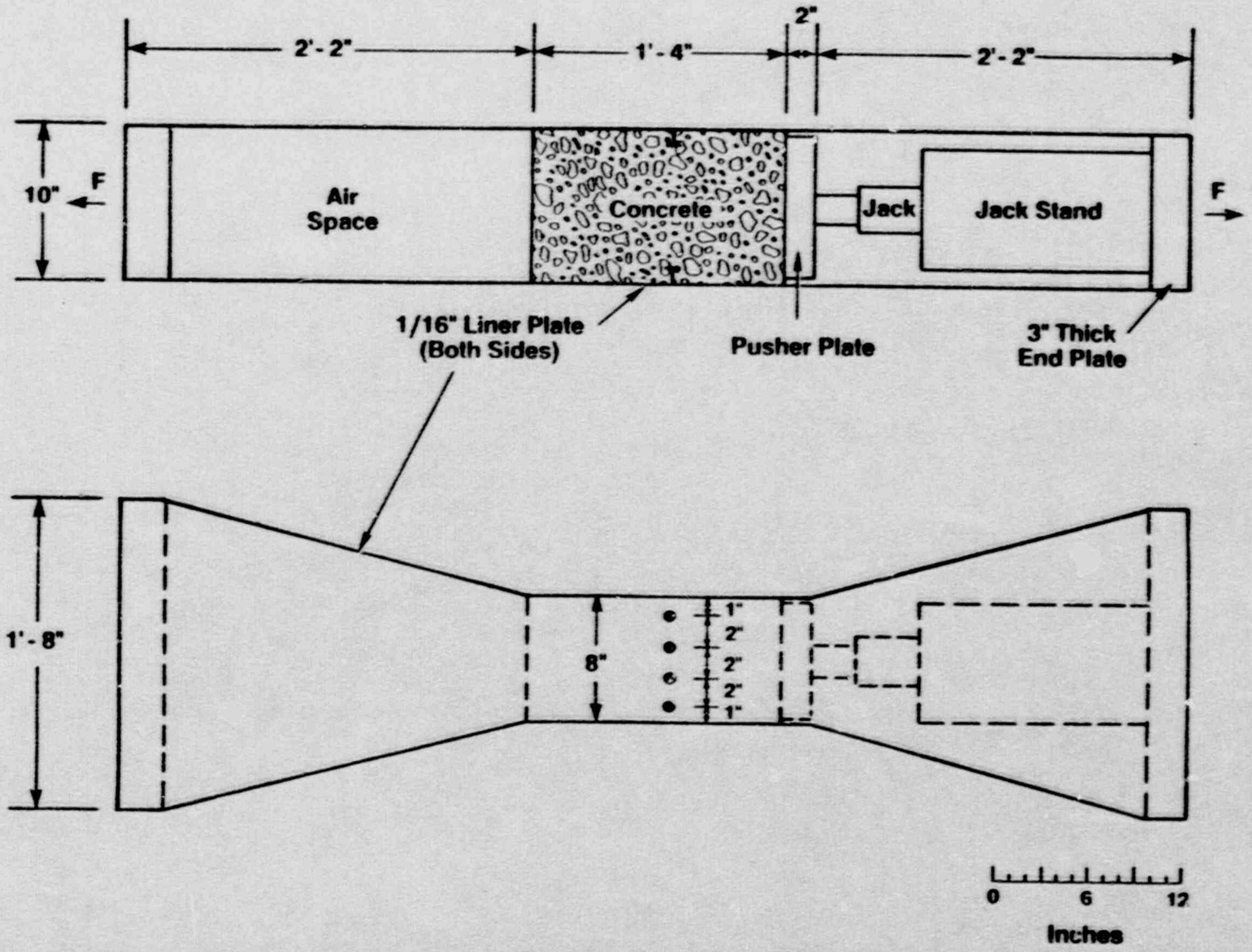
Test Series	# of Tests	d/t Ratio[1]	Friction Modeled?	Preload to Liner?	Questions Addressed/(Remarks)
1:6-scale tests:					
1A	3	2.37	no	no	Does the stud fail in shear? What is the shear load at failure?
1B	3	2.37	no	yes	Does membrane yielding affect the liner anchorage failure mode? What is the shear load at failure?
Full-scale tests:					
1C	3	2.37	no	yes	With a typical full-scale thickness, does liner tearing occur? (Material properties may be different; effect of surface loading may depend on thickness. Full scale testing is also more credible and studs and welding techniques can be more realistic.)
1D	1	na	yes	vary	How much shear can be transferred by friction, i.e., what is the relation between the frictional shear force and slip deformation? (No studs are used, and test is nondestructive so many load cycles can be applied, i.e., increment shear force at different pressure levels. These measurements could be used to predict the effect of friction in the following tests.)
1E	3	2.37	yes	yes	Does friction change the failure mode of the liner anchorage system? Does friction cause a significant change in the shear load at which failure occurs? (Same as 1C with friction added; comparison of 1C and 1E should determine the significance of friction.)
1F	3	1.00	no[2]	yes	Is the failure mode different for smaller ratios of stud diameter to liner thickness? To what extent is the shear load at failure changed?
1G	3	1.67	no	yes	Is the failure mode different for smaller ratios of stud diameter to liner thickness? To what extent is the shear load at failure changed?

---

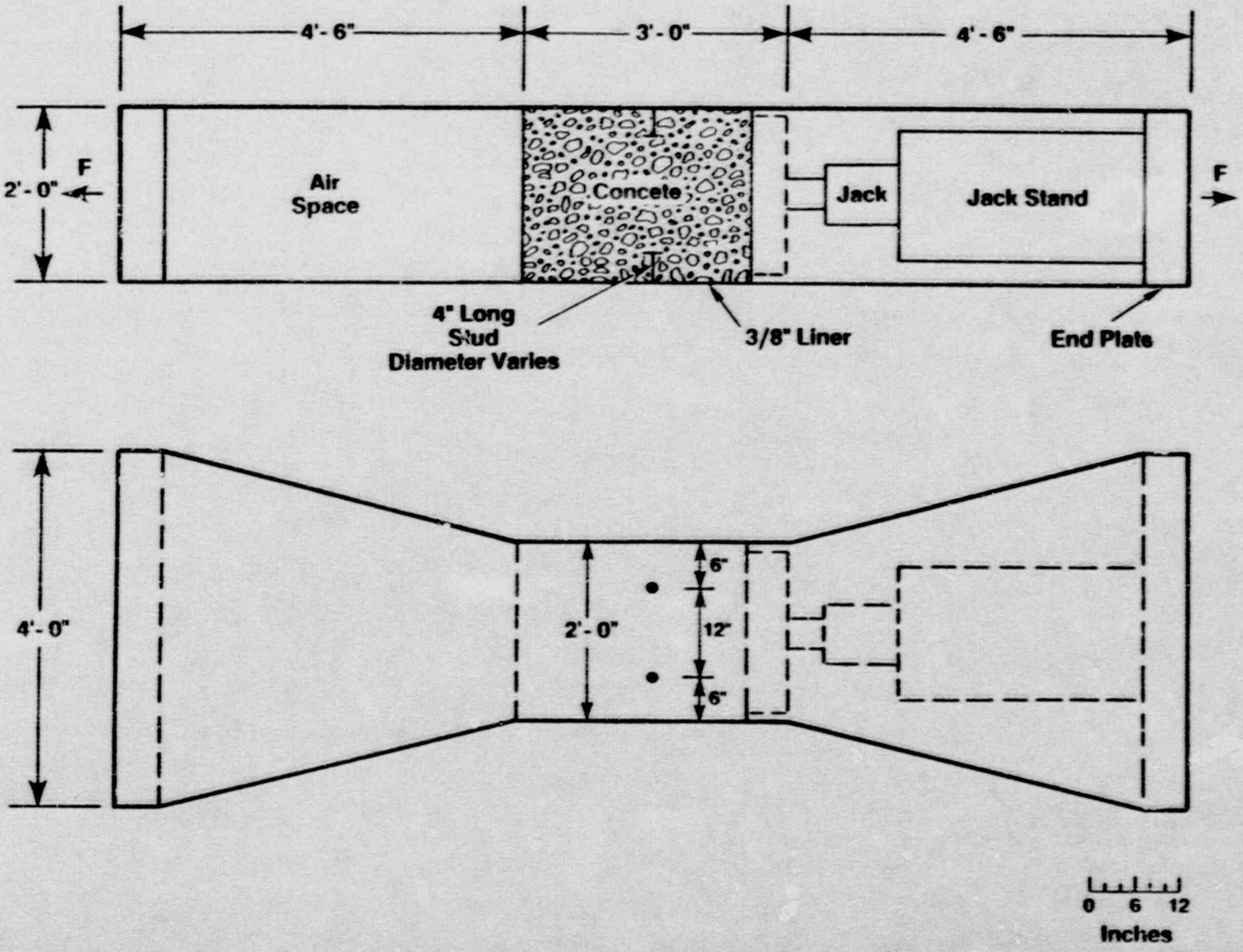
[1] d is the stud diameter and t is the liner thickness; in the 1:6-scale model tests (1A and 1B) the liner thickness is 1/16 in., and in the full-scale tests the liner thickness is 3/8 in.

[2] In tests 1F and 1G, the conclusion regarding the importance of friction drawn from a comparison of tests 1C, 1D and 1E will determine whether or not friction is modeled. Tests 1F and 1G are also included in the test matrix based on the assumption that liner tearing occurs when the liner is preloaded both in the 1:6-scale liner thickness and the full-scale liner thickness test specimens. This assumption could change as the testing program progresses.

PHASE 1 (TEST 1A - 1B) TEST SPECIMEN



PHASE 1 (TEST 1C - 1G) TEST SPECIMEN

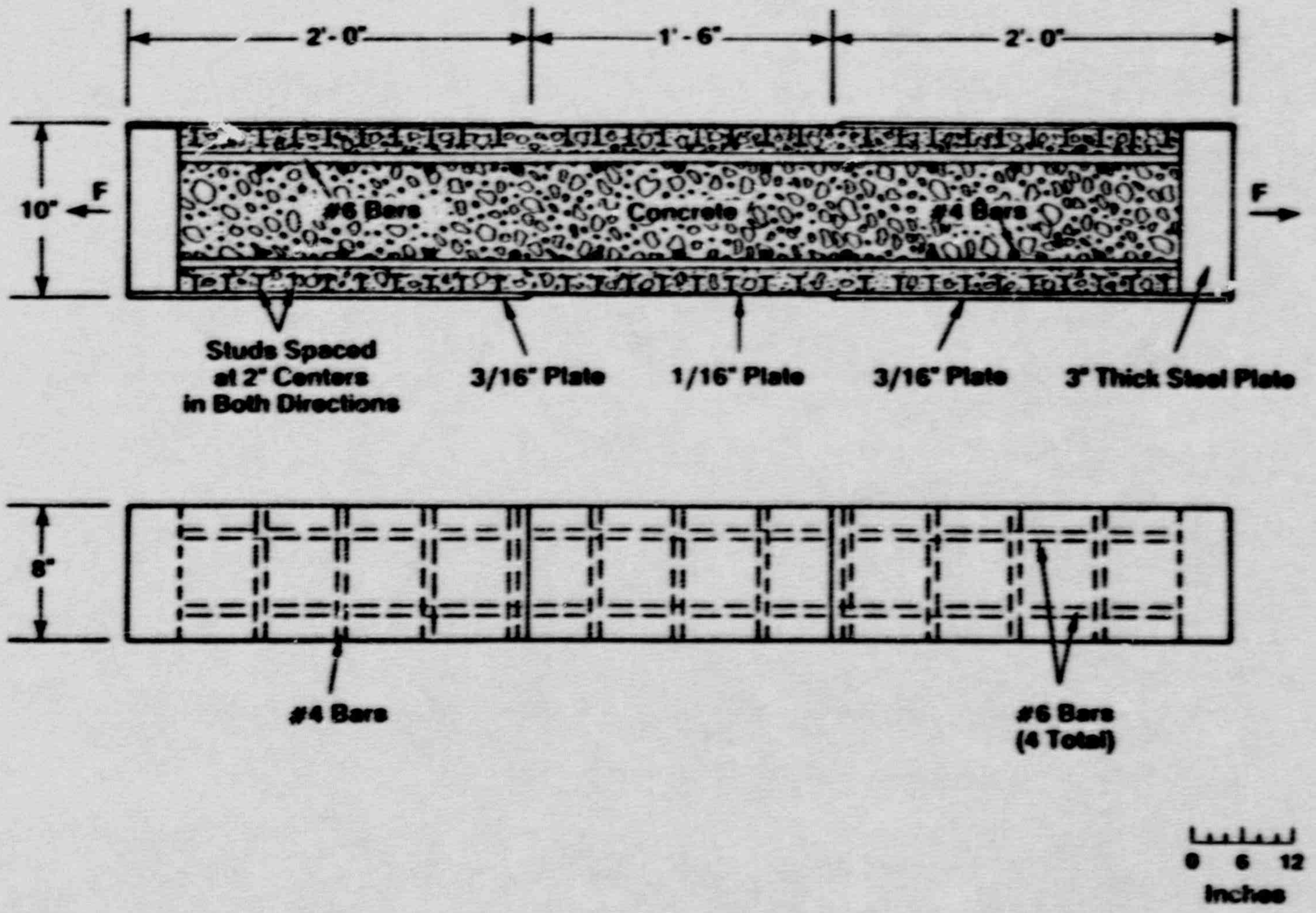


TEST MATRIX FOR PHASE 2 TESTING.

(All 1:6-scale tests)

Test Series	# of Tests	Stud Spacing	Friction Modeled?	Questions Addressed/(Remarks)
2A	1	NA	no	What is the strain concentration due only to the presence of the insert plate? Where do the peak strains occur? At what peak strain do tears initiate? How do the tears propagate? (This test establishes a baseline to determine the effect of studs. This is a pull test of the liner material including the insert plate.)
2B	3	2"	no	How do studs affect the magnitude and location of the peak strain? Are the insert plate and studs the essential features that must be included to predict liner tearing, i.e., how well does this test replicate the 1:6-scale model result? (Use symmetric specimen - same liner on both sides of concrete with studs, use same ratio of rebar area to liner area in 1:6-scale model.)
2C	3	2"	yes	Does friction significantly reduce the slip resisted by the studs, thereby reducing the stud shear forces and the magnitude of the peak strain? (Use same test specimen as in 2B.)

PHASE 2 TEST SPECIMEN



POSSIBLE FUTURE TESTING

1) **Parameter studies of stud and line anchorage systems (size and spacing of anchorage).**

2) **Insert plate studies (shape and thickness variations).**

3) **Temperature effects**

4) **Pull-out failure mode**

SUMMARY OF CONTAINMENT PENETRATION RESEARCH PROGRAMS

M. Brad Parks
Containment Technology Division
Sandia National Laboratories

presented to
ACRS Subcommittee on Structural Engineering
January 24-25, 1990

STATUS OF PENETRATION PROGRAMS

COMPLETED:

- Electrical Penetration Assemblies (NUREG/CR-5334)
- Personnel Airlock (NUREG/CR-5118)
- Compression Seals and Gaskets (NUREG/CR-4944,5096)
- Inflatable Seals (NUREG/CR-5394)

ONGOING TESTS:

- Pressure-Unseating Equipment Hatch

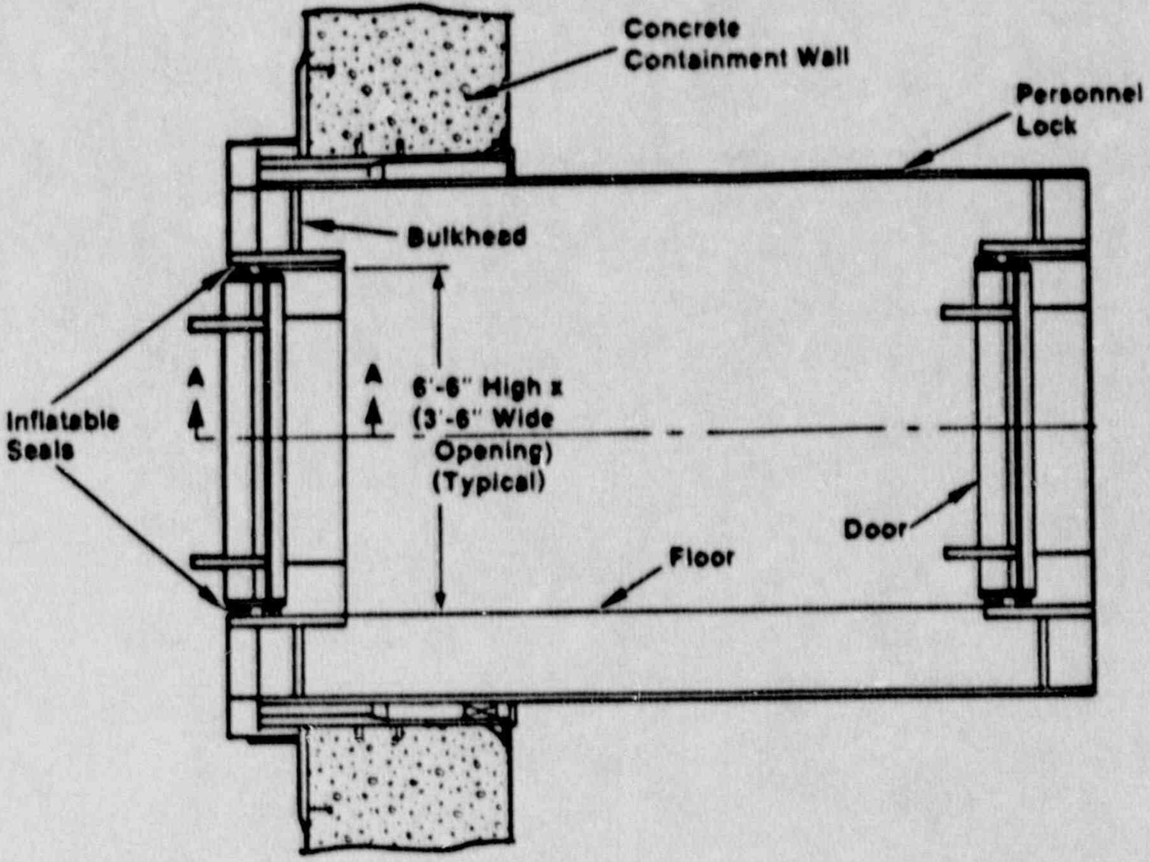
TO BE TESTED:

- Bellows

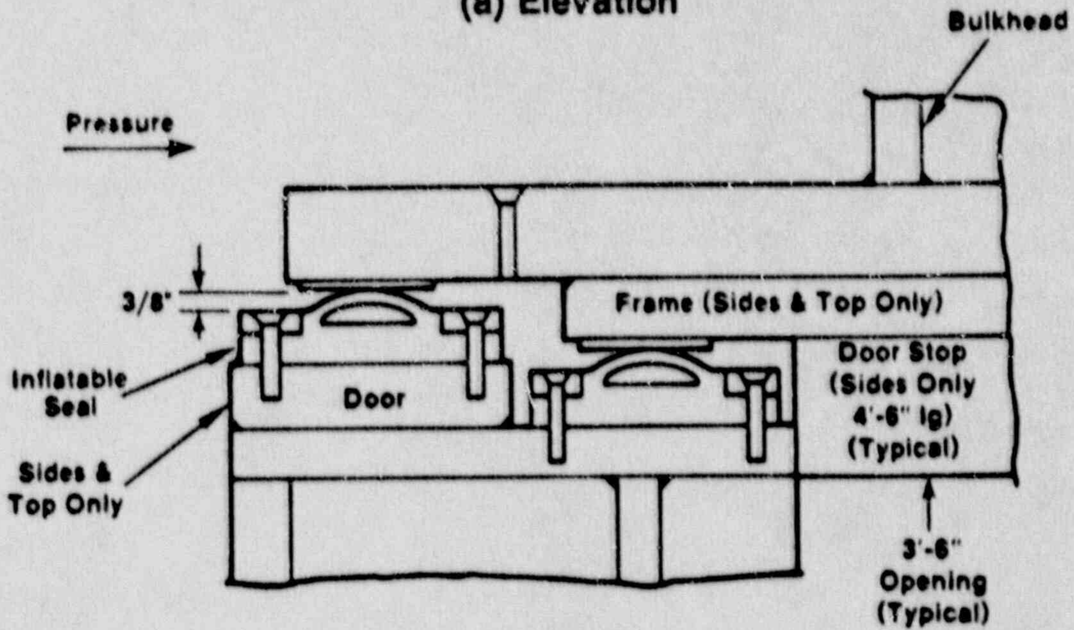
INFLATABLE SEALS

Background Information:

- Used to prevent leakage around personnel and escape lock doors
- Currently installed or planned for use in thirteen commercial nuclear power plant containments (Approx. 10% of all commercial containments)
- All installations are in either PWR or Mark-III type containments
- Normal operating seal pressure varies from 50 to 110 psig depending on the nuclear power plant



(a) Elevation



(b) Section A-A

Typical Application of Inflatable Seals in a Personnel Airlock

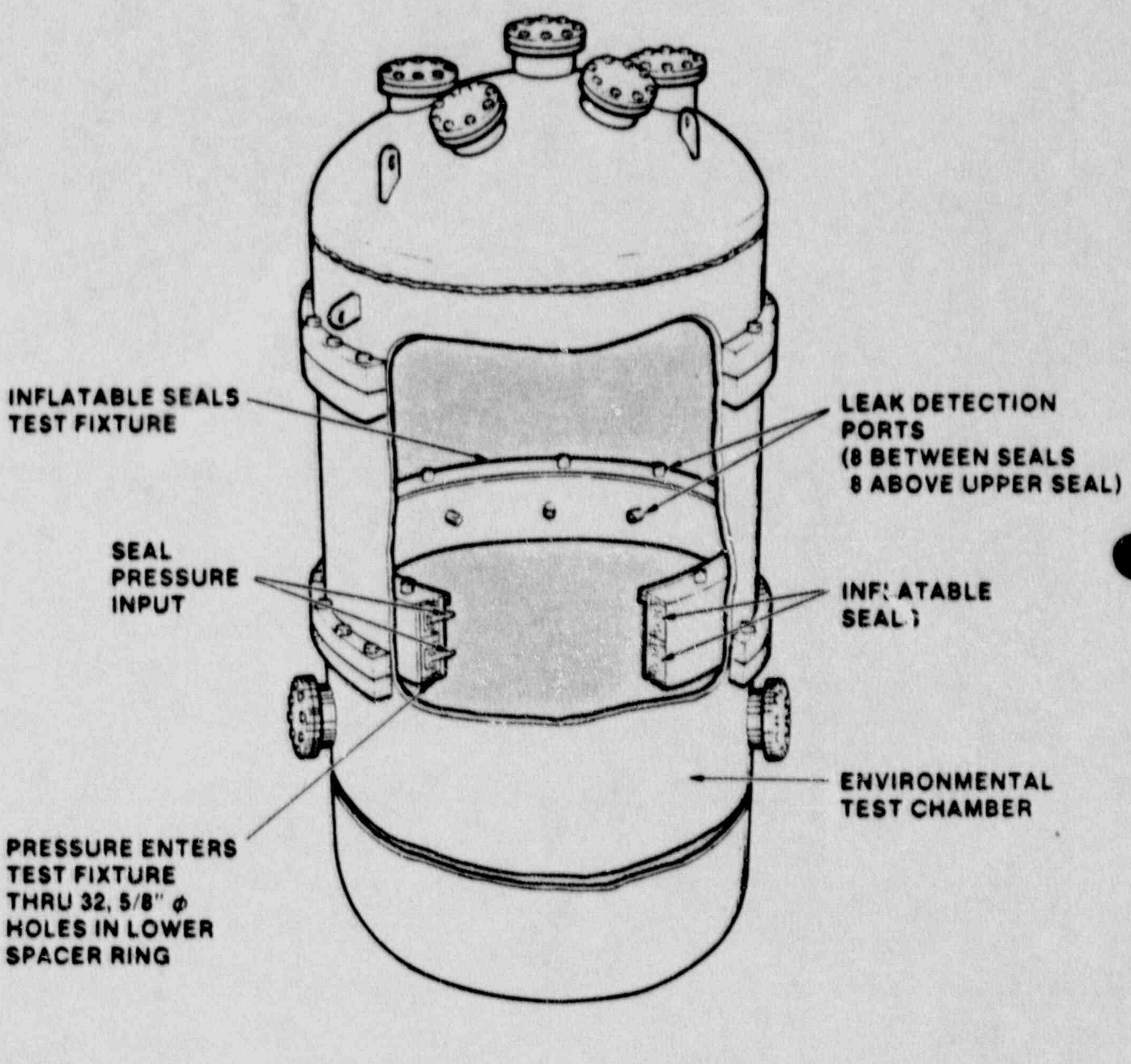
TEST MATRIX

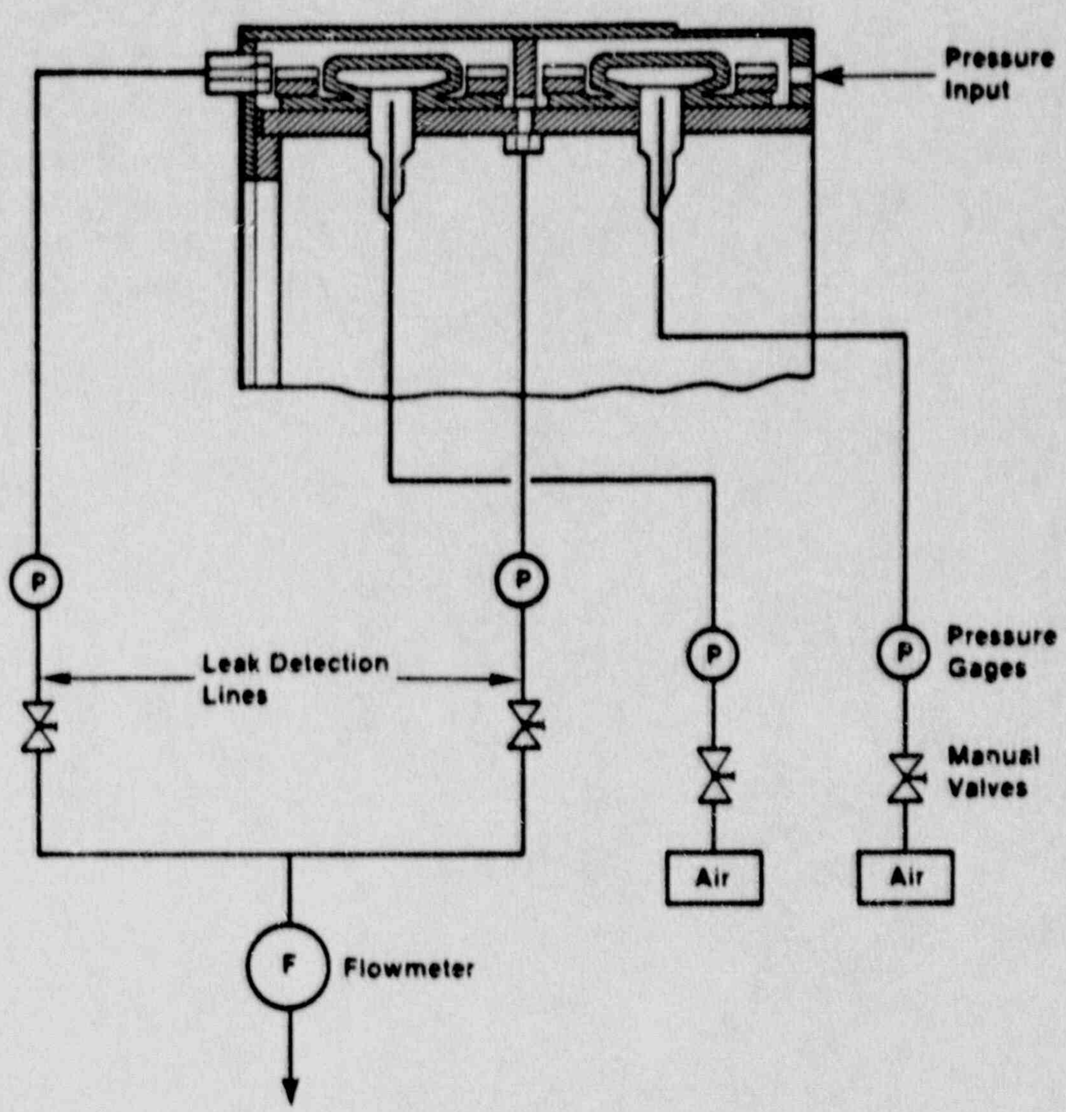
Test Series	Seal Design	Seal Condition	Loading
1	Old	Unaged	Air, Room Temp. & 400°F
2	Old	Aged	Air, Room Temp. & 300°F
3	New	Unaged	Air, Room Temp. & 300°F,350°F
4	New	Aged	Air, Room Temp. & 300°F

INFLATABLE SEALS

Primary Test Objectives:

1) To determine the containment pressure and temperature, for a given internal seal pressure, to produce significant leakage past inflatable seals.
2) Once leakage begins, to determine the rate at which leakage increases for further increases in containment pressure.


INFLATABLE SEALS TEST FIXTURE
LEAK DETECTION PORTS (8 BETWEEN SEALS 8 ABOVE UPPER SEAL)
SEAL PRESSURE INPUT
INFLATABLE SEALS
ENVIRONMENTAL TEST CHAMBER
PRESSURE ENTERS TEST FIXTURE THRU 32, 5/8" ϕ HOLES IN LOWER SPACER RING



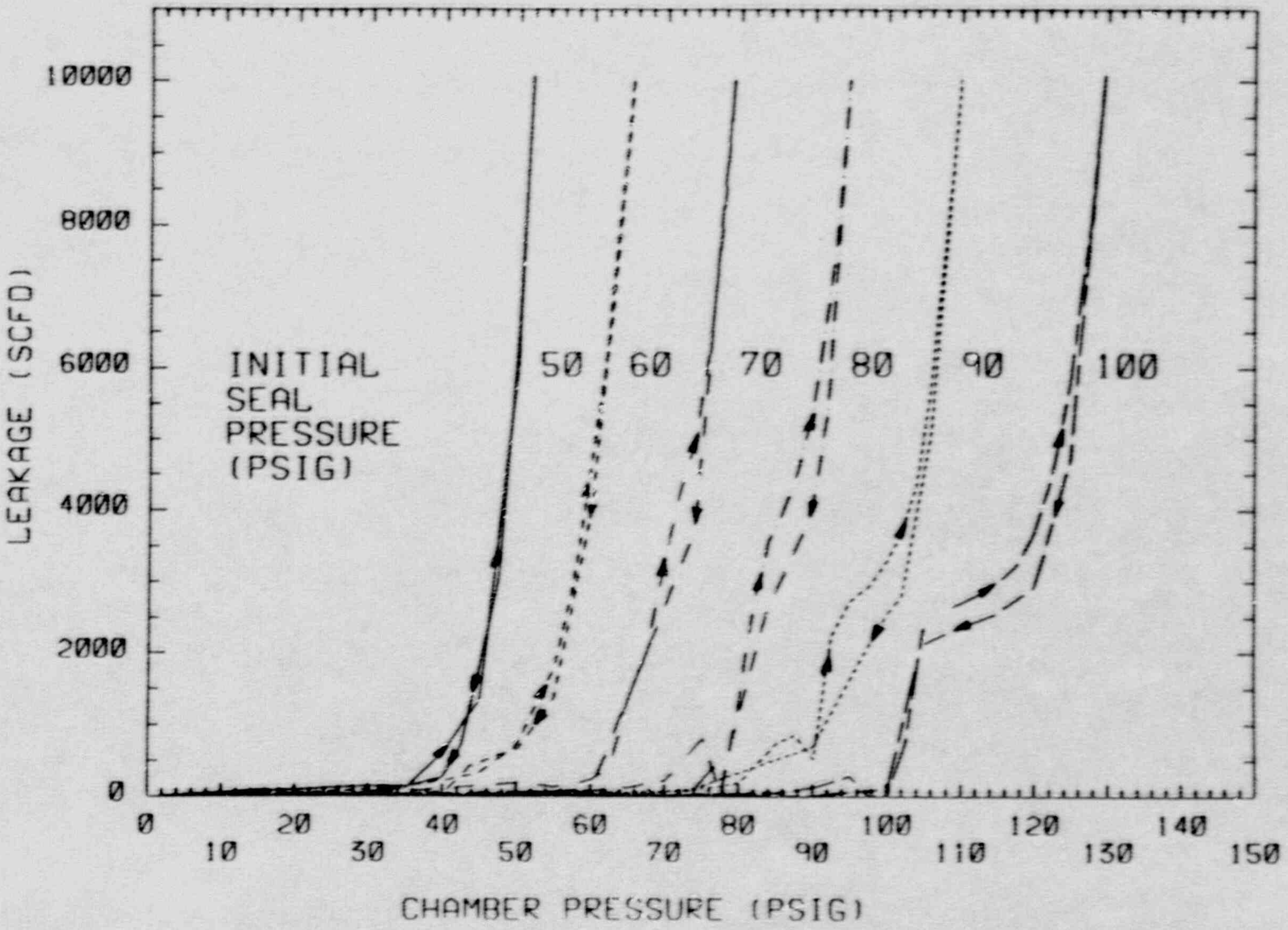
Schematic of Test Setup

SUMMARY OF ROOM TEMPERATURE TESTS
TEST SERIES 1 THRU 4

Initial Seal Pressure (psig)	Chamber Pressure (psig) for Leakage Past Both Seals of 10,000 scfd				
	Test Series 1	Test Series 2	Test Series 3 (Round 1)	Test Series 3 (Round 2)	Test Series 4
50	51.1	---	93.0	58.2	---
60	65.4	79.0	98.5	76.9	100.5
70	79.0	---	104.3	97.4	---
80	94.7	---	125.1	129.1	---
90	109.9	---	140.1	---	---
100	129.6	---	---	---	---
60C*	---	---	60.8	---	---
90C*	---	---	92.6	---	---

---

*Seal pressure maintained constant throughout test.



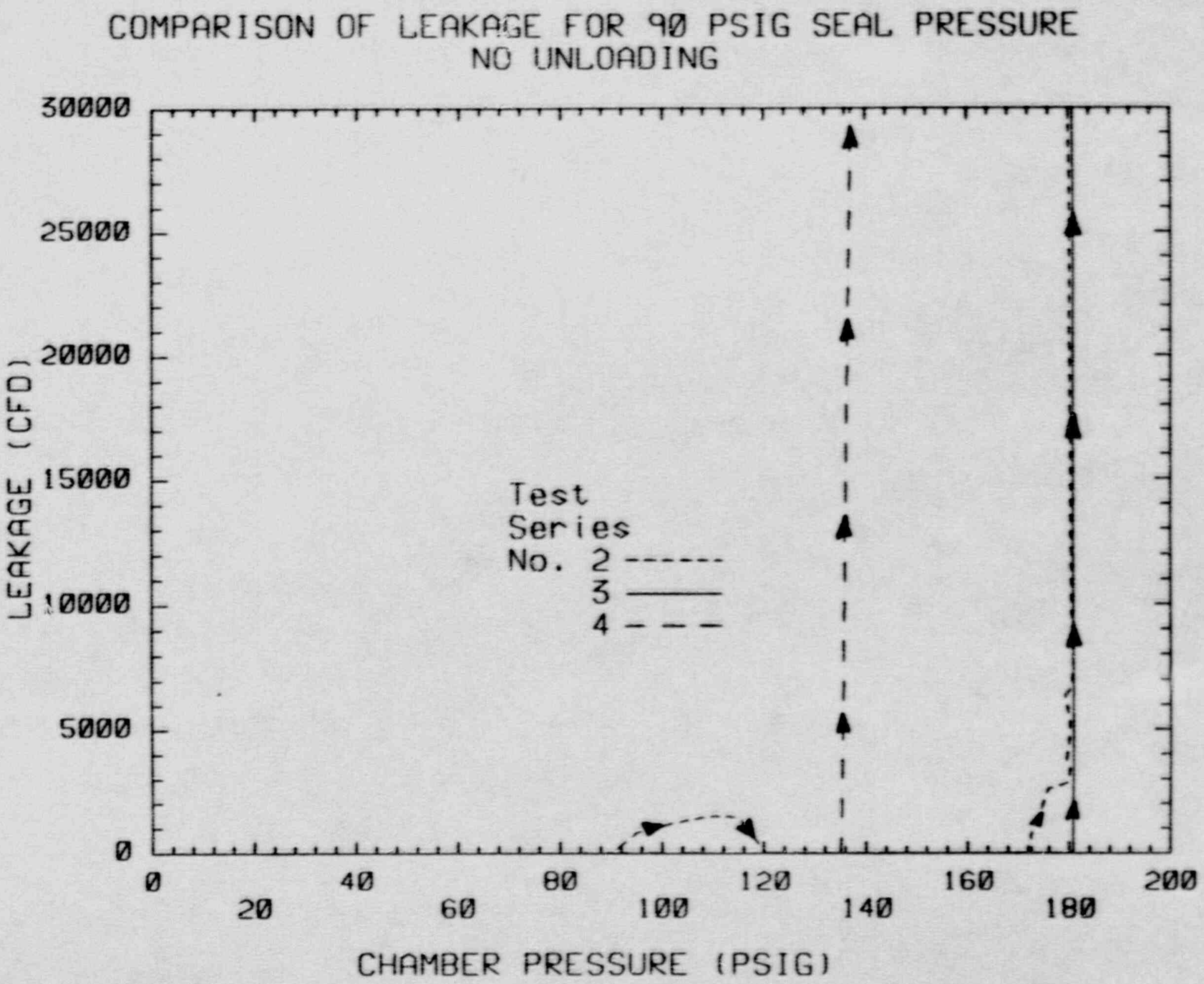
Leakage Vs. Chamber Pressure for Various Seal Pressure Levels
Test Series 1 - Seals Isolated From Pressure Source - Room Temperature

SUMMARY OF ELEVATED TEMPERATURE TESTS
TEST SERIES 1 THRU 4

Seal Pressure at Room Temperature (psig)	Test Temperature (°F)	Chamber Pressure (psig) at Failure* of Seals			
		Test Series 1	Test Series 2	Test Series 3	Test Series 4
50	400	132	---	---	---
90	300	---	180	180	138
90	350	---	---	145	---

---

*Failure is defined as leakage past both seals in excess of 30,000 scfd.



Comparison of Leakage at 300°F for Test Series 2, 3, and 4 (90 psig Initial Seal Pressure)

SUMMARY OF TEST RESULTS

1) Regardless of test conditions, <u>significant leakage did not occur until the chamber pressure exceeded the initial seal pressure</u>.

2) Leakage increased rapidly for small increases in chamber pressure.

3) For temperatures up to 350°F, there were no indications of degradation of the seal material. However, between 350°F and 400°F (the maximum test temperature), signs of a breakdown in the composite seal material began to occur.

4) Test validated methods have been developed to predict the containment pressure, for a given seal pressure and temperature, at which leakage past inflatable seals can be expected.

Evaluation of Leakage Potential of Pressure Unseating Equipment Hatches and Drywell Heads

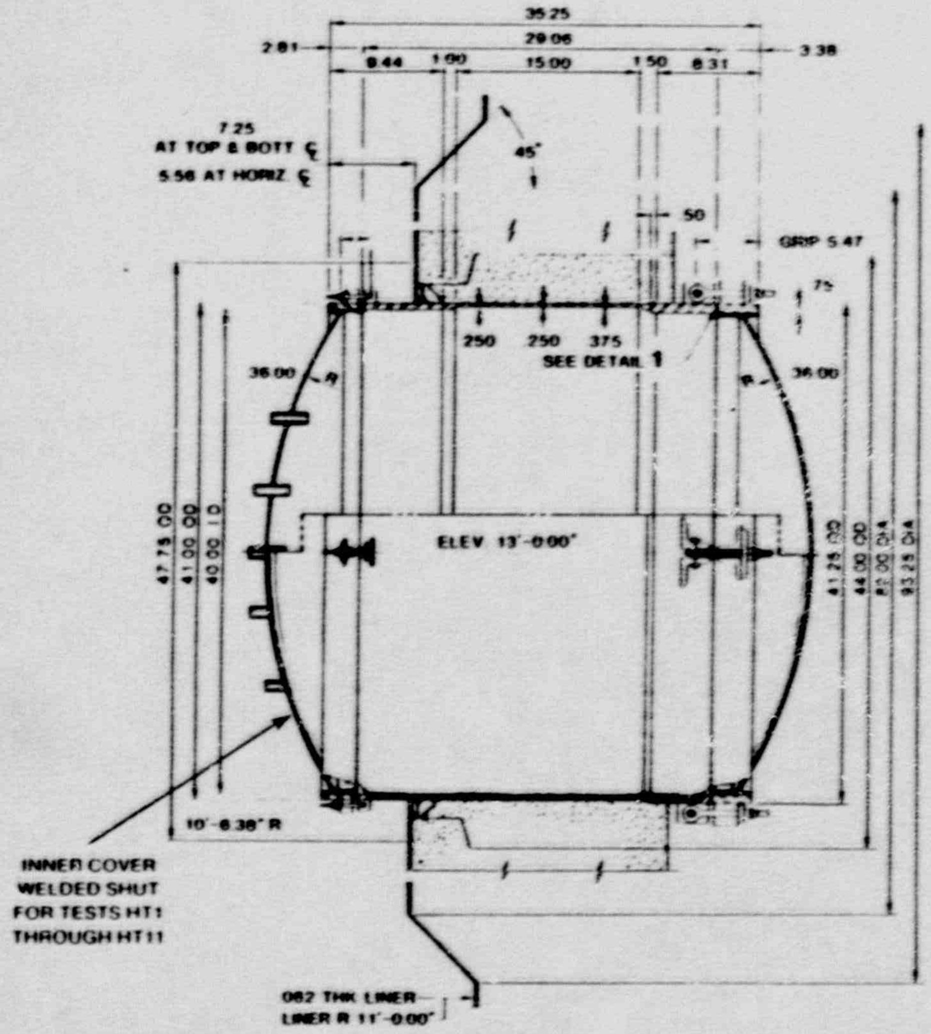
Analytical method has already been developed:

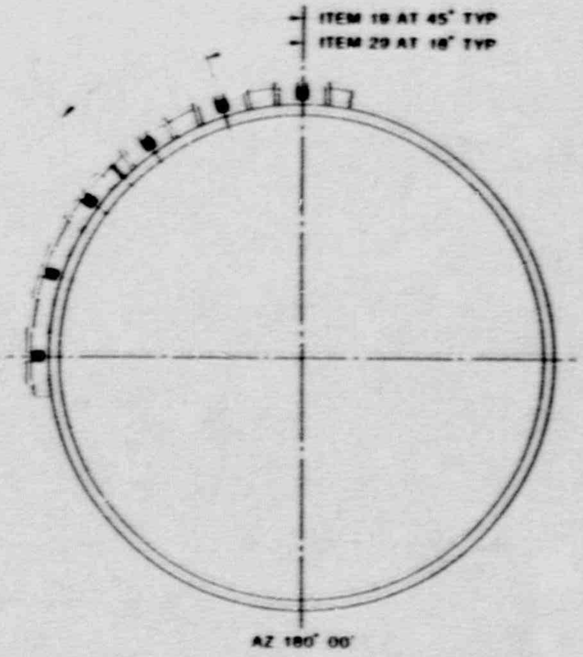
- **Structural response determined from strength of materials**
- **Empirical criteria for evaluating leakage initiation, based on gasket available springback**
- **Leakage from fluid mechanics**

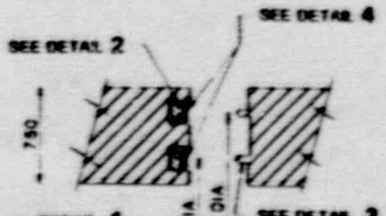
Tests are underway on the pressure-unseating hatch in the 1:6-scale model to validate this analytical approach. Parameters being varied include:

- **Gasket material**
- **Aging history**
- **Aggregate bolt preload**
- **Aggregate bolt stiffness**
- **Loads**

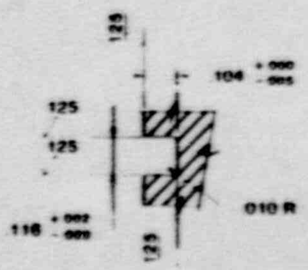
Ambient temperature tests have been completed.



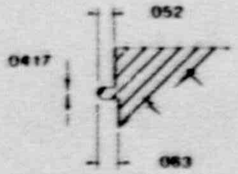




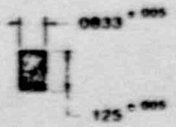
DETAIL 1



DETAIL 2



DETAIL 3



DETAIL 4

NOTES

1 UNLESS OTHERWISE SPECIFIED DIMENSIONS ARE IN INCHES

Table 1
Revised Test Matrix for Investigating the Leakage Potential of a Pressure-Unseating Equipment Hatch

Test Designator	Gasket Material	Aging Duration[1] (hours)	Bolt Preload (kips)	Number of Bolts	Test Load[2]
LP3[3]	SI	118	45.7	10	A
HT1	SI	144	57.2	10	A
HT2	EP	Unaged	68.7	10	A
HT3	EP	Unaged	91.5	20	A
HT4	EP	168	91.5	20	A
HT5	EP	Unaged	91.5	20	B
HT6	EP	144	91.5	20	B
HT7	EP	168	114.4	20	B
HT8	SI	168	91.5	20	B
HT9	EP	Unaged	91.5	20	C
HT10	EP	144	91.5	20	C
HT11	SI	144	91.5	20	C

Notes:

1. Gaskets will be aged in-place at 300°F for the indicated time to simulate both radiation and thermal aging. Data from Reference 4 indicates that compression set retention of EP and SI seals is most sensitive to radiation aging. Exposure to as little as 50 Mrads results in compression set retention of 75% for EP and 90% for SI. Typically, a radiation dose of 200 Mrads has been used in other experiments and, although it may represent an overtest, the compression set retention is about 95% for EP and about 97% for SI at this level of exposure. Since only thermal aging is practical for the equipment hatch tests, the aging time (and possibly temperature also) should be adjusted to achieve compression set retention of the gaskets between 80% and 95%. Dimensional measurements of the gasket will be made three times: when the gaskets are first placed in the grooves and before the cover has been installed; before pressure testing and after the cover has been in place with the bolts torqued to 40 ft-lbs for at least one day (or, if applicable, after aging); and after pressure testing (unless the gaskets are not intact).

2. A - Stepwise pressurization at ambient temperature.
   B - Stepwise pressurization and heating; temperature held equal to the steam saturation temperature at the current pressure.
   C - Hold gas temperature at level sufficient to maintain the gasket at or above its degradation temperature as defined in Reference 5 for at least two hours; maintain temperature and initiate a stepwise pressurization.
   For all three cases, pressurization with nitrogen will continue until significant leakage is detected or until the maximum allowable pressure, as defined in the SOP, is reached, whichever comes first.

3. Test LP3 was conducted in July 1987 during the time that the pressure testing of the 1:6-scale model was conducted.

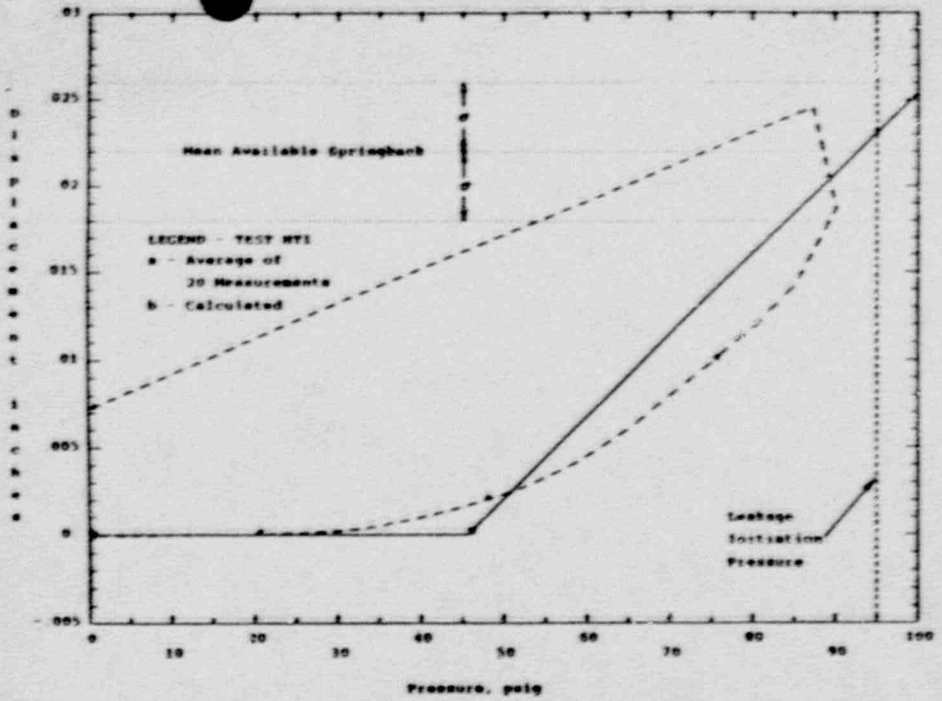
Table 2
Summary of Calculated and Measured Behavior

Test No.	HT1	HT2	HT3	HT4
Maximum Test Pressure	95	115	200	180
Leakage Initiation Pressure[1] (psig)				
Measured	90-95	110-112	195-197	>180
Calculated	93	120	222	166
Leak Rate at Max Test Pressure (scfm)				
Measured	25	30	13	-
Calculated	80	0	0	570
Separation[2] (mils)				
Measured[3]	25	36	35	32
Calculated	23	32	33	28
Available Springback (mils)				
Mean	22	39	39	25
Standard Deviation	4	6	6	2

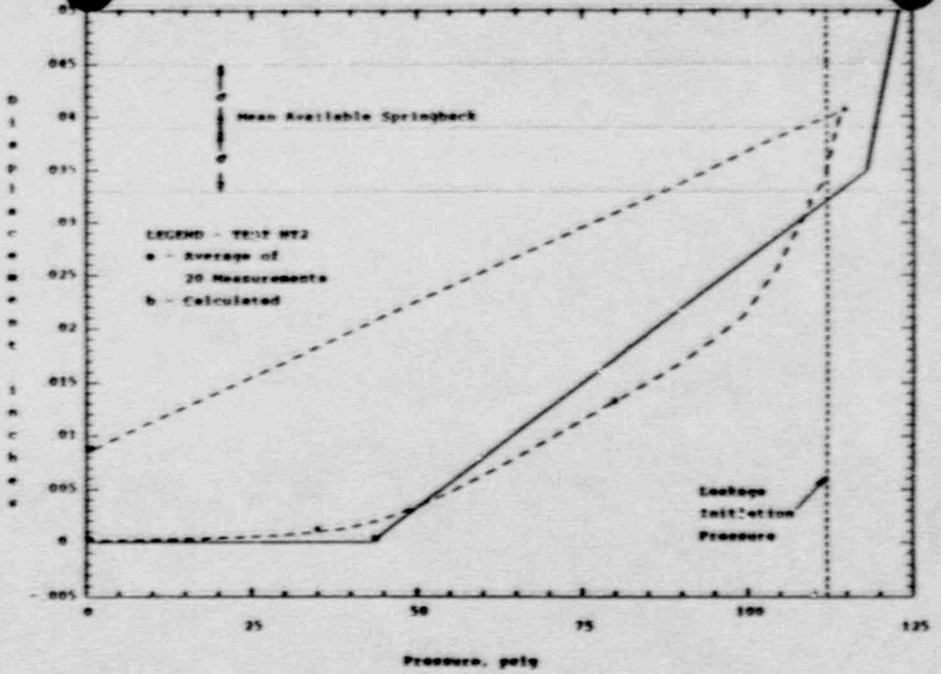
Notes:

1. Measured value arbitrarily defined as pressure at which leakage first exceeded 5 scfm; calculated value corresponds to the initiation of leakage.
2. Measured and calculated result given at the pressure corresponding to the higher value in the range listed for measured leakage initiation pressure.
3. Average of all 20 displacement transducers.

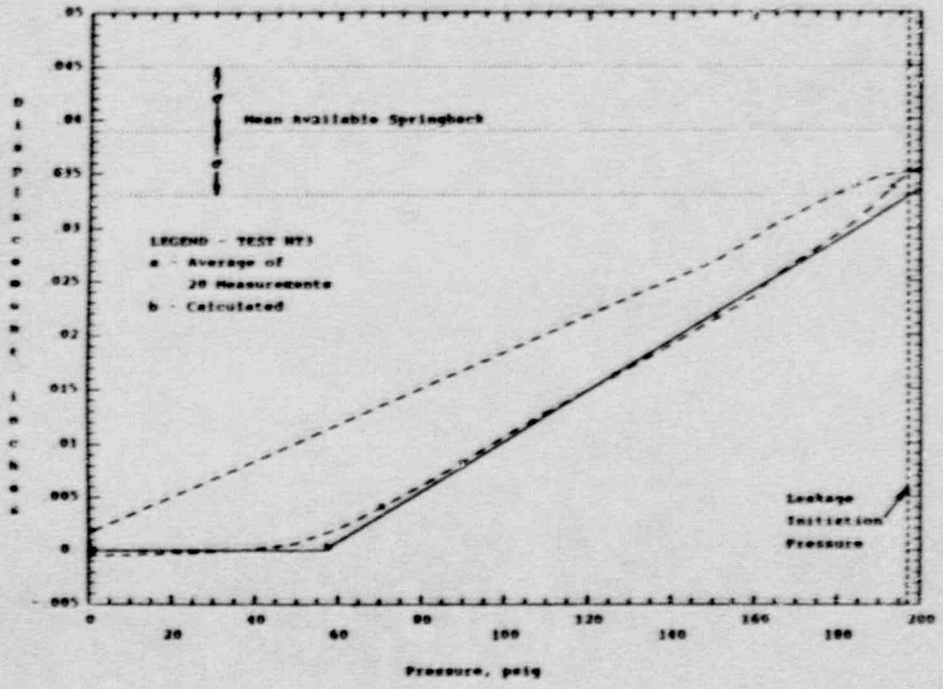
Test HT1



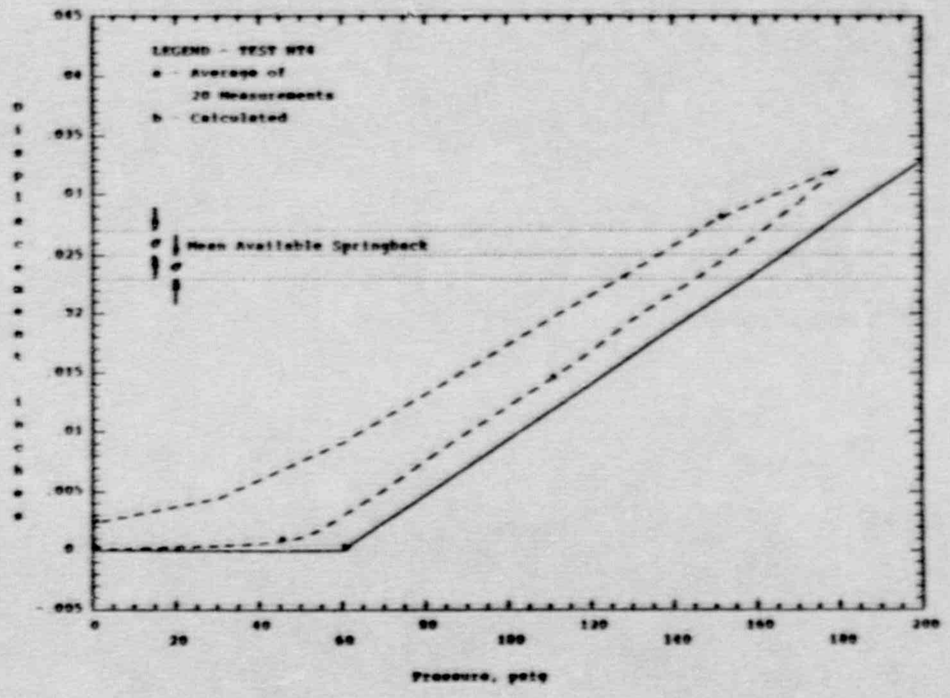
[illegible]
Average Separation Displacement



Results of Equipment Hatch Test HT3
Average Separation Displacement



Results of Equipment Hatch Test HT4
Average Separation Displacement



Preliminary Conclusions

In three of the four tests, significant leakage first occurred when the separation displacement was within one standard deviation of the mean available springback.

The mean available springback is a reasonably accurate measure of gasket performance.

Average response can be used with available springback to predict leakage initiation with reasonable accuracy.

Leakage is very sensitive to the available springback.

The method for calculating leak rate significantly overestimates the actual leakage.

BELLOWS

USED PRIMARILY IN STEEL CONTAINMENTS TO MINIMIZE PIPING LOADS IMPOSED ON THE CONTAINMENT SHELL

TWO MAIN TYPES:

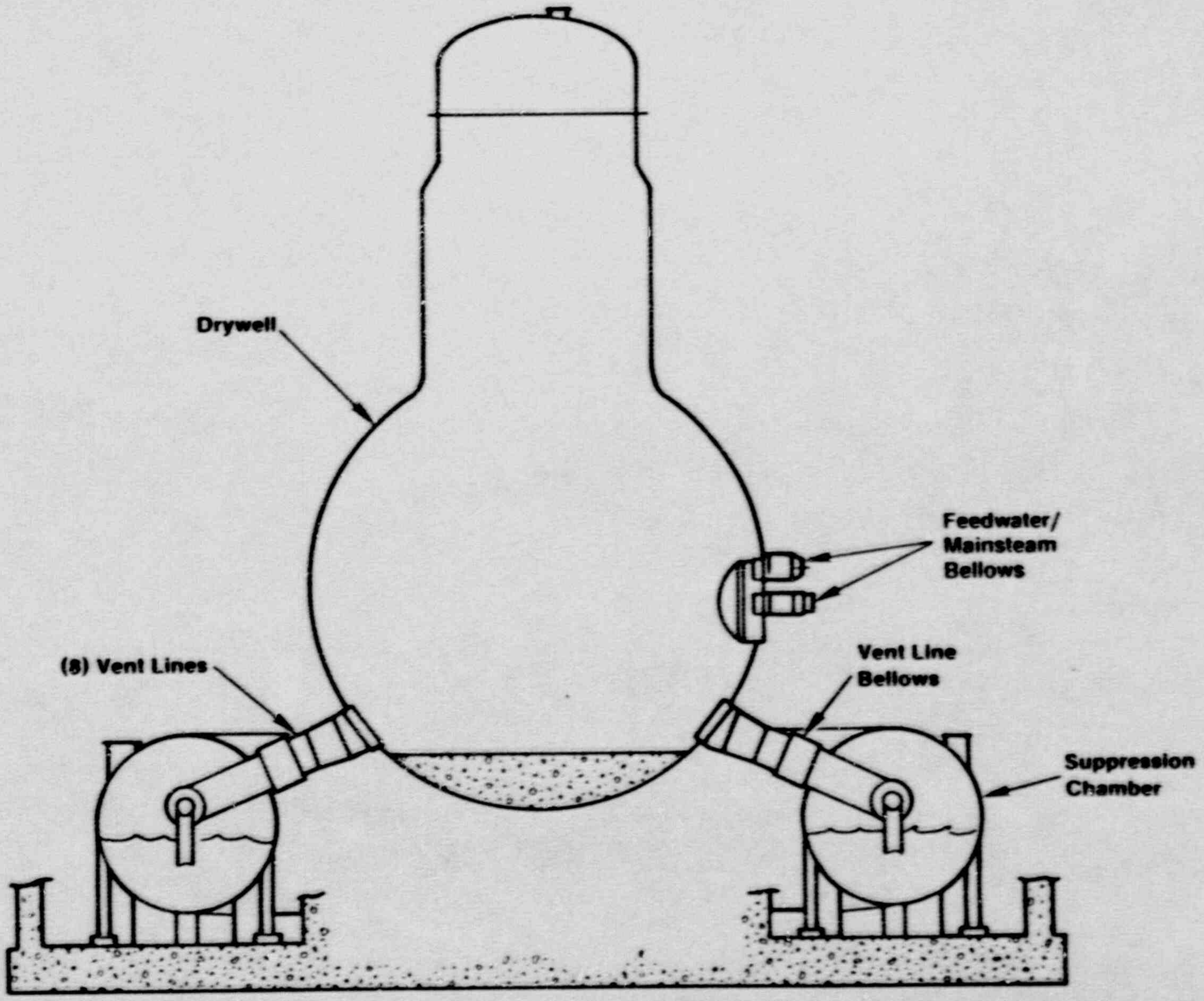
1) VENT LINE BELLOWS

   BWR MK-1 ONLY
   65-125" DIA.

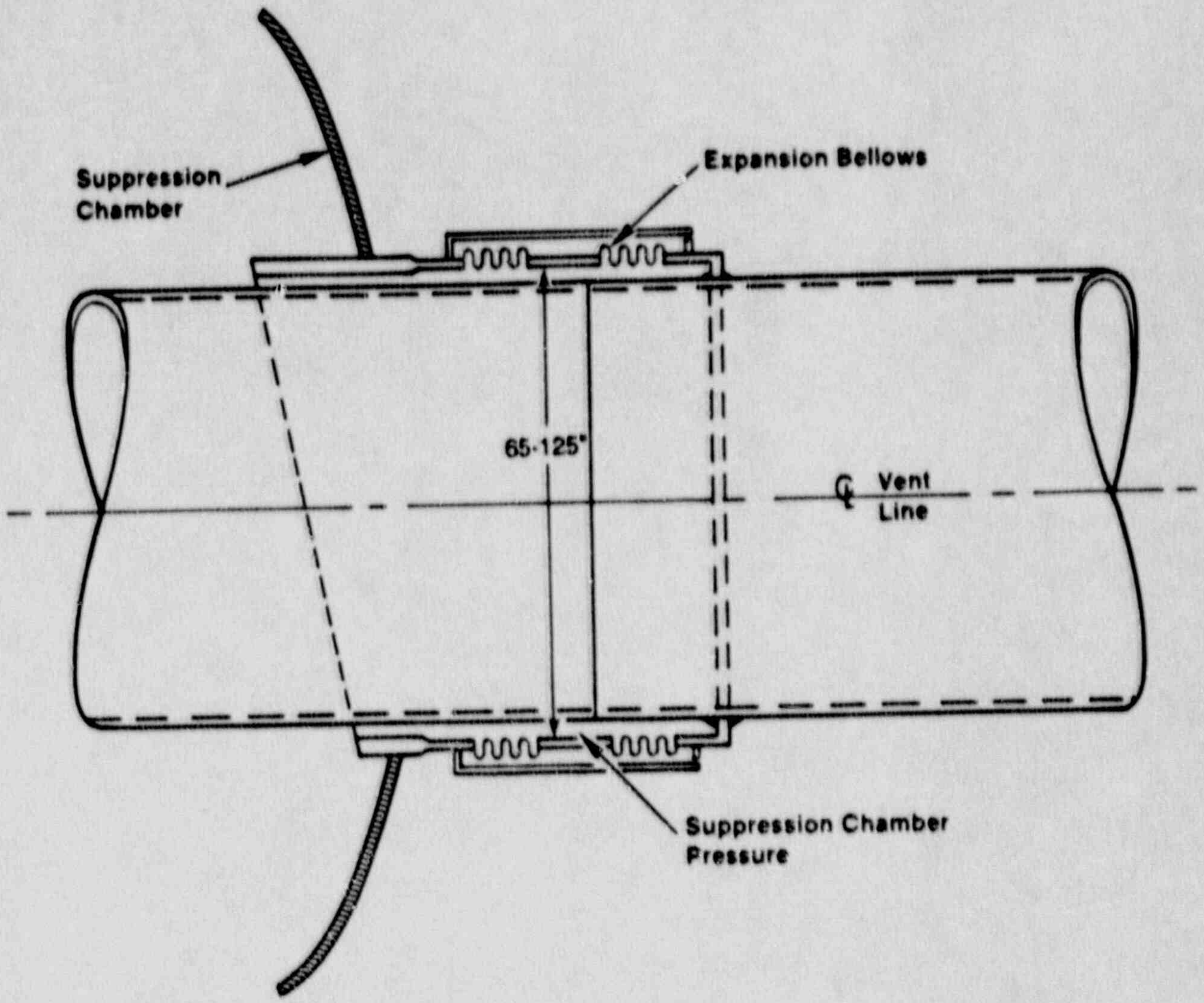
2) PROCESS PIPING BELLOWS

   BWR AND PWR CONTAINMENTS
   6-60" DIA.

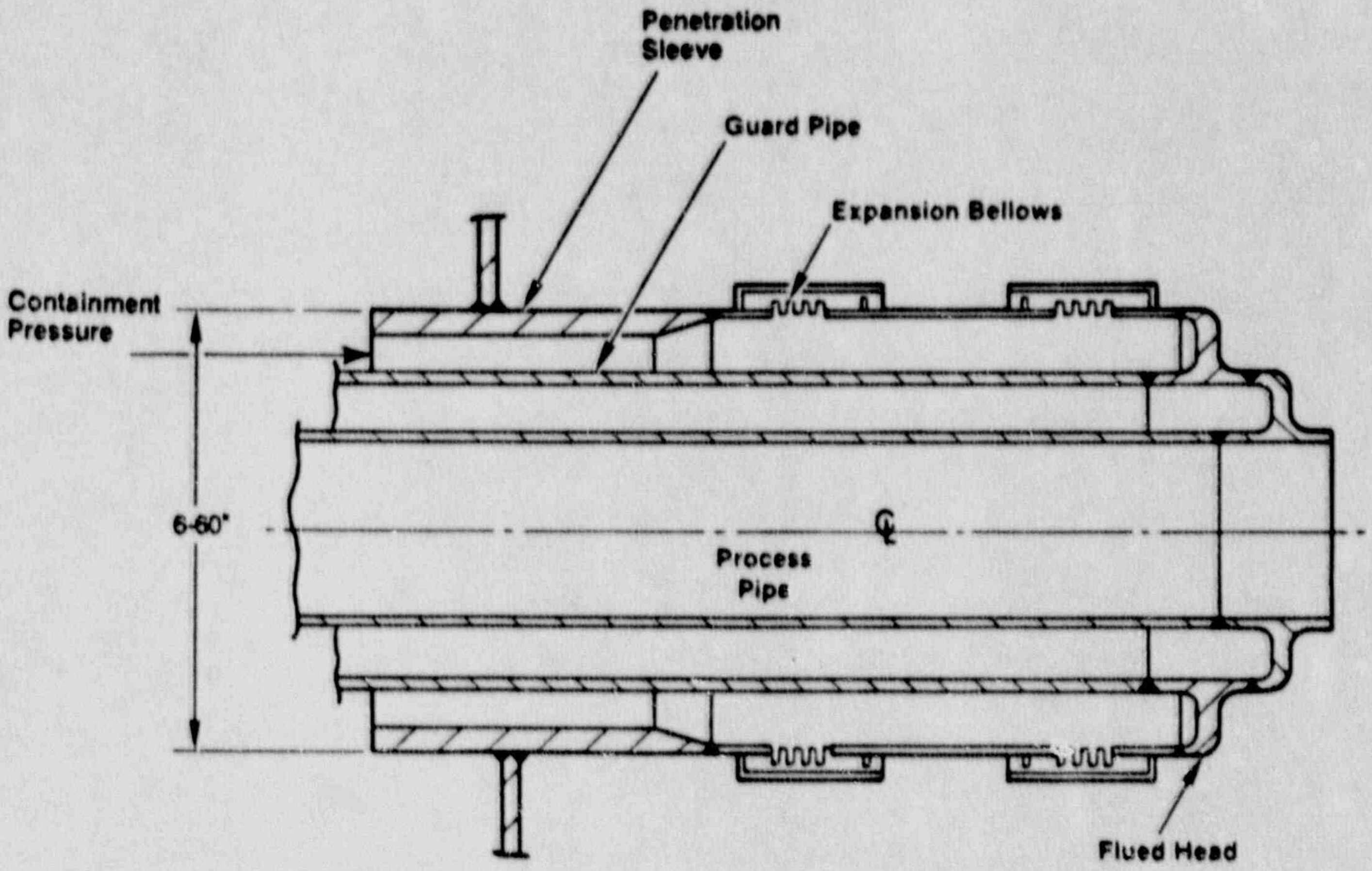
MATERIAL: TYPE 304 STAINLESS STEEL



BWR Mark-I Containment



Typical Vent Line Bellows



Typical Process Piping Bellows

CONTAINMENT BELLOWS DESIGN

DESIGN CONDITIONS - SUPPLIED BY A/E FOR CONTAINMENT

- Internal Pressure
- External Pressure
- Axial Deflection
- Lateral Deflection
- Rotation Due to Bending
- Rotation Due to Torsion (in a few cases)

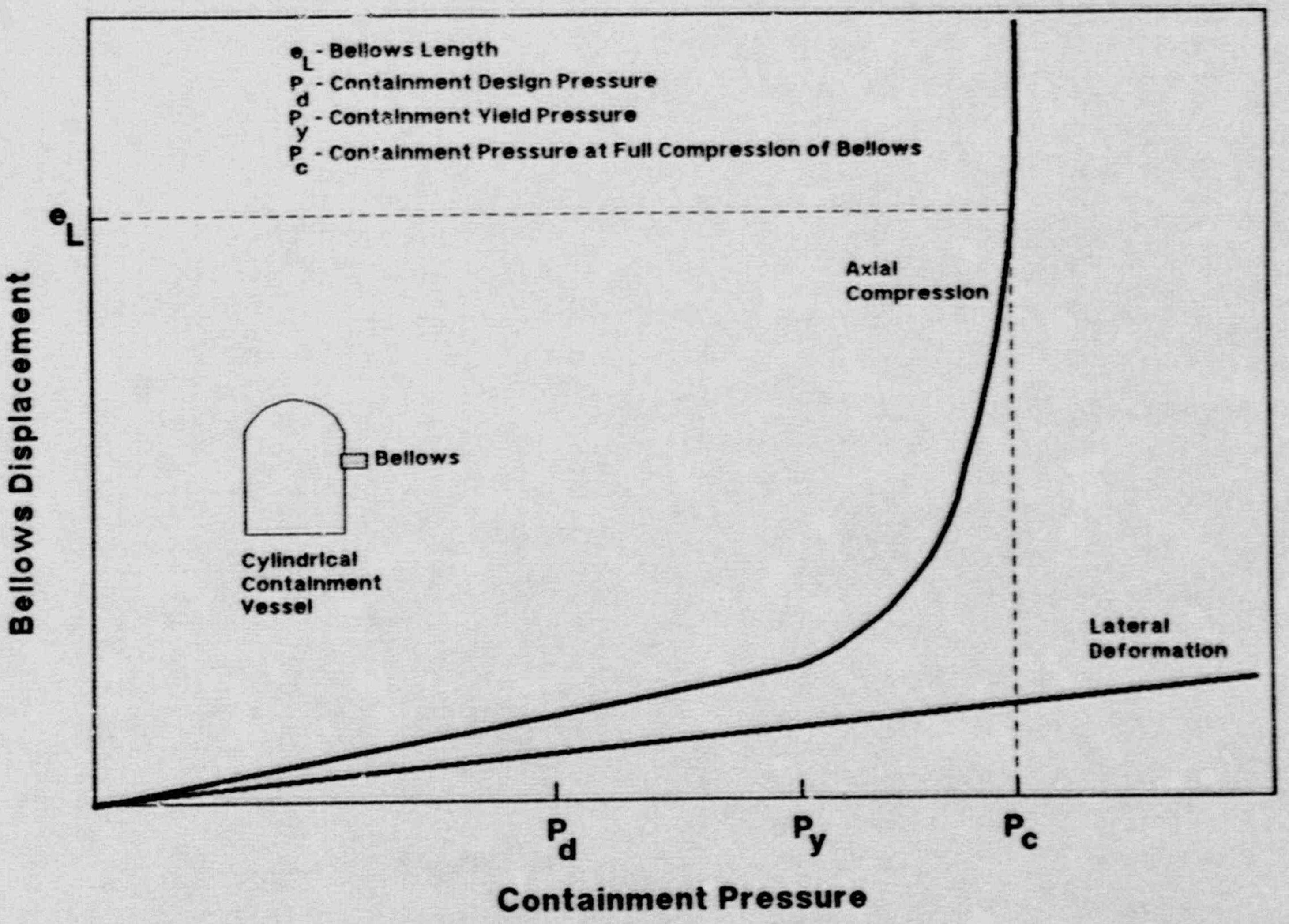
Based on a worst case combination of normal operating plus SSE plus LOCA conditions.

OBJECTIVES OF CONTAINMENT BELLOWS PROGRAM

1) TO DETERMINE IF CONTAINMENT PENETRATION BELLOWS ARE A POSSIBLE MODE OF FAILURE IN THE EVENT OF A SEVERE ACCIDENT.

2) IF SO, TO DEVELOP METHODS TO ESTIMATE THE SEVERE ACCIDENT CONDITIONS THAT WOULD LIKELY CAUSE A BELLOWS FAILURE.

   - LITERATURE SEARCH OF ANALYTICAL AND EXPERIMENTAL PROGRAMS
   - FINITE ELEMENT ANALYSIS
   - IF NECESSARY, CONDUCT ADDITIONAL TESTING



Typical Bellows Loadings vs. Containment Pressure

TWO IMPORTANT QUESTIONS:

1) HOW DOES P_c COMPARE TO THE PRESSURE ASSOCIATED WITH OTHER FAILURE MODES?

2) WILL BELLOWS REMAIN LEAKTIGHT UP TO P_c?

PRELIMINARY STUDY

- CONDUCTED AN EXTENSIVE, WORLD-WIDE SEARCH FOR APPLICABLE BELLOWS TEST DATA AND ANALYTICAL METHODS
- EXPERTS FROM THE U.S., JAPAN, GERMANY, FRANCE, AND ENGLAND WERE CONSULTED
- NO APPLICABLE TEST DATA IS AVAILABLE

- ALSO, FINITE ELEMENT ANALYSES HAVE BEEN CONDUCTED TO ESTIMATE THE ULTIMATE PRESSURE AND DEFORMATION CAPACITY OF A PROCESS PIPING BELLOWS
- BECAUSE OF THE LARGE DEFORMATIONS APPLIED TO BELLOWS DURING A SEVERE ACCIDENT, THE ANALYSES COULD NOT BE CONTINUED UNTIL BELLOWS FAILURE.

CONCLUSIONS FROM PRELIMINARY STUDY

- CONTAINMENT PENETRATION BELLOWS CAN NOT BE ELIMINATED AS A POSSIBLE MODE OF FAILURE DURING A SEVERE ACCIDENT.

- SEVERE ACCIDENT TESTING OF CONTAINMENT BELLOWS IS ESSENTIAL.

__REASONS:__

- EXISTING ANALYTICAL METHODS TO ESTIMATE SEVERE ACCIDENT CAPACITY OF BELLOWS ARE INADEQUATE

- NO AVAILABLE TEST DATA TO 'PROVE' THAT CONTAINMENT BELLOWS WILL REMAIN LEAKTIGHT DURING A SEVERE ACCIDENT

FUTURE ACTIVITIES

1) CONDUCT SEVERE ACCIDENT TESTING OF REPRESENTATIVE CONTAINMENT BELLOWS

2) BASED ON TEST RESULTS, DEVELOP METHODS TO PREDICT BELLOWS ULTIMATE PRESSURE AND DEFORMATION CAPACITY

TENTATIVE TEST OUTLINE

TWO LOAD CONDITIONS:

A) Simultaneous application of axial compression, lateral deformation, and internal pressure

B) Simultaneous application of axial elongation, lateral deformation, and external pressure

TYPES OF PLANNED TESTS:

1) Typical universal process piping bellows - Load Case A

2) Typical single process piping bellows - Load Case A

3) Typical scaled-down vent line bellows - Load Case A

4) Typical scaled-down vent line bellows - Load Case B

IMPACT OF STRUCTURAL RESPONSE WITH REDUCED STIFFNESS ON PLANT RISK

METHODOLOGY AND

APPLICATION TO THE PEACH BOTTOM BWR

BY

MICHAEL P. BOHN

SANDIA NATIONAL LABORATORIES

PRESENTED TO

ACRS SUBCOMMITTEE MEETING

ALBUQUERQUE, NM

JANUARY 25, 1990

PROGRAM OBJECTIVES

TO ASSESS THE IMPACT OF DECREASED NATURAL FREQUENCIES OF CONCRETE SHEAR WALL STRUCTURES ON

- DETERMINISTIC DESIGN CALCULATIONS
- OVERALL SEISMIC PLANT RISK

LASL DATA SHOW SIGNIFICANT DECREASES IN FIRST MODE MODEL FREQUENCIES

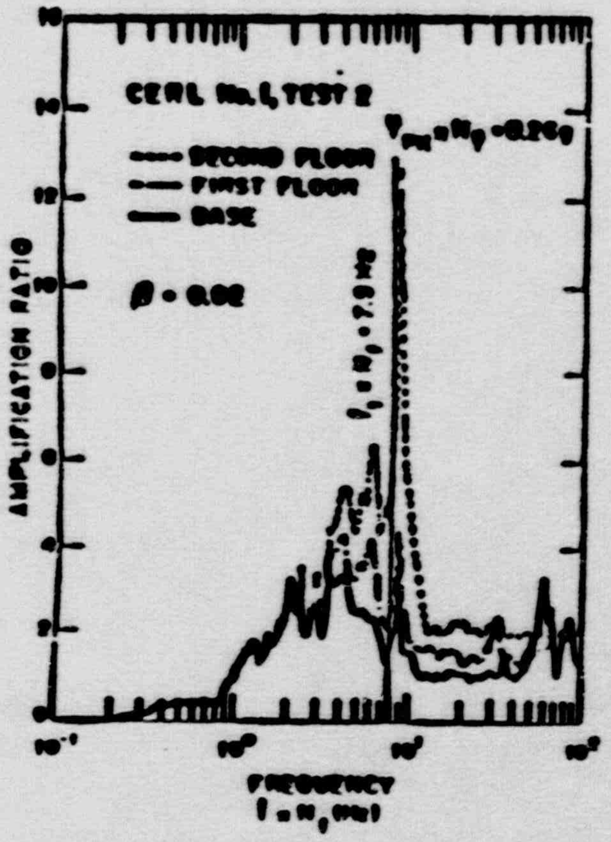


Figure 2-8 Floor Response Spectra: $Y_{pk}/N_y = 0.26$ g

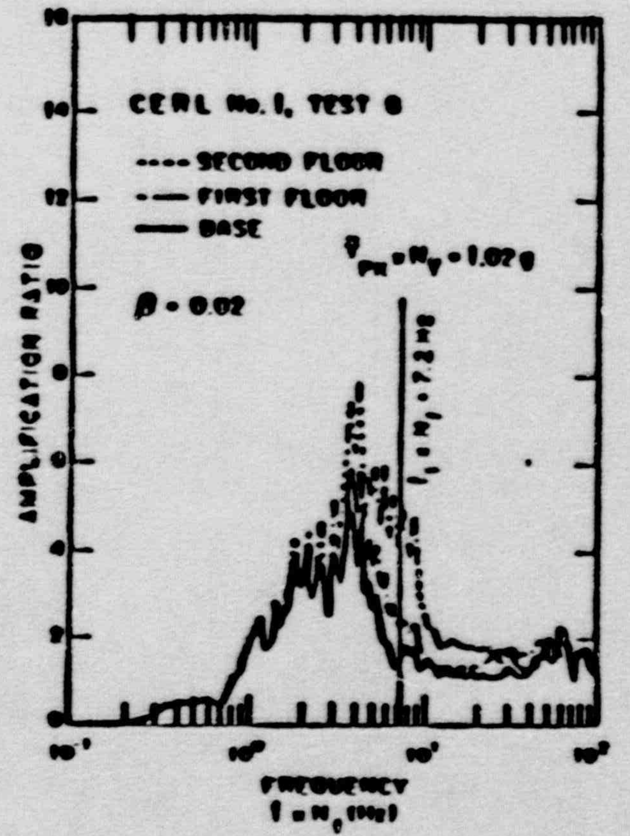


Figure 2-9 Floor Response Spectra: $Y_{pk}/N_y = 1.02$ g.

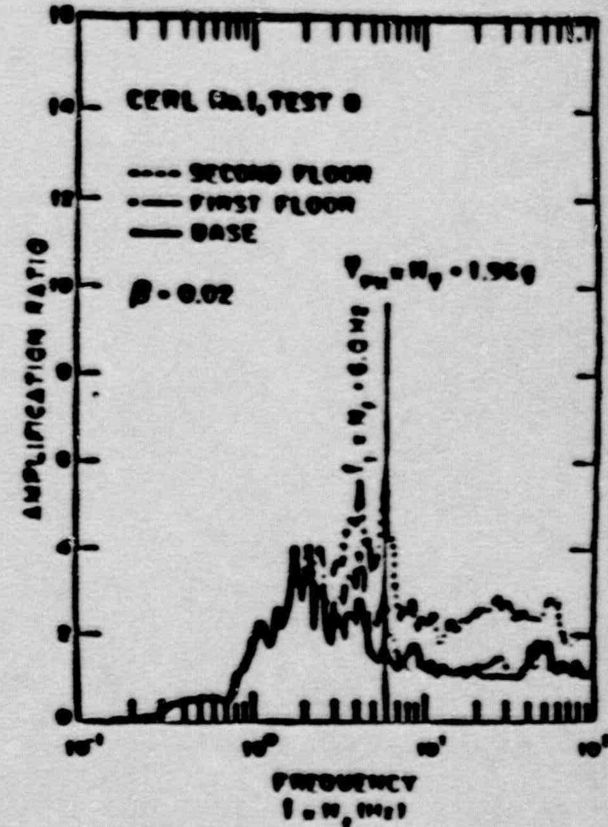


Figure 2-10 Floor Response Spectra: $Y_{pk}/N_y = 1.96$ q.

DECREASED (FIXED-BASE) STRUCTURE FREQUENCIES AFFECT

- OVERALL BUILDING RESPONSE TO EARTHQUAKES THROUGH SSI
- WALL SHEAR AND MOMENT LOADS
- FLOOR SLAB ACCELERATIONS AND SPECTRA
- SPECTRAL ACCELERATIONS EXPERIENCED BY COMPONENTS

Initial Stiffness

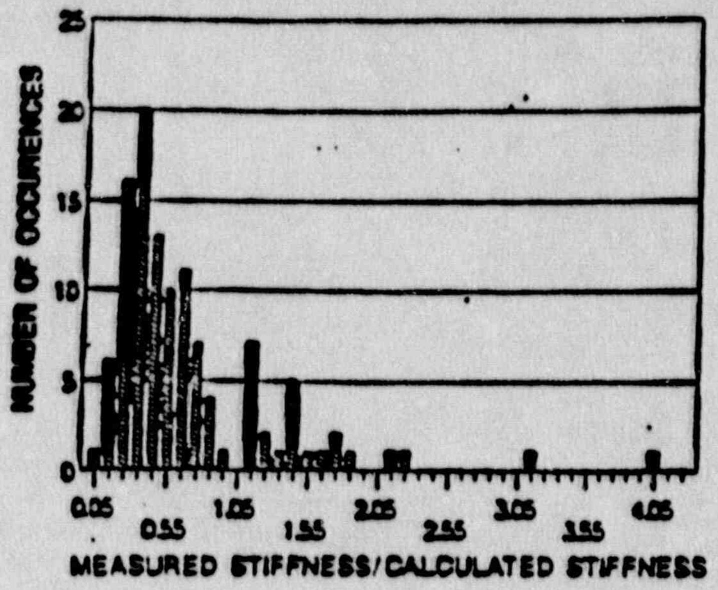


Figure 4-1 Histogram of Measured Stiffness/Calculated Stiffness

(Data from Dr. Mete Sozen, University of Illinois in draft report of ASCE Working Group on Stiffness of Concrete Shear Wall Structures)

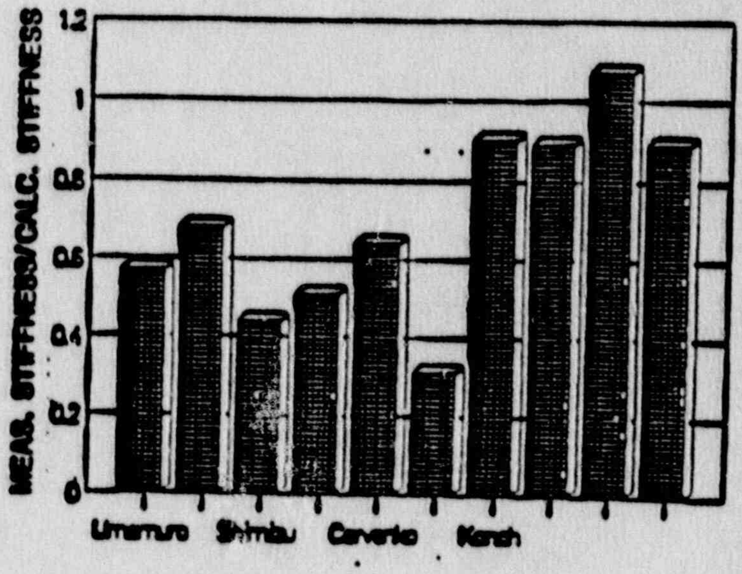


Figure 4-2 Japanese Data on Initial Measured/Calculated Stiffness (from draft report of ASCE Working Group on Stiffness of Concrete Shear Wall Structures)

PROBABILISTIC IMPACT ASSESSMENT

GENERAL APPROACH TO ASSESSING PROBABILISTIC IMPACT ON RISK

STEP 1 CHOOSE EXISTING SEISMIC PRA(S) AS BASE CASE(S).

STEP 2 RE-COMPUTE STRUCTURE RESPONSE WITH REDUCED FIXED-BASE NATURAL FREQUENCIES USING BEST-ESTIMATE SSI CALCULATIONS

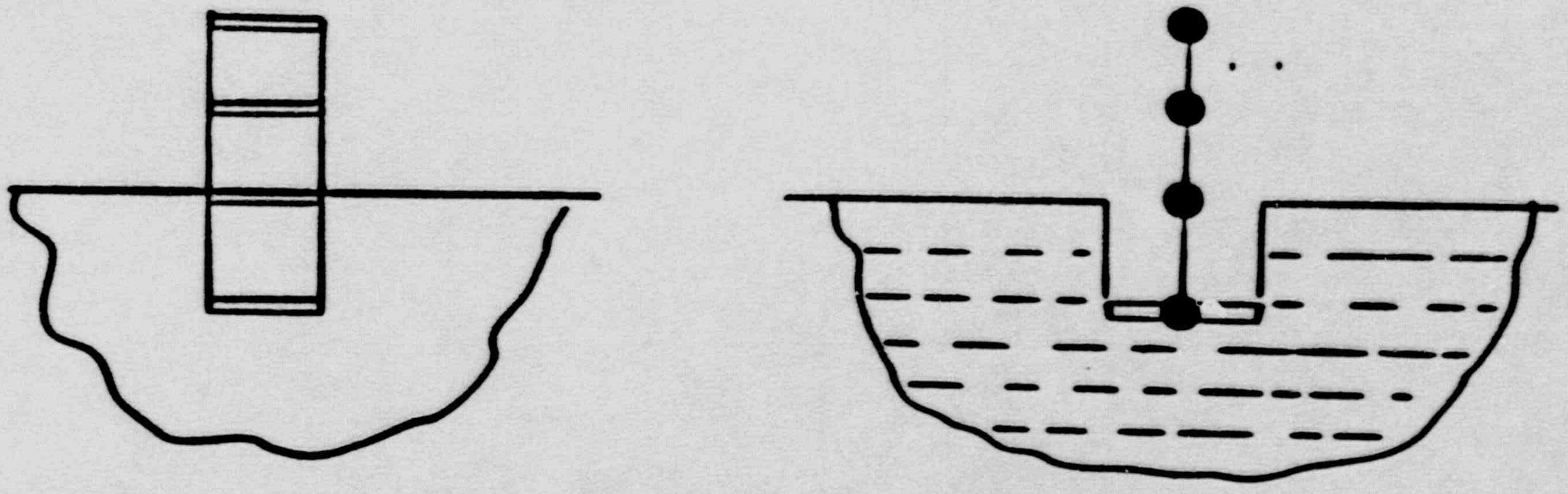
STEP 3 RE-EVALUATE CAPACITY OF STRUCTURES WITH NEW MEDIAN LOADS AND UNCERTAINTY DISTRIBUTIONS

STEP 4 RE-COMPUTE FLOOR SPECTRA FOR CRITICAL COMPONENTS (MEDIAN AND UNCERTAINTY DISTRIBUTIONS)

STEP 5 RE-EVALUATE CRITICAL COMPONENT FRAGILITIES

STEP 6 COMPUTE ACCIDENT SEQUENCE PROBABILITIES WITH NEW STRUCTURE AND COMPONENT FRAGILITIES AND COMPARE WITH ORIGINAL PRA RESULTS

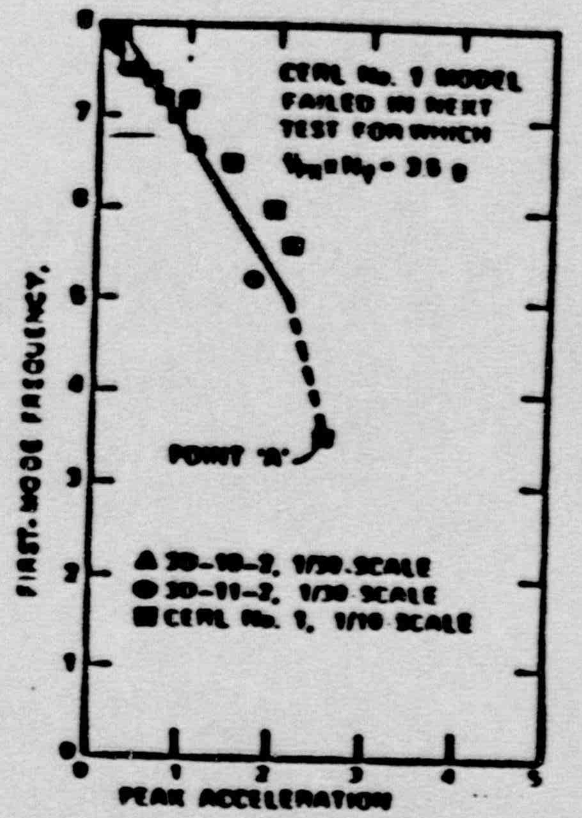
RE-COMPUTE STRUCTURE RESPONSE



- OBTAIN DESIGN FIXED BASE MODELS OF STRUCTURES
- COUPLE WITH BEST-ESTIMATE SOIL MODEL
- DEFINE 10 TIME HISTORIES WITH ENSEMBLE MEAN MATCHING ORIGINAL GROUND MOTION SPECTRA
- PERFORM 10 TIME HISTORY SSI CALCULATIONS INCLUDING VARIATIONS IN STRUCTURE AND SOIL PROPERTIES AND REDUCED BUILDING NATURAL FREQUENCIES

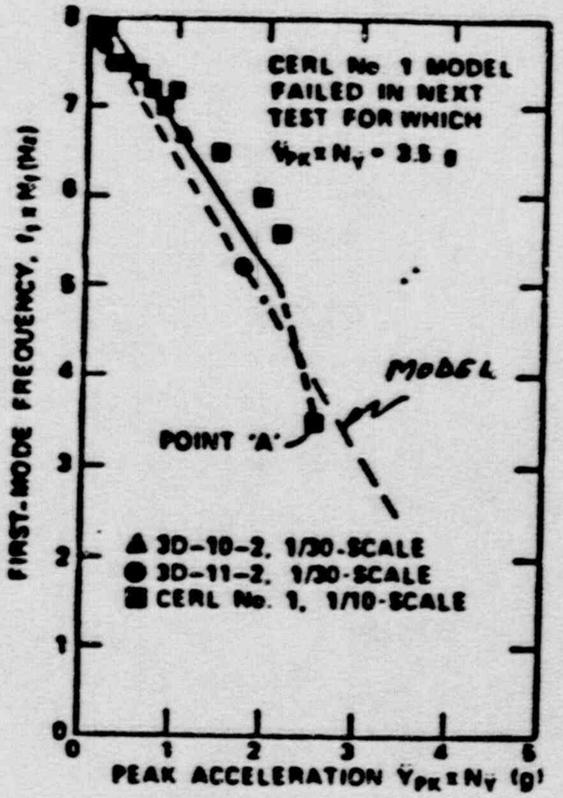
REDUCTION IN MEDIAN BUILDING NATURAL FREQUENCIES BASED ON LASL TESTS

- REDUCTION UP TO 50%
- REDUCTION A FUNCTION OF PGA
- MODEL FOR REDUCTION OF "HIGHER" NATURAL FREQUENCIES TO BE DETERMINED

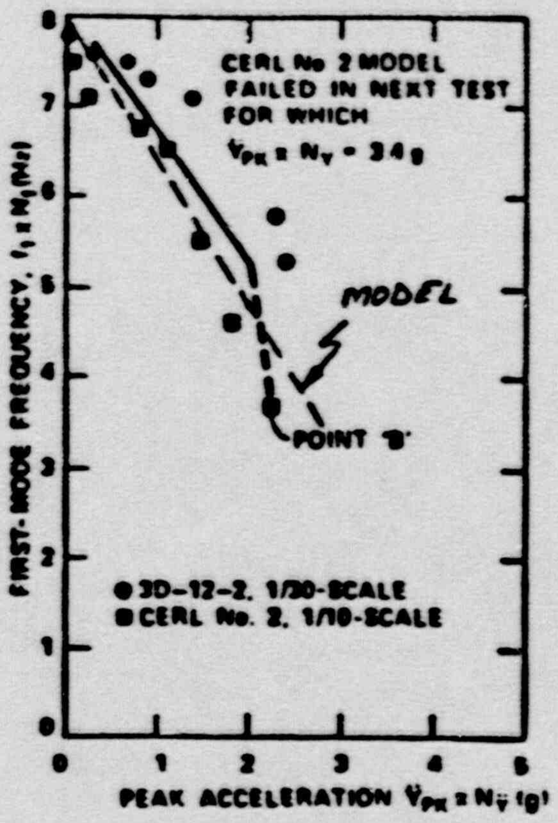


PRELIMINARY LINEAR FREQUENCY REDUCTION MODEL FIT MOST LASL DATA WELL

$$f_1 = 0.6 f_1^{nom} - 0.2 * pga$$



1983 TESTS



1984 TESTS

OTHER THAN BUILDING RESPONSE CHANGES, RE-EVALUATION OF RISK FOLLOWS ORIGINAL SEISMIC PRA

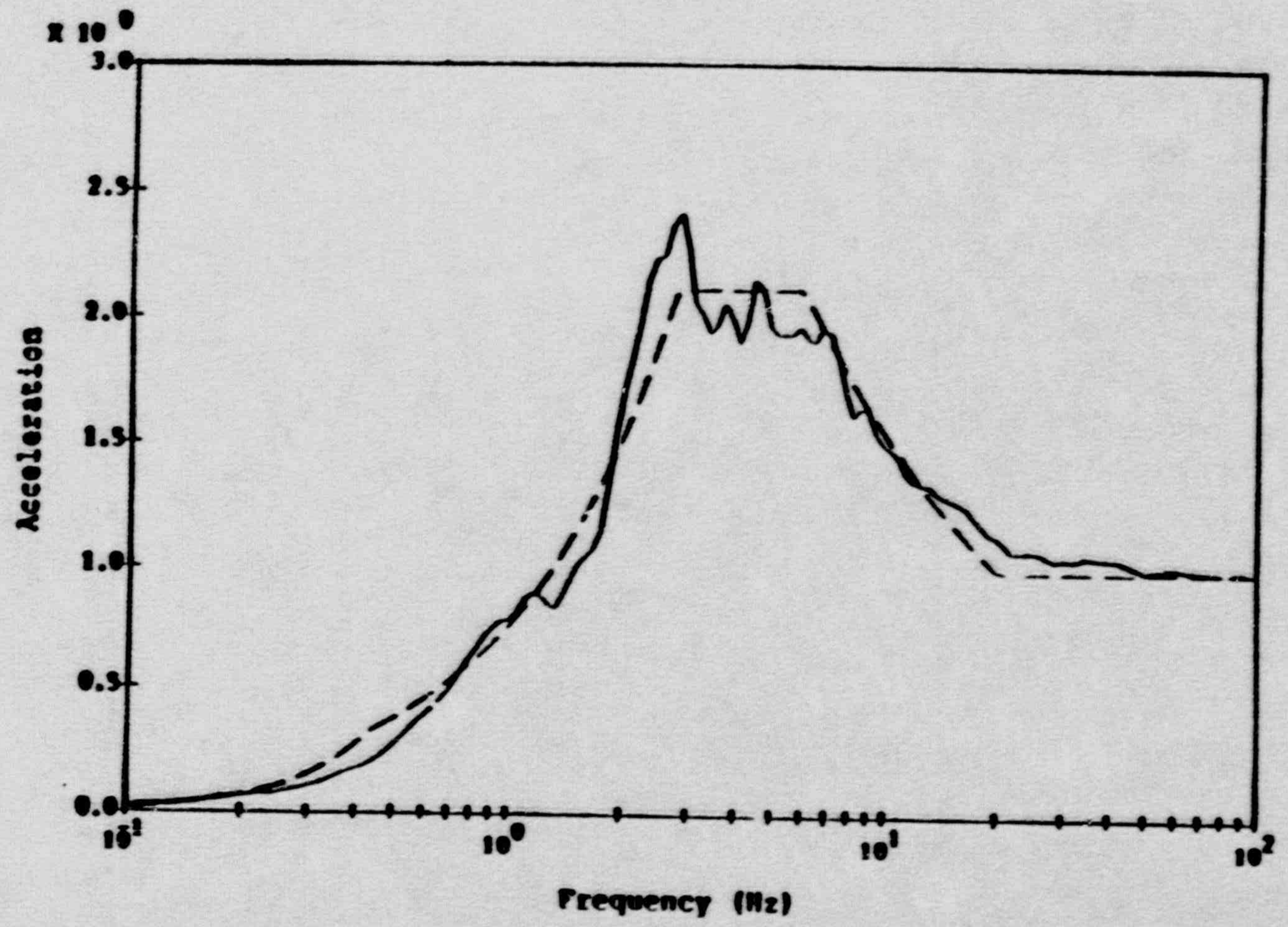
- SAME HAZARD CURVE(S) AND UNCERTAINTY
- SAME EVENT TREES AND FAULT TREES
- SAME RANDOM AND FRAGILITY CHARACTERIZATIONS FOR COMPONENTS
- BUILDING FRAGILITIES CHANGED DUE TO RESPONSE CHANGES
- FULL MONTE CARLO UNCERTAINTY ANALYSIS OF CORE DAMAGE FREQUENCY IS PERFORMED

PRELIMINARY APPLICATION TO PEACH BOTTOM

INITIAL EVALUATION OF EFFECT USED NUREG 1150 PEACH BOTTOM SEISMIC PRA AS BASE CASE

- ROCK SITE
- INPUT GROUND MOTION SPECTRA BASED ON 10 RECORDED EARTHQUAKE TIME HISTORIES
- HAZARD CURVE FROM LLNL EUS HAZARD PROGRAM
- BUILDING FRAGILITIES DEVELOPED FOR SITE
- COMPONENT FRAGILITIES BOTH GENERIC AND SITE-SPECIFIC

PEACH BOTTOM NUREG 1150 GROUND MOTION INPUT



Legend:

Median of 10 Comp. ————————

Rock 50% [1] — — — — — —

Notes:

All spectra calculated at 5% damping

Acceleration in units of g

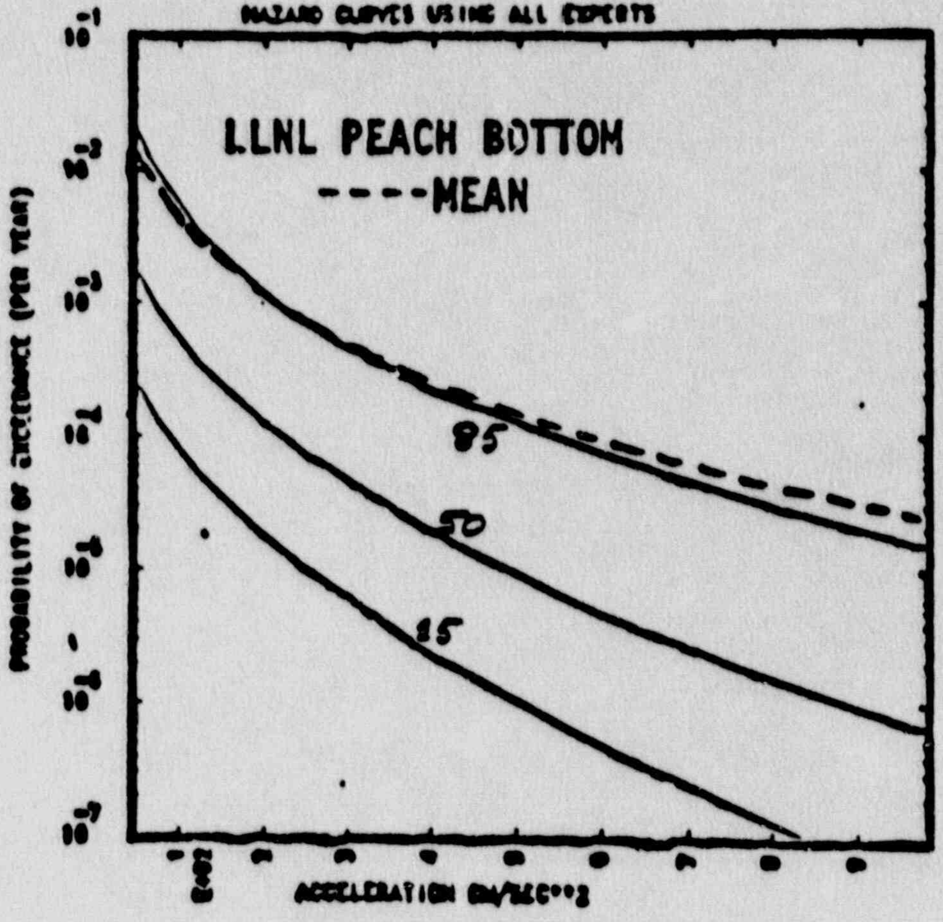


Figure 3.1. Peach Bottom Hazard Mean, Median, 85 Percent and 15 Percent Curves

PEACH BOTTOM NUCLEAR POWER STATION

SAFETY RELATED STRUCTURES

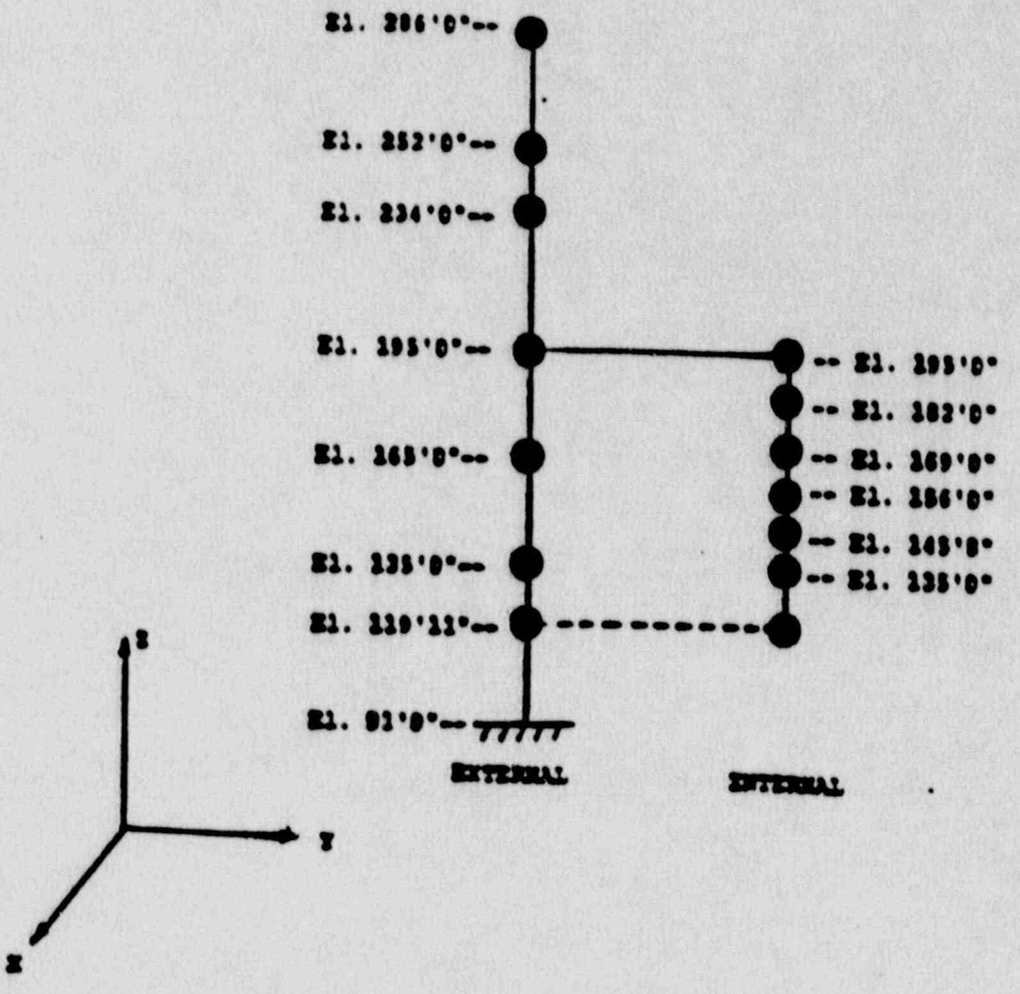
RADWASTE - TURBINE BUILDING
TURBINE BUILDING
CIRCULATING WATER PUMP HOUSE
EMERGENCY COOLING TOWER STRUCTURE
REACTOR BUILDING

CRITICAL COMPONENTS

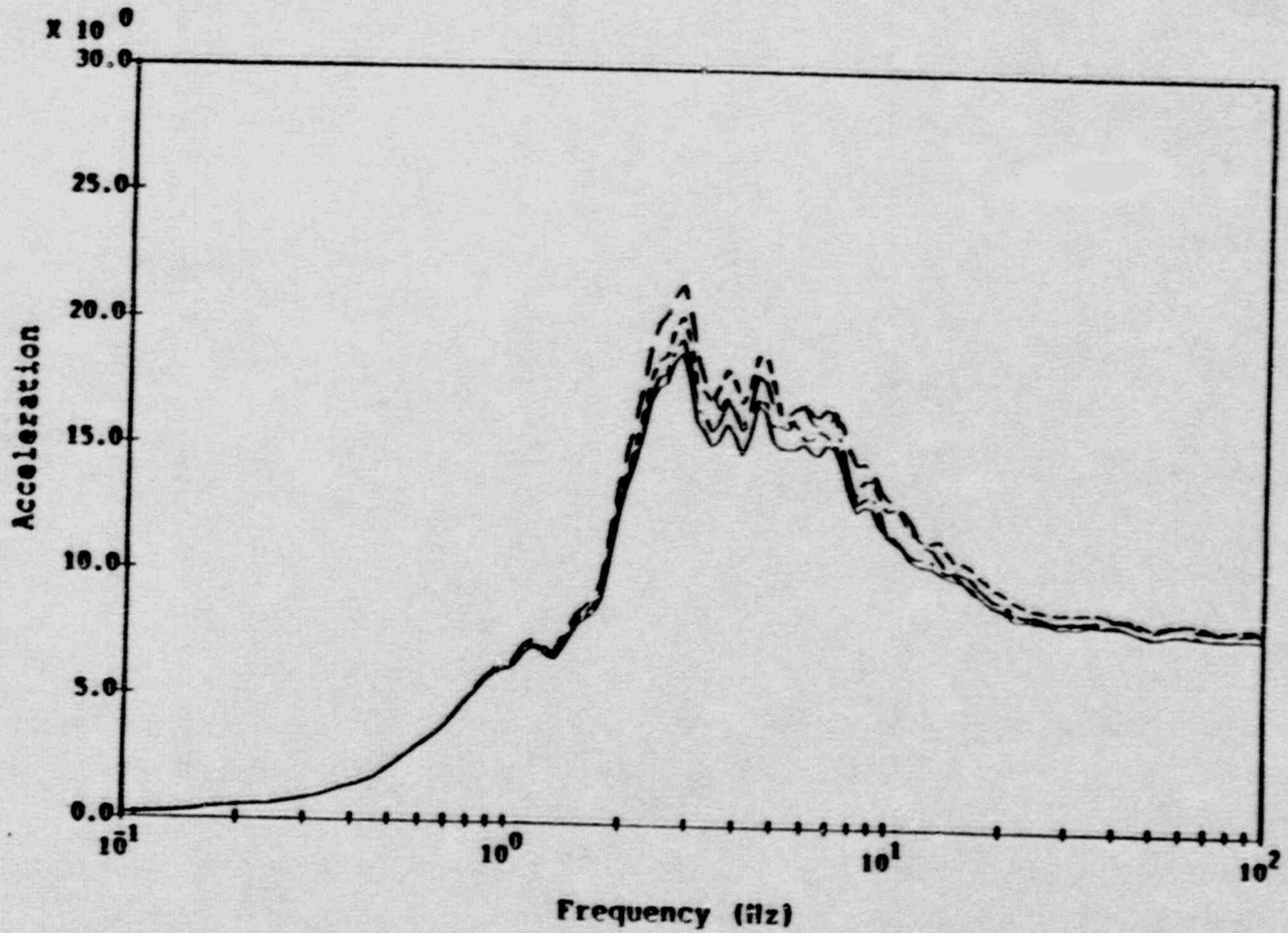
CERAMIC INSULATORS
ESW/ECW PUMPS
DIESEL GENERATOR DAY TANK
4KV BUSSES

REACTOR BUILDING MODEL NATURAL FREQUENCIES

Direction	Nominal		0.6K		0.2K	
	HZ	% Mass	HZ	% Mass	HZ	% Mass
N-S	7.06	69	4.84	69	3.18	68
E-W	7.63	71	5.24	72	3.45	72
N-S	20.4	16	13.99	17	9.28	12
E-W	22.74	16	15.58	16	10.26	15



REACTOR BUILDING SHOWED LITTLE EFFECT AT LOWER ELEVATIONS



Legend:

Free-field ————————

El. 119'-11"

.2*K — — — — —

.6*K - - - - - . - . -

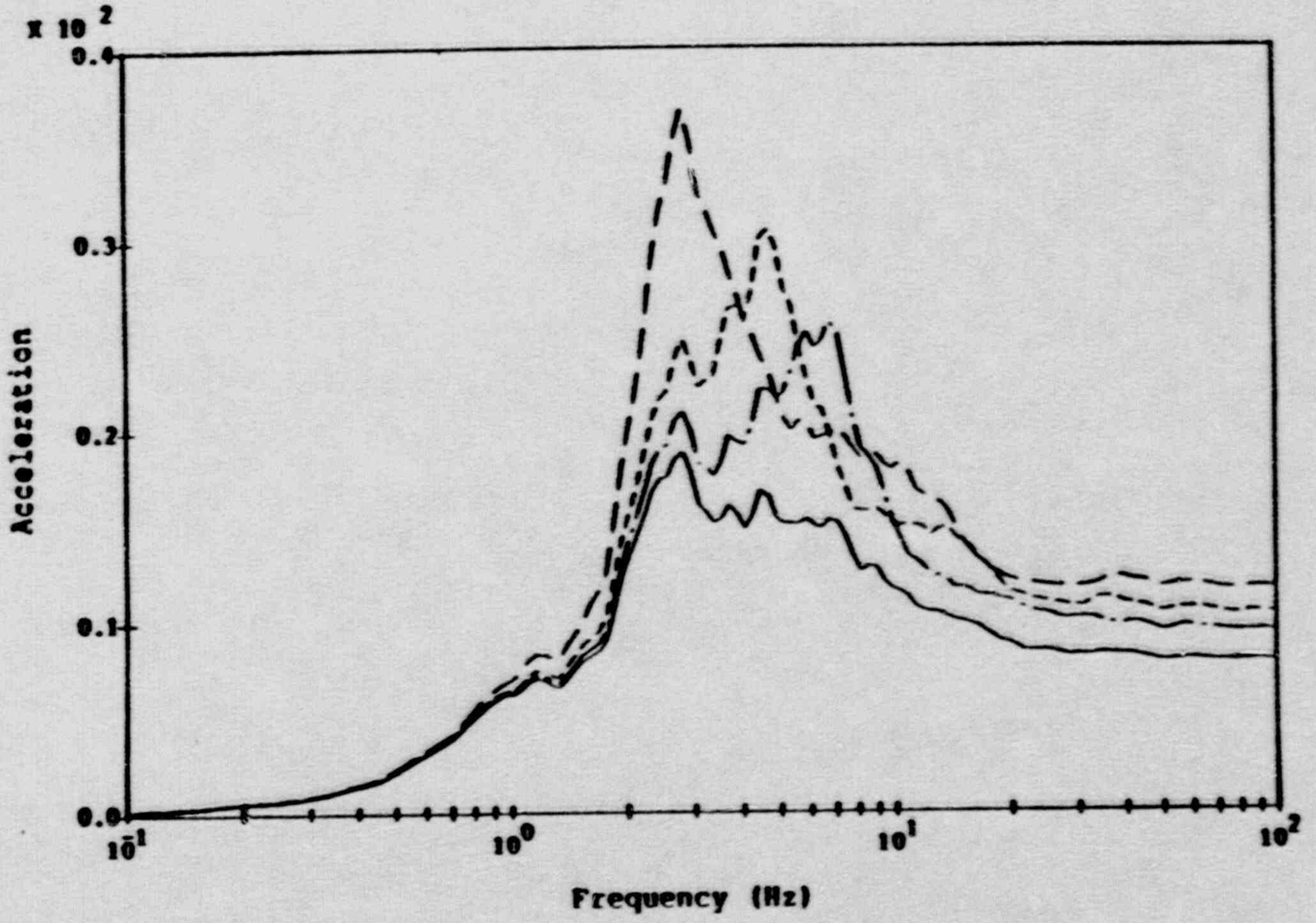
1.0*K ——— . ——— . .

Notes:

spectra calculated at 5% damping

accelerations in units of ft/s/s

AMPLIFICATION AND SHIFT INCREASED AT HIGHER REACTOR BUILDING ELEVATIONS



Legend:

Free-field

El. 165'-0"

.2*K

.6*K

1.0*K

Notes:

spectra calculated at 5% damping

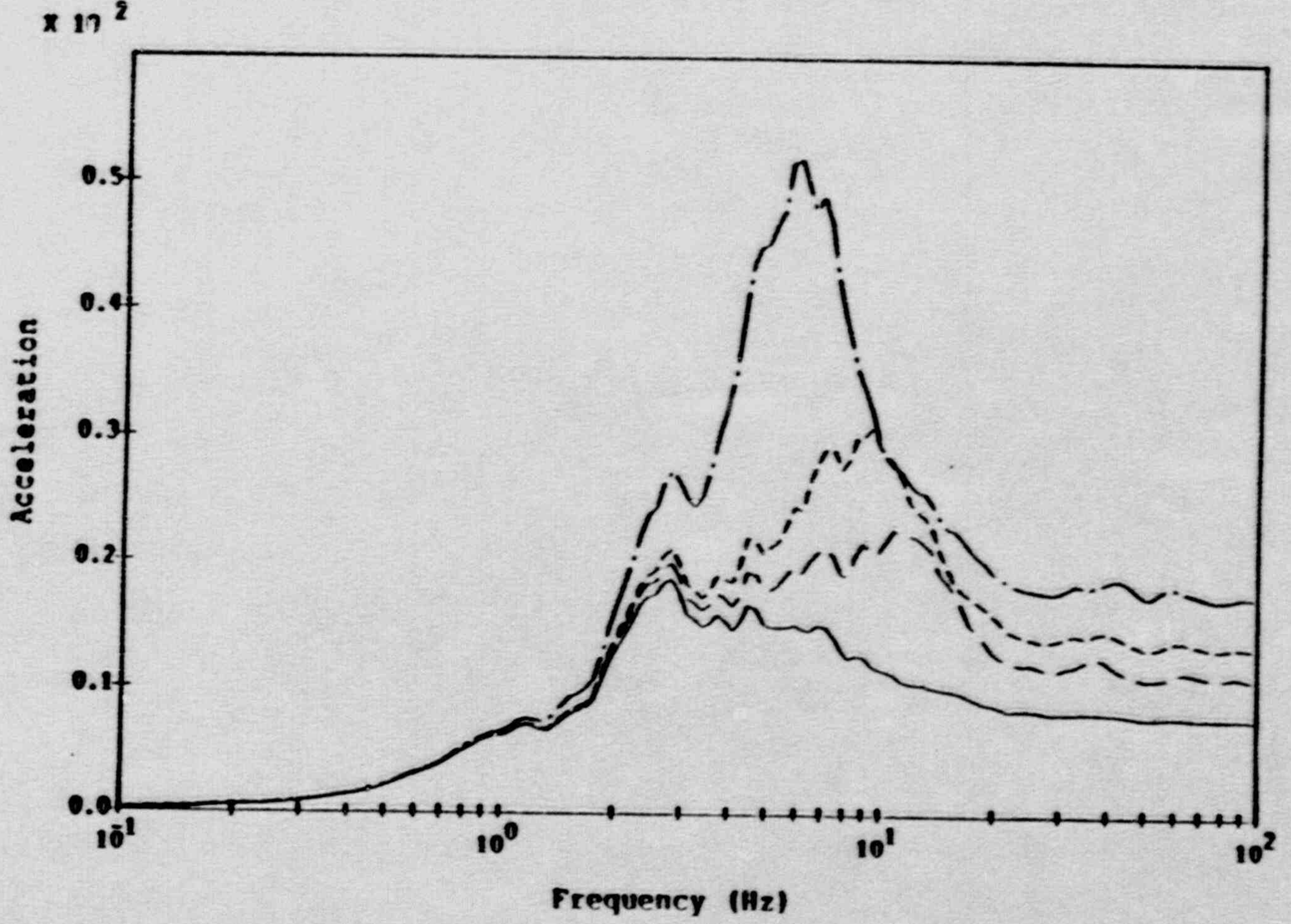
accelerations in units of ft/s/s

RESULTS OF ANALYSES SHOWED THE CRIB HOUSE AND EMERGENCY COOLING TOWER WERE MOST AFFECTED BY STIFFNESS REDUCTION

- THIS RESULTED FROM LOWERING INITIAL FREQUENCIES (10 - 14HZ) DOWN INTO AMPLIFIED ACCELERATION REGION OF INPUT GROUND MOTION SPECTRA

- THESE STRUCTURES PLAYED A CRITICAL ROLE IN FINAL PRA RESULTS

THE CIRCULATING WATER PUMP HOUSE (CRIB HOUSE) SHOWED SUBSTANTIAL AMPLIFICATION IN 5-10HZ RANGE



Legend:

Free Field ————————

1.0*K — — — — — —

0.6*K - - - - - - - - -

0.2*K

Notes:

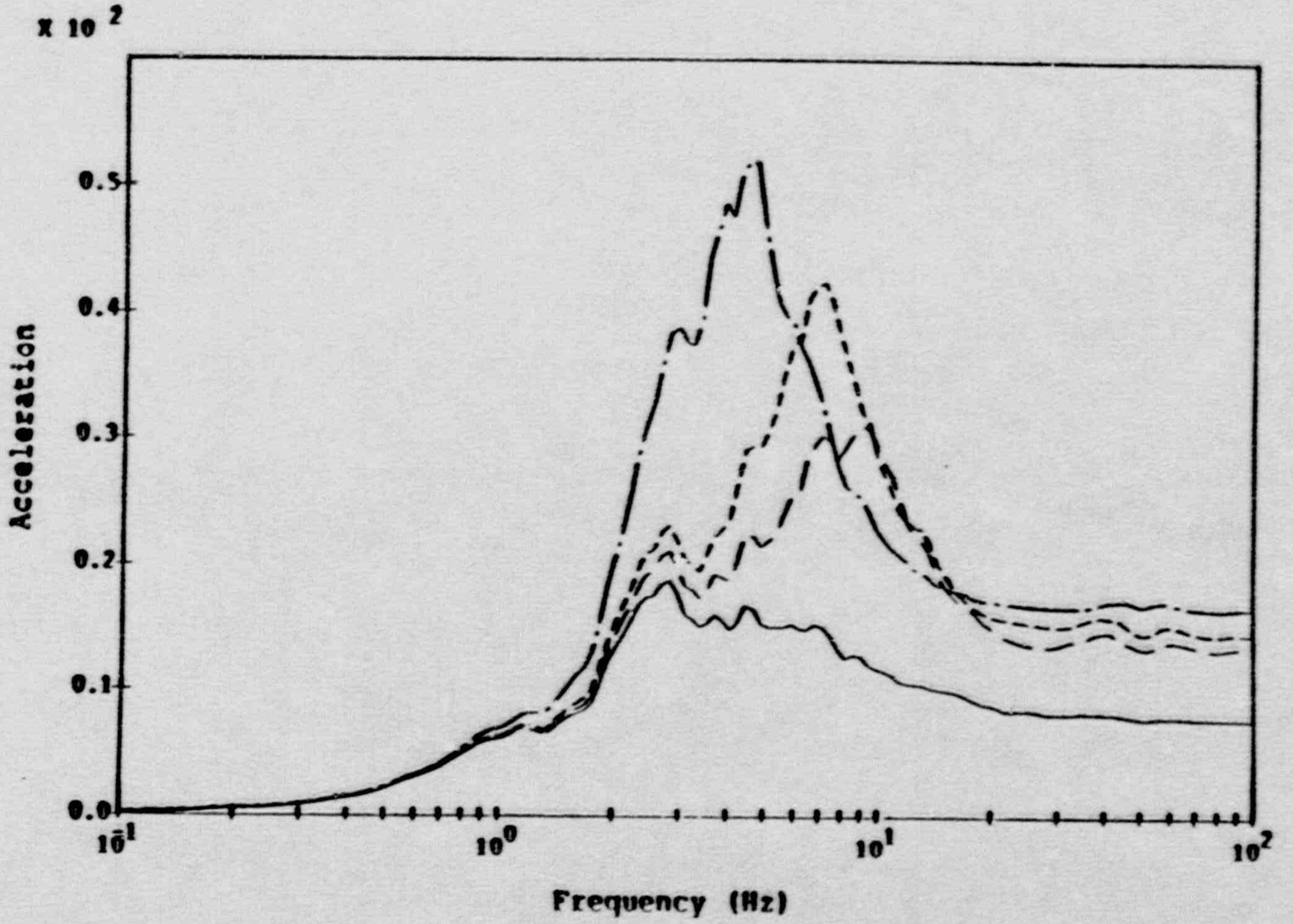
spectra calculated at 5% damping

accelerations in units of ft/s/s

elevation at 130'-6"

EMERGENCY COOLING TOWER STRUCTURE SHOWED SUBSTANTIAL AMPLIFICATION

IN 3-7HZ RANGE



Legend:

Free Field	———————
1.0*K	— — — — —
0.6*K	- - - - - - - - -
0.2*K	—·—·—·—

Notes:

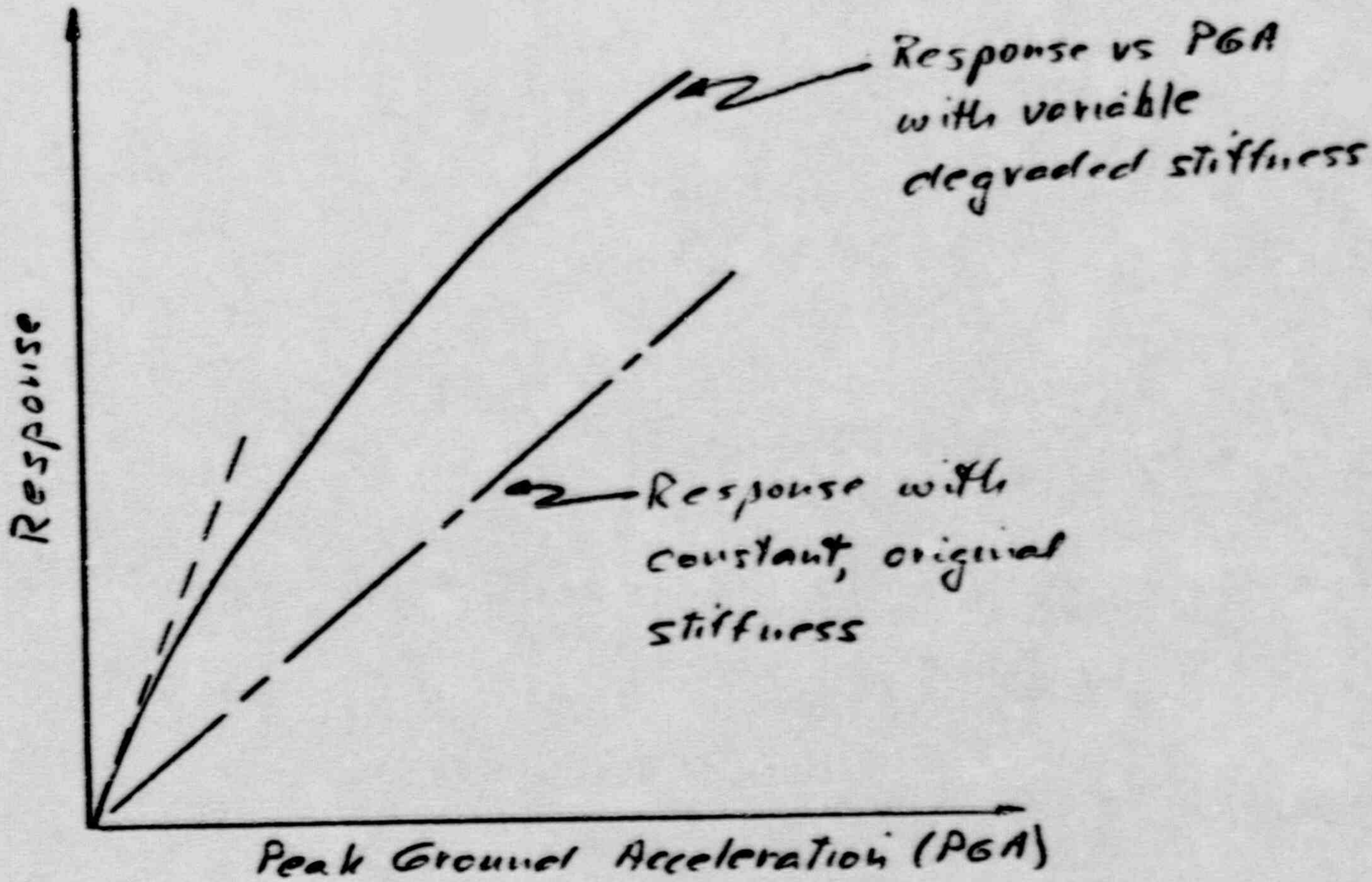
spectra calculated at 5% damping
accelerations in units of ft/s/s
elevation at 168'-0"

AMPLIFICATION OVER PGA FOR ALL RESPONSES

		Median Response PGA				
		$1.0k_o$	$0.6k_o$	$0.2k_o$	Loc/Elev.	Freq.
	1	1.00	same*	same	Free-field	ZPA
	2	2.08				2-5
	3	1.90				5
	4	1.78				5-10
	5	1.90				7
	6	1.20	same	same	RWT 135	ZPA
	7	2.50	2.98	2.33		5-10
	8	1.40	1.70	1.70	150	ZPA
	9	3.00	3.37	3.37		5-10
	10	1.60	1.90	1.90	165	ZPA
	11	3.30	4.15	3.89		5-10
	12	1.00	same	same	RB 91	ZPA
	13	1.80				5-10
	14	1.80				7
	15	1.80				5
	16	1.10			116	ZPA
	17	2.10	2.14	2.10		7
	18	1.10	same	same	135	ZPA
	19	2.10	2.53	2.01		7
	20	1.30	same	same	165	ZPA
	21	3.00	2.59	2.59		7
	22	1.00	same	seme	DG 127	ZPA
	23	1.80				5-10
	24	1.80				5
	25	1.00			TB 116	ZPA
	26	1.90				7
	27	1.30	1.70	2.10	CWPS 114	ZPA
ECW Pumps →	28	2.40	3.50	5.20		7
	29	1.40	1.40	1.50	ECT 153	ZPA
ESW Pump →	30	2.60	4.00	3.50		7
	31	2.60	4.00	3.50		5

*same indicates that all three cases have the same median response

AMPLIFICATION RATIOS AT 0.6K AND 0.2K USED TO BUILD DEGRADATION MODEL AS A FUNCTION OF PGA



ACCIDENT SEQUENCE AND TOTAL CORE DAMAGE MEAN FREQUENCIES

	ORIGINAL	WITH STIFFNESS REDUCTION
Accident Sequence	Mean Frequency (per year)	Mean Frequency (per year)
RVR-1	8.92E-6	8.86E-06
ALOCA-17	1.23E-7	4.26E-07
ALOCA-30	1.84E-5	2.29E-05
S_1LOCA-25	2.02E-8	2.08E-08
S_1LOCA-70	6.67E-6	6.50E-06
S_1LOCA-80	6.72E-7	5.35E-07
S_2LOCA-21	2.86E-7	3.10E-07
S_2LOCA-42	1.20E-6	1.05E-06
RVT-1	2.76E-6	2.91E-06
RVT-2	2.94E-7	3.10E-07
RVT-3	6.26E-9	6.63E-09
RVT-4	6.26E-10	6.63E-10
T1-25	2.98E-7	2.64E-07
T1-32	1.18E-10	7.92E-11
T1-33	3.69E-5	7.97E-05 ←
T1-36 to S2-41	2.86E-8	2.35E-08
T1-36 to S2-42	1.11E-11	7.54E-12
T1-40 to S1-70	1.27E-10	9.64E-11
T1-40 to S1-80	5.67E-13	4.03E-13
T1-43 to ALOCA-30	2.53E-7	4.06E-07
T3A-1 to T2-1-29	1.45E-9	1.00E-09
T3A-1 to T2-1-36	4.48E-10	2.96E-10
	TOTAL 7.66E-5	TOTAL 1.24E-04

INCREASE IN P(CORE DAMAGE) DUE TO INCREASES IN FLOOR ACCELERATION (AT 7HZ) IN CRIB HOUSE AND ECT

- THESE INCREASE PROBABILITY OF FAILURE OF ECW AND ESW PUMPS
- IF ECW/ESW PUMPS FAIL AND LOSP OCCURS, THEN GET STATION BLACKOUT TRANSIENT (T_1-33) DUE TO LOSS OF COOLING TO DIESEL GENERATORS

DETERMINISTIC IMPACT ASSESSMENT

GENERAL APPROACH TO ASSESSING DETERMINISTIC IMPACT

STEP 1 FOR THE SAME PLANT(S), IDENTIFY DESIGN TIME HISTORIES, DAMPING AND DESIGN RULES AND PARAMETERS.

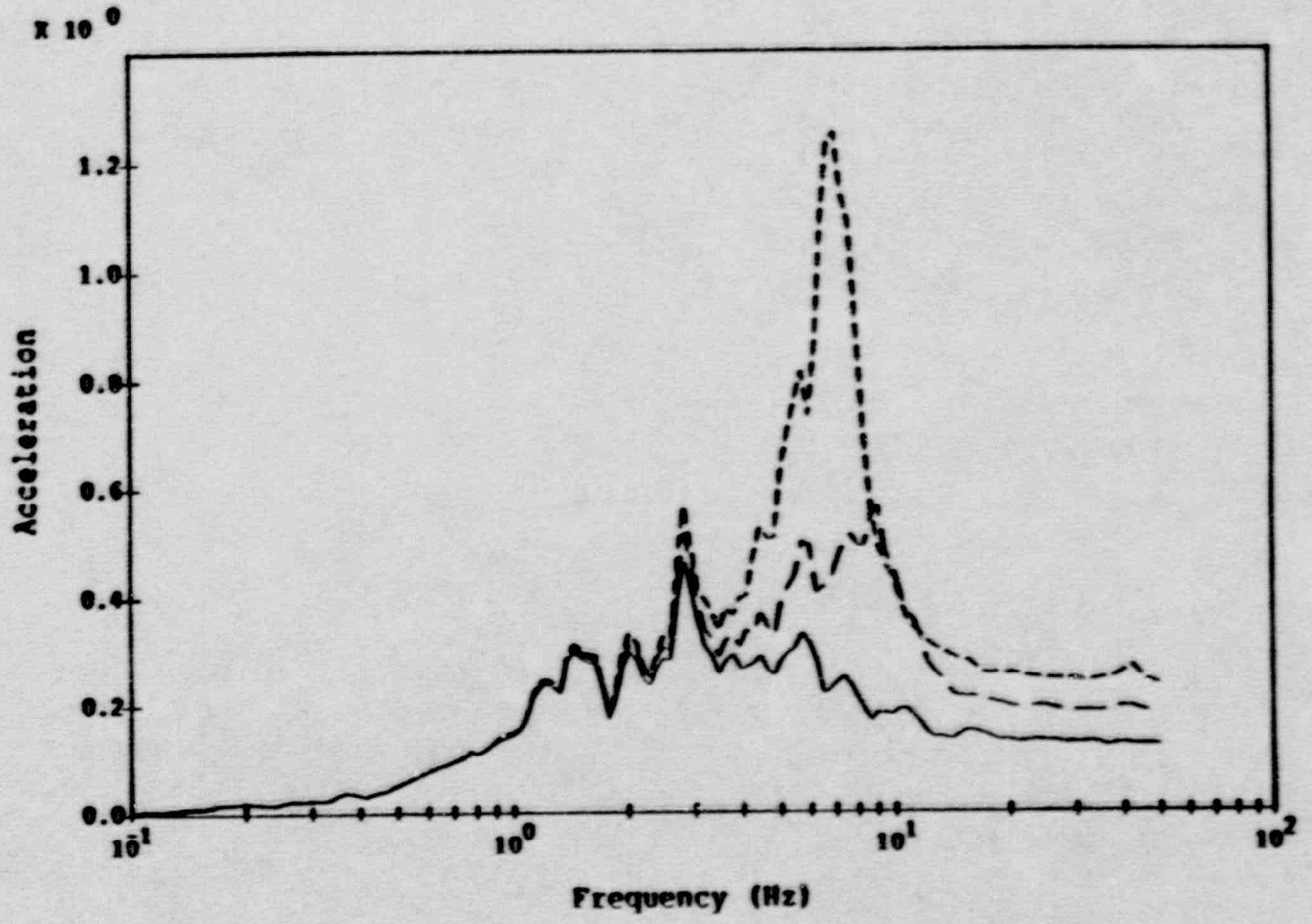
STEP 2 COMPUTE A "DESIGN-LIKE" CALCULATION OF FLOOR SLAB ACCELERATIONS, FLOOR SPECTRA AND NET WALL LOADS WITH NO STIFFNESS REDUCTION. (SSE LEVEL ONLY).

STEP 3 REPEAT STEP 2 WITH REDUCED WALL STIFFNESSES.

STIFFNESS DEGRADATION RESULTED IN INCREASES IN DESIGN - TYPE WALL LOADS

STRUCTURE	NET SHEAR	NET MOMENT
REACTOR BLDG	28%	25%
EMERGENCY COOLING TOWER	16%	28%
DIESEL GENERATOR BLDG	19%	23%
CRIBHOUSE	30%	30%
RADWASTE/TURBINE BLDG	22%	30%

DESIGN - TYPE SPECTRA, WITH AND WITHOUT STIFFNESS DEGRADATION - RWT BUILDING



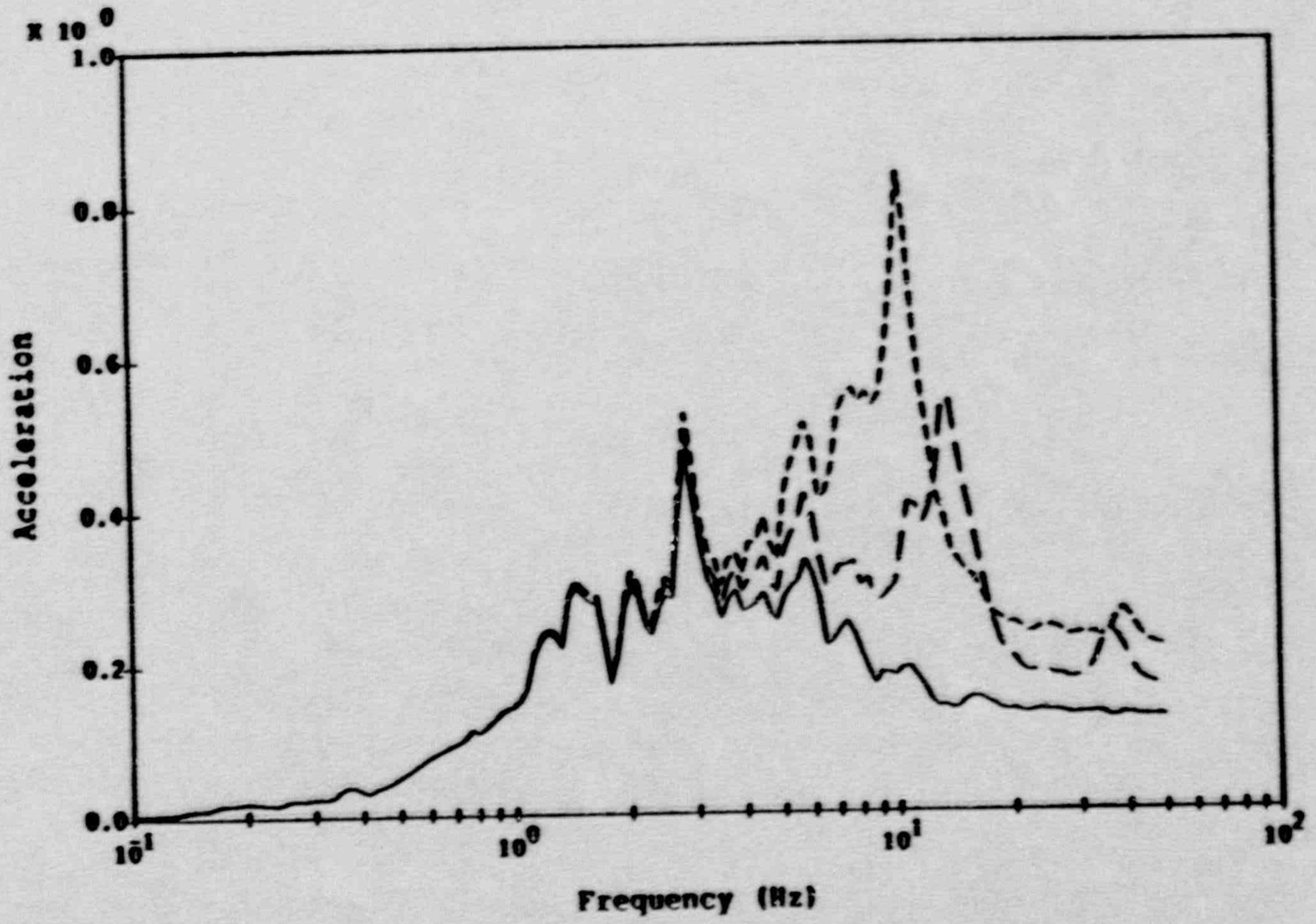
Legend:

Free Field	———————
el 150'-0"	
1.0*K	— — — — —
0.5634*K	- - - - - - - - -

Notes:

spectra calculated at 5% damping
accelerations in units of g's

DESIGN - TYPE SPECTRA, WITH AND WITHOUT STIFFNESS DEGRADATION, CRIB HOUSE



Legend:

Free Field ————

el 130'-6"
  1.0*K — — — — —
  0.5634*K - - - - - - - - -

Notes:

spectra calculated at 5% damping
accelerations in units of g's

CONCLUSIONS

- THE REDUCTION IN STIFFNESS CAN SIGNIFICANTLY AFFECT RISK, BUT EFFECT IS PLANT/BUILDING SPECIFIC

- THE COMBINED DETERMINISTIC AND PROBABILISTIC RISK RE-EVALUATIONS WILL PROVIDE:

  - EVALUATION OF DECREASE IN DESIGN MARGIN OF SAFETY DUE TO FREQUENCY REDUCTION

  - ESTIMATE OF POTENTIAL RISK IMPACT OF FREQUENCY REDUCTION

FUTURE WORK

- COMPLETE ANALYTICAL CUMULATIVE DAMAGE MODELING STUDIES BY FEBRUARY 1990
- APPLY FINAL STIFFNESS MODEL TO ROCK SITE PWR
- APPLY FINAL STIFFNESS MODEL TO SOIL SITE PWR (ZION) INCLUDING SELECTED PIPING SYSTEM MODELS

THE SEISMIC CATEGORY I STRUCTURES PROGRAM

1985 - 1989

CHARLES R. FARRAR

MEE-13 ENGINEERING MECHANICS   LOS ALAMOS

ISSUES

- This program was originally established as part of the NRC's Margins to Failure Program
- Initial program objective was to investigate the dynamic response of seismic Category I reinforced concrete structures subjected to seismic loads beyond their design basis

2

PROGRAM OBJECTIVES

- Address the seismic response of reinforced concrete Category I structures, other than containment
- Develop experimental data for determing the sensitivity of structural behavior in the elastic and inelastic ranges to variations in configuration, design practices, and earthquake loadings
- Provide experimental data to validate computer codes

MEE-13 ENGINEERING MECHANICS LOS ALAMOS

3

PROGRAM OBJECTIVES (cont.)

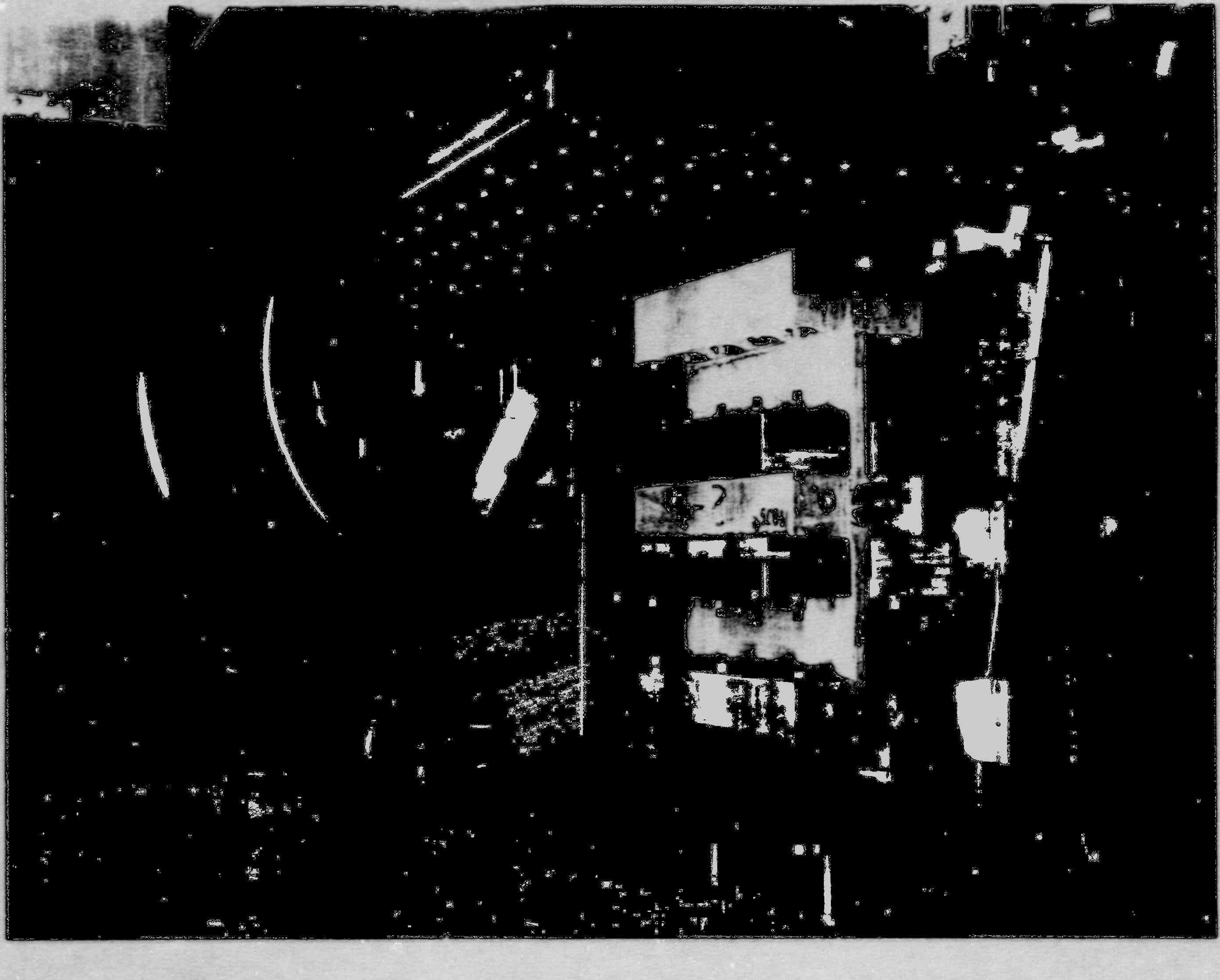
- Investigate changes in floor response spectra as the structure's response goes from the elastic to the inelastic region
- Develop a method for representing damping in the elastic and inelastic ranges of response
- Support plant risk studies being done at Sandia

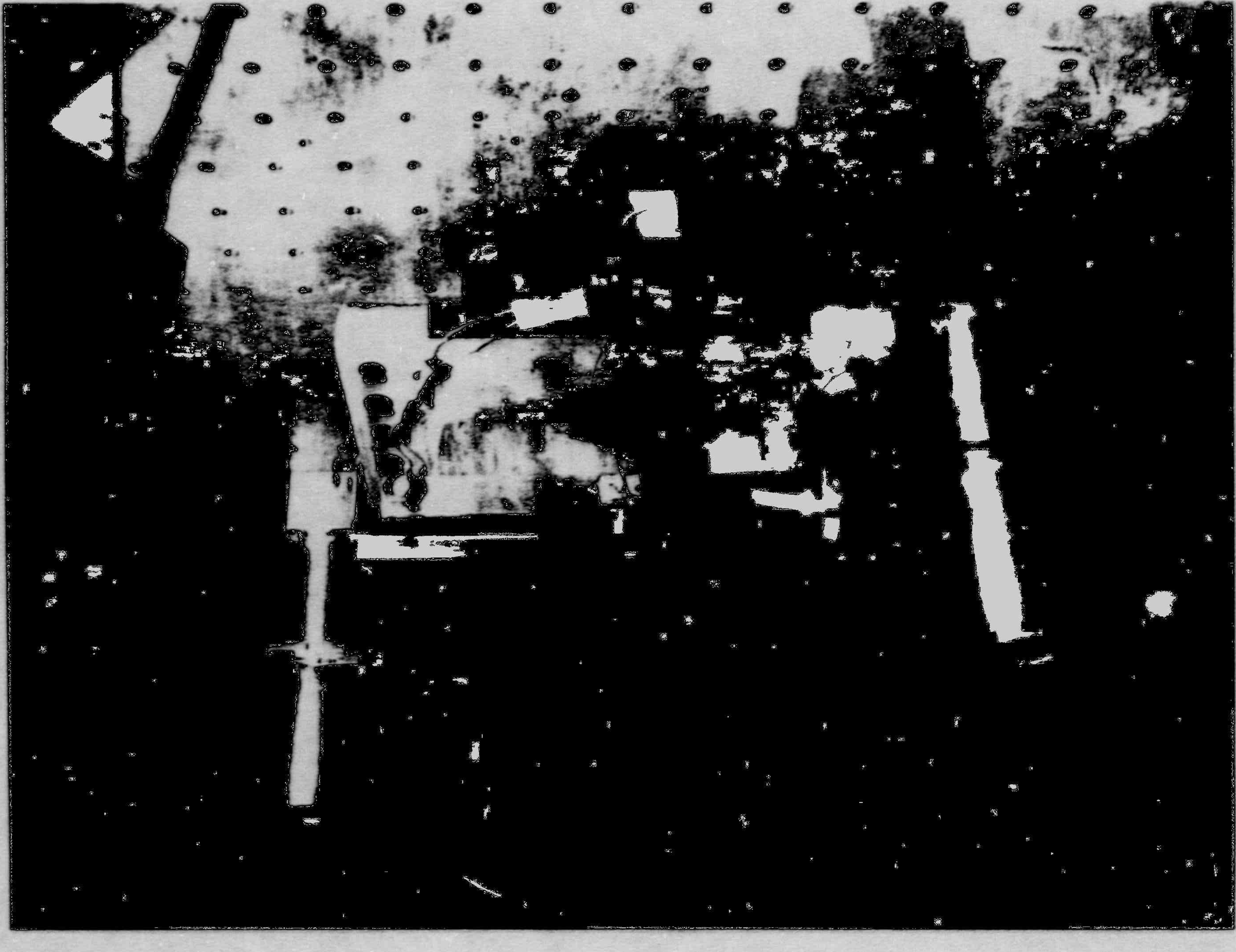
EARLY TESTING PROGRAM
FY 80-84

- Because of the size of prototype structures and because we were investigating the nonlinear response, scale model testing was employed

- Program began by testing 1/30-scale (1-in.-thick) isolated shear walls both statically and dynamically

- Next, scale model diesel generator buildings and auxiliary buildings (1- and 3-in.-thick walls) were subjected to simulated seismic inputs





VIEWGRAPH 8 1/30-SCALE DIESEL GENERATOR BUILDING MODEL

VIEWGRAPH 9 1/10-SCALE DIESEL GENERATOR BUILDING MODEL

VIEWGRAPH 10 1/14-SCALE AUXILIARY BUILDING MODEL

(PHOTOS NOT REPRODUCABLE)

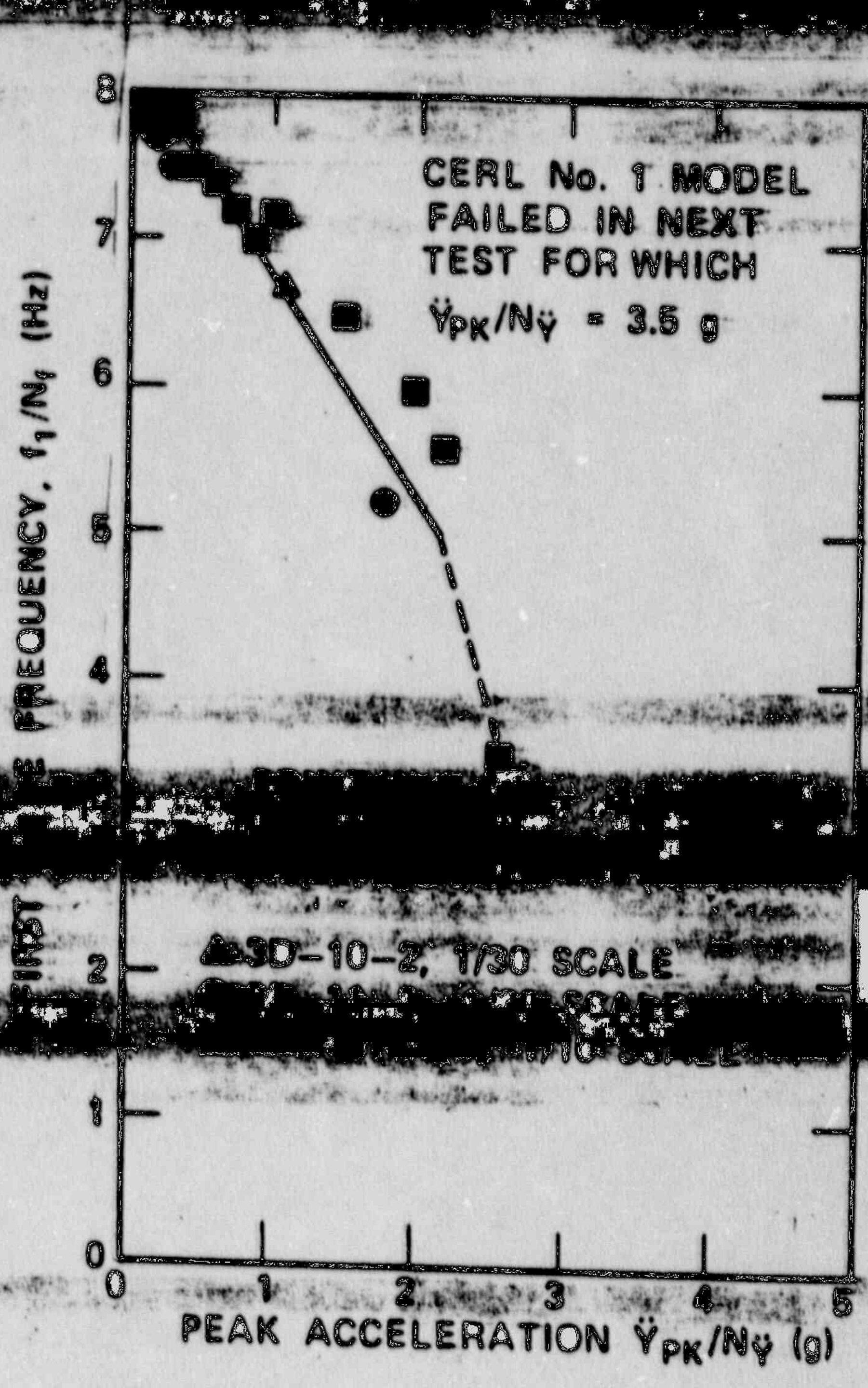
EARLY TEST RESULTS
(FY 80-84)

- From scaled test results, prototype structures are expected to withstand earthquakes in excess of 2 g's peak horizontal ground acceleration. This implies significant reserve margin

- Stiffness, measured directly in static tests and determined indirectly from frequency measurements in dynamic tests, were as much as a factor of 4 below values that industry would use in the design process

EARLY TEST RESULTS
(FY 80-84)
(CONT.)

- Scalability between different size MICROCONCRETE models was demonstrated in the elastic and inelastic response region



NOTES:

FOR 1/30 SCALE, N_f = 1/11.8, $N_{\ddot{y}}$ = 1/4.6

FOR 1/10 SCALE, N_f = 1/6.8, $N_{\ddot{y}}$ = 1/4.6

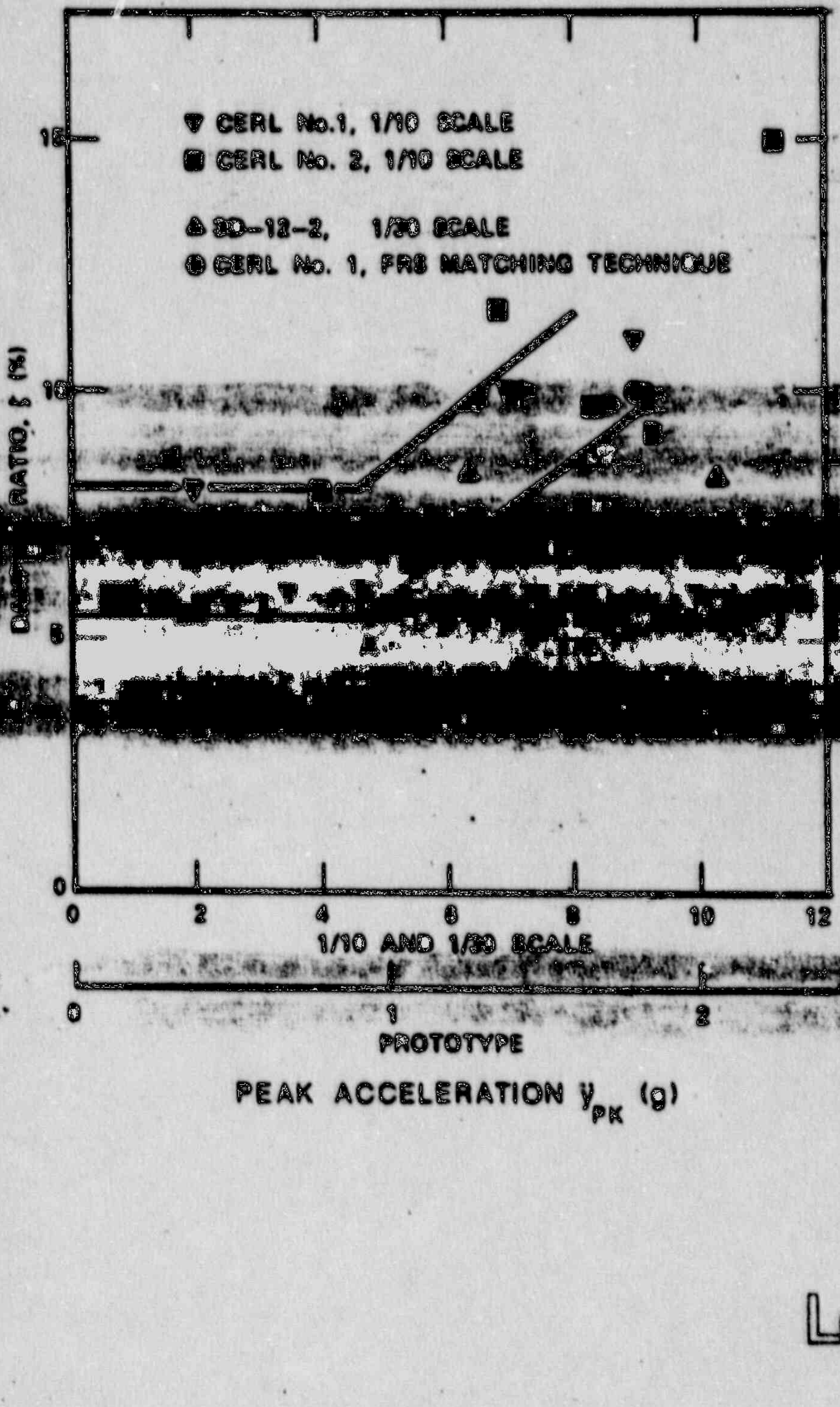
EXAMPLE:

<u>AT POINT 'A' CERL TEST No.1</u>

$f_{1_{PROT.}}$ = 24 x 1/6.8 =3.5 Hz

$\ddot{Y}_{PK_{PROT.}}$ = 12 x 1/4.6 = 2.6 g

15


▼ CERL No.1, 1/10 SCALE
■ CERL No. 2, 1/10 SCALE
▲ SD-12-2, 1/30 SCALE
● CERL No. 1, FRS MATCHING TECHNIQUE
DAMPING RATIO, ζ (%)
0
5
10
15
0
2
4
6
8
10
12
1/10 AND 1/30 SCALE
0
1
2
PROTOTYPE
PEAK ACCELERATION $\ddot{y}_{PK}$ (g)

Los Alamos

PROGRAM STATUS AT THE END OF FY 84

- 23 microconcrete scale model structures had been tested statically or dynamically
- Technical Review Group was most concerned with the reduced stiffness values measured at low load levels (less that 50 psi nominal base shear stress)
- Test results showed that the structures have significant reserve margin despite the reduced stiffness

MEE-13 ENGINEERING MECHANICS LOS ALAMOS

TRG Members

• Dr. Wilfred Baker	Wilfred Baker Engineering
• Dr. Ken Buchert	Southern Illinois University
• Mr. Don Denton	Tennessee Valley Authority
• Dr. Robert Kennedy	RPK Consulting
• Prof. Mete Sozen	University of Illinois
• Dr. John Stevenson	Stevenson and Associates

CONCERNS OF THE TECHNICAL REVIEW GROUP

- At this point in the program the TRG focused its concerns on the Reduced Stiffnesss issue
- Cat I structure design is based on an uncracked cross-section analysis, stiffness reductions of 4 are not accounted for

CONCERNS OF THE TECHNICAL REVIEW GROUP (cont.)

- Plant equipment could have been designed to the inappropriate response spectra

- Reduced stiffness would, in general, shift the resonant frequency of the structure into the frequency range where an earthquake has its peak energy

POSSIBLE SOURCES OF REDUCED STIFFNESS

- Does microconcrete respond in a different manner than conventional concrete? (no tests were done on conventional concrete structures)
- Were the structures damaged prior to testing (shipping or curing) ?
- What were the actual boundary conditions during the tests?

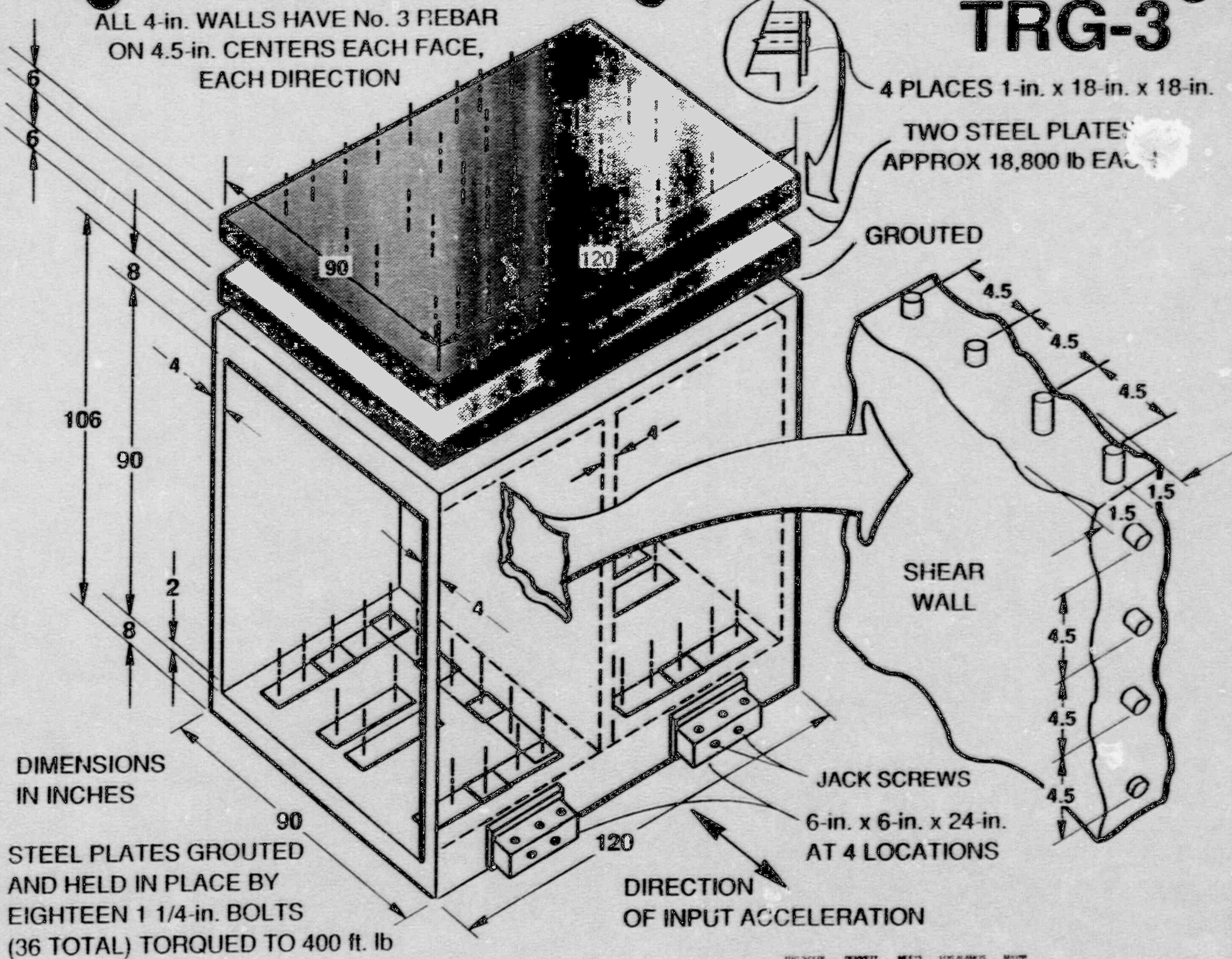
CURRENT PROGRAM EMPHASIS

- TRG and NRC feel a need to resolve the "Reduced Stiffness" issue

- TRG proposes an "ideal" test structure geometry to investigate the Reduced Stiffness issue

- ASCE Dynamic Analysis Subcommittee of the Nuclear Structures and Materials Committee forms a working group to investigate the Reduced Stiffness issue

TRG-3



ALL 4-in. WALLS HAVE No. 3 REBAR ON 4.5-in. CENTERS EACH FACE, EACH DIRECTION

4 PLACES 1-in. x 18-in. x 18-in.

TWO STEEL PLATES APPROX 18,800 lb EACH

DIMENSIONS IN INCHES

STEEL PLATES GROUTED AND HELD IN PLACE BY EIGHTEEN 1 1/4-in. BOLTS (36 TOTAL) TORQUED TO 400 ft. lb

6-in. x 6-in. x 24-in. AT 4 LOCATIONS

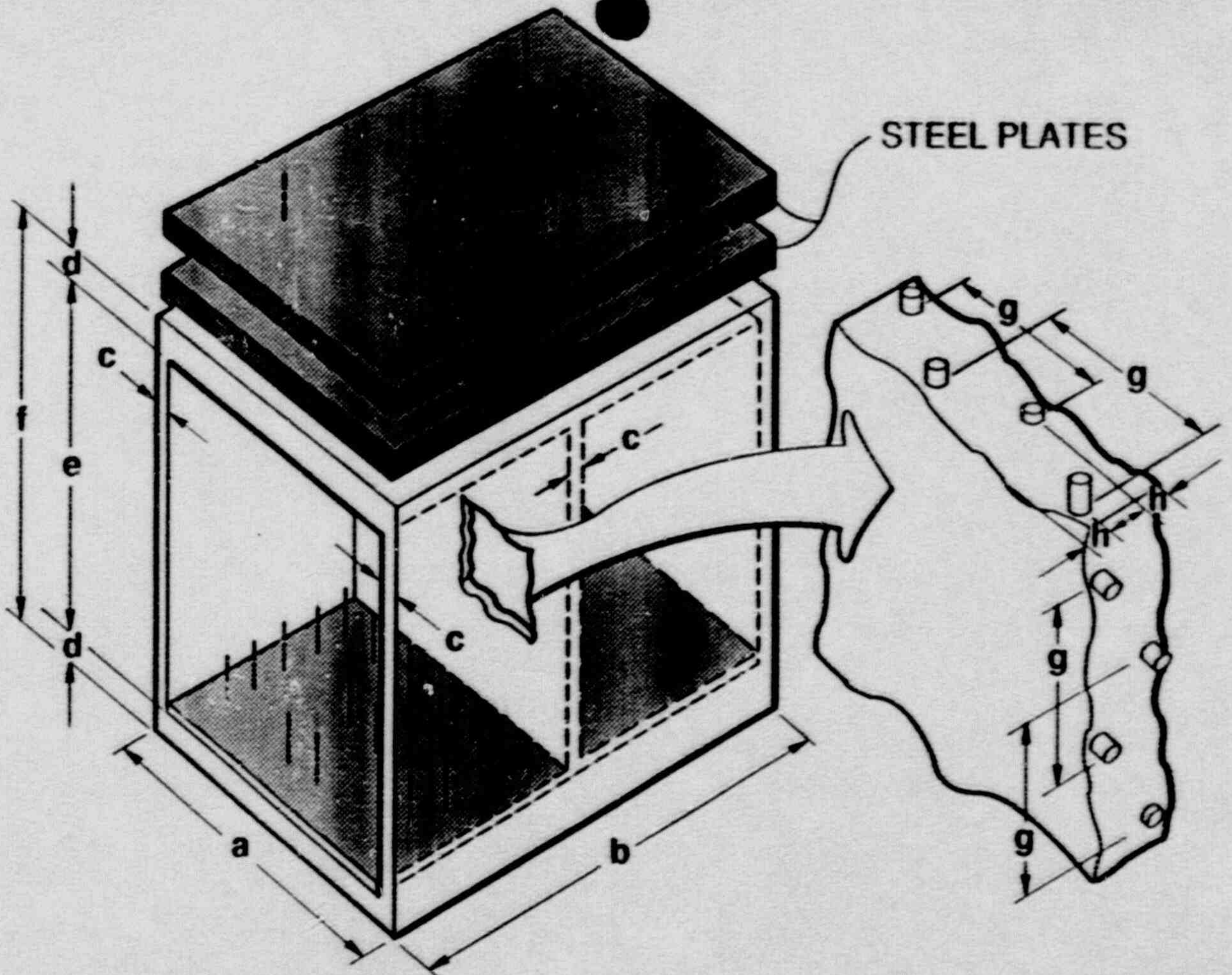
DIRECTION OF INPUT ACCELERATION

TRG TEST SEQUENCE

- 15 structures (conventional and microconcrete) were tested statically and dynamically (shake table and experimantal modal analysis)

- Results were primarily used to address the Reduced Stiffness issue

- Results are also being used to :
  1) address the scalability of static and dynamic responses of microconcrete structures, to conventional concrete structures
  2) address cumulative damage affects
  3) compare static response to dynamic response



STRUCTURE	DIMENSIONS (in.)								ADDED WEIGHT (lbs)	REBAR diam (in.)	AGGREGATE SIZE (in.)
	a	b	c	d	e	f	g	h			
TRG 1, 2	30	40	1	2	30	34	0.25*	0.5*	575	0.042	micro
TRG 3, 5	90	120	4	8	90	106	4.5*	1.5*	37,000	0.375	0.75
TRG 4	90	120	6	8	90	106	14.5	1	37,000	0.375	0.75
TRG 6	90	120	6	8	24	40	7.25	1	37,000	0.375	0.75
TRG 7 - 13	30	40	2	2.67	30	35.3	6	0.5	1350	0.14	0.375
TRG 14 - 16	30	40	2	2.67	30	35.3	6	0.5	1350	0.14	micro

*ONE LAYER OF REINFORCEMENT DOWN THE CENTER OF THE WALL IN BOTH THE HORIZONTAL AND VERTICAL DIRECTION

TESTS ON TRG-1 AND -3

(TRG-1 WAS A 1/4-SCALE MODEL OF TRG-3)

- **PURPOSE:** Determine if a conventional concrete shear wall will exhibit reduced stiffness, and demonstate scalability between microconcrete and conventional concrete

- **TESTS:** Experimental modal analysis, static monotonic loading, and simulated seismic excitation on a shake-table

VIEWGRAPH 25 TRG-3 STRUCTURE ON A SHAKE TABLE

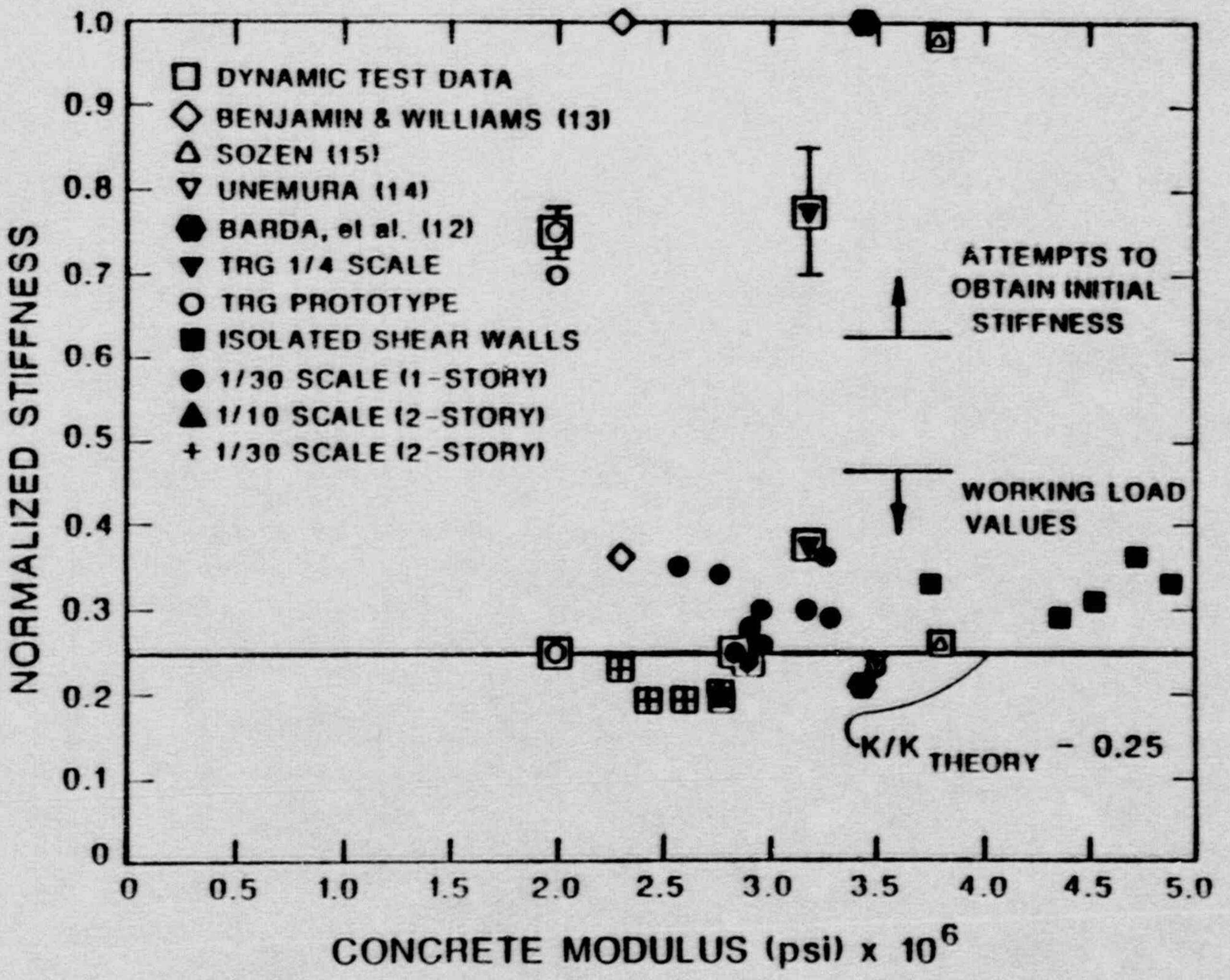
(PHOTOS NOT REPRODUCABLE)

RESULTS FROM TRG-1 AND -3

- The conventional concrete structure showed more stiffness reduction than the microconcrete model when subjected to simulated seismic base excitation at comparable levels

- Scalability could only be demonstated during the low-level static testing and the experimental modal analyses

- Question: Were the structures damaged during the transportation to the seismic test facilities?


NORMALIZED STIFFNESS
1.0
0.9
0.8
0.7
0.6
0.5
0.4
0.3
0.2
0.1
0
0
0.5
1.0
1.5
2.0
2.5
3.0
3.5
4.0
4.5
5.0
CONCRETE MODULUS (psi) x 10^6
□ DYNAMIC TEST DATA
◇ BENJAMIN & WILLIAMS (13)
△ SOZEN (15)
▽ UNEMURA (14)
⬢ BARDA, et al. (12)
▼ TRG 1/4 SCALE
○ TRG PROTOTYPE
■ ISOLATED SHEAR WALLS
● 1/30 SCALE (1-STORY)
▲ 1/10 SCALE (2-STORY)
+ 1/30 SCALE (2-STORY)
ATTEMPTS TO OBTAIN INITIAL STIFFNESS
WORKING LOAD VALUES
K/K_{THEORY} - 0.25

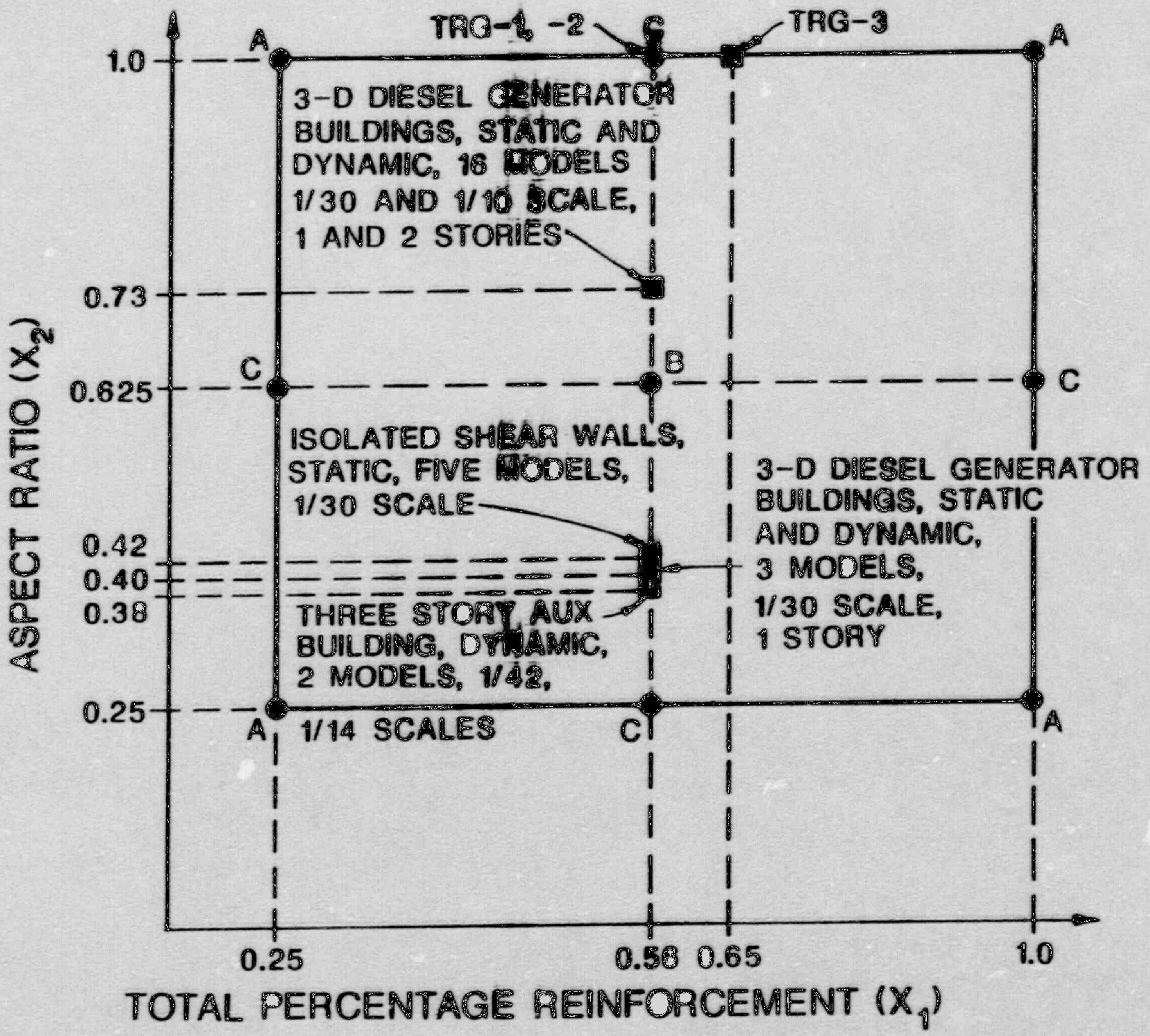
27

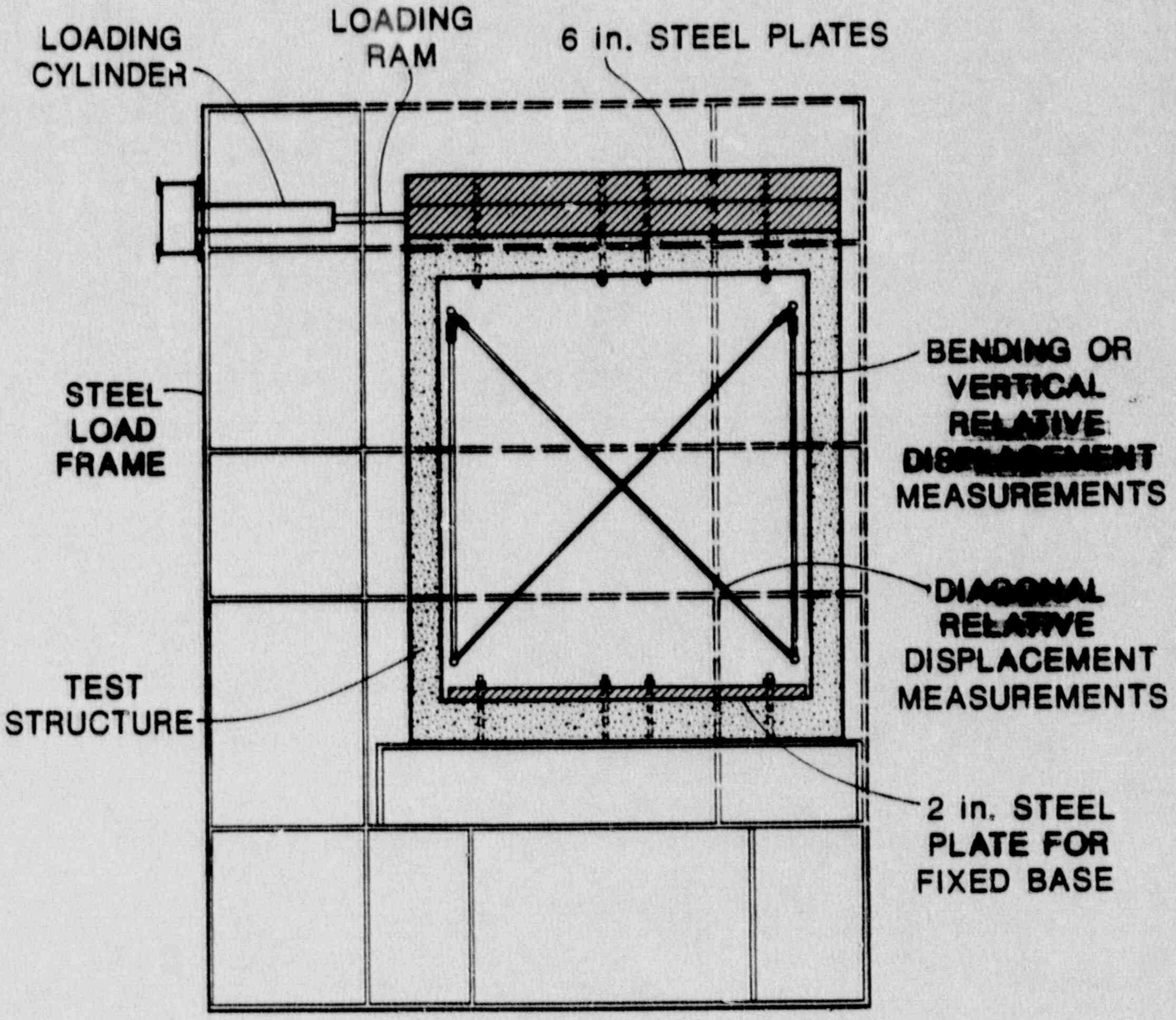
TESTS ON TRG-4,-5,-6

- **PURPOSE:** Determine if carefully constructed and handled conventional concrete structures will demonstrate theoretical stifness during well instumented static cyclic tests. These tests were initially part of a series to examine reduced stiffness as a function of aspect ratio and percent reiforcement.

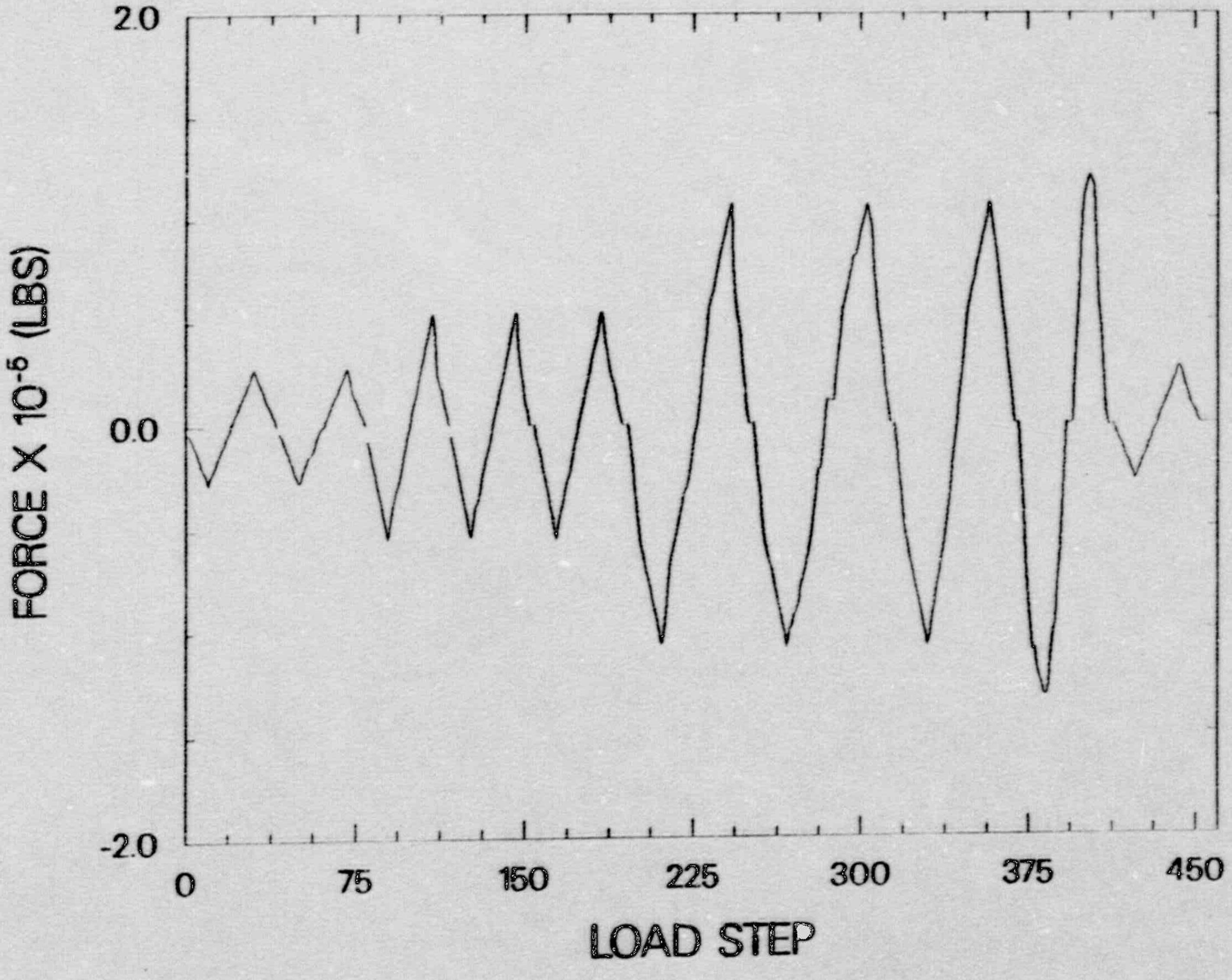
- **TESTS:** Experimental modal analysis, static cyclic loading to failure

24


TRG-1, -2
TRG-3
A
C
A
3-D DIESEL GENERATOR BUILDINGS, STATIC AND DYNAMIC, 16 MODELS 1/30 AND 1/10 SCALE, 1 AND 2 STORIES
0.73
C
B
C
ISOLATED SHEAR WALLS, STATIC, FIVE MODELS, 1/30 SCALE
3-D DIESEL GENERATOR BUILDINGS, STATIC AND DYNAMIC, 3 MODELS, 1/30 SCALE, 1 STORY
0.42
0.40
0.38
THREE STORY AUX BUILDING, DYNAMIC, 2 MODELS, 1/42, 1/14 SCALES
0.25
A
C
A
1.0
0.625
ASPECT RATIO (X_2)
0.25
0.56
0.65
1.0
TOTAL PERCENTAGE REINFORCEMENT (X_1)

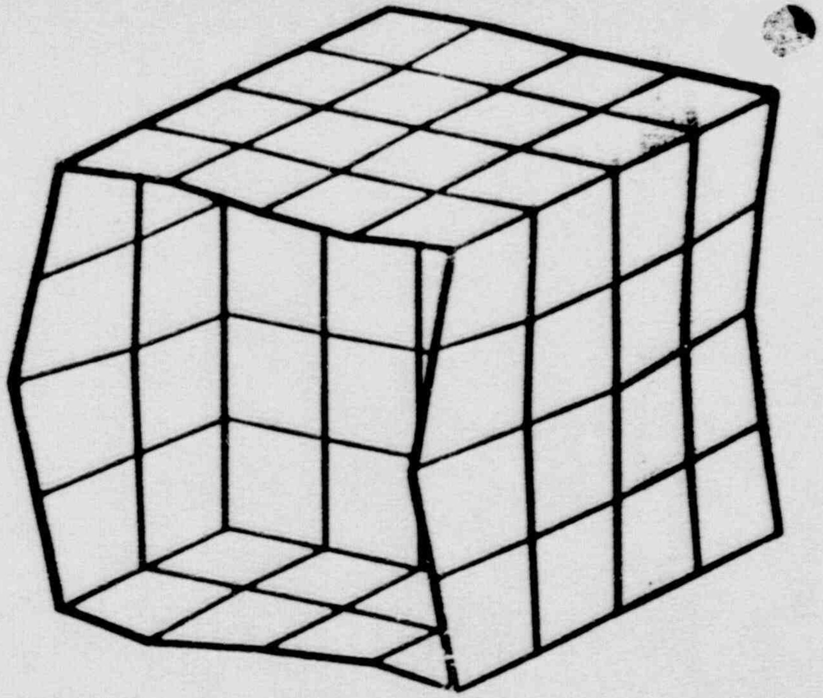

LOADING CYLINDER
LOADING RAM
6 in. STEEL PLATES
STEEL LOAD FRAME
BENDING OR VERTICAL RELATIVE DISPLACEMENT MEASUREMENTS
DIAGONAL RELATIVE DISPLACEMENT MEASUREMENTS
TEST STRUCTURE
2 in. STEEL PLATE FOR FIXED BASE

THE LOAD VS. LOAD STEP HISTORY

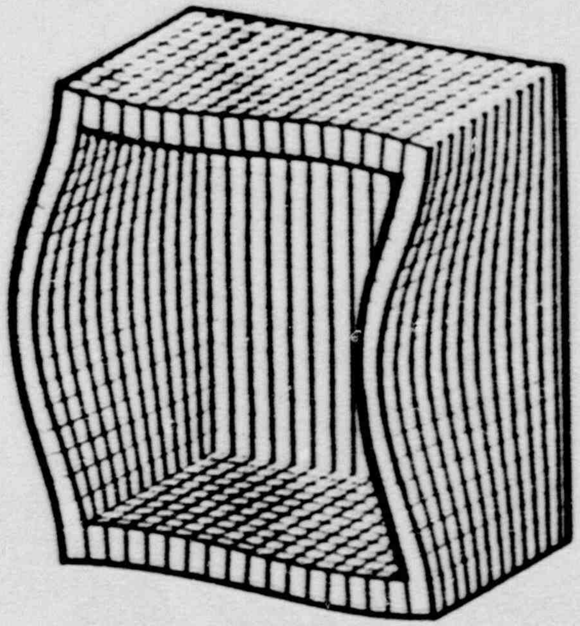


RESULTS FROM TRG-4,-5,-6

- Experimental modal analysis results agreed almost exactly with finite element modal analysis results
- Total stiffness as well as the shear and bending components of stiffness agreed with S.O.M. theory until the first structural cracks appeared
- These results contradict previous findings in this test program



MODE 5, 111 Hz
EXPERIMENTALLY DETERMINED



MODE 5, 111 Hz
DETERMINED FROM FINITE ELEMENT ANALYSIS

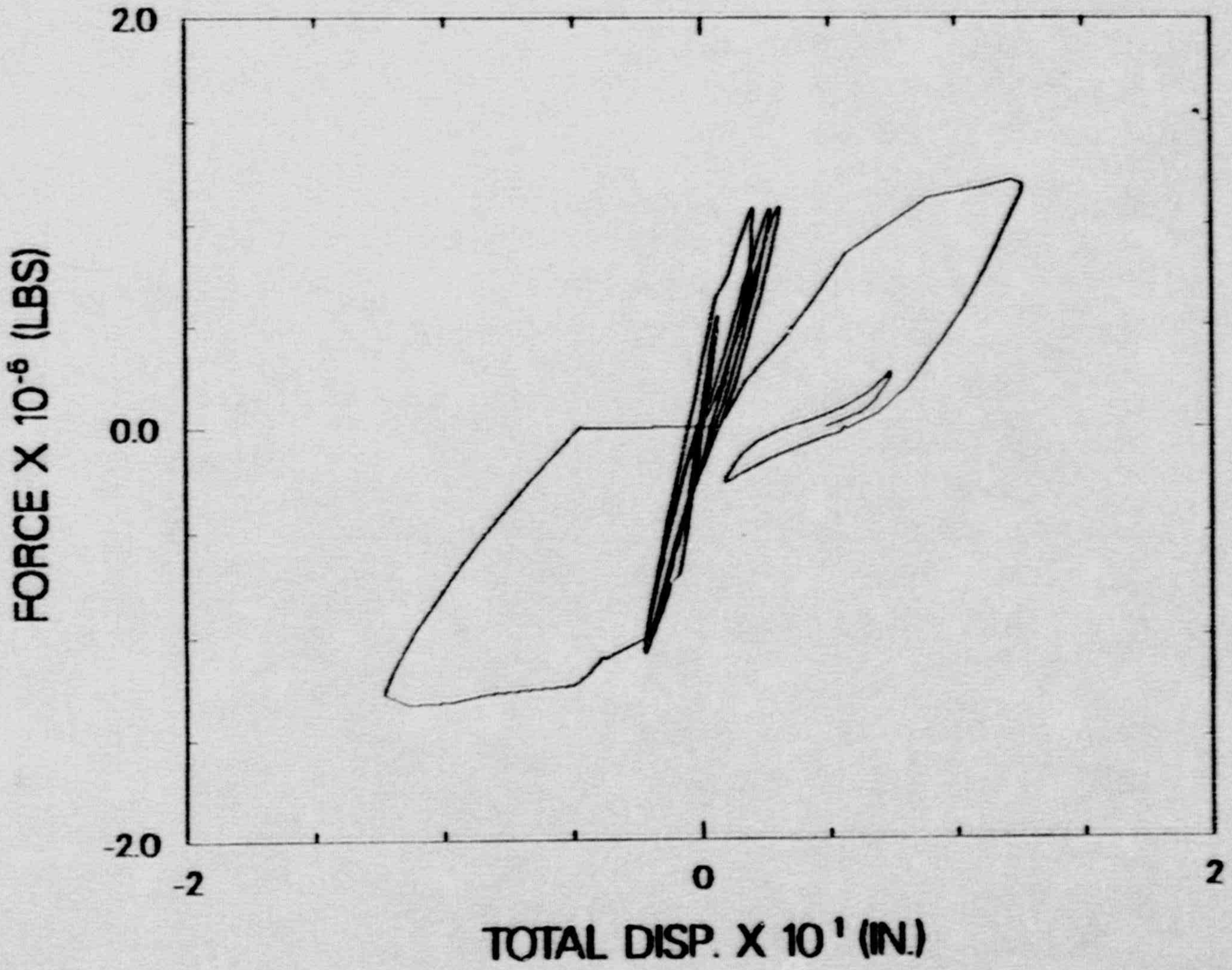
Resonant Frequencies From Experimental Modal Analysis Before and After Cracking Compared With Resonant Frequencies From Finite Element Analysis of The Shear Wall

Mode	Experimental Before Cracking (Hz)	Finite Element Analysis (Hz)	Experimental After Cracking (Hz)
1	37.1	36.3	28.2
2	79.2	77.8	*
3	88.3	86.0	*
4	100.	102.	*
5	111.	111.	82.0
6	122.	120.	*

---

* Not identified.

ENTIRE LOAD CYCLE HISTORY - TRG4

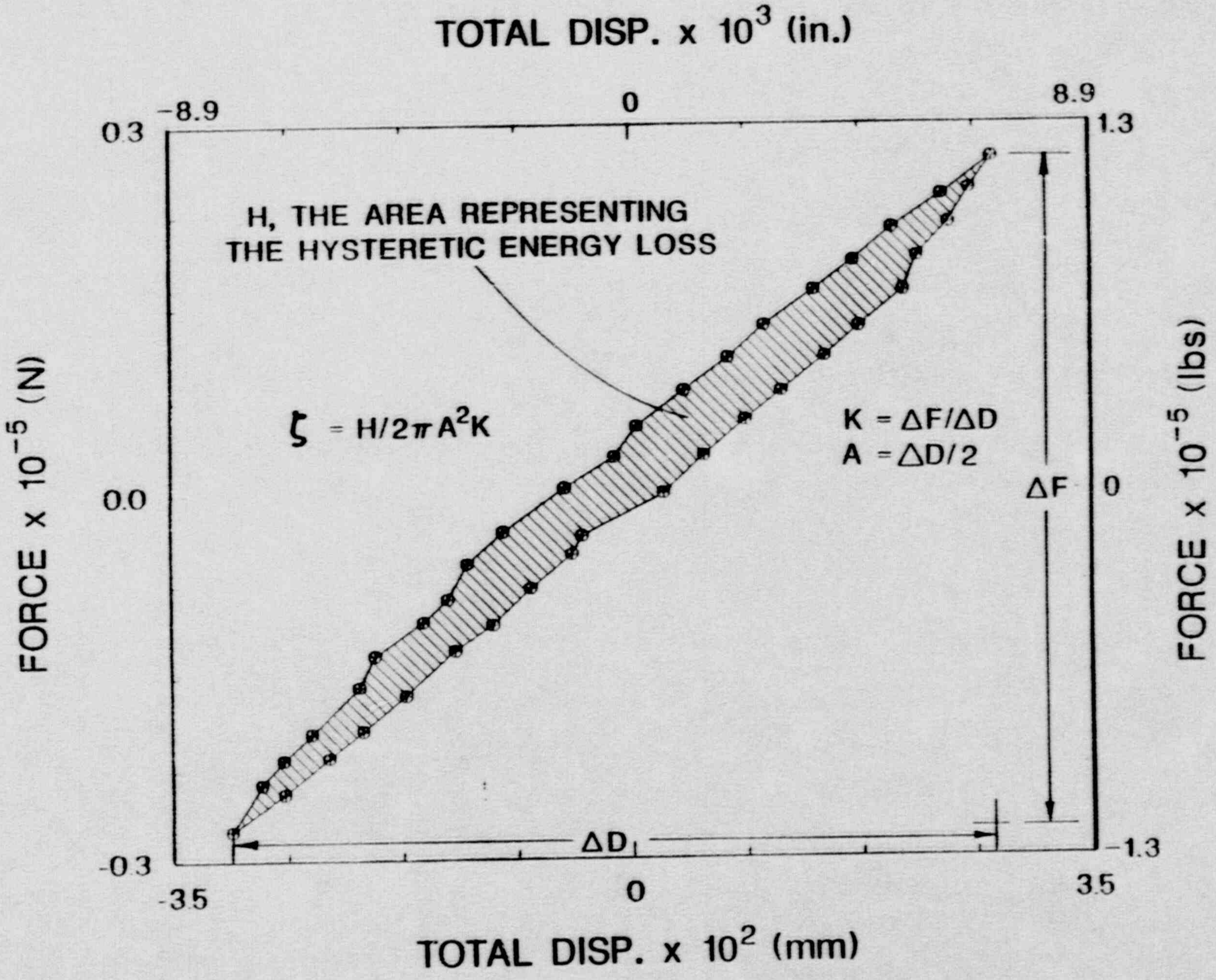


TRG-4 STIFFNESS COMPARISON

- **S.O.M. THEORY:**
  - BENDING $=50.6\times10^{6}$ LB/IN
  - SHEAR $=10.1\times10^{6}$ LB/IN
  - TOTAL $=8.42\times10^{6}$ LB/IN
- **MEASURED:**
  - BENDING $=52.6\times10^{6}$ LB/IN
  - SHEAR $=10.2\times10^{6}$ LB/IN
  - TOTAL $=8.50\times10^{6}$ LB/IN

TRG-4 RESULTS COMPARED WITH TRG-3 RESULTS

TRG-4 (static)	Stiffness was approx. 100% of S.O.M. theory until first cracking that occured at 130 psi NBSS and 171 psi MNTS
TRG-3 (seismic)	Stiffness was 25% of S.O.M. theory during the first seismic pulse that produced a NBSS of 91 psi and a MNTS of 92 psi

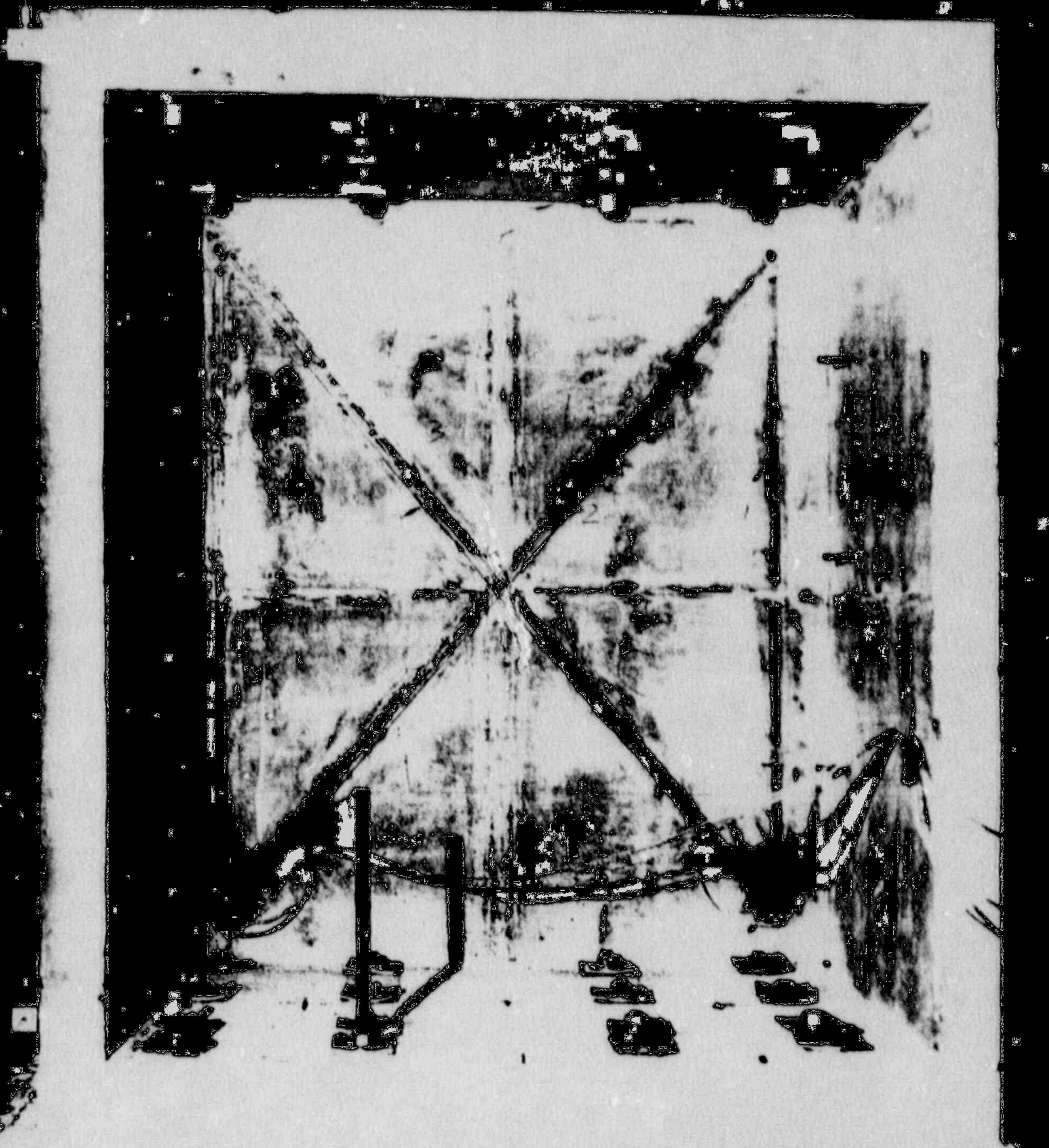

TOTAL DISP. x 10^3 (in.)
-8.9
0
8.9
0.3
1.3
H, THE AREA REPRESENTING THE HYSTERETIC ENERGY LOSS
ζ = H/2πA²K
K = ΔF/ΔD
A = ΔD/2
ΔF
0.0
0
FORCE x 10^-5 (N)
FORCE x 10^-5 (lbs)
ΔD
-0.3
-1.3
-3.5
0
3.5
TOTAL DISP. x 10^2 (mm)

TESTS ON TRG-7 THROUGH -16

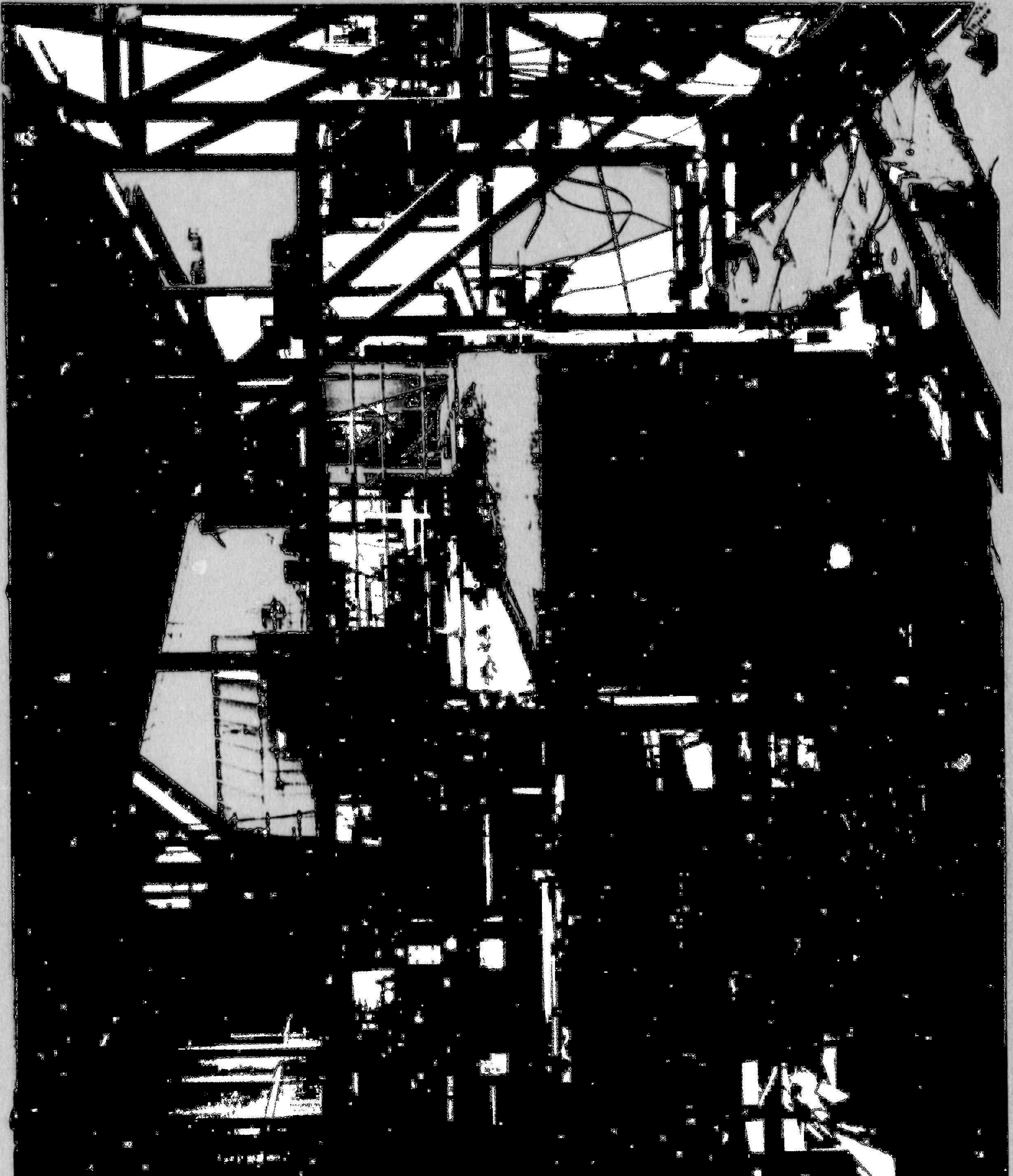
(These structures were 1/3-scale models of TRG-4)

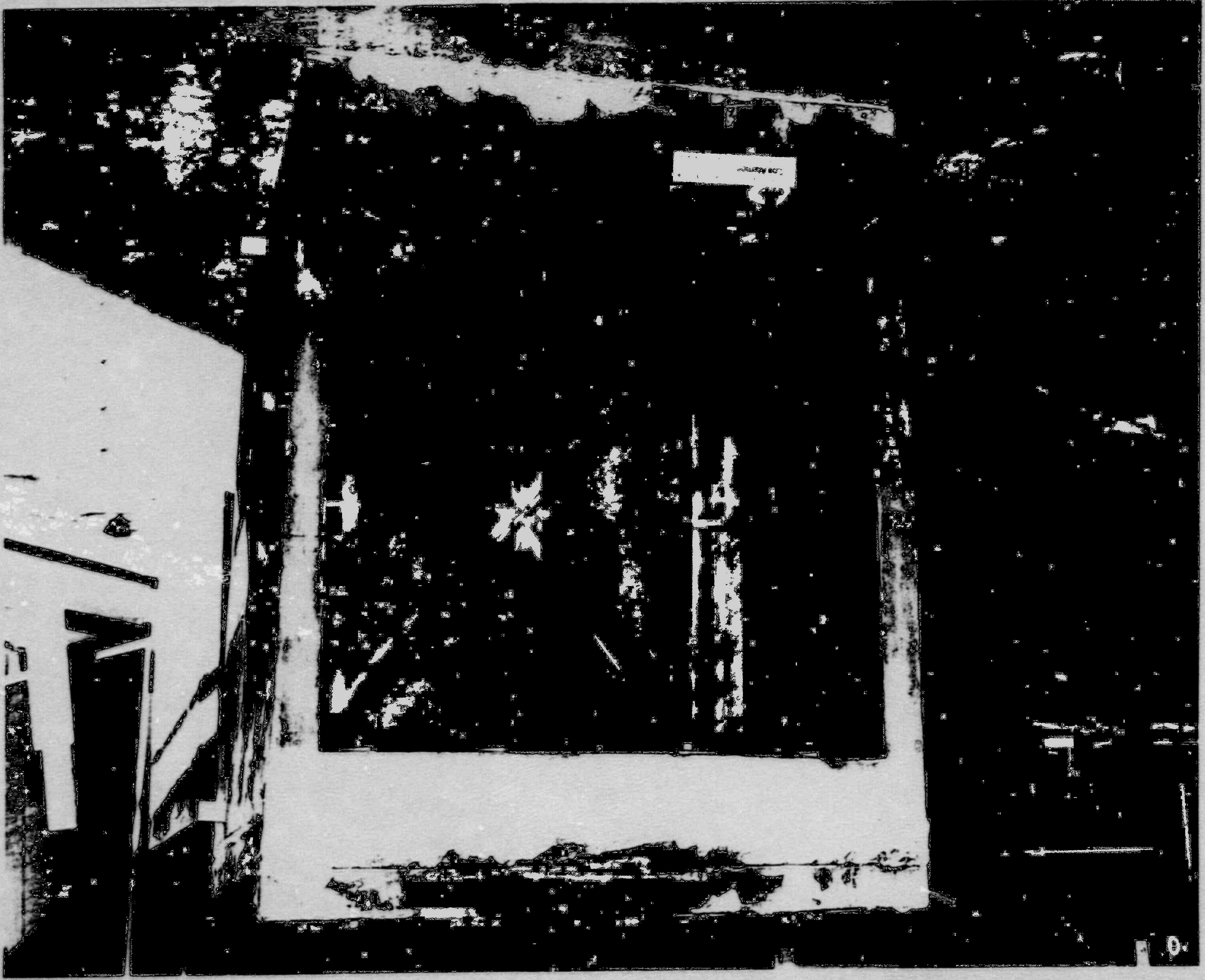
- **PURPOSE:** provide information on cumulative damage effects, further address the scalability issues, measure stiffness in a more direct manner during dynamic tests

- **TESTS:** Experimental modal analysis, static cyclic tests, simulated seismic excitation on a shake-table



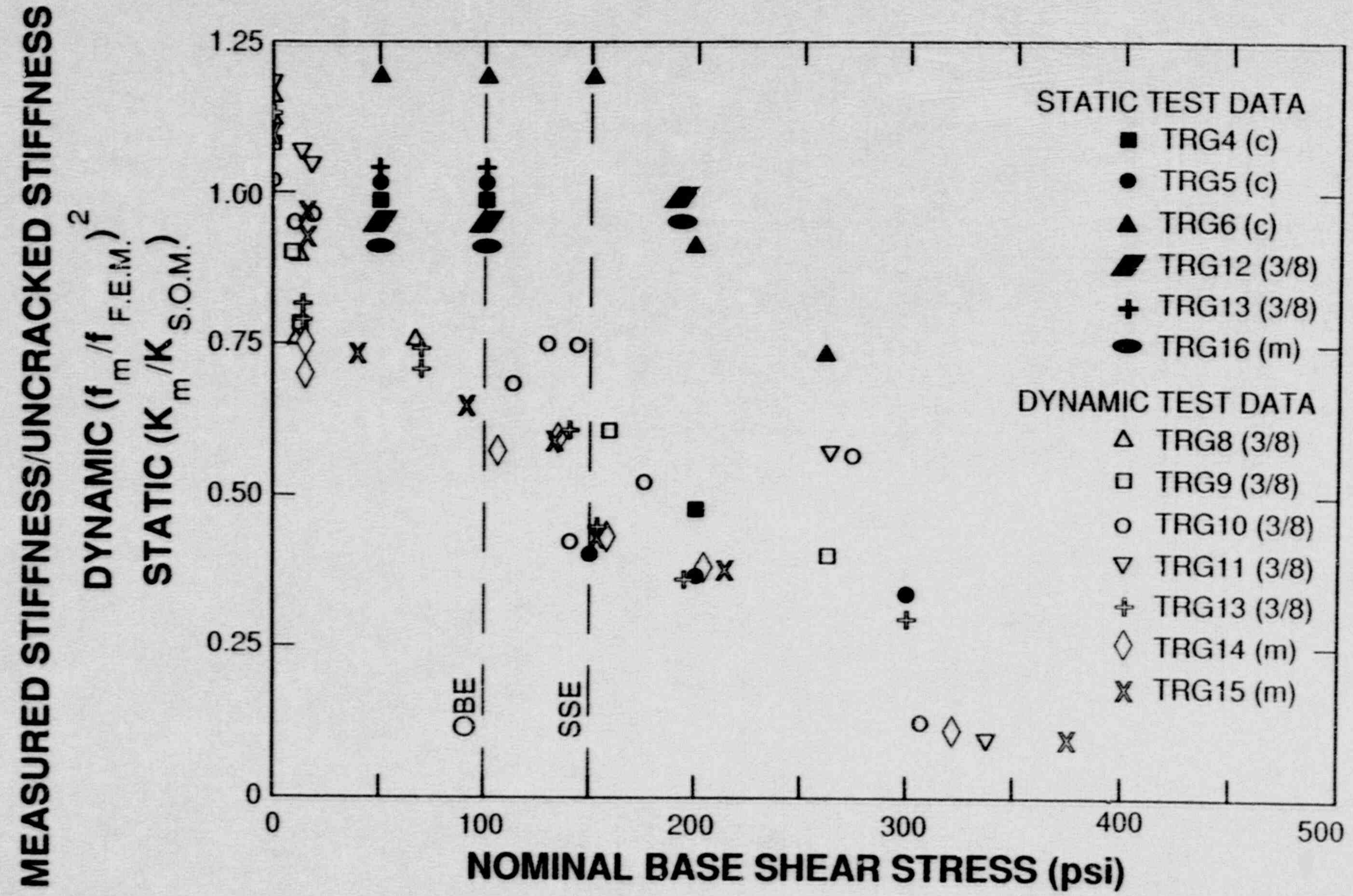

TRG 7



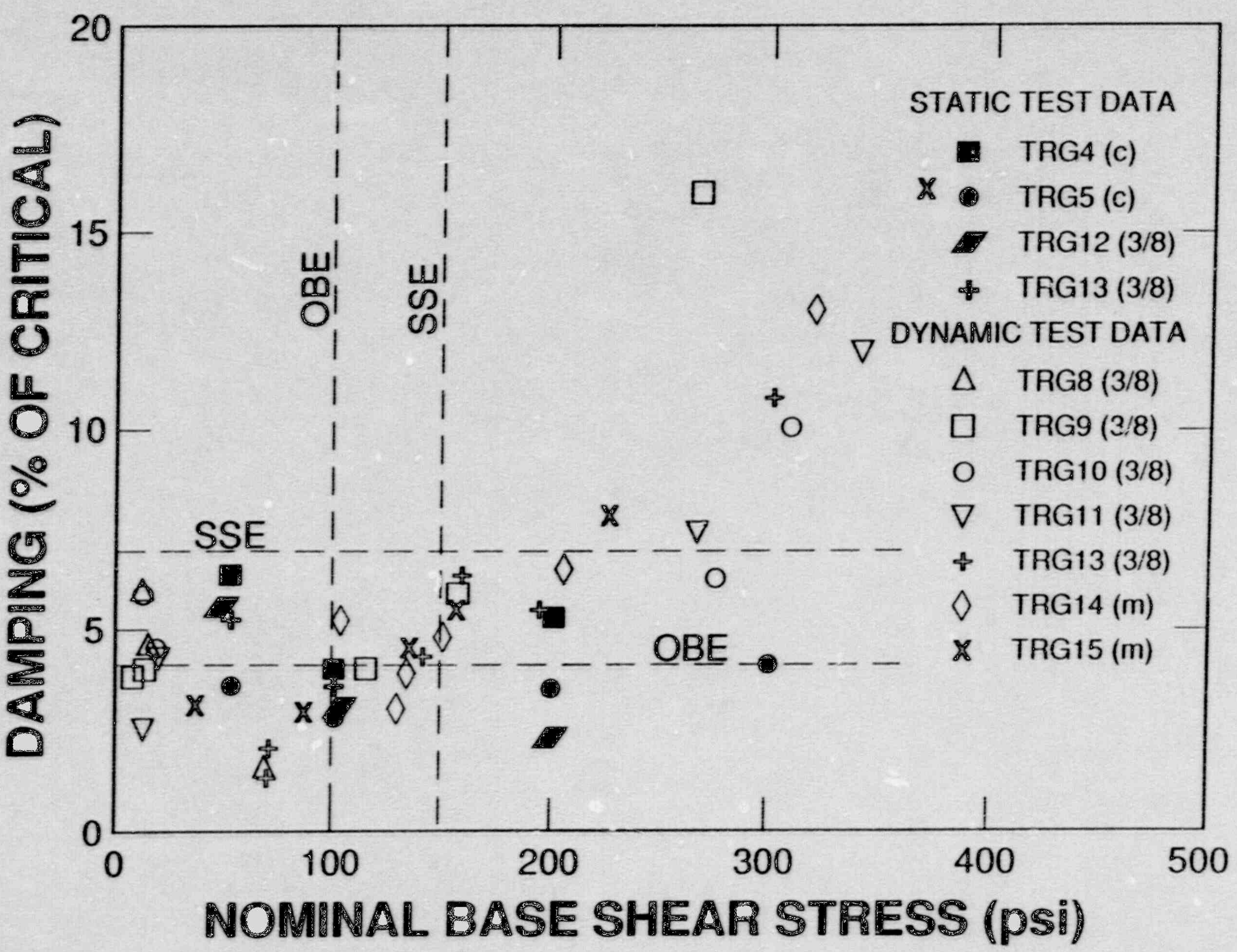


DYNAMIC TEST SEQUENCE

	EXCITATION LEVEL (CODE ULTIMATE STRENGTH)				
MODEL	25%	50%	75%	100%	150%
1	1(2.1G)	2	3	4	?
2		5(4.2G)	6	7	?
3			8(6.3G)	9	?
4				10(8.4G)	?



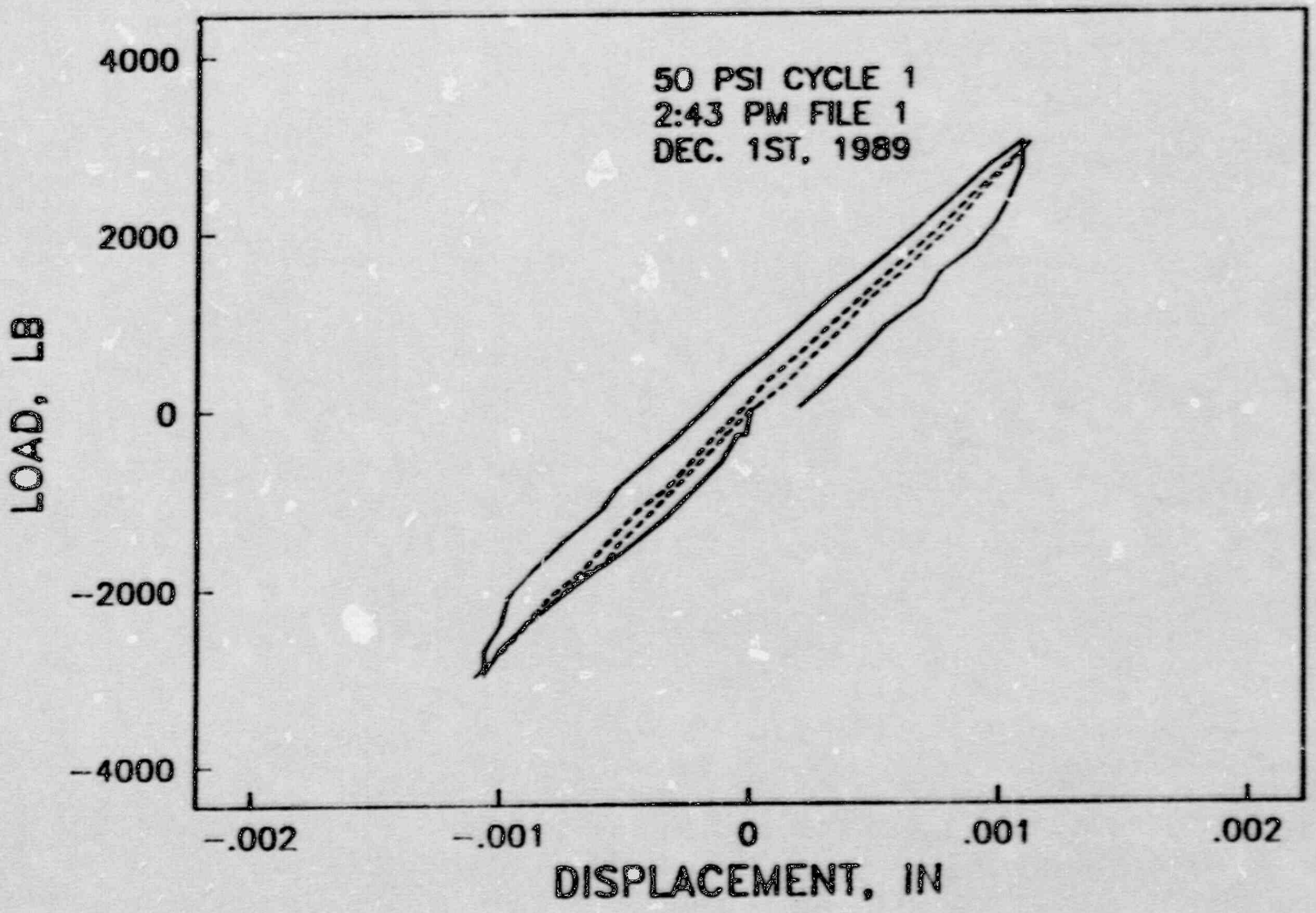
TRG DATA (rev) - FARRAR - MEE13 - LOS ALAMOS - JUN89(rev 0190)



DAMPING vs NOMINAL BASE SHEAR STRESS - FARRAR - MEE13 - LOS ALAMOS - JUL89 (rev. 0190)

9

TRG 16
TOTAL DEF. STRG AND DISP



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TRG TEST SEQUENCE RESULTS

- Reduced stiffness does not appear to be as large as initially thought. Reductions in stiffness of 4 (from theory) were probably related to damage prior to testing and boundary conditions. Currently, it appears that stiffness reductions at OBE levels will be 70% of theory at worst.

- Scalability of microconcrete response to conventional concrete response was demonstrated in the elastic range

- No cumulative damage effects were noted

WHERE WE STAND WITH THE SIMILITUDE ISSUE

- EXPERIMENTAL MODAL ANALYSIS

Similitude has been demonstrated for the dynamic properties (resonant frequencies, mode shapes, modal damping)

micro to 3/8-in aggregate S.F.=1

micro, 3/8-in to conventional concrete S.F.=3

Scaling Of Experimental Modal Analysis Results

Mode	Measured on TRG-4	Predicted From TRG-7 Results
1	37.1 Hz	107 R*/3 = 35.1 Hz
2	79.2 Hz	230 R/3 = 75.4 Hz
3	88.3 Hz	258 R/3 = 84.6 Hz
4	100.0 Hz	310 R/3 = 102 Hz
5	111.0 Hz	337 R/3 = 110 Hz

* R = sqrt (E_c TRG 4 / E_c TRG 7)

SIMILITUDE (CONT.)

- STATIC, CYCLIC TESTING

Similitude has been demonstrated in the linear response region, failure mechanism in structures with conventional rebar is different than models with wire mesh because of different rebar ductilities

micro to 3/8-in aggregate S.F.=1
(entire load history)

micro, 3/8-in to conventional concrete S.F.=3
(elastic response only)

SIMILITUDE (CONT.)

- **SEISMIC EXCITATION**

At this point there is no data providing a direct comparison between small scale (micro or 3/8-in aggregate) structural response and a conventional concrete prototype

Similitude has been demonstrated between microconcrete and 3/8-in concrete structures of the same size (S.F.=1)

ASCE COMMITTEE ACTIVITIES

- Currently we are involved with two ASCE committee working goups. Both groups are part of the Dynamic Analysis subcommittee of the Nuclear Structures and Materials Committee (Structural Division)

  1.) Shear wall stiffness working group

  2.) Stuctural capacity and failure working group

- Interaction with these working goups provides additional peer review

ASCE COMMITTEE ACTIVITIES (CONT.)

- These committees also provide a means to disseminate data developed under NRC sponsored research programs to the technical community

SHEAR WALL STIFFNESS WORKING GROUP

- Currently, the group is in the process of completing a position paper on how to compute shear wall stiffness

- The working goups position is that at nominal stress levels below 100 psi the NRC's response spectra broadening (+ or - 15% in frequency) will account for reduced stiffness. Above 100 psi designs should examine two stiffness values: 100% of theory and 50% of theory.

STRUCTURAL CAPACITY AND FAILURE WORKING GROUP

- Provide a summary of available experimental and experience data on shear walls as well as other nuclear power plant structural components
- Show how this information is used in PRA and margins studies
- Identify areas where more experimental data is needed

PROGRAM CONCLUSIONS

- NRC has initiated a program to investigate the plant risk significance of reduced stiffness

- This program will conclude by issuing special topical reports on the particular program objectives (stiffness, damping, floor response spectra, etc.) and a final summary report